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Essays in Macroeconomics and Fiscal Policy

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Contents

Title Page i

Table of Contents ii

Abstract v

Acknowledgements vii

1 The Role of Corporate Tax Policy on Monetary Effectiveness: A Quasi-Experimental Approach **1**

1.1 Introduction 1

1.2 Dataset 6

1.2.1 Firm level variables 6

1.2.2 Summary statistics 13

1.2.3 Monetary Policy Shocks 14

1.3 Empirical Framework 15

1.3.1 Empirical Specification 15

1.3.2 Results 18

1.3.3 Robustness 20

1.4 Theoretical Model 24

1.4.1 Households 24

1.4.2 Production 25

1.4.3 Government 29

1.4.4 Model Results 29

1.5 Conclusion 32

1.6 Figures . . . 33
1.7 Tables . . . 70
1.8 Appendix . . . 73
1.8.1 Data Construction . . . 73
1.8.2 Additional Figures . . . 78
1.8.3 Model Appendix . . . 83

2 Dynamic Effects of US Corporate Tax Policy: Firm-Level Evidence 91
2.1 Introduction . . . 91
2.2 Dataset . . . 96
2.2.1 Firm Level Variables . . . 96
2.2.2 Corporate Tax Variables: Statutory Rate and Investment Tax Credit . . . 97
2.3 Empirical Framework . . . 100
2.3.1 Empirical Specification . . . 100
2.3.2 Baseline Results . . . 101
2.3.3 Heterogeneity Analysis . . . 102
2.3.4 Robustness . . . 105
2.4 Conclusion . . . 106
2.5 Figures . . . 107
2.6 Tables . . . 117
2.7 Appendix . . . 119
2.7.1 Data Definitions . . . 119
2.7.2 Additional Figures . . . 121

3 Asymmetric Effects of Monetary Policy in Micro-data 124
3.1 Introduction . . . 124
3.2 Dataset . . . 128
3.3 Empirical Framework . . . 131
3.3.1 Empirical Specification . . . 132
3.3.2 Baseline Results . . . 133
3.3.3 Robustness . . . 135

3.3.4 Heterogeneity Analysis 137
3.4 Conclusion 143
3.5 Figures 145
3.6 Tables 160
3.7 Appendix 161

Bibliography **183**

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Essays in Macroeconomics and Fiscal Policy

Abstract

This dissertation is a compilation of three essays focusing on the following topics: understanding the heterogeneous effects of corporate tax policy on businesses, understanding how tax policy influences the effectiveness of monetary policy, and exploring asymmetric effects of monetary policy. In doing so, I draw on frontier methods in quasi-experimental design paired with rich micro-level datasets that allow me to study the effectiveness of aggregate policy measures. I also complement my empirical tools with theoretical models to pin down transmission mechanisms.

The first chapter examines the impact of corporate tax policy in altering the efficacy of monetary policy. In recent decades there has been much focus in the macroeconomics literature on understanding heterogeneous effects of monetary and fiscal policy. Although numerous empirical studies have explored monetary policy transmission via various financial and non-financial factors, the theoretical frontier has continuously highlighted that separate analysis of monetary and fiscal policy may overlook important policy interactions. In this chapter, I use narratively-identified variation in marginal tax reforms in the US and show that the average impact of monetary policy depends on preceding corporate tax changes. Specifically, I find that monetary policy is more effective on employment, sales and investment for firms facing an increase in the statutory rate relative to those with stable statutory tax rates. Moreover, I document that monetary policy is least effective on firms that received marginal tax cuts. The empirical findings are rationalized using a New Keynesian model featuring capital and corporate taxes. The theoretical model and empirical results demonstrate that tax shifts may significantly amplify or dampen the effectiveness of monetary policy.

The second chapter of my dissertation analyzes post-WWII US corporate tax policy changes. This is part of joint work with James Cloyne and Paolo Surico, where we build two new proxies on marginal tax rate and investment tax credits and investigate the causal effects of corporate tax changes on business investment dynamics. We find that both marginal tax rates and investment

tax credits are tools genuinely effective across the firm distribution, yet on average marginal tax rate changes are more effective at raising firms' investment rates than investment tax credits. Exploiting micro-level heterogeneity, we also show that small, low-leverage, and high-growth firms are more sensitive to corporate tax policy changes, which suggests that firm characteristics may have a role in propagating the effectiveness of these tax tools. Overall, our findings provide a comparison of the two most common tools used in corporate tax policy and highlight the role of firm characteristics in studying US corporate tax policy.

Lastly, Chapter 3 documents the first micro-level evidence on the asymmetric effects of monetary policy in the US. Various studies on monetary transmission literature have documented non-linear effects of monetary policy at the aggregate level. However, there are few studies that use micro-data to explore sign-dependent monetary effectiveness at the disaggregate level. Focusing on firm-level data from 1980q3 to 2016q3, I find that monetary contractions have larger effects on firm-level employment, investment and sales than monetary expansions. These results are consistent with the aggregate findings in the literature and are robust to sample selection, sector-level analysis and alternative monetary policy shocks. Furthermore, I examine the role of financial characteristics in propagating the asymmetric effects of monetary policy. My findings show a larger employment and investment response to a monetary tightening for firms with small size, low leverage, high liquidity or no-dividend paying status. For the employment results, I also find firms with low leverage or high liquidity to respond more to monetary expansions; however, this effect is much weaker as compared to monetary contractions. These results provide evidence that financial characteristics have a role in propagating the asymmetric effects of monetary policy.

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Chapter 1

The Role of Corporate Tax Policy on Monetary Effectiveness: A Quasi-Experimental Approach

1.1 Introduction

Corporate taxes and monetary policy have been at the center of policy debates in recent decades. However, despite the intertwined nature of these two policies, most of the current empirical literature studies them in isolation. This paper aims to explore the intersection of these two policies by asking the following questions. Do underlying tax structures matter for monetary effectiveness? Can changes in the US corporate taxes weaken the effectiveness of monetary policy? The answers to these questions are crucial for public policy and understanding how tax policy influences monetary policy outcomes.

