European University Institute
DEPARTMENT OF ECONOMICS
EUI WORKING PAPERS
ECO 2014/10
Department of Economics

THE INVESTMENT EFFECT OF FISCAL CONSOLIDATION

Silvia Albrizio and Stefan Lamp

European University Institute
Department of Economics

The Investment Effect of Fiscal Consolidation

Silvia Albrizio and Stefan Lamp

EUI Working Paper **ECO** 2014/10

ISSN 1725-6704

Printed in Italy
European University Institute
Badia Fiesolana
I – 50014 San Domenico di Fiesole (FI)
Italy
www.eui.eu
cadmus.eui.eu

The investment effect of fiscal consolidation. *

Silvia Albrizio
OECD
European University Institute †‡

Stefan Lamp
European University Institute
§

September 2014

Abstract

This paper investigates the relationship between fiscal consolidation, business plans and firm investment. Based on a detailed narrative of tax changes in Germany covering 40 years of fiscal adjustments, we define and exploit the exogenous variation of tax bills to quantify the effect of tax changes on firm future investment plans as well as on realized investment. We find that firms in the manufacturing sector revise downwards their planned investment by about 4% subsequently to a tax increase equal to 1% of the value added in the total manufacturing industry. On the contrary realized investment growth drops by around 8% at impact. Furthermore we find that income and consumption taxes are most harmful to investment and that firms base their investment plans considering laws currently under discussion, anticipating future tax changes. Not taking into account this anticipation effect would lead to strongly biased estimates.

JEL classification: E22, E62, H32
Keywords: Firm investment, fiscal shocks, narrative identification, business confidence

*The authors would like to thank Jerome Adda, Evi Pappa, Fabiano Schivardi, Antonia Diaz, Antonello d'Agostino, Marek Razcko, and Tommaso Oliviero for useful comments and discussion. Furthermore we would like to thank the workshop participants at the EUI Applied Macroeconometrics working group, the Banca d'Italia Workshop on Fiscal Policy 2014, the ZEW Public Finance Conference 2014 as well as the participants of the OECD internal seminar series.

†silvia.albrizio@oecd.org

‡This paper does not represent the view of the OECD or of its member countries. The opinion expressed and the arguments employed are those of the author.

§stefan.lamp@eui.eu

1 Introduction

Fiscal consolidation represents one of the main challenges that policy makers are currently facing in most OECD countries. Understanding how different fiscal consolidation measures (i.e. spending cuts and tax increases) affect growth is therefore crucial. In a recent paper, Alesina, Favero, and Giavazzi (2012) show empirically that tax-based fiscal adjustments have a statistically significant different effect on output compared to spending-based adjustments. The former ones are not only more costly in terms of output loss than spending adjustments, but they can be also linked to longer-lived recessions. The macro analysis of Alesina, Favero, and Giavazzi (2012) focuses on a large set of OECD countries and points out that the strong effect of tax-based consolidation on output is driven by shifts in business investment. Understanding further the links between fiscal consolidation, business confidence and firm investment is even more crucial in periods of excessive debt and/or deficit, when the economy needs an effective growth policy agenda. Therefore, our analysis focuses on tax adjustments and tries to shed light in the interconnection between tax adjustments, business confidence and investment. Previous studies have been unable to capture the causal link between these elements either due to the aggregate nature of the data, which does not allow matching firm expectations with their investment behavior, or due to the endogeneity of the fiscal policy, as one of the key issues in estimating the impact of economic policy is the identification of exogenous fiscal shocks.

To deal with the unavailability of firm investment expectations, previous literature focuses mainly on realized investment both at the macro and at firm level. Alesina and Perotti (1996), using case studies, stress the "credibility effect" that a decisive discrete change in the fiscal policy stance may have on interest rates which would crowd in private investment. Alesina, Ardagna, Perotti, and Schiantarelli (1999) associate one percentage point of GDP increase in labor tax with a decrease of aggregate investment over GDP by 0.17 on impact and a cumulative effect of about 0.7 in five years. Confirming these results, Cloyne (2011), Mertens and Ravn (2009), and Hayo and Uhl (2013) find a negative, sizable and statistically significant effect of tax increase on investments at the aggregate level. At firm level, previous literature builds heavily on neoclassical models of investment based on the user cost of capital and the Q-theory[1]. In the user-cost framework, higher taxes affect investment negatively through the increase in user cost of capital. Cummins, Hassett, and Hubbard (1996) exploit cross-sectional variation in

[1]See Bond and Van Reenen (2007) for a comprehensive overview of microeconometric models of investment and employment.

user cost due to major tax reforms. They find significant effects with an implied long-run elasticity of the capital stock with respect to the user cost between -0.5 and -1.0 [2]. Chirinko, Fazzari, and Meyer (1998) analyze UK firm investment behavior using both the underlying Q-theory and user cost of capital, and their estimated effect reduces to -0.25. Finally, micro evidence based on cointegration models (Caballero, Engel, and Haltiwanger (1995) among others) estimates an average long-run relationship between capital-output ratio and the user cost of -0.1, where estimates range between -0.01 and -2.

Regarding the second limitation, the identification of exogenous fiscal shocks, the economic literature distinguishes three main methodologies. The first branch of literature follows the structural vector autoregressive approach (SVAR). In this approach, exogenous fiscal shifts are unobservable and identification is achieved using sign restrictions derived from economic theory (Mountford and Uhlig (2009)) or by taking into account institutional features of tax and transfer systems (Blanchard and Perotti (2002)). The VAR approach has led to a wide range of estimates of the spending multiplier (see Ramey (2011) for a literature survey). The second group of studies consists mainly of case studies (Giavazzi and Pagano (1990), Alesina and Ardagna (2010), and Alesina and Ardagna (2012)) find that spending based adjustments can have a very small or no output cost at all. Finally, a more recent method that found increasing attention in the economic literature is the narrative approach. Identification is based on observable exogenous shifts in fiscal stance by considering official documents, and hence by definition focusing only on fiscal adjustments that are motivated by deficit reducing purposes. As pointed out in Mertens and Ravn (2013), an attractive feature of this approach is that the narrative record summarizes the relevant features of a potentially very large information set.

This paper aims at filling the above described research gap, investigating further the set of correlations and causality between tax adjustments and private investment, in order to provide clear insights on the impact of fiscal reforms on firm incentives, and therefore on growth. In particular, we contribute to the debate in three ways: Firstly, by considering micro level data we move one step further in establishing a causal link between tax-based fiscal consolidation, business confidence and investment. Taking advantage of the information on firms' planned investment provided by the IFO investment survey[3], we are not only able

[2] Additional firm-level evidence on the user-cost elasticity of the investment rate is given by Schwellnus and Arnold (2008) and Johansson (2008).

[3] EBDC Business Investment Panel, http://www.cesifo-group.de/ifoHome/facts/EBDC.html

to compare our micro-based results with the previous findings from the macro literature, but also to take into consideration forward-looking behavior of the firms. Secondly, the detailed structure of the dataset allows us to disentangle the effect in two different dimensions: a heterogeneous effect depending on firm size and on the industry sub-sector. In line with Romer and Romer (2010) and Pescatori, Leigh, Guajardo, and Devries (2011), we employ the narrative approach to identify exogenous tax adjustments. Based on a detailed narrative created by Uhl (2013) for Germany, we revise 40 years of documented tax legislation (1970-2009) in order to create a dataset of tax adjustments that are not cyclically driven nor dictated by long-term growth considerations. We further investigate the timeline of tax adjustment not only considering the publication date, as provided by Uhl (2013), but also looking for the date when the public discussion of the adjustment started. To do so, based on the LexisNexis database, we collect journalistic documents that discuss each of the tax changes we considered.

Finally, focusing on one country only, we are not only able to consider a much more accurate policy dataset, testing the results for different shock reference dates (discussion date, publication and first implementation date) but also to disentangle the effect according to the type of tax change (income tax, business and corporate tax, or consumption tax). In fact, as pointed out in Mertens and Ravn (2013) and Cloyne and Surico (2013), there is little reason to expect that the different types of taxes available to governments all have the same impact on the economy.

The remainder of this paper is structured as follows. Section 2 introduces the series of exogenous tax shocks as developed by Uhl (2013), and which have been adopted for the purpose of this paper, as well as the firm level investment data. Section 3 describes in detail the identification and the estimation strategy, while the main results are discussed in section 4. Section 5 further elaborates on heterogeneity and section 6 performs a series of robustness and sensitivity checks. Finally, section 7 concludes.

2 Tax shocks and firm investment data

Narrative of German tax changes

The series of tax changes is based on Uhl (2013), who elaborates an extensive record of tax legislation in Germany[4]. In order to identify all relevant tax law

[4]The analysis in Uhl (2013) is based mainly on the Finanzbericht and Bundesfinanzplan of the Federal Republic of Germany. In order to recover all budgetary details of individual tax laws we re-

changes Uhl (2013) uses in a first step a size criteria of the budgetary impact of tax changes. Tax shocks are thus considered important and are included in the narrative if their budgetary impact reaches 0.1% of GDP in a given year[5]. This first criterion led to the identification of 95 important tax changes that are revised in a detailed fashion in Uhl (2013) and that are classified according to their main motivation in "endogenous" and "exogenous" tax measures in line with the previous literature (see for example Romer and Romer (2010), Pescatori, Leigh, Guajardo, and Devries (2011) and Cloyne (2013))[6].

Key in the narrative approach is to identify the exact motivation behind each tax change, as this allows excluding tax policy changes dictated by business cycle fluctuations and changes correlated with the dependent variable throug other unobserved factors. As pointed out in Romer and Romer (2010), simply regressing output growth on all legislated tax changes will lead to biased estimates, given the fact that some tax changes might be correlated with the error term. Moreover, this bias might be even more emphasized in case the researcher does not account for the fact that the policy makers might adjust their policy measures to the current state of the economy, for example employing countercyclical policies. Even controlling in the regression framework for known macroeconomic shocks and conditions would not solve the issue of identification, as firstly it would be impossible to proxy for all information about future output movement that the policy maker may have had and secondly the response to tax changes is likely to vary from period to period and may be hence correlated with other unobserved factors in the error term. Thus it is crucial to identify the exact motivation behind individual tax changes.

We align our classification of the motivation of tax changes with Uhl (2013), however we revise each of the Uhl tax shocks and regroup them according to "exogenous" and "endogenous" for our analysis of investment. Uhl (2013) classified spending driven tax changes, countercyclical policies and tax changes due to macroeconomic shocks as "endogenous" measures. On the other hand, "exogenous" measures are those dealing with budget consolidation and structural con-

vised the Finanzbericht for the years 1970-2009 and the four-year budget plans (Bundesfinanzplan) for the time period 1990-2009.

[5]Tax shocks are also included if the measure is (close to but) below the 0.1% GDP threshold but tax law changes consist of individual well defined measures. Other narratives, such as Pescatori, Leigh, Guajardo, and Devries (2011) do not state a precise cutoff rule, however for their full dataset of fiscal adjustments, only 5 out of 173 fall below the 0.1% rule, none for Germany.

[6]As the previous literature building on the narrative approach we slightly abuse terminology and consider "exogenous" all changes that are not systematically correlated with current or lagged output and investment.

siderations. While consolidation measures are related only to past spending and are exogenous to the current macroeconomic stance, the category of structural tax changes is more controversial as it includes both measures that aim at long-term growth, incentivizing investment, as well as tax changes that have been induced by court-rulings and that are hence unrelated to investment activity. Therefore, building on the previous narrative-literature, in our reclassification we define as "exogenous" only those structural changes that are not cyclically driven nor motivated by long-term growth considerations and hence aimed at investment. The appendix provides some examples of tax changes and their classifications[7].

Given the fact that we have exact information on the timing of individual tax measures - the date the first draft was introduced to the parliament, the date the tax law was published and information on the public discussion in the newspapers - we test for the impact at different dates. Differently from other studies that use this approach, we consider the budgetary impact at announcement. This choice relieves us from difficult considerations regarding revisions that are potentially correlated with investment and the contingent economic situation[8] as well as from potential measurement errors. Furthermore, to avoid heterogeneous displacement effects, we focus on exogenous tax shocks that are announced and implemented within the same period.[9] Figure 1 depicts the full series of important tax changes in Germany announced and implemented within the same period for both "exogenous" and "endogenous" motivations for the period 1970-2009, using half-yearly data frequency. As the graph shows clearly, endogenous tax changes are on average larger and more frequent than the exogenous category. In total, we count with 19 exogenous shocks and 31 endogenous ones. The correlation between the two time series is 0.09, and is not statistically significant (p-value of 0.53).

Given data availability our main analysis focuses on the period 1970-2010[10]. As explained in more detail in the following section, we group tax changes in both yearly and half-yearly periods in line with our firm level investment data. The original tax shock series, expressed in billions of Euros (governmental budgetary

[7]For a complete overview of all important tax measures in the Federal Republic of Germany, see Uhl (2013).

[8]Examples of factors correlated with investment which could drive the revisions are: resistance from trade unions, deterioration of the economic situation, etc.

[9]This is in line with the previous literature. See for example Mertens and Ravn (2011) that exclude tax changes with implementation lag exceeding one quarter. In the robustness section we also control for shocks that are announced but that are implemented in subsequent periods.

[10]Our last fiscal shock is observed in 2009, however we include one additional year of firm investment data to capture the lagged investment effect.

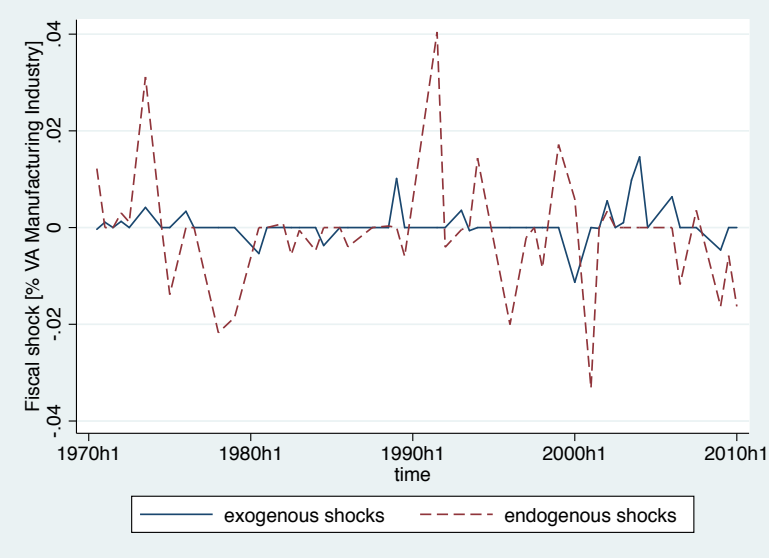

Figure 1: Legislated tax changes. Half-yearly frequency

impact), has been first deflated using the gross fixed capital formation deflator for the manufacturing industry[11] and divided by total value added (VA) in the manufacturing industry in 2005, in order to have the main regression variables at a similar scale, which allows for easier interpretation of the coefficients. The exogenous shock series contains both positive and negative tax measures ranging from -0.011 to 0.014 with a mean absolute impact of 0.002 and a standard deviation of 0.004.[12]

In terms of timing, focusing on the subset of exogenous shocks, the average length from the date the draft of the law is introduced to the parliament and the date of publication of the same is around five months [13]. On the other hand the average time between publication and first implementation of the tax measure is two months. However a detailed revision of the shocks brings the fact to light that most of the shocks are induced by a lengthy public discussion prior to the initi-

[11]The deflator is based on STAN Industry Rev. 3, 2008 (OECD) Database. Investment and financial variables are deflated in the same way.

[12]We use the mean absolute impact rather than the simple mean, given both positive and negative shocks. Alternatively the mean impact of the 12 positive shocks has been 0.005 (0.004) shocks, and for the seven negative shocks -0.004 (0.004).

[13]The exact draft date can only be reconstructed for shocks posterior to 1977.

ation of the legal process of tax change. The media and newspapers report these discussions and we refer to the date of the first article mentioning as "discussion date". In order to check for this possibility we look at the timing of news coverage of tax measures prior to the draft date using the online database LexisNexis. We find that the average time lag between initial discussion of the tax measure and its publication is one year. The appendix provides an overview table containing discussion, draft and publication date of selected tax shocks[14].

Firm investment data

Data on firm investment is obtained from the IFO investment survey (IVS). As pointed out in Seiler (2012) the IVS was originally introduced in 1955 and considers the manufacturing sector in Germany, however annual investment data is available only from the mid 1960s onwards. While the initial questionnaire has been distributed only once a year, from 1993 onwards the survey has been performed bi-annually, in spring and autumn of the same year, leading to an even richer data structure[15].

The IVS questionnaire focuses mainly on firm investment activity and includes both forward and backward looking statements of realized and planned investment. As the questionnaire includes only a small list of potential control variables, the dataset has been enriched by the Economic and Business Dataset Center (EBDC) with balance sheet data obtained from Amadeus and Hoppenstedt[16]. The merged investment data counts with a total of 202,368 observations that belong to 5,590 firms. In principle the dataset is longitudinal however the number of firm that exit at some point in time the panel is high, so that there are few firms reporting the entire sample period. In terms of representativeness, in 2009 the IVS sample covered 31% of all employment in the German manufacturing sector (7% of companies), with better representation of bigger firms (2% of employment size class < 50 and 66% of employment class size >1000).[17]

For the purpose of our analysis, the original dataset has been first converted to Euros, using the fixed Euro-DM exchange rate and then deflated with the OECD

[14]Using LexisNexis, we were able to track back news coverage for tax adjustments for the period 1992 to 2010.

[15]Data previous to 1991 corresponds to West Germany, while data posterior to 1991 includes also firm from former Eastern Germany.

[16]The exact merging procedure is described in Seiler (2012).

[17]The authors would like to thank Heike Mittelmeier and Christian Seiler from the EBDC for providing this information regarding the IVS.

deflator for gross fixed capital formation in the manufacturing industry. Furthermore we drop IFO sector 210 from the analysis, manufacture of mining products, as it does not find a clear correspondence in the ISIC manufacturing classification. Converting the dataset to an annual data structure, and constructing the change in realized investment as log difference of investment at time t and investment at time $t-1$, we are left with 64,310 observations belonging to 5,186 distinct firms[18] [19]

Most of the literature dealing with firm level investment considers as dependent variable the ratio of investment (defined as the change in capital stock) over capital. Even though the IFO data provides a direct measure of investment, it does not provide us with an initial capital stock[20]. Therefore, as alternative measure we normalize investment by firm specific average asset stock over the sample period, which is available for the subset of firms that have been merged with the Amadeus and Hoppenstedt databases. Nevertheless also this procedure reduces the sample coverage considerably. Therefore we use this specification only as robustness check for our findings, estimating a dynamic firm-level investment model as derived in Bond, Harhoff, and van Reenen (2005) (see section 6).

Our empirical analysis focuses both on realized investment growth and on updates of planned investment. However updates of planned investment are only available for the subsample period 1993-2010, in which the IVS has been conducted at a bi-annual frequency. In each round firms are asked to provide an estimate for their planned investment for the same year. In addition, in spring firms are asked how much they have been investing in the previous year (realized investment in $t-1$) and, in autumn, how much they are planning to invest next year ($t+1$). Therefore the richness of the IFO investment dataset allows us considering both realized investment changes and updates in planned investment.

[18] Conditioning our sample on firms that report in two consecutive periods does not change significantly the size composition: For the full sample (sample in differences) there are 17.6% (15.6%) in size group up to 49 employees, 31.9% (31.2%) in size group up to 199 employees, 34.7% (35.8%) in the size group up to 999 employees and 15.8% (17.3%) in the category >1000.

[19] We allow for zero growth in case a firm reports zero investment in two consecutive years. As robustness check we further experiment with a second specification, imputing a small, but positive number for investment in years t or t-1 in case a firm reports in either of the two periods zero investment. Given that this procedure leads to additional variability, for the analysis we cut the variable at the first and 99th percentile to make the measure outlier proof. We find that our results are not affected by the specification of the dependent variable.

[20] Backtracking the capital stock using inventory methods would be only meaningful for balanced data or data with few gaps.

Formally, realized investment growth in year t is defined as:

$$\Delta ln(I_t) = ln(I_{t,A}) - ln(I_{t-1,A}) \tag{1}$$

while the change in planned investment is defined for reference year t, respectively in each period p=1 between 1 October (t-1) and 31 March (t) and p=2 between 1 April (t) and 30 September (t), as:

$$\Delta ln(PI_{t,1}) = ln(PI^t_{t,S}) - ln(PI^t_{t-1,A}) \tag{2}$$

$$\Delta ln(PI_{t,2}) = ln(PI^t_{t,A}) - ln(PI^t_{t,S}) \tag{3}$$

where the subscript indicate the year and the survey round (S=spring, A=autumn) when the plan is revealed, while the superscript refers to the forecast horizon, i.e. the year the investment is supposed to take place. The exact timing of the half-yearly investment structure is depicted in Figure 6 in the appendix.

2.1 Summary statistics and representativeness

Table 1 shows the main variables of interest for our analysis at annual frequency for the full sample period and two subsample periods 1970-1990 (West Germany only) and 1991-2010. The main dependent variable, realized investment growth is small in absolute terms, however as the standard error suggests there exists considerable variation across firms. The alternative measure (investment over average capital stock) has a mean of 0.25 (median of 0.18), which however includes more bigger firms. The exogenous fiscal shock measured in terms of total value added in the manufacturing industry is very similar for the two time periods in terms of the average, however the standard error in the later period (1991-2010) is almost the double. For comparative purposes Table 1 also reports the aggregate control variables for the interest rate as well as sales growth and firm size (number of employees), as these variables are reported for all firms in the questionnaire [21]. While the interest rate has been around 1% higher in the early subsample (1970-1991), average sales growth was nearly double compared to the second sample period. These tendencies are related to general structural changes in the German economy.

In order to provide further evidence on the representativeness of our data, Figure 7 in the appendix compares realized changes in aggregate investment in

[21] As mentioned, other financial covariates, such as assets and liabilities, are only available for a subset of firms (those listed in either Amadeus or Hoppenstedt and that could be merged).