This paper presents the first empirical evidence on how corporate tax policy influences the effectiveness of monetary policy in micro-data. I employ macro identification of exogenous tax changes using narrative methods,[1] and build a novel quasi-experimental design testing the impact of underlying tax structures on monetary effectiveness. The main findings show that preceding tax cuts (increases) cause consequent monetary policy to be less (more) effective. In other words, changes in tax policy generate considerable variation in monetary effectiveness. I also build a conventional New Keynesian model featuring capital and corporate taxes that rationalizes the empirical find-

[1]Narrative accounts use historical records such as archives of congressional reports and presidential speeches to isolate exogenous discretionary policy interventions.

ings. Both theoretical model and empirical findings demonstrate that tax policy could influence monetary policy effectiveness.

There are a set of empirical challenges in identifying the impact of monetary policy conditional on tax policy. First of all, aggregate policy measures are usually actions taken in response to prevailing economic conditions, hence it is challenging to isolate causes and effects of these policy changes. Second, identifying monetary and tax policy jointly in pure time-series setting may not yield sufficient variation to study the interaction of these two policies.

I tackle these issues using distinct institutional features of monetary and tax policy and a novel empirical design employing micro and macro data. First of all, monetary and tax policy has distinct cross-sectional features which can be pinned down in micro-data. Specifically, monetary policy is levied uniformly across the distribution of firms, whereas tax policy has considerable cross-sectional variation with multi-brackets nature in the tax code. From 1968 to 2006, US corporate tax code incorporated 12 historically stable income brackets and sizable cross-sectional variation across these brackets. Hence, I use statutory rate changes on different income brackets to pin down tax treatment variation across firms.[2] Second, I use Romer and Romer (2010, 2009) and Mertens and Ravn (2012, 2013) tax narrative accounts which provide historical records to isolate exogenous discretionary tax reforms in the United States. Combining the narrative tax reforms with corporate statutory rate changes allows me to pin down time-series and cross-sectional variation in statutory tax changes across income brackets over time.

Monetary and tax policies also differ significantly in terms of their effectiveness window. Tax reforms occur at much lower frequency and have longer spans. In contrast, monetary policy changes occur at a much higher frequency than tax reforms. This feature allows me to test over-time effects of tax reforms on monetary effectiveness. In sum, I use cross-sectional and time-series features of monetary and tax policy to design a novel quasi-experimental research design.

I employ annual Compustat dataset for the period 1969-2006, which contains rich income statement and balance sheet information on 20,798 publicly traded US firms. I combine this data with monetary policy shocks, constructed by Romer and Romer (2004) and updated by Wieland and

[2]This approach builds on the dataset used in Chapter 2. Chapter 2 studies the effects of corporate tax changes on firm capital formation, whereas Chapter 1 studies the implications of firms' tax treatments on monetary effectiveness.

Yang (2020) and narratively-identified IRS statutory rate changes in corporate income taxes. Then, I construct a taxable income measure and sort firms into respective income brackets over time. The final data has disaggregated statutory rate changes, matched to micro data, which allows me to pin down the tax treatment of firms and test its implications on monetary effectiveness. Last, I use flexible research design of local projections (LP) (Jordà, 2005) which allows me to test the effectiveness of monetary policy conditional on firms' tax treatment using a variety of specifications.

The key results can be summarized as follows. First, changes in tax policy play an important role in monetary policy transmission. Specifically, firms facing a 1 percentage point tax increase cut employment by .25% more than firms with constant taxes in response to a contractionary monetary policy shock. The same group of firms also show on average 0.6% and 0.14% larger response in investment and sales to monetary policy, respectively. Second, I find that monetary contractions are least effective on firms receiving persistent tax cuts. As compared to firms with stable statutory tax rates, the firms facing tax cuts have considerably muted response in employment, investment and sales. These findings suggest that the direction of tax shifts may significantly amplify or reduce the effectiveness of monetary policy. In other words, tax reforms could explain a sizable amount of variation in monetary policy effectiveness.

The findings of this paper are robust to a series of checks including alternative empirical strategies, additional controls and underlying assumptions. In addition, I test the role of firm financial and non-financial characteristics in the baseline responses. These estimates suggest that the baseline results are robust to various firm characteristics, although some factors such as being small, holding high liquidity and low leverage significantly contribute to indirect effects of taxes on monetary effectiveness.

Taken together, this paper makes two contributions to the literature. First, the results of this paper presents the first empirical evidence, to the best of my knowledge, quantifying the nature of interaction between monetary and corporate tax policy in the micro-data. This is a previously unexplored dimension that is different than the earlier approaches used in the monetary transmission literature. The main finding of my paper is that the effectiveness of monetary policy depends significantly on the dynamics of tax system. In other words, depending on the nature of preceding tax changes, monetary policy can be more or less effective than it otherwise would be.[3] Second and more importantly, my findings can shed light on weaker effects of monetary policy documented in the last decades (Boivin et al., 2010). Bringing together the historical downward trend in corporate taxes and the evidence from my findings may account for the muted effects of monetary policy in recent decades.

Related Literature This paper connects to literatures on transmission of monetary policy, tax narrative accounts and interaction of monetary and fiscal policy. First, there is a growing literature studying heterogeneity in the effects of monetary policy using firm-level data. These papers provide evidence on how financial and non-financial factors like balance sheet conditions (Gertler and Gilchrist, 1994; Ottonello and Winberry, 2020), firm age-dividend status (Cloyne et al., 2019), liquidity conditions (Jeenas, 2019; Fazzari et al., 1988; Kashyap et al., 1994; Gilchrist and Himmelberg, 1995) and collateral assets (Bahaj et al., 2020) play a role in the transmission of monetary policy. I contribute to this literature by providing the first empirical evidence documenting the role of corporate tax policy in changing firms' responsiveness to monetary policy. This is a new dimension that has not been addressed previously in the literature and my results provide evidence for tax policy induced variation in monetary policy outcomes.

Second, this paper connects to narrative macroeconometric studies. Employing tax reforms for identification, recent studies estimate short and medium run effects of tax policy on real economic activity (Romer and Romer, 2010; Mertens and Ravn, 2013, 2012; Mertens and Olea, 2018; Zidar, 2019; Cloyne, 2013; Hayo and Uhl, 2014; Cloyne et al., 2021; Barro and Redlick, 2011; Hussain and Liu, 2018; Nguyen et al., 2016). I contribute to this literature by providing the first analysis using corporate tax narratives joint with monetary policy in the micro-data.[4] Among the tax narrative literature, Mertens and Olea (2018), Zidar (2019) and Cloyne et al. (2021) share the most similarities to my analysis in their use of counterfactual statutory rate analysis. Both Zidar (2019) and Mertens and Olea (2018) examine the effects of personal income tax changes at an aggregate

[3] See also the ECB speech by J. M. Gonzlez-Pramo in 2005 that highlights this aspect with regard to coordination of fiscal and monetary policy in European Union countries.