	Total sample: 1970-2010		Subsample: 1970-1990		Subsample: 1991-2010	
	Mean	std	Mean	std	Mean	std
Realized investment change	-0.0110	(-1.046)	0.0297	(0.965)	-0.0424	(1.104)
Investment / Average total assets	0.2520	(0.229)	0.2580	(0.234)	0.2487	(0.226)
Exogeneous fiscal shock	0.0011	(-0.006)	0.0013	(0.004)	0.0010	(0.007)
3 month interbank rate	2.4670	(-1.616)	3.0911	(1.904)	2.0205	(1.186)
Sales growth	0.0231	(-0.261)	0.0312	(0.225)	0.0164	(0.288)
Total employment last year	837	(5195)	948	(5247)	753	(5154)
Observations	64310		27936		36374	

Note: Investment / Average total assets counts with a total of 39751 observations.

Table 1: Summary statistics: main variables

the manufacturing sector in Germany obtained from STAN (OECD, Rev.3 2008) with aggregation based on our sample data. The figure indicates that the series co-move closely over the entire sample period but that our aggregation based on firm data shows slightly more variability than the official statistics. Furthermore the appendix provides some first evidence for the negative correlation of our fiscal shock measure and aggregate investment growth. The two series show a correlation coefficient of -0.15 (Figure 8). We present the same evidence by ISIC 3 industry sub-sector and by size group (Figure 9 and Figure 10 in the appendix).

3 Identification and empirical specification

As pointed out above, the key assumption behind the narrative approach is that both the tax changes itself and their composition are "exogenous" i.e. tax changes are not dictated by business cycle fluctuations nor long-term growth concerns. In line with the previous literature (see for example Cloyne and Surico (2013)), we test for exogeneity using a four-variable VAR at annual frequency including the tax shock series (for both the endogenous and exogenous category), GDP growth, the three month interbank rate and the average investment change as main dependent variable [22]. We construct the aggregate change in investment as log difference of average investment in period t and t-1 weighted by employment shares [23]. The selection-order criterion suggests in most specifications unani-

[22] In an alternative specification, we also account for the structural break due to the German reunification (1990) and the recent financial and economic crisis (post 2007); our results are robust to the inclusion of these exogenous dummies.

[23] We also test for other measures of aggregation, using changes in total investment from period t to period $t+1$, and hence conditioning on firm presence in two consecutive years, or using simple unweighted average investment change. The main results hold for all definitions of aggregate

mously a lag structure of order one for the VAR. Table 3 in the appendix provides evidence from the granger causality tests, showing that the exogenous tax shock series implemented in the same period of the publication date cannot be predicted neither by macroeconomic conditions in the last year, nor by past investment activity. On the other hand, the "endogenous" tax adjustments can be predicted by economic growth (p-value 0.063). The three excluded series jointly (investment growth, GDP growth and interest rate) moreover carry information to forecast the endogenous fiscal shock series at 10%. These results strongly support our key identification assumptions [24].

As second test for exogeneity of our fiscal shock series we run an ordered probit regression to see if the government's decision to adjust taxes can be predicted by past macroeconomic data. The same approach has been taken by Cloyne and Surico (2013) and Mertens and Ravn (2009). We hence construct an indicator variable ω_t equal to 1 if the government implements a positive fiscal shock, zero if no action has taken place and -1 if there has been a negative fiscal adjustment. Results are presented in Table 4 in the appendix and indicate that while movements in the exogenous shock cannot be predicted neither by lagged changes in aggregate investment nor by lagged levels of GDP, the endogenous shocks are correlated to lagged investment growth. As additional test, we run the ordered probit model on official data from the manufacturing sector (Table 5) using both changes in gross fixed capital formation (GFCF) and levels of GFCF from the OECD (STAN) database. While the results for GFCF growth are fully comparable with our in-sample findings (only lag 2 of GFCF growth) is significantly correlated with the endogenous shock, for the levels equation we find strong evidence that movements in the endogenous series are highly correlated with both lagged levels of investment and GDP. The shocks that have been classified "exogenous" on the other hand are not predictable.

Using the exogenous tax adjustment series, our analysis first focuses on the revision of investment plans, and secondly, we study how realized investment is affected. Both analysis are based on the following main regression specification:

$$\Delta I_{i,j,t} = \alpha + \beta_m(L_m)\tau_t + \psi m_{t-1} + \rho g_{t-1} + \nu \Delta z_{i,t-1} + D_{90} + D_{07} + \theta_j + \epsilon_{i,j,t} \tag{4}$$

investment. We furthermore test that the investment series are stationary, using an augmented Dickey-Fuller test.

[24] Given the fact that our tax shock series includes both structural and consolidation motivated shocks, as sensitivity check, we furthermore exclude all shocks with structural motivation. The presented findings are robust to the selection of shocks.

where $\Delta I_{i,j,t}$ is the growth rate of realized investment for firm i, in sector j, in period t. The investment changes are defined separately for realized and planned investment as introduced in section II. The fiscal shock τ_t is the exogenous tax adjustment published at time t, and is uncorrelated with other shocks to investment by construction. Macro-level controls consist of the monetary policy stance m_{t-1} (previous period three-month interbank rate) and economic condition g_{t-1} (lagged levels of GDP). Dummies to account for the crisis period 2007-2010 (D_{07}) and for the structural change 1990 (D_{90}) are included in the regression equation[25]. Finally, lagged sales growth at firm level ($\Delta z_{i,t-1}$) is part of the regression controls to proxy for current and future demand conditions at firm level. In all specifications we include furthermore sectorial fixed effects θ_j and standard errors are clustered at firm level [26]

4 Main regression results

The following section presents the main regression results for both planned and realized investment growth at firm level. Table 13 in the appendix also provides some evidence for the effect of fiscal shocks on realized investment changes aggregated at sub-sector level.

Planned Investment

As previous contributions have suggested (see for example Alesina, Favero, and Giavazzi (2012)), business confidence and private investment are found to be the main drivers of the output effect of fiscal consolidation. Studying the change in future investment plans at micro level helps to understand and pin down the business expectation and confidence channel. As mentioned in section 2, in the IVS firms are asked about their investment plans for next period. Given the opportunity cost of investments, these plans, and in particular their revisions, incorporate business expectations and anticipation about future economic and policy conditions.

[25]To account for the structural break in the statistical data more than the actual historical date of the German reunification.

[26]Given the fact that our main explanatory variable is aggregated at annual level, we potentially could cluster on years, however clustering on year assumes that firm level errors are uncorrelated from one year to another, an assumption that is unlikely to hold. Alternatively we test for clustering at industry sub-sector (branch). The main findings are unaffected by the choice of the clustering variable.

Insert Table 6 here

We observe updates on planned investment for the period 1993 to 2010 at a bi-annual frequency. For this period, we count with a total of 10 exogenous fiscal shocks with a mean impact of 0.001 and a standard deviation of 0.0048. Moreover given the fact that our analysis focuses on the announcement effect of fiscal policy, we use the shock publication date. Table 6 presents the estimates of the effect of a tax change equal to 1% of total manufacturing value added on the revision of planned investment. Block 1 (column (1) - (3)) includes only lags of the fiscal shock, while block 2 (column (4) - (6)) includes also leads. For the rest, the two blocks include the same set of covariates: the first column of each block includes a set of aggregate controls (lagged GDP, lagged three month interbank rate, and a dummy accounting for the recent financial crisis) in addition to industry fixed-effects, the second column includes additionally lagged firm level sales growth, and finally the third column includes firm level fixed effects. In all specifications we furthermore include a separate dummy for the second half-year (autumn), in order to account for potential differences in volatility of the two revisions[27], which results to be highly significant in all specifications.

Block 1 shows that there is a significant and negative effect of tax shocks on planned investment. A shock equal to one standard deviation of the exogenous fiscal shock[28] hence translates to a decrease in planned investment of around 1.2% in the next investment plan. Once we additionally include leads, in order to test for a potential anticipation effect in block 2, the lagged effect on planned investment becomes quantitatively larger. We furthermore confirm that agents anticipate the fiscal adjustment as both lead 1 and 2 show up to be significant in all three specifications. Note additionally that all control variables (but lagged GDP in some specifications) show up to be statistically significant with the expected sign. The R^2 is low even when including firm level fixed effects, which indicates that investment changes are indeed very lumpy and volatile [29].

The forward looking behavior of the firms can be explained by the average length of the legislative process for tax changes in Germany. To test this hypoth-

[27]Due to a lower degree of uncertainty, the autumn investment update might be more accurate and hence less volatile than the spring update. The authors would like to thank Antonello d'Agostino for pointing this out in his discussion at the Banca d'Italia Fiscal Policy workshop 2014.

[28]As the shock can take on both positive and negative values, we standardize using a standard deviation measure. Alternatively we could use the mean of the absolute shock impact in order to quantify the shock impact on investment growth, which is very similar in magnitude.

[29]Note furthermore that the R^2 from the firm level fixed effect regressions, column (3) and (6) are adjusted and hence lower than the other columns, that report an unadjusted regression fit.

esis we investigate in a more detailed fashion the legislative timing, starting from the moment when the draft of the law is discussed in the public (media coverage in major German newspapers and news magazines). Therefore we search for news contents related to the discussion of fiscal shock measures employing the database LexisNexis [30]. In fact we find clear evidence that between the time of public discussion and publication of the law, on average, there passes one year. Compared to the draft date, the date when the law is officially introduced in the parliamentary discussion, the public discussion happens around half a year earlier. Table 2 in the appendix provides an overview of mayor exogenous tax shocks since 1992 including their official publication dates, draft dates and periods of public discussion in the media (discussion dates). Given these findings, we re-estimate our main regression model focusing on the discussion date as "true" announcement date of the shock.

The results are reported in Table 7. We find that once we consider the media discussion date, controlling for firm-level sales growth or using firm-level fixed effects, no forward lag shows up to be significant. In fact compared to the publication date, the fiscal shock is only significantly (and negatively) correlated with changes in planned investment at impact, i.e. when the news is announced. [31] Generally, using the discussion date, we find quantitatively similar, but more stable effects of downward revision of -3.5% to -4% for a shock equal to 1% value added in the manufacturing industry. A shock equal to one standard deviation hence led to a downward revision of investment plans by 1.9% for the sample period 1993-2010.

Insert Table 7 here

To sum up, when firms make their plans for next period investment, they are influenced by laws currently under discussion and laws published in the previous half year. Given the fact that we are interested in identifying the announcement effect of fiscal consolidation measures on planned and realized investment, we hence use the discussion date as main specification in the remaining sections of this paper.

[30] LexiNexis contains all major German newspaper and covers news contents from the beginning of the 1990s.

[31] We also tried alternative specifications including additional lags up to lag 4, but the only significant impact remains at lag zero.

Realized investment

After analyzing firm behavior in terms of investment expectation, it is interesting to apply a similar analysis to realized investment in order to be able to compare our findings with the previous macro-level results. We consider firms' annual investment growth from 1970 to 2010 as defined in section 2. Table 8 presents the point estimates of the effect of a tax change equal to 1% of total manufacturing value added on investment growth. Column (1) does not include any controls while column(2) includes aggregate controls and column (3) furthermore lagged sales growth at firm level. Column (4) presents the results for realized investment for the period 1991-2010, while column (5) for the earlier period and Western Germany alone (1970-1990).

Insert Table 8 here

Interestingly, we find that the fiscal shock has a negative and significant impact on realized investment that is strongest in the year of public discussion[32] but has also a lagged effect. The initial impact is stable to the inclusion of additional aggregate and firm level controls (column 2 and 3), however once we include the set of controls, we find a more persistent effect. Adding up the significant lags in column 3, the total impact of a one percent tax shock on investment growth is around -15.6%, which however is smaller when evaluated at the mean absolute impact or the standard deviation measure: -5.7%. In fact, for the annual shock series, there are a total of 19 fiscal shocks with a mean value of 0.0007 and a standard deviation of 0.0037. All aggregate control variables show up to be significant and show the expected sign. The sample split in column 4 and 5 suggests two clearly different patterns: while in the more recent period 1991-2010, the fiscal shock shows quantitatively the same impact as for the entire sample period (-8.8%), the earlier subsample shows a significant lagged effect that is biggest at lag 1. As for the half yearly analysis there are 10 shocks for the subsample post 1991, with a mean impact of 0.00096 (0.0047) and 9 shocks for the first subsample referring to column 5 with a mean impact of 0.0004 (0.0027). Hence the different fiscal policy over the period considered translates into bigger an more volatile shocks in more recent years. In addition to differences in the fiscal shock series, firms might have changed their behavior over the last 20 years, using more technology and respond faster to changes in the companies legal and fiscal environment.

[32]Using as true announcement date the date of public discussion as introduced in the previous section.

Generally, the results are in line with the macro level findings even though the magnitudes are not directly comparable. Alesina, Favero, and Giavazzi (2012) for instance find that a one percent GDP tax shock has a negative and significant lagged effect on fixed capital formation growth in Germany that increases from -4% in the first quarter after the adjustment to around -6% one year after the adjustment. In fact, while in the macro literature the shock is standardized by GDP, in our micro set-up it makes more sense to re-scale the expected budgetary impact using the value added of the total manufacturing sector. Moreover, another difference between our framework and the macro analysis is the difference in timing.

In order to verify that fiscal shocks, defined as exogenous, are not correlated with the shocks that were announced in the past but implemented at time t, we reestimate our regression model including both the previously announced shocks and in a second step also the shocks that we classified as endogenous. Running our main specification (column (3), containing both aggregate and firm level controls), and including the shocks previously identified as endogenous, we get results very much in line with those presented in Table 7. While the leads do not show up to be significant, at impact we estimate an effect of -7.65, at lag1 of -5.87 and at lag 2 of -2.95, all significant at 1%[33]. On the other hand, including the anticipated tax shocks, we confirm these findings: while the leads are not statistically significant, at impact we estimate an effect of -8.29, at lag1 of -6.39 and at lag2 of -4.89. These findings can be seen as a first robustness check for our main regression results.[34]

While section 5 reports the results for heterogeneity of realized investment changes depending on type of tax adjustment, firm size and the sub-sector of the manufacturing industry, section 6 performs further robustness checks, providing also evidence for the negative and significant effect of tax adjustments using a rigorous difference-in-difference strategy that allows us controlling for other unobserved factors potentially correlated with the fiscal shock series and investment growth.

[33]This results hold independent of the inclusion of control variables.

[34]Table 13 presents the effects of fiscal consolidation at industry level, provides similar evidence. Including previously announced shocks or shocks considered endogenous does not alter our main findings.

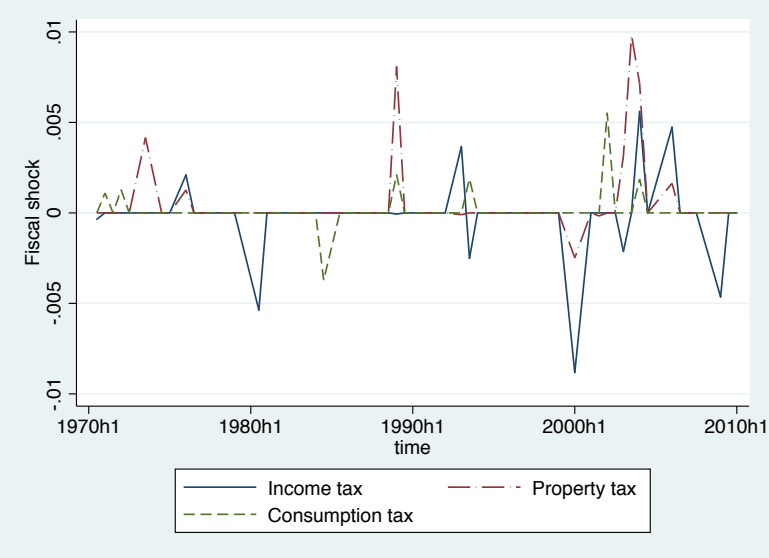

Figure 2: Exogenous tax adjustments; by type of tax

5 Heterogeneous effects

The long time span of available data for realized investment growth allows us studying the effect of tax changes by looking at three main dimensions: type of tax adjustment, heterogeneous effects by firm size and by manufacturing subsector as well as their interactions.

Looking at GDP per capita, Johansson (2008) find that corporate taxes are most harmful for growth, followed by personal income taxes and consumption taxes. To test for the effect of exogenous tax changes on realized investment we group the shocks in different categories. As depicted in Figure 2, we distinguish three main tax categories:

- personal income tax, pension & savings tax
- corporate & business tax, energy tax, property tax
- consumption tax

Breaking down the tax shock into these subcategories, we are able to distinguish 11 tax measures for the first category, 11 for category two and 7 for the third category. In order to identify these categories we revised a total of 42 tax law changes

that consist of 184 individual tax measures.

Insert Table 9 here

Including the three fiscal shock series in our reduced form estimation, both in the same regression (Table 9, column 1) and in separate regressions, we find important differences with respect to the previous estimates found in Johansson (2008): consumption tax shocks have a strong negative and lagged impact, while tax adjustments affecting income tax seem to have the biggest impact within the same year. Property and corporate taxes, on the other hand, have a smaller effect at impact. These findings support a recent hypothesis[35] which highlights the importance of the demand channel for the transmission of fiscal shocks. Consumption taxes affect demand and consequently firms' investment in the successive periods through future demand expectations.

In order to compare our results with the aggregate findings on realized investment (section 4), we look at the standard deviation measure of the distinct categories of fiscal shocks and find that while income (0.0021) and property (0.0022) adjustments nearly have the same variability, consumption shocks are smaller, almost half (0.0011). Using the estimated coefficients from column (1) this leads to an effect of a standard deviation fiscal adjustment on investment growth of -4.1% for income tax, -1.76% for property tax and -1.9% for consumption tax. In order to contrast these results, we aggregate fiscal shocks in an alternative way, considering income and property tax as direct taxes and the consumption tax as indirect taxes. Results are presented in Table 10 and show the same pattern that is stable to the inclusion of additional controls, fixed effects and also to the inclusion of previously excluded tax shocks. While direct taxes show a negative effect at impact, indirect taxes only lead to a downward revision of realized investment in the subsequent period, and hence providing further evidence for the demand channel hypothesis.

Recent firm-level literature has furthermore stressed the importance of considering heterogeneous and distributional effects of fiscal and other policies in general. To test for different impact in terms of firm size we use the IFO firm class sizes of employees (1-49, 50-199, 200-999, $>$1000) and run the regression for each subgroup separately. Given the potential residual correlation across size classes,

[35] See for example the discussion of Aghion and Kharroubi (2013) at the annual BIS conference (June 2013) by Reichlin.

we adopted a seemingly unrelated regression (SUR) framework. The results highlighted in Figure 3 show that at impact all size classes are negatively and significantly affected by the tax adjustment. Furthermore the effects are larger for firms that belong to size group 1 to 3. The largest firms show the smallest coefficient. Moreover we confirm that the lagged effect is present for all size classes but for the smallest firms (size group 1), where lag1 does not show up to be significant. This finding might be due to the fact that the smallest group is highly heterogeneous, as it is also suggested by the wide confidence band. The magnitude of the effect is in line with the aggregate findings for the impact and slightly larger for lag1.

In a next step, we investigate if distinct tax shocks have different effects by firm size. The tax effects might differ by size group as firm size is also a proxy for the legal status. Figure 4 shows the results for direct and indirect tax shocks at impact and for lag1 for the distinct size groups. As pointed out above, given the strong heterogeneity in the smallest size group, we cannot confirm any significant effect for either tax category. On the other hand we can confirm the main pattern that we found when looking at type of tax shocks. Direct tax adjustments have a negative impact at lag 0 that is quantitatively smaller than the impact for indirect (consumption) taxes at lag 1. Furthermore the impact is larger for smaller firms (coefficient for size group 2 $>$ size 3 $>$ size 4, for both direct and indirect taxes), which might indicate that smaller firms are on average more credit constraint and hence a fiscal shock translates to a stronger effect (see Zwick and Mahon (Working Paper) for recent evidence from the US).

A final dimension of heterogeneity that we test is the response by sub-sectors of the manufacturing industry. For that purpose, we divide the firms in our sample into 12 sub-sectors based on the two-digit ISIC 3 classification with some aggregations[36]. We apply the same SUR methodology as used for firm size, and regress investment growth on contemporaneous and lagged fiscal shocks, including furthermore our set of control variables. The results for lag 0 are displayed in Figure 5 [37]. We find that almost all sub-sectors show a negative and significant impact at lag 0, but the sub-sectors "food, beverages & tobacco", "leather", "non-metallic

[36]The manufacturing industries covered are food, beverages and tobacco (1516), textiles and wearing apparel (1718), Leather industry (1900), wood (2000), pulp, paper and printing (2122), chemical, rubber, plastics and fuel products (2325), other non-metallic mineral products (2600), basic metals and fabricated metal products (2728), machinery and equipment n.e.c. (2900), machinery and equipment (3033), transport equipment (3435) and manufacturing n.e.c. and recycling (3637).

[37]For lag one we only find a significant (and negative) effect for sub-sectors 1718, 2122, 2900, and 3033.

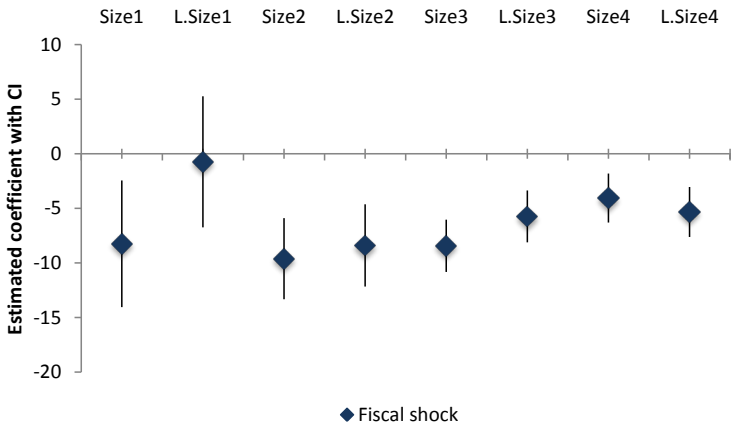

Figure 3: Heterogeneous effect by firm class size

mineral products", and "transport equipment"[38]. The significant coefficients range from -5 to -11 and are hence in line with our previous findings.

Using the narrative identification for fiscal shocks allows us considering and aggregating a wide range of shocks, and thus identifying a robust average effect of tax adjustments; however, at the same time, and given the shock heterogeneity, the narrative approach makes it difficult to pin down a single channel.