[4] Note that recent work by Cloyne et al. (2020) also studies tax and spending narrative joint with monetary policy. Cloyne et al. (2020) highlights the role of monetary policy on the effectiveness of fiscal policy using international data.

level, either across income distribution or regions.[5] Instead, my paper is a micro-data application on corporate taxes using narrative identification.[6] In this respect, this paper connects more to my earlier work, Cloyne et al. (2021), in which we build narratively-identified changes in statutory tax rates. My paper differs from Cloyne et al. (2021) with its topic and empirical strategy. In Cloyne et al. (2021), we study the effects of corporate tax changes on firm capital formation and employment growth, whereas my paper explores the intersection of tax and monetary policy by questioning the short and medium term effects of tax changes on monetary effectiveness.

Third, this paper also connects to the literature focusing on regime dependent effects of monetary policy. Studies such as Tenreyro and Thwaites (2016); Auerbach and Gorodnichenko (2012); Berger and Vavra (2014); Angrist et al. (2018); Matthes and Barnichon (2015); Owyang et al. (2013) explore whether policy interventions have differential effectiveness based on whether the economy is in a recession or expansion, or based on uncertainty. My paper shares some characteristics with these studies, yet also has a fundamentally different approach. First, most of these studies estimate impulse response functions in regime-switching environments where regimes are computed at the aggregate level with transition probabilities across regimes. In my paper, a tax regime starts following a discrete event of persistent statutory rate changes. Moreover, since I use disaggregated changes in statutory rates, my approach can be categorized as a micro-regime application where every firm gets exposed to its own shock. Taken together, my paper addresses the state dependent effects of monetary policy in the aftermath of tax shifts. However, as compared to the aggregate literature, I define regimes at the micro level and do not employ regime switching models.

Last, there is an influential strand of literature theoretically modeling the interaction of fiscal and monetary policy rules (Canzoneri et al., 2010; Chen et al., 2020; Davig and Leeper, 2011; Sargent and Wallace, 1984; Aiyagari and Gertler, 1985; Leeper, 1991). These papers study policy outcomes under alternative coordination of monetary and fiscal policy. Specifically, they characterize a non-cooperative game between the government and central bank to consider implications of active and

[5] Mertens and Olea (2018) analyze the changes in *average* marginal tax rate across years and study the elasticity of reported income along the income distribution. Zidar (2019) constructs regional tax liability changes in personal income tax changes and estimate employment effects using income distribution differences across states.

[6] This is an important feature that differs my work from Mertens and Olea (2018) and Zidar (2019) as it allows me to capture clean treatment effects. Moreover, it deals with the concerns for general equilibrium effects that may arise in studies using aggregate data. Note that the decomposition approach is particularly feasible in corporate taxation due to the historically stable brackets.

passive monetary and fiscal policy interactions in altering the effectiveness of alternative stimulus policies as well as determining price stability. My paper diverges from these studies in that it does not explicitly model the endogenous interaction of fiscal and monetary policy. Instead, I study effectiveness of monetary transmission conditional on underlying tax structures. In order to motivate my empirical predictions, I also present a medium scale New Keynesian model featuring capital and corporate income taxes. The theoretical predictions reconcile the findings of the quasi-experimental exercise that preceding persistent tax policy interventions can influence the effectiveness of monetary policy.

The rest of the paper is organized as follows. Section (1.2) discusses firm-level data, tax policy variables and monetary policy shocks. Section (1.3) presents the empirical strategy, main results and robustness checks. Section (1.4) provides the theoretical model and conducts an experiment to motivate the empirical results. Section (1.5) concludes.

1.2 Dataset

1.2.1 Firm level variables

This paper uses the annual Compustat database on the publicly traded C corporations in North America. Compustat provides high-quality information on balance sheet and income statement components of active and inactive companies. The sample spans from 1969 to 2006 and consists of 205,342 firm-by-year observation from a total of 20,357 firms. The main explanatory variables I analyze are the number of employees (*emp*, Compustat item 29), investment (defined as capital expenditures, *capx*, Compustat item 128) of firm j in period t and net sales (*sale*, Compustat item 12). Other variables of interest are book value of total assets (*at*, Compustat item 6), liquidity ratio[7], leverage (total debt divided by the book value of total assets)[8] and cash dividends paid (*dv*, Compustat item 127). Details of data construction is discussed in Appendix.

Using Compustat data in this paper is advantageous for a couple of reasons. First, Compustat is a

[7]Liquidity ratio is calculated as the share of cash and short term investments (*cheq*, Compustat item 1) to total assets.

[8]Total debt is calculated as the sum of debt in current liabilities (*dlc*, Compustat item 34), and long term debt (*dltt*, Compustat item 9).

long enough panel to study within-firm variation. I analyze thirthy-seven years of annual firm level data where the average firm is observed for about 11 years. Second, Compustat has a rich cross-sectional dimension. The rich balance-sheet information in Compustat allows me to construct a new taxable income measure, test alternative hypotheses and conduct heterogeneity analysis.

There are a few limitations of using Compustat data. First, Compustat only consists of publicly held companies, hence the estimates represent only the effects of the corporate tax code on the behavior of publicly traded C-corporations. Second, despite the good coverage across different sized firms, Compustat data may disproportionately feature large companies, and therefore may underrepresent small firms. Last, Compustat is mainly a report of financial statements, hence the gross income and tax variables are not reported for the purposes of tax books. Although a growing public finance literature (Kleven et al., 2016) increasingly suggests the use of third-party information on business records in developed countries to be legitimate and accurate with little discrepancy between the tax reports and the third-party information[9], extracting the corporate tax variable out of Compustat (despite being second-party information) may still be subject to measurement error and bias.

Since both marginal tax rates and taxable income are unobserved data in the generic financial statements in Compustat, I construct a taxable income measure using balance sheet variables. The taxable income variable is measured using the following:

$$\begin{aligned} TI = \text{Net Income} - \text{Interest Paid} - \frac{\sum_{n=t-3}^{n=t-1} \text{Tax Loss Carryforward}_n}{3} + \text{Special items} \\ - \text{Depreciation and Depletion Expense} + \frac{\text{Income from extraordinary items}}{(1-mtr)} \end{aligned} \tag{1.1}$$

According to the definition, taxable income is generated using firms' profits net of allowable cost deductions.[10] The definition mainly builds on existing definitions in the literature (Graham, 1996; Blouin et al., 2010; Shevlin, 1990), and further supplements them with the 1984 IRS instructions on corporate taxes.[11] The definition accounts for firms' incentives to allocate income across time

[9]Kleven et al. (2016) further suggests this transparency to be especially true for the large firms where the tax enforcement through auditing is strong.

[10]The deductions are guided by the IRS instructions to the extent of data availability.

[11]See the 1984 IRS corporate income tax return form 1120-A.

through carryforwards and allows for forward-looking behavior. The goal is to generate a taxable income definition closest to the actual reports, to the extent of data availability. In Appendix 1.8.1, I detail the construction of taxable income as well as comparisons to Graham (1996) and Blouin et al. (2010) measures.

Corporate tax variable

This section constructs firm-level measure of exogenous statutory corporate tax changes over time. This is a two stage procedure. In the first stage, I select tax reforms from the tax narrative accounts of Romer and Romer (2009) focusing on corporate income tax reforms that have persistent statutory rate changes and are classified as *exogenous* by Romer and Romer (2009) and Mertens and Ravn (2013).[12] According to Romer and Romer (2009) categorisation, a tax reform is exogenous if its motivation is to address inherited budget deficits or to achieve some long-run goal such as increasing fairness or changes in philosophy of the government.[13] Hence, discarding the changes in tax liabilities that are related to the current state of the economy fulfills the required assumption on the orthogonality of tax reforms, and therefore form the exogenous series. Next, many corporate reforms are implemented with either delay or have gradual multiyear phase-ins such as Economic Recovery Tax Act of 1981 and Tax Reform Act of 1986. To strip the policy variation with elements of surprise, I focus on tax changes implemented within one quarter of their legislation to avoid *anticipation* effects (Mertens and Ravn, 2012). After the elimination of tax changes based on exogenous, unanticipated and persistent ones, the selection procedure yields 5 tax reforms between 1969 and 2006 with significant and immediate impact on corporate statutory tax rates. Table 1.1 lists these reforms together with their direct impact on the corporate statutory rate schedule.[14]

In the second step, I follow the approach in my earlier work Cloyne et al. (2021) and decompose the selection of Romer and Romer (2009, 2010) tax reforms into a panel of statutory rate changes across

[12]The narrative accounts use historical records of congressional reports and presidential speeches to construct exogenous discretionary tax shocks with descriptive information on size, timing and motivation. Mertens and Ravn (2013) disaggregated tax narratives are originated from Romer and Romer (2009) aggregate series and provide exogenous tax policy shocks on the US corporate and personal income tax separately.

[13]Similarly, a tax reform is considered endogenous if the tax reform is influenced by current economic conditions.

[14]There are 10 total number of statutory tax changes from 1969 and 2006.

taxable income brackets. Tracking the statutory rate changes across 12 distinct income brackets over years allows the 5 benchmark tax reforms to generate a large amount of variation in statutory tax rates. The details of this step are explained below in detail.

Corporate income tax code US corporate income tax code is a piecewise linear system where the taxable income is divided into brackets where marginal tax rates are fixed within but vary across these brackets. Table 1.2 provides an overview of the IRS historical statutory marginal tax rates across different taxable income brackets from 1968 to 2016.[15] This table reveals a number of unique features of the corporate tax code. First, corporate tax code has adhered to 12 historically *stable* taxable income brackets ranging from under 25,000 dollars to over 18.333 million dollars.[16] Second, the gradual rate structure has consistently been an important characteristic of corporate tax code since the late 1960s. Specifically, the Revenue Act of 1978, the 1986 Tax Reform Act[17] and the Omnibus 1993 shows examples of detailed size-dependent tax rates. Third, the tax legislations change the statutory rates in the same direction for all taxable income brackets, but there is considerable heterogeneity in the dose of tax changes. For example, the 1975 legislation suggests an 18 percentage point (ppt) decline in top statutory tax rate for firms with taxable income from $50,000 to $75,000 and a 2 ppt decline in the top statutory tax rate for firms with taxable income above $100,000.[18] Next, there is considerable variation in the tax cuts and tax increases. Of the five exogenous tax reforms, two (1984 and 1993) are tax increases and three (1979, 1982, 1987) are tax cuts.[19] Last, not all the tax brackets receive statutory rate shocks at every new legislation. For example, in the 1993 tax reform only large firms with income greater than $10 million faced a statutory tax change. All these features form the basis of my quasi-experimental research design exploiting time-series and cross-section variation in marginal tax rates.

Figure 1.1 sketches the basic research design. Consider an economy with alternative marginal tax

[15] See the full historical data on IRS historical Table 24.

[16] These brackets are $0 - $25,000, $25,000 - $50,000, $50,000 - $75,000, $75,000 - $100,000, $100,000 - $335,000, $335,000 - $1,000,000, $1,000,000 - $1,405,000, $1,405,000 - $10,000,000, $10,000,000 - $15,000,000, $15,000,000 - $18,333,000 above $18,333,000.

[17] 1986 Tax Reform Act implemented two step changes occurring in years 1987 and 1988.

[18] However, not surprisingly since the 18 ppt is only levied on the $25,000 income from $50,000 to $75,000, the dollar impact of the 18 percentage points cut is actually quite small.The liability impact of 18 ppt change is $4,500 at maximum. Note that this feature of varying progressiveness is an obstacle in terms of making direct comparisons of marginal tax rate changes across firms of different income brackets. In order to facilitate this type of comparison, I use a liability based measure across firms through time.

[19] The larger number of tax cuts are also due to the lack of indexation in the tax code.

rates, τ_1, τ_2, τ_3 and τ_4 on alternative taxable income brackets.[20] At time t, a new legislation is executed which changes the marginal tax rate for some brackets and leaves it unchanged for some others. I take this feature and form a quasi-experimental research design using variation in the direction and level of tax treatment across income brackets through time. My identification strategy has two layers. First, I exploit the direction of statutory rate changes where the treatment group consists of the firms which are in the taxable income brackets receiving a statutory rate change and the control group consists of the firms in the brackets that do not receive any change in statutory rate. Specifically, I generate a binary measure of tax treatment across income groups that pins down the direction of statutory tax rate change. Second, I explore tax treatment dose across the treated groups, which allows me to test the role of intensity of tax treatment across firms. The next section details the construction of the two measures and underlying identifications strategies.