6 Robustness checks

6.1 Sensitivity analysis

In addition to the first model checks presented in the main section, we further elaborate on robustness in the present section. First, given the strong impact that the recent financial and economic crisis had on the economic activity in Germany (negative changes in realized investment of around 30 % in 2009 alone), a first sensitivity check consists of excluding the period 2007-2010 from our analysis. As

[38]While "food, beverages & tobacco" are a very heterogeneous group of firms, "leather" and "non-metallic mineral products" are very small and specialized sub-sectors within the German manufacturing industry. The fact that we do not find a significant effect for the transport equipment sector might be related to the strong export orientation of this sub sector, which includes the entire German car industry.

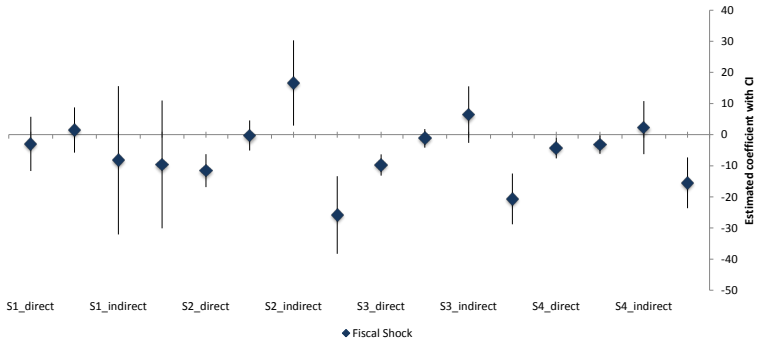

Figure 4: Heterogeneous effect by size and tax type

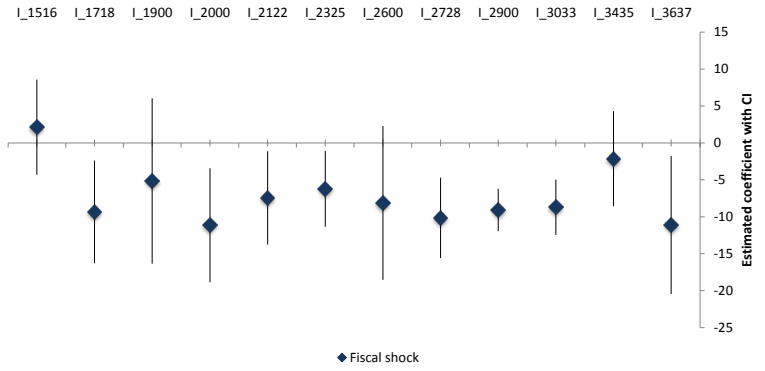

Figure 5: Heterogeneous effect by ISIC sector classification: at impact

pointed out in the methodological section, in the original regression specification we already control with a dummy for the recent crisis period, however excluding the period completely represents a good robustness check for our findings. Dropping the period post 2007, we are left with 38950 observations. For our preferred regression specification, including both aggregate and firm-level controls we find that the leads are not predictable and that the estimated coefficients for the fiscal shocks show the same sign and magnitude as before: -8.76, -5.54, and -3.05 for lag 0 to lag 2.

Another important robustness check is to exclude the biggest single sub-sector within manufacturing (manufacture of machinery and equipment) and to see if our results are stable. Dropping 17,710 observations from the annual dataset does not affect our results to an important degree and the estimated coefficients are directly aligned with our analysis of annual realized investment changes: -7.99, -4.21, and -2.96 for lag 0 to lag 2. Moreover, given the potential concern that structural shocks differ from consolidation shocks in their nature, i.e. they are based on "structural" considerations, these shocks might be correlated to past output and investment levels. We hence exclude them from our regression analysis and re-estimate the model using only shocks that are labeled unambiguously consolidation shocks in both Uhl (2013) and our classification. Again, our results are strongly aligned with the ones presented previously.

Finally, and in order to follow the literature on firm level investment, we model firm investment as in Bond, Harhoff, and van Reenen (2005). We hence estimate a dynamic model of firm investment focusing on the investment rate rather than on investment growth [39]. Due to data availably, we normalize investment by average assets of the company rather than by the capital stock at time $t-1$. The investment model specifies that current investment, the dependent variable, is explained by lagged investment, current and lagged sales growth, levels of sales, current and lagged cash flow to capital ratio and the second lag of the difference between capital stock and sales $(k-y)$. As explained in Bond, Harhoff, and van Reenen (2005), for consistency with the error correction specification, we require the coefficient of $(k-y)$ to be negative. For stability we furthermore require that the coefficient of lagged investment is lower than one in absolute terms.

As the investment rate depends on investment in the previous period, the model has to be estimated by general method of moments (GMM) [40]. Given the

[39] The interested reader is referred to Bond, Harhoff, and van Reenen (2005), where the error correction model of firm investment is derived in detail.

[40] For efficiency considerations, we adopt the system GMM approach as in Bond, Harhoff, and

fact that the GMM estimator is a large N, small T estimator, we focus on the sample period 1991 to 2004 in order to maximize the numbers of tax shocks and firm observations, but repeat the exercise for the full sample with very similar findings. In a first step, we estimate the model as in Bond, Harhoff, and van Reenen (2005), including time fixed effects, in order to account for the economic cycle and other unobserved factors (Table 11, column 1). In order to estimate the effect of our annualized fiscal shocks, we replace the time fixed effects by aggregate controls (column 2) and confirm that the main results do not change. Finally, the fiscal shock is included in column (3). Similar to our previous findings on investment growth, we find a negative and significant effect for fiscal shocks on the investment rate at impact and lag1. The coefficients can be interpreted as a 1% tax adjustment in terms of VA in the manufacturing industry leads to a decrease in investment by -1.4% at impact and -1.1% at lag one, and hence a total aggregate effect of -2.5%. The test statistics for column 3 indicate that the Hansen-statistic of non-valid instruments can be rejected, while the model shows clear evidence of autocorrelation only at lag1.[41].

Additional evidence on causality

Using a narrative identification strategy for fiscal shocks should overcome any type of endogeneity by construction. Nevertheless, taking advantage of the micro-level dataset and the detailed shock breakdown, we can provide further evidence that the investment response is indeed driven by the fiscal shock and that there are no unobserved factors driving the investment response, using a difference-in-difference approach. In order to do so, we focus on one specific type of shock that is likely to affect only some sub-sectors of the manufacturing industry. This identification strategy can help us to get closer to a causal interpretation of investment impact of fiscal consolidation.

For this purpose, we focus on tax changes that affect the cost of energy. Our assumption is that controlling for a set of aggregate and firm level factors, some energy intensive sectors will be highly affected by this type of tax adjustment, while other sectors will not respond to this tax change. Key is that both sectors,

van Reenen (2005)

[41] As additional model check we ignore the potential correlation between lagged investment and the error term and estimate the investment equation by both OLS and fixed effect regression. Given the induced bias the true value for lagged investment should be in-between the two naive approaches. We find that his is the case with an OLS estimate of 0.45 and FE estimate of 0.09 for the lagged investment coefficient.

belonging to the manufacturing industry, share the same unobserved trends and hence any difference in outcome can be assigned to the effect of the tax shock. The pulp and paper industry seems a good candidate to test this hypothesis, given its high energy dependence[42]. As control groups we consider the food and tobacco industry (ISIC 1516) and the group of non-classified manufacturing (ISIC 3637). Even though some firms in the food and tobacco industry might be dependent on energy in their production process, both control sectors are highly heterogeneous in terms of products and production processes and hence it is likely for energy tax changes not to show any aggregate effect.

Our "treatment" group "paper" consists of 10,357 observations and the combined group of "controls" has a total of 10,946 observations for the sample period 1970-2010. For this period we count a total of 4 energy shocks[43]. Investment change for the entire sample period for the control group has a mean value of -0.012 (1.01) and for the treatment group 0.001 (1.36). The regression results are reported in Table 12, where the first column (1) refers to a pooled regression, column (2) includes fixed effects for the individual sub-branches summarized in the two categories, and column (3) includes firm level fixed effects. The results show that there exists a strong negative lagged effect for pulp and paper, while the control sector does not show any significant response to energy tax increases. Adding firm level fixed effects in column (3) alters the estimated coefficients only slightly, but leads to a higher level of significance for lag 1. In order to compare the magnitude of the coefficients with our previous findings, we evaluate them at the mean impact of energy shocks. Given a standard deviation of energy shocks of 0.002, firms in the pulp and paper industry respond to an average shock by reducing their investment growth by -4.8%. The results are hence highly aligned with our previous findings.

7 Conclusion

Private investment has been shown to be one of the main drivers of aggregate output during periods of fiscal consolidation. Nevertheless, previous literature has failed to provide a causal link between fiscal adjustment, business confidence and

[42]On a worldwide scale the pulp and paper industry is considered the fifth largest consumer of energy. One additional advantage of the pulp and paper industry is that the products and manufacturing processes are highly standardized and hence a shock on energy prices (tax increase) is likely to affect all companies in the industry in a very similar fashion.

[43]Shocks in 1972, 1980, 1987 and 2001. Given the small number of shocks, we focus on realized investment changes rather than updates in planned investment.

firm investment. The urge for understanding this channel is even more relevant in periods of excessive debt and/or deficit when the economy needs an effective growth policy agenda.

Based on a detailed narrative record for tax changes in Germany (Uhl (2013)), we reclassify 40 years of fiscal shocks into "exogenous" and "endogenous" changes with respect to investment and to the contingent state of the economy. Exploiting this exogenous variation, we study the effect of a tax change on firms' realized and planned investment, considering the IFO investment survey dataset. We find that recently published laws and laws under current discussion in the media and in the parliament shape future investment plans. Taking into account the forward looking behavior and adjusting the announcement dates according, we find that an increase in tax equal to 1% of the value added of the total manufacturing industry leads to a lagged decrease in planned investment of about 4%. For realized investment growth we estimate an average effect of 8%

Finally, the use of micro-level firm data allows us to elaborate further on heterogeneity in terms of firm size, industry sub sector as well as by type of tax shock. Differently from the previous literature, we find that consumption taxes and income tax adjustments are most harmful for growth as they have the strongest negative and persistent effect on investment growth at firm level. The finding thus support recent hypotheses that highlight the importance of the demand channel in the transmission of fiscal policies, and may act through future demand expectation.

References

Alesina, A., and S. Ardagna (2010): *Large Changes in Fiscal Policy: Taxes versus Spending*pp. 35–68. The University of Chicago Press.

Alesina, A., S. Ardagna, R. Perotti, and F. Schiantarelli (1999): "Fiscal Policy, Profits, and Investment," Working Paper 7207, National Bureau of Economic Research.

Alesina, A., C. A. Favero, and F. Giavazzi (2012): "The output effect of fiscal consolidations," Working Papers 450, IGIER (Innocenzo Gasparini Institute for Economic Research), Bocconi University.

Alesina, A., and R. Perotti (1996): "Fiscal Adjustments in OECD Countries: Composition and Macroeconomic Effects," Working Paper 5730, National Bureau of Economic Research.

Alesina, A. F., and S. Ardagna (2012): "The design of fiscal adjustments," Working Paper 18423, National Bureau of Economic Research.

Blanchard, O., and R. Perotti (2002): "An Empirical Characterization Of The Dynamic Effects Of Changes In Government Spending And Taxes On Output," *The Quarterly Journal of Economics*, 117(4), 1329–1368.

Bond, S., D. Harhoff, and J. van Reenen (2005): "Investment, R&D and Financial Constraints in Britain and Germany," *Annales d'Economie et de Statistique*, (79-80), 433–460.

Caballero, R. J., E. M. R. A. Engel, and J. C. Haltiwanger (1995): "Plant-Level Adjustment and Aggregate Investment Dynamics," *Brookings Papers on Economic Activity*, 26(2), 1–54.

Chirinko, R. S., S. M. Fazzari, and A. P. Meyer (1998): "What Do Micro Data Reveal About the User Cost Elasticity?: New Evidence on the Responsiveness of Business Capital Formation," Macroeconomics 9805011, EconWPA.

Cloyne, J. (2011): "What are the Effects of Tax Changes in the United Kingdom? New Evidence from a Narrative Evaluation," CESifo Working Paper Series 3433, CESifo Group Munich.

——— (2013): "Discretionary Tax Changes and the Macroeconomy: New Narrative Evidence from the United Kingdom," *American Economic Review*, 103(4), 1507–28.

Cloyne, J., and P. Surico (2013): "Household Debt and the Dynamic Effects of Income Tax Changes," Discussion paper.

Cummins, J. G., K. A. Hassett, and R. Hubbard (1996): "Tax reforms and investment: A cross-country comparison," *Journal of Public Economics*, 62(12), 237 – 273, Proceedings of the Trans-Atlantic Public Economic Seminar on Market Failures and Public Policy.

Giavazzi, F., and M. Pagano (1990): "Can Severe Fiscal Contractions be Expansionary? Tales of Two Small European Countries," Working Paper 3372, National Bureau of Economic Research.

Hayo, B., and M. Uhl (2013): "The macroeconomic effects of legislated tax changes in Germany," *Oxford Economic Papers*.

Johansson, e. a. (2008): "Taxation and Economic Growth," OECD Economics Department working papers 620, OECD.

Mertens, K., and M. O. Ravn (2009): "Empirical evidence on the aggregate effects of anticipated and unanticipated US tax policy shocks," Working Paper Research 181, National Bank of Belgium.

——— (2013): "The Dynamic Effects of Personal and Corporate Income Tax Changes in the United States," *American Economic Review*, 103(4), 1212–47.

Mountford, A., and H. Uhlig (2009): "What are the effects of fiscal policy shocks?," *Journal of Applied Econometrics*, 24(6), 960–992.

Pescatori, A., D. Leigh, J. Guajardo, and P. Devries (2011): "A New Action-based Dataset of Fiscal Consolidation," IMF Working Papers 11/128, International Monetary Fund.

Ramey, V. A. (2011): "Can Government Purchases Stimulate the Economy?," *Journal of Economic Literature*, 49(3), 673–85.

Romer, C. D., and D. H. Romer (2010): "The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks," *American Economic Review*, 100(3), 763–801.

Schwellnus, C., and J. Arnold (2008): "Do Corporate Taxes Reduce Productivity and Investment at the Firm Level?: Cross-Country Evidence from the Amadeus Dataset," OECD Economics Department Working Papers 641, OECD Publishing.

Seiler, C. (2012): "The Data Sets of the LMU-ifo Economics & Business Data Center - A Guide for Researchers," Ifo Working Paper Series Ifo Working Paper No. 138, Ifo Institute for Economic Research at the University of Munich.

Uhl, M. (2013): "A History of Tax Legislation in the Federal Republic of Germany," MAGKS Papers on Economics 201311, Philipps-Universitarburg, Faculty of Business Administration and Economics, Department of Economics (Volkswirtschaftliche Abteilung).

Zwick, E., and J. Mahon (Working Paper): "Do Financial Frictions Amplify Fiscal Policy? Evidence from Business Investment Stimulus," .

8 Appendix

Narrative & Investment data

This section shows some examples of tax changes as discussed and classified in Uhl (2013). For our purpose of analyzing the effect of exogenous fiscal tax changes on investment we revise all structural and consolidation tax measures in Uhl and reclassify them accordingly in "endogenous" and "exogenous" measures.

An example for an exogenous structural tax measure is given by shock number 20 in Uhl (2013) ,"Gesetz zur Fortentwicklung der oekologischen Steuerreform". It corresponds to the continuation of the ecological tax reform, published on 22 December 1999, with a total budgetary impact of 10,635 billion Euros it represents a tax measure with structural motivation that is included in our analysis. Even though the revenues from the original ecological tax reform were aimed at reforming the retirement scheme in Germany from a pure pay-as-you go system to a more capital oriented system (the so-called "Riester Rente"), and hence might have indirect impact on investment, the continuation law discussed here did not directly contribute to the structural reform of the pension scheme, and revenues were not used to reduce the contribution rates to the social security system. The main argument that dominated the parliamentary debate was that that additional block grants were used to avoid future increases. We label the tax measure structural and include it in our analysis.

On the contrary, shock number 28 in Uhl (2013) " Gesetz zur Senkung der Steuersaetze und zur Reform der Unternehmensbesteuerung", represents a good example of structural shock that we consider endogenous, differently from Uhl (2013) . It refers to a law that has the objective to decrease taxes and reform company taxation (published in October 2000). This law implemented one of the most extensive tax reforms in Germany and substantially reduced income - and corporate tax burden. Furthermore the corporate tax imputation system was replaced by a 50 percent income taxation rule. The introduction of the bill clearly postulated that the motivation behind the law is to promote growth and employment by reducing the tax burden. Tax reductions were supposed to stimulate consumption, employment and investment. Therefore we do not included it in our analysis as it is directly aimed at increasing firm investment activity.

Finally, a good example for a consolidation shock is given by shock number 62 in Uhl (2013), a law published in March 1981, with the objective to increase petroleum tax and taxes on spirits (Mineraloel und Branntweinsteuer-Aenderungsgesetz

1981). As pointed out in Uhl (2013), the main motivation behind the law was budgetary consolidation. Although structural effects cannot be excluded completely (in order to improve the structure of tax revenues), consolidation considerations dominated the discussion.

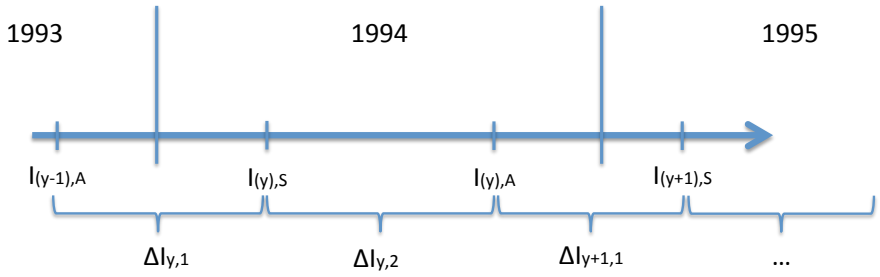

Figure 6: Timing of the half-yearly investment survey

Tax shocks: Number in Uhl (2013)	Parliament Publication date	Parliament Draft date	Newspaper coverage Discussion date	Comments
No 5. Law for the continuation of the legal situation 2006 for commuter's tax allowances	Dec-08	Mar-2009, Constitutional court ruling(Dec-2008)	Oct-07	Case rulings in 2007 made clear that the constitutional court likely declares the previously introduced measure for the "Pendlerpauschale" unconstitutional.
No 10. Tax reform act 2007	Jul-06	May-06	Nov-05	Negotiation of the coalition agreement in November 2005 defined list of important changes, amongst others the commuter's allowance.
No 12 .Law for limitation of the loss incorporation in the context of tax deferral	Dec-05	Nov-05	Apr-04 to Jun-05	Already in April 2004 changes in the classification of certain type of life insurances. End of 2004, same applies to closed real estate investment funds. Law as extension of these measures.
No 13. Law to increase tax compliance	Dec-03	Jul-03	Feb-03	Presentation of a white paper by the Financial Ministry in February 2003 to increase the declaration of unreported earnings from abroad.
No 15. Reform of the retirement income (AltEinkG)	Jul-04	Dec-03	Mar-03	Commission established in 2002. Report on the reformation of the pension scheme published in 2003.
No 16. Act for the change of the tobacco tax and other consumption taxes	Dec-03	Jul-03	May-03	First press news in May 2003, however possibility to increase consumption taxes due to financial situation has been already discussed in coalition negotiations after the federal elections in autumn 2002.
No 20 Law for the continuation of the ecological tax reform	Dec-02	Nov-02	Jun-01	Discussion of the further development of the ecological tax.
No 24. Law for the reform of the pension insurance and the promotion of capital pension schemes (AVmG)	Jun-01	Nov-00	Early 2000	Discussion of aging society and unsustainability of the Pay-as-you-go pension scheme in early 2000. Presentation of the pension scheme reform in May 2000.
No 42. Law for the new regulation of the interest taxation	Nov-92	Apr-92	Jan-92	Press notes on the discussion of the consistency with the constitution of the revised interest taxation in early January 1992.
No 44. Law for the implementation of the federal consolidation package (FKPG)	Jun-93	Mar-93	Feb-91	Coalition agrees on substantial tax increases in order to finance the burden of the German reunification. Biggest impact has the solidary surcharge on income and wages.

Table 2: Previous news coverage of fiscal shocks

Summary statistics and regression tables

This section presents evidence for the representativeness of our sample data for the overall manufacturing sector in Germany. We compare aggregate firm level data, obtained as log difference of total change at time t and time t-1 (d_inv_t) and a size-weighted average measure of investment changes (d_inv_a_w), with the benchmark for realized investment changes (gross fixed capital formation data obtained from STAN Industry Rev.3 2008 (OECD).