Treatment in marginal tax rate

This section constructs a measure of statutory rate changes that deals with the endogenous nature of marginal tax rates. I follow the earlier methods in Mertens and Olea (2018), Zidar (2019) and Cloyne et al. (2021) where the estimated statutory change in year t is calculated as the difference between a counterfactual statutory rate calculated using year $(t-1)$ taxable income and year t rates and the actual year $(t-1)$ statutory tax rate. The use of the previous year's taxable income is to strip away the behavioral responses of firms in adjusting their income.[21] Equation 1.2 formulates Δmtr_t measure which is interpreted as the change in the statutory rate on an additional \$1 income earned today. This is a proxy for the change in statutory rate, with no income response.

$$\Delta mtr_t = \tau_t(TI_{t-1}) - \tau_{t-1}(TI_{t-1}) \tag{1.2}$$

I repeat this exercise for 12 distinct taxable income brackets from 1968 to 2006 and generate a new time-series measure capturing variation in statutory rate changes *across income brackets*. Figure

[20]The marginal tax rate is defined as the statutory rate that incurs on the additional dollar of income. Throughout the paper, I use marginal and statutory tax rates interchangably.

[21]On the corporate income side, the tax brackets are matched to brackets on a nominal basis at every year, hence I do not deflate the taxable income measure through time. Similarly in the liability changes, I use a *share* of liability change measure which also does not require deflating.

1.2 plots the output of this exercise where the y axis has the percentage point change in the statutory tax rate (Δmtr_t) of different income brackets from 1968 to 2006. Positive numbers show statutory rate increases and negative numbers show statutory rate cuts. In order to visualize the labels clearly, the figure is split across large and small firms such that Figure 1.2a(1.2b) covers firms with taxable income less (higher) than 1 million dollars. Figure 1.2 shows three interesting features of statutory corporate income tax changes. First, the figure uses cross section of income brackets which allows us to visualize time-series variation by brackets. For example, Figure 1.2a shows that firms with taxable income between $25,000 and $1,000,000 taxable income faced a statutory tax increase of 4.8 percentage points in the year 1968. The same group of firms also receive a tax cut of 3.6 and 1.2 percentage points in the following years in 1970 and 1971, respectively. Second, Figure 1.2 illustrates the variation difference within tax legislations. For instance, the Revenue Act of 1978 act lowered statutory tax rates across firms at four different rates for firms of different sizes. Third, both Figure 1.2a and Figure 1.2b suggest that there is sizable variation in the sign of treatment across different brackets. Particularly for large firms, there are three years of tax increases and five years of tax cuts that impact various brackets. The selected exogenous statutory rate changes in Romer and Romer (2009) and Mertens and Ravn (2013) are marked with stars and will be used in the baseline results. These also correspond to reforms summarized in Table 1.1. After generating different tax treatments across income brackets, I match the Compustat constructed taxable income variable to the right taxable income brackets which allows me to generate a firm level measure of tax treatment over time.[22]

Next, in order to study monetary policy in the aftermath of tax legislations, I define the term, *tax regime* to refer to periods following discrete and persistent tax policy changes in micro-data. Using the sign of tax change, I assign firms to three possible tax regimes: expansionary, contractionary and neutral. For example, if a firm has received a tax cut at year t, it enters an expansionary tax regime. Similarly, if a firm has received a tax increase at year t, it enters a contractionary tax regime. The firms that have not received any change in statutory rate are assigned to the neutral regime. This exercise labels the tax treatment of firms in the aftermath of persistent tax shifts. Once a firm is allocated to a new regime, unless an opposite tax change occurs in the subsequent

[22]This part of my methodology follows from my earlier work Cloyne et al. (2021).

years, the firm continues to stay in the relevant regime for up to five years. The choice of five years is guided by the persistent nature of specific tax legislations and the maximum length of years in between exogenous reforms.[23]

Figure 1.3 plots example tax regimes for the sample of large firms. The left panel shows the marginal tax treatment of large firms (same as Figure 1.2) and the right panel sketches the tax regimes across income brackets using the shaded colors. Figure 1.3 left panel shows that firms with taxable income greater than $1,405,000 received a tax increase of 4.8 percentage points in year 1968, a tax cut of 2.6 percentage points in 1970 and another tax cut of 1.2 in year 1971. In the right panel, I shade the respective tax regimes for firms in this bracket, which marks 1968 to 1970 as contractionary tax regime years and 1970 to 1974 as expansionary tax regime years. Since this particular income bracket does not receive any more treatment in the following years, the firms switch to neutral regime in 1974, five years after the reform. Hence, repeating this exercise across income brackets allows me to construct micro tax regimes of income brackets through time. Using exogenous tax reforms, 4 percent of the sample is identified to receive a persistent tax increase and 18 percent of the sample is identified to receive a persistent tax cut.[24]

One of the underlying assumptions of this framework is that it allocates firms to a regime using last year's taxable income which may overlook potential income responses. In order to not rely on the assumption of static taxable incomes through time, I restrict my sample to firms who continue to stay in the treated brackets when the policy changes. In other words, once a firm is allocated to a regime, I continue to track its income for the next year to confirm that they continued to stay in that particular reform's treated brackets. Although this certainly decreases the number of firms allocated to a regime, it minimizes the measurement error regarding the assumption on dynamics of taxable income. I also drop the regime observations of firms who jump more than one neighboring income bracket after a tax reform. This also automatically deals with firms that switch to zero or negative taxable income after the tax reform as well.

Constructing tax treatment dose The US tax code has a non-linear structure, hence it restricts statutory treatment comparisons across brackets. In order to overcome this, I construct

[23]Figure 1.23 provides robustness on the maximum regime length.

[24]The final data consists of 8,251 observations in contractionary regime and 32,960 observations in expansionary regime, forming 4% and 18% of the total estimation sample.

a firm-level *dose* measure that captures treatment variation in statutory rate changes. Using the difference between tax liabilities based on the previous and the new legislation, I calculate the changes in share of tax burden for each firm that has received a statutory rate change. In order to facilitate comparison across firms and time, I also scale liability changes with lagged taxable income:

$$Dose = \frac{\Delta\text{mtr} \times (\text{TI}_{t-1} - \overline{\text{TI}}_{t-1})}{TI_{t-1}}$$

where $\overline{\text{TI}}$ is the threshold taxable income starting that particular bracket. Appendix Figure A1.1 presents a detailed example. Repeating this exercise for every firm at every tax legislation generates a firm-level dose variable which measures share of tax burden changes after every tax reform.