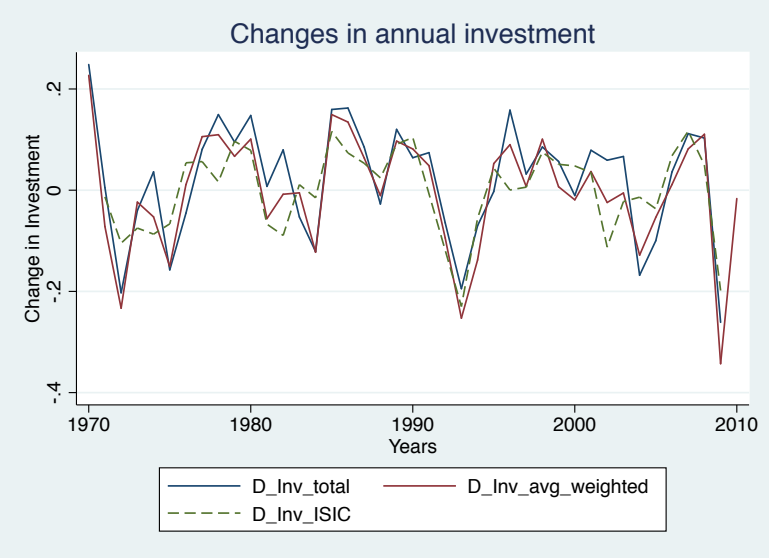

Figure 7: Change in aggregate investment: STAN vs. sample aggregation

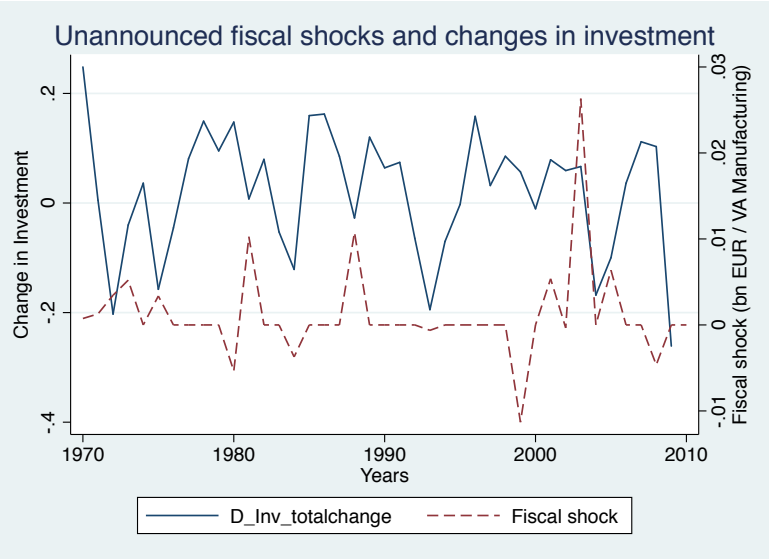

Figure 8: Change in aggregate investment vs. exogenous fiscal shock series

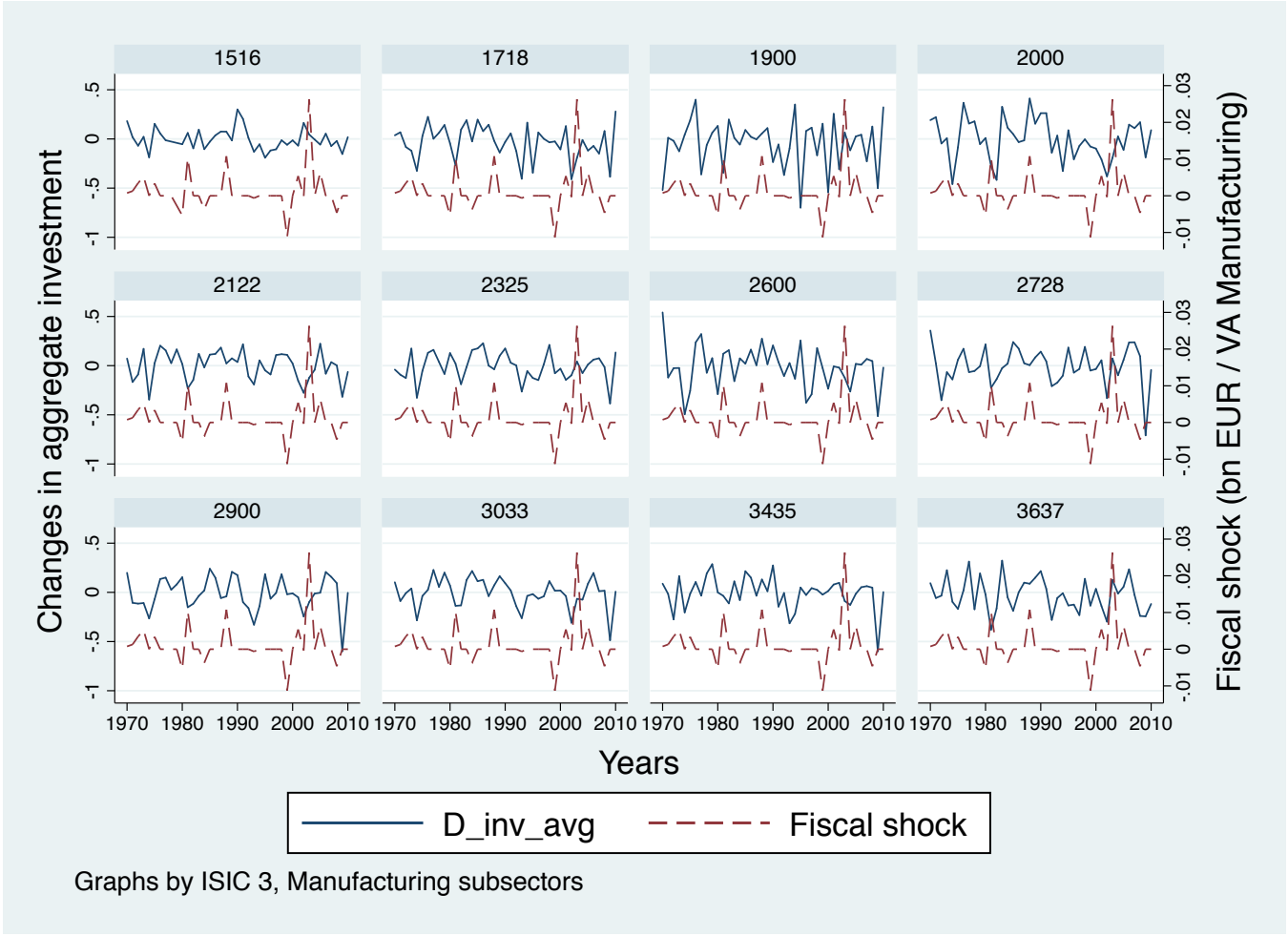

Figure 9: Change in aggregate investment in ISIC3 sub sectors vs. exogenous fiscal shock series

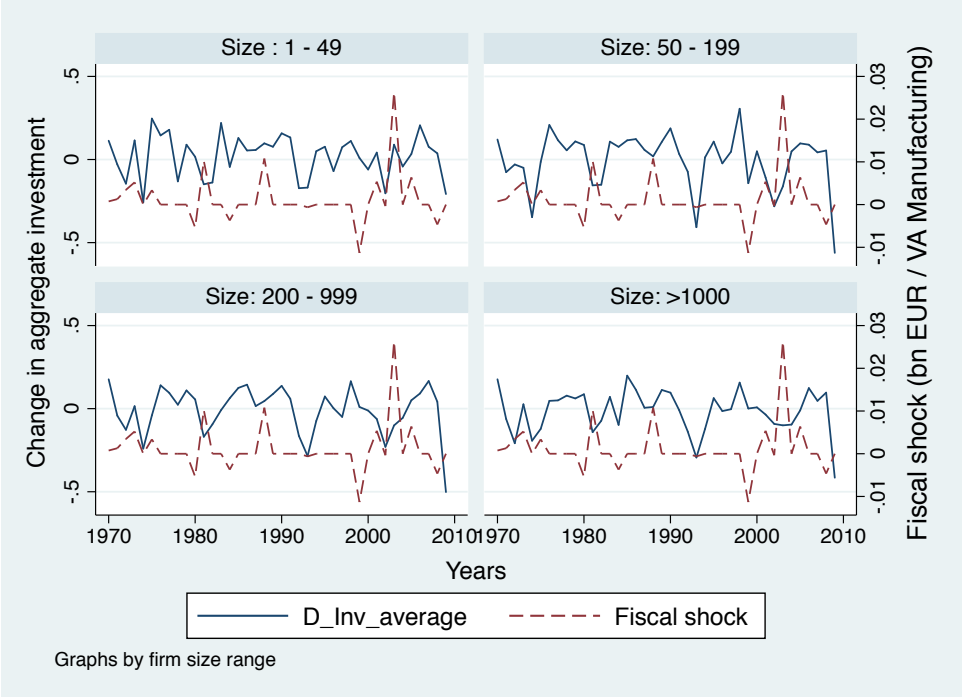

Figure 10: Change in aggregate investment by size class vs. exogenous fiscal shock series

Table 3 and Table 4 present results from the aggregate VAR analysis and provide evidence that the shock series cannot be predicted by macroeconomic variables or lagged investment changes. On the other hand, all announced shocks at time t seem to have an impact on changes in investment (Table 4); the null hypothesis of no granger causality can be rejected at the 10% significance level.

Equation	Excluded	chi2	df	Prob > chi2
Exog. fiscal shock	Interest rate (3month)	0.003	1	0.959
Exog. fiscal shock	GDP growth	0.689	1	0.407
Exog. fiscal shock	Change in investment	0.172	1	0.678
Exog. fiscal shock	ALL	1.461	3	0.691
D.investment	Exog. fiscal shock	0.020	1	0.887
D.investment	Interest rate (3month)	1.426	1	0.232
D.investment	GDP growth	1.620	1	0.203
D.investment	ALL	2.732	3	0.435

Exogenous fiscal shock and investment change (39 obs.)

Equation	Excluded	chi2	df	Prob > chi2
Endog. fiscal shock	Interest rate (3month)	2.572	1	0.109
Endog. fiscal shock	GDP growth	3.447	1	0.063
Endog. fiscal shock	Change in investment	2.141	1	0.143
Endog. fiscal shock	ALL	6.283	3	0.099
D.investment	Endog. fiscal shock	0.297	1	0.586
D.investment	Interest rate (3month)	1.040	1	0.308
D.investment	GDP growth	1.964	1	0.161
D.investment	ALL	3.028	3	0.387

Endogenous fiscal shock and investment change (39 obs.)

Table 3: Granger causality test based on 4 variable VAR

Dependent variable: Exogenous fiscal shock	beta	se
L.1 Change in investment	-2.626	(2.108)
L.2 Change in investment	1.670	(1.942)
L.1 GDP	0.000	(0.000)
L.2 GDP	0.000	(0.000)
Observations	39	
Pseudo R2	0.06	

For the sample period 1970-2010 there are 6 negative adjustment, 25 periods of no action and 10 years with positive shocks.

Dependent variable: Endogenous fiscal shock	beta	se
L.1 Change in investment	-0.335	(-2.005)
L.2 Change in investment	4.766**	(-2.099)
L.1 GDP	0.000	(0.000)
L.2 GDP	0.000	(0.000)
Observations	39	
Pseudo R2	0.09	

For the sample period 1970-2010 there are 14 negative adjustment, 15 periods of no action and 12 years with positive shocks.

Table 4: Ordered Probit: Insample

Dependent variable: Exogenous fiscal shock	beta	se
L.1 GFCF	-2.93E-11	(7.81e-11)
L.2 GFCF	5.03E-11	(6.73e-11)
L.1 GDP	-.000	(0.000)
L.2 GDP	0.000	(0.000)
Observations	38	
Pseudo R2	0.05	

For the sample period 1970-2010 there are 6 negative adjustment, 25 periods of no action and 10 years with positive shocks.

Dependent variable: Endogenous fiscal shock	beta	se
L.1 GFCF	1.82E-10**	(8.15E-11)
L.2 GFCF	-7.13E-11	(6.71E-11)
L.1 GDP	-0.002**	(0.001)
L.2 GDP	0.001**	(0.001)
Observations	38	
Pseudo R2	0.12	

For the sample period 1970-2010 there are 14 negative adjustment, 15 periods of no action and 12 years with positive shocks.