Figure 1.4 plots histogram of tax treatment dose in my sample. I use mean tax burden changes by each regime and allocate firms into groups that received a high or low dose tax treatment. The final tax treatment variable consists of the following five categories: contractionary high, contractionary low, expansionary high, expansionary low and neutral. For instance, if a firm has received a low dose tax cut (e.g. the change in tax burden is lower than the cutoff tax burden change), it is assigned to expansionary low regime. Alternatively, if a firm has received a high dose tax cut, it is assigned to expansionary high regime. The new measure of groups allows me to explore both the direction and the intensity of the tax treatment which facilitate treatment comparisons across time and firms. Furthermore, the use of a liability based measure allows me to test the implications of cash flow or balance sheet channel across firms.

1.2.2 Summary statistics

Table 1.3 presents summary statistics of key variables of interest in the firm-level data covering the sample period 1969-2006. The sample contains 205,342 observations on 20,357 firms. Since the sample consists of public firms, the average size (total assets) is $1,169 million and average taxable income is $105 million over the sample. The right-skewed size distribution of firms motivates the use of log variables in regressions. The average number of employees in the sample is 6.8 thousand.

The average leverage ratio is 29 percent and the average marginal tax rate is 27 percent. The taxable income and marginal tax rate are variables constructed in the paper and income taxes (*txt*) is a tax variable reported in Compustat. The numbers at different percentiles of the "income tax" variable and the generated "taxable income" variable suggest that the constructed variables are in line with the reported data.

The summary statistics are also provided across different tax regimes. Neutral regime firms account for 80 percent of the total sample and consists of 20,333 unique firms with 164,131 observations. Contractionary tax regime firms account for 4 percent of the total sample and consists of 2,027 firms with 8,251 observations in total. Expansionary tax regime firms account for 18 percent of the total sample and consists of 5894 firms with 32,960 observations. The comparison across tax groups suggests expansionary tax regime firms are larger in size than contractionary or neutral regime firms. Comparisons of employment, asset and income taxes also confirms this. Regarding leverage, firms that are subject to tax increases seem to have lower debt to asset ratio than the rest of the groups. Regarding the marginal tax rate, all groups seem to have sizable variation, which confirms the sizable heterogeneity in the treatment groups.

Finally, Figure 1.5 presents descriptive charts that lay out characteristics of each tax regime. In terms of number of firms, contractionary regime consists of 2,027 unique firms, expansionary regime consists of 5,894 firms. The total number of firms is 20,357. Figure 1.5 shows that contractionary regime firms heavily consist large firms (high assets and high sales) with high debt to asset ratios and low investment rates. In contrast, expansionary and neutral regime firms are more homogeneous across alternative firm characteristics. In the robustness section, I provide alternative specifications that address confounding effects of firm characteristics in the main results.

1.2.3 Monetary Policy Shocks

I use the Romer and Romer (2004) monetary policy shock series (Romer shocks) that have recently been updated by Wieland and Yang (2020). These are residuals from a regression of the federal funds rate on lagged values and the Federal Reserve's information set using Greenbook forecasts. The series are summed to an annual frequency and span from 1969 to 2006. Following Gertler and Karadi (2015), I instrument changes in 1 year treasury rate with the Romer shocks. Since my paper is a micro-data application using aggregate shocks, I use panel-data feasible local projection (Jordà, 2005) method with instrumental variables (LP-IV) as in Jordà et al. (2015).

There are many alternative approaches to identify monetary policy shocks in the monetary policy literature. First of the many prominent approaches is using a SVAR framework complemented with Cholesky decomposition, evidence or theory-based sign restrictions or calibrated elasticities. Although SVAR approaches provide a valid characterization of the transmission mechanism, they are not feasible for micro-data applications. Another alternative measure is to use high frequency shocks to fed funds futures rate (Jeenas, 2019; Gurkaynak et al., 2005; Gertler and Karadi, 2015). These studies employ a hybrid approach to identify exogenous shocks via high frequency surprises on interest rate futures around policy shocks. However, many of these measures are only available starting late 1980s and 1990s due to the record of federal funds meeting dates. Since I need a larger window to capture enough variation in both tax and monetary policy, they are not a good candidate for this paper.

1.3 Empirical Framework

This section provides the empirical framework that explores the role of tax changes on the effectiveness of monetary policy. Given the lack of existing work on the interaction of tax and monetary policy, pinning down the precise estimation is not straightforward. My empirical strategy is geared towards examining heterogeneous effects of monetary policy conditional on preceding tax treatment of firms.

1.3.1 Empirical Specification

I use local projections instrumental variable (LP-IV) approach combining local projections (Jordà, 2005) with instrumental variables. The LP-IV allows me to estimate a flexible specification without imposing VAR dynamics on the main variables. Equation (1.3) presents the baseline LP-IV estimating dynamic causal effects of exogenous monetary policy changes subject to alternative tax treatment of firms:

$$\Delta_h log(y_{j,t+h}) = \alpha_j^h + \beta^h \Delta\,\mathrm{R}_{\,t} + \Gamma^h \Delta\,\mathrm{R}_{\,t}\,\mathrm{Dose}_{j,t} + \theta^h \mathrm{Dose}_{j,t} + \Omega'(L)\,Z_{j,t-1} + \epsilon_{\,j,t+h} \tag{1.3}$$

where horizon is $h = 0, 1, ..4$ years and j and t denote firm and time, respectively. The left hand side of equation 1.3 is the cumulative change in the outcome variable y, $\Delta_h log(y_{j,t+h}) \equiv log(y_{j,t+h}) - log(y_{j,t-1})$, where y is log employees, log real investment and log real sales. The specification regresses the dynamic cumulative change in variable y on monetary policy changes subject to firms' tax treatment. α_j^h denotes firm fixed effect which soaks up permanent differences across firms and allows me to explore within firm variation. $\Delta\,\mathrm{R}_{\,t}$ is the changes in the 1-year treasury rate instrumented with extended Romer and Romer (2004) monetary policy shocks. The policy rate is scaled such that the shock raises the one-year treasury rate by 25 basis points on impact. The main coefficient of interest, β^h, gives the impulse response of left hand side variable at time t+h to a monetary policy change at time t. Γ^h captures the marginal effect of tax treatment on firms' responsiveness to monetary policy, where the dose variable is calculated as the percentage change in liability share of firms using the previous and the next year's rate.[25] The interaction term is instrumented with the interaction of the Romer and Romer (2004) shocks and Dose variable. In the robustness section, I also discuss a less parametric estimation using bins of tax treatment.[26] Both specifications confirm the main findings that tax policy generates differential monetary policy outcomes.

The identifying assumptions for this model are as follows. First, monetary policy shocks should satisfy the conventional instrument validity and exogeneity conditions, where the former suggests the shocks should be correlated with movements in 1-year treasury rate and the latter suggests that the shocks should be uncorrelated with all other shocks. I address these two conditions by using plausibly exogenous monetary shocks of Romer and Romer (2004) which isolates changes the federal funds rate that are orthogonal to the information set reported in the Greenbook forecasts. Regarding the instrument validity, the first-stage F statistic is above the threshold value of 10

[25] Note that *Dose* is positive when statutory tax rate increases, implying firms to have higher tax liabilities and vice verse.

[26] See Figure 1.15a that provides the less parametric estimation. The results will be discussed in the robustness section.

proposed by Stock and Yogo (2005), suggesting Romer shocks is a relevant instrument for changes in one year treasury rates.

Second, dynamic structure of LPs requires monetary policy innovations to be exogenous with respect to other current and lagged endogenous variables (Stock and Watson, 2018; Nakamura and Steinsson, 2018). In my particular setting, since I test the implications of an aggregate shock on micro data, the analysis does not suffer from reverse causality which would imply firm-level variables to affect aggregate shocks. However, since my particular research design incorporates two aggregate policy measures, I test the current and lagged exogeneity within the tax and monetary policy measures.[27] Appendix Table A1.2 provides two-way Granger causality estimates that suggest orthogonality between monetary shocks and statutory rate changes. The Granger causality results are discussed in detail in robustness section.

Finally, specific to my research design, I use narratively identified exogenous and unanticipated tax reforms which deals with anticipation effects and endogenous selection into tax treatment (Romer and Romer, 2009; Mertens and Ravn, 2013). In addition, I use lagged taxable income to compute tax regimes of firms which ensures that monetary policy do not affect firms tax treatment through income changes.

$Z_{j,t-1}$ includes the following control variables: change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Firm level controls in logs help soak up differences in cross-sectional characteristics in financial and nonfinancial variables. The variables in log difference or growth form are to capture the time-series trend. All the control variables in $Z_{j,t-1}$ are measured at the end of last year before the monetary shocks and tax changes to ensure exogeneity with respect to the shock. The estimation is done up to horizon of $H = 4$ years and the lag structure on control variable is 2 year.[28] The standard errors are clustered two-way at the firm by year level where serial correlation adjustment is set to 2 year using Driscoll and Kraay (1998) methodology. This is a standard method to account for serial correlation at the firm level and through time (See Cloyne et al. (2019), Bahaj et al. (2020)). Following Mertens and Olea (2018), I use year dummy on

[27] Note that since the policy shocks only contain variable realized at date t or earlier, the lead exogeneity requirement is less concerning (See Stock and Watson 2018, page 10)

[28] See Appendix 1.8.1 for the sample selection procedure.

1981 and 2001 as a dependent variable. These correspond to a period of relative macroeconomic turbulence and unusual policy variation associated with Volker era and Dot Com recession. Finally, note that the baseline specification imposes linearity in the effect of the tax treatment on explaining firms' responsiveness to monetary policy changes.

1.3.2 Results

This section presents the impulse responses of specification 1.3 where I plot estimated coefficients as well as their 95% confidence intervals on the number of employment, investment and sales, respectively.

Figure 1.6 plots the estimates on the number of employees. For neutral regime firms, shown in Figure 1.6a, I find that the number of employees have an average semi-elasticity of -0.4 percent to monetary policy. The peak effect is -0.7 percent occurring two years after the monetary policy shock and the effect is not significant across the horizon. For firms with alternative tax treatments, shown in Figure 1.6b, I find sizable heterogeneity in monetary policy outcomes conditional on the changes in statutory rate. Specifically, firms with tax cuts respond monotonically less to contractionary monetary policy. In contrast, firms facing a statutory tax increase experience a larger fall in employees relative to the neutral regime firms. Figure 1.6b estimates suggest that on average firms facing 1% tax hikes (cuts) respond to monetary policy about 0.25 percent more (less) than the firms with stable taxes. Since firms with tax cuts have a sizable share (18%) of the total sample of firms, these findings highlight an important result that contractionary monetary policy is significantly ineffective on firms facing statutory tax cuts.[29] Finally, Figure 1.6c plots the interaction coefficient $\hat{\Gamma}^h$ where the shaded areas capture 95% confidence intervals.

Next, Figure 1.7 shows the impulse responses of investment, where the dependent variable is the cumulative change in log real capital expenditures. For neutral regime firms, shown in Figure 1.7a, I find -0.3 percent average semi-elasticity of investment to monetary policy shock. The peak effect is reached two years after the monetary shock, at a value around -1.1 percent. In comparison, Figure 1.7b suggests that firms with statutory tax increases show a noticeable on-impact drag on

[29] See Appendix Figure A1.2 for standard errors of Figure 1.6b.

investment following a monetary innovation. The estimates are mostly negative across the horizon for firms with increasing taxes. Comparison of the average effects suggest that firms facing 1% tax increase (decrease) show a -0.6 percent larger (smaller) semi-elasticity to monetary shocks. The estimates of expansionary regime firms is mostly positive across the horizon and significant in the first two years after the monetary shock.[30]

Last, Figure 1.8 presents the estimates on log real sales using equation 1.3. Figure 1.8a suggest that firms in the neutral regime have on average -0.2 percent semi-elasticity of sales to monetary policy shocks, where the peak effect is -0.5 percent occurring four years after the monetary shock. Similar to the previous results, firms experiencing a contractionary tax display a larger response to monetary shocks, where average semi-elasticity is -0.45 percent. In contrast, firms with tax cuts have much less response to monetary policy relative to the contractionary and neutral regime firms. The average semi-elasticity of a firm with 1% tax cut is 0.14 percent lower on average than firms in neutral regime. The effect of monetary policy for expansionary regime firms is not significantly different from zero across the forecast horizon.[31]

Taken together, there are two main results that emerge from Figures 1.6, 1.7 and 1.8. First, firms facing tax increases exhibit larger responses to monetary policy shocks than firms in expansionary or neutral tax regimes. Second, firms facing statutory rate cuts have much lower responses to monetary innovations. These findings suggest that underlying tax shifts can account for a considerable amount of variation in monetary effectiveness. In other words, the underlying tax treatment of companies can amplify or reduce their responsiveness to monetary policy.

Last, I confirm the baseline continuous interaction results using a less parametric estimation. In this specification, I use bins of tax regimes as described in the data section and estimate the marginal effects of the 1 percent contractionary or expansionary tax change on the effectiveness of monetary policy. Equation 1.2 provides the details of the specification and Figure 1.15-1.17 plot the impulse responses. Overall, the results from the less parametric specification align with the results of the baseline specification. Specifically, in all three outputs we can observe that receiving a 1 percent tax cut offsets the effectiveness of monetary policy and receiving a 1 percent tax increase amplifies the

[30] See Appendix Figure A1.3 for standard errors of Figure 1.7b.
[31] See Appendix Figure A1.4 for standard errors of Figure 1.8b.

effectiveness of monetary policy. The results are more pronounced for employment than investment and sales. Overall, both continuous interaction and group-based estimation results underscores the interaction between firms' tax treatments and the responsiveness to monetary policy.

1.3.3 Robustness

This section conducts a large number of checks that show the robustness of the baseline results. In particular, I confirm that the results are robust to alternative estimation strategies, control set and underlying assumptions. I also test the confounding effects of firm characteristics, address several potential threats to identification and discuss measurement issues.

Orthogonality of tax and monetary shocks One of the biggest concerns related to the identification strategy is that federal tax reforms may influence monetary policy innovations. This paper uses *exogenous* changes in federal funds rate and *exogenous* and unanticipated changes in tax reforms where original sources (Romer and Romer, 2009, 2004) address general endogeneity concerns. However, it is still necessary to address orthogonality of monetary policy with respect to tax policy.

To verify monetary policy shocks are not endogenous to the Romer and Romer (2009) tax reforms, I perform Granger causality test using aggregate data. Appendix Table A1.2 provides the test results suggesting Romer monetary policy shocks are orthogonal to the tax reforms. Similarly, Appendix Table A1.3 ensures that exogenous set of tax reforms are not Granger caused by monetary policy shocks as well. The specifications include alternative versions with different aggregate controls, none provides evidence on endogeneity of the two policy measures. Furthermore, Figure 1.9 plots the distribution of monetary policy shocks across tax regimes which suggests that monetary innovations have sufficient randomness and mean-zero distribution in the neutral tax regime. The contractionary and expansionary regimes also have comparable left-skewed distribution of monetary shocks.[32]

Next, one may also be worried that the income groups that received tax changes might be predicted from the political party in power. Regarding the tax scheme specific anticipation effects, I match

[32] The negative shock in the expansionary regime in Figure 1.9 belongs to year 1981, which is controlled in the regressions due to unusual policy variation of Volker era.

the tax reforms in Table 1.1 to respective political parties. Out of three tax cuts, two are legislated under Republican party and one is legislated under Democratic party.[33] Out of two tax increases, Deficit Reduction Act of 1984 is legislated under Republican party under Reagan and Omnibus 1993 has passed under Democratic party. There are also no conventions on the income range that receives a statutory rate change. For example, Deficit Reduction Act of 1984 - despite being legislated under Republican party - has increased the statutory taxes by 5 percentage points for large firms with income in between $1 and $1.4 million. Thus, there is sufficient randomness in the specifics of exogenous tax shifts and preferences of political parties.

Baseline robustness checks One of the main assumptions in my empirical strategy is that it allocates firms to a certain tax treatment using last year's taxable income which may overlook potential income responses. In the baseline regressions, I restrict my sample to firms who continue to stay in the treated income bracket for two years around the policy change. The results in Figure 1.21 re-estimates the baseline specification when firms stay in the treated income brackets for three years around the policy change. The results confirm the baseline findings that tax shifts account for a considerable degree of heterogeneity in firms' responses to monetary innovations. Note that the movement of taxable income per se is not crucial as long as the firm stays in the same bracket.

Second, the baseline analysis sets an upper bar of firms in each regime as 5 years. This is guided by the largest gap year within the exogenous reforms. It also captures persistence feature of tax reforms and ensures that average firm life across regimes are comparable. In Figure 1.23 and Figure 1.24, I change the maximum regime life of a firm to 8 years. Comparison with the baseline charts suggest that the results are robust to regime length as well.

Next, I explore the possibility that the baseline estimates could be prone to omitted variable bias and test alternative financial variables that might explain the differences in responses. First, Figure 1.19 re-estimates equation 1.3 with additional controls on leverage and liquidity ratio of firms. The results are quite robust to additional controls.

Following Ottonello and Winberry (2020), I also test whether heterogeneity in other observable firm characteristics can drive the main results. Figure 1.27-1.29 re-estimate the main results using

[33]Revenue Act of 1978 is legislated under Democratic Party. Economic Recovery Tax Act of 1981 and Tax Reform Act of 1986 passed under Republican party.

specification 1.3 where monetary shocks are interacted with various other firm characteristics. In each case, the coefficient on monetary and tax regime interaction remains robust suggesting that the main results are not driven by asset growth, leverage or sales growth differences. In other words, these alternative channels of monetary policy do not offset the corporate tax channel I explore.

Next, I re-estimate the baseline analysis on the full set of tax reforms: exogenous and endogenous. Figure 1.25 plots the impulse responses for employment, investment and sales. The results slightly deviate from the baseline findings which is expected since endogenous tax reforms may be correlated to monetary innovations.

Last, I also conduct a less parametric specification by binning firms across different regime-dose categories. For this exercise I use tax regimes and treatment doses, and allocate firms to the following five groups: contractionary high, contractionary low, expansionary high, expansionary low and neutral (See Section 1.2 for details). Figure 1.30 plots impulse responses from estimation of equation 1.2 on employees on different tax treatment categories. The first row plots the effects of monetary policy when a firm is in neutral regime. The second (third) row plots the effects of monetary policy when the firm is facing a increase (cut) in marginal tax rate. Column one and two reflect the dose of treatment being low and high, respectively. The tax group based results confirm the linear interaction findings. Especially, in Figure 1.30-1.32, we can pin down the monotonic effects high dose groups carry towards the monetary responsiveness which underscores the interaction