Table 5: Ordered Probit: Official Statistics (OECD STAN)

Table 6: Revision in planned investment

Dependent variable: Revision in planned investment	(1) β / (SE)	(2) β / (SE)	(3) β / (SE)	(4) β / (SE)	(5) β / (SE)	(6) β / (SE)
F2.fiscal shock				−5.520***	−4.021*	−5.012**
				(2.076)	(2.148)	(2.175)
F.fiscal shock				−2.945*	−4.141**	−4.033**
				(1.769)	(1.830)	(1.928)
Fiscal shock	−1.601	0.103	−0.398	0.050	2.504	1.838
	(2.170)	(2.264)	(2.370)	(2.071)	(2.184)	(2.201)
L.fiscal shock	−2.573*	−3.750**	−2.149	−5.293***	−6.575***	−5.816***
	(1.544)	(1.755)	(1.892)	(1.631)	(1.691)	(1.901)
L2.fiscal shock	−1.587	−2.018	−0.794	−0.187	−0.325	0.289
	(1.681)	(1.815)	(1.833)	(1.679)	(1.790)	(1.839)
Dummy_autumn	0.095***	0.099***	0.102***	0.083***	0.084***	0.086***
	(0.014)	(0.015)	(0.015)	(0.014)	(0.015)	(0.015)
Dummy_crisis	−0.089***	−0.061***	−0.050**	−0.130***	−0.116***	−0.113***
	(0.018)	(0.020)	(0.021)	(0.020)	(0.021)	(0.023)
L.GDP	0.000	−0.000	−0.000*	0.000***	0.000*	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
L.3 month interbank rate	−0.030***	−0.042***	−0.031**	−0.025***	−0.026***	−0.027**
	(0.009)	(0.011)	(0.013)	(0.009)	(0.010)	(0.012)
L.Sales growth		0.090***	0.046		0.100***	0.061*
		(0.029)	(0.032)		(0.028)	(0.032)
Observations	25189	19525	19525	23151	19525	19525
R^2	0.006	0.009	0.007	0.007	0.009	0.006
Industry FE	Y	Y	N	Y	Y	N
Firm FE	N	N	Y	N	N	Y

* p < 0.1, ** p < 0.05, *** p < 0.01. Clustered standard errors at firm level in parentheses.

Table 7: Halfyearly: discussion date of the fiscal shock

Dependent variable:			
Revision in planned investment	(1)	(2)	(3)
	β / (SE)	β / (SE)	β / (SE)
F2.fiscal shock	−4.831**	−3.149	−3.268
	(2.452)	(2.551)	(2.645)
F.fiscal shock	1.566	1.632	0.933
	(2.265)	(2.287)	(2.406)
Fiscal shock	−4.132***	−3.418**	−3.941**
	(1.498)	(1.565)	(1.590)
L.fiscal shock	−1.548	−1.736	−1.810
	(1.510)	(1.564)	(1.651)
L2.fiscal shock	−1.910	−0.813	−1.012
	(1.816)	(1.888)	(1.892)
Dummy_autumn	0.081***	0.081***	0.085***
	(0.014)	(0.015)	(0.015)
Dummy_crisis	−0.116***	−0.100***	−0.097***
	(0.020)	(0.021)	(0.022)
L.GDP	0.000**	0.000	0.000
	(0.000)	(0.000)	(0.000)
L.3 month interbank rate	−0.017**	−0.018**	−0.020*
	(0.008)	(0.008)	(0.010)
L.Sales growth		0.095***	0.056*
		(0.028)	(0.032)
Observations	23151	19525	19525
R^2	0.007	0.008	0.006
Industry FE	Y	Y	N
Firm FE	N	N	Y

* p < 0.1, ** p < 0.05, *** p < 0.01. Clustered standard errors at firm level in parentheses.

Table 8: Annual: realized investment change

Dependent variable:					
Investment growth	(1)	(2)	(3)	(4)	(5)
	β / (SE)	β / (SE)	β / (SE)	β / (SE)	β / (SE)
F2.fiscal shock	2.659***	1.239	0.584	2.126*	3.085
	(0.922)	(0.940)	(0.945)	(1.150)	(2.022)
F.fiscal shock	0.895	0.056	−0.020	0.642	4.563**
	(0.896)	(0.904)	(0.910)	(1.208)	(1.923)
Fiscal shock	−8.949***	−8.502***	−8.724***	−8.789***	−2.359
	(0.952)	(0.960)	(0.960)	(1.245)	(1.811)
L.fiscal shock	−2.901***	−4.682***	−4.853***	−1.704	−7.072***
	(0.849)	(0.876)	(0.883)	(1.056)	(1.824)
L2.fiscal shock	0.858	−1.757*	−2.164**	0.529	−3.275*
	(0.941)	(0.986)	(0.993)	(1.248)	(1.895)
Dummy_90		−0.270***	−0.276***		
		(0.018)	(0.018)		
Dummy_crisis		−0.098***	−0.097***	0.065	
		(0.032)	(0.036)	(0.051)	
L.GDP		0.000***	0.000***	−0.000	0.000***
		(0.000)	(0.000)	(0.000)	(0.000)
L.3 month interbank rate		−0.028***	−0.028***	−0.047***	−0.047***
		(0.002)	(0.003)	(0.009)	(0.005)
L.Sales growth			0.032	0.053	0.000
			(0.031)	(0.034)	(0.056)
Observations	43738	43738	42046	23024	19022
R^2	0.003	0.007	0.008	0.007	0.013
Industry FE	Y	Y	Y	Y	Y

* p < 0.1, ** p < 0.05, *** p < 0.01. Clustered standard errors at firm level in parentheses.

Table 9: Heterogeneous effects: Tax type

Dependent variable: Investment growth	(1) β / (SE)	(2) β / (SE)	(3) β / (SE)	(4) β / (SE)
Income tax	−13.956***	−18.096***		
	(2.298)	(1.925)		
L.Income tax	−5.670**	−5.808***		
	(2.403)	(1.963)		
Property and Corp tax	−7.958***		−12.959***	
	(2.052)		(1.575)	
L.Property and Corp tax	0.947		−6.606***	
	(2.037)		(1.474)	
Consumption tax	0.176			−6.754***
	(2.998)			(2.533)
L.Consumption tax	−17.392***			−17.800***
	(3.044)			(2.550)
Dummy_90	−0.225***	−0.213***	−0.239***	−0.191***
	(0.016)	(0.015)	(0.016)	(0.015)
Dummy_crisis	−0.227***	−0.207***	−0.240***	−0.216***
	(0.017)	(0.016)	(0.016)	(0.016)
L.GDP	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
L.3 month interbank rate	−0.023***	−0.026***	−0.027***	−0.024***
	(0.002)	(0.002)	(0.002)	(0.002)
L.Sales growth	−0.002	−0.002	0.002	0.006
	(0.022)	(0.022)	(0.022)	(0.022)
Observations	54261	54261	54261	54261
R^2	0.008	0.007	0.007	0.006
Industry FE	Y	Y	Y	Y

* p < 0.1, ** p < 0.05, *** p < 0.01. Clustered standard errors at firm level in parentheses.

Table 10: Heterogeneous effects: direct vs. indirect taxes

Dependent variable:			
Investment growth	(1)	(2)	(3)
	β / (SE)	β / (SE)	β / (SE)
Direct taxes	−9.720***	−10.228***	−9.869***
	(1.086)	(1.114)	(1.102)
L.Direct taxes	−0.906	−1.641	−0.959
	(1.081)	(1.106)	(1.087)
Indirect taxes	3.054	2.449	3.298
	(3.053)	(3.097)	(3.160)
L.Indirect taxes	−16.265***	−15.903***	−16.055***
	(2.778)	(2.822)	(2.844)
Observations	53164	53164	53164
R^2	0.01	0.01	0.01
Controls	Y	Y	Y
Anticipated shocks	N	N	Y
Industry FE	Y	N	Y
Firm FE	N	Y	N

* p < 0.1, ** p < 0.05, *** p < 0.01. Clustered standard errors at firm level in parentheses.

Table 11: Results from GMM Model (Bond et. al (2003))

Dependent variable:			
Investment / Assets	(1)	(2)	(3)
	β / (SE)	β / (SE)	β / (SE)
L. Investment / Assets	0.222***	0.218***	0.236***
	(0.020)	(0.020)	(0.021)
Sales growth	0.294***	0.256***	0.254***
	(0.088)	(0.081)	(0.111)
L.Sales growth	0.145***	0.137***	0.153***
	(0.026)	(0.023)	(0.030)
L2.(Assets - Sales)	−0.103***	−0.100***	−0.124***
	(0.022)	(0.020)	(0.030)
F.fiscal shock			−0.094
			(1.001)
Fiscal shock			−1.461**
			(0.681)
L.fiscal shock			−1.101*
			(0.665)
Hansen (p-value)	0.01	0.05	0.13
Arellano-Bond (AR1)	-17.34	-17.72	-15.91
Arellano-Bond (AR2)	1.67	1.75	1.76
Observations	10761	10761	9524
Firms	1875	1875	1798
Year FE	Y	N	N
Aggregate controls	N	Y	Y

* p < 0.1, ** p < 0.05, *** p < 0.01. Estimation by SYS-GMM using the one-step estimator. Hansen test (p-value) for over identification restrictions reported. We follow the same selection of instruments as in Bond et. al (2003)

Table 12: Results from Diff-in-Diff: Energy tax

Dependent variable: Investment growth	(1) (β / (SE)	(2) (β / (SE)	(3) (β / (SE)
Energy tax X paper industry	3.608	3.410	6.921
	(13.553)	(13.554)	(14.011)
L.Energy tax X paper industry	−23.134*	−23.532*	−24.323**
	(12.269)	(12.272)	(12.289)
L2.Energy tax X paper industry	−20.403	−20.356	−20.427
	(12.683)	(12.697)	(13.105)
Energy tax	−1.535	−1.517	−3.133
	(8.436)	(8.435)	(8.806)
L.Energy tax	4.927	5.158	4.569
	(7.952)	(7.958)	(7.972)
L2.Energy tax	−10.747	−10.927	−12.773
	(9.005)	(9.020)	(9.230)
Pulp & Paper		0.039***	
		(0.013)	
Observations	12960	12960	12960
R^2	0.004	0.004	0.004
Controls	Y	Y	Y
Industry FE	N	Y	N
Firm FE	N	N	Y

* p < 0.1, ** p < 0.05, *** p < 0.01. Clustered standard errors at firm level in parentheses.

Table 13: Aggretated results: by ISIC 3 subsector

Dependent variable:				
Investment growth	(1)	(2)	(3)	(4)
	β / (SE)	β / (SE)	β / (SE)	β / (SE)
Fiscal shock	−3.689***	−3.689***	−2.301*	−2.278**
	(1.120)	(0.816)	(1.054)	(0.864)
L.fiscal shock	−2.674**	−2.682***	−2.279**	−2.958***
	(1.182)	(0.760)	(0.772)	(0.740)
L2.fiscal shock	−.635	−0.640	−1.091	−1.493
	(1.557)	(2.124)	(2.065)	(2.021)
Fiscal shock anticipated			−0.018	
			(0.689)	
L.fiscal shock anticipated			0.181	
			(0.421)	
L2.fiscal shock anticipated			2.011*	
			(0.989)	
Fiscal shock endog.				1.209*
				(0.649)
L.fiscal shock endog.				−1.129
				(0.679)
L2.fiscal shock endog.				−3.316***
				(0.589)
Dummy_90	−0.248***	−0.249***	−0.253***	−0.202***
	(0.0402)	(0.027)	(0.029)	(0.038)
Dummy_crisis	−0.151***	−0.151***	−0.176***	−0.1678***
	(0.043)	(0.037)	(0.046)	(0.034)
L.GDP_index	0.696***	0.697***	0.689***	0.589***
	(0.135)	(0.101)	(0.097)	(0.122)
L.3 month interbank rate	−.0171***	−0.171***	−0.0183***	−0.011*
	(0.006)	(0.004)	(0.004)	(0.005)
Observations	465	465	465	465
R^2	0.11	0.11	0.12	0.16
Industry FE	N	Y	Y	Y

* p < 0.1, ** p < 0.05, *** p < 0.01. Clustered standard errors at firm level in parentheses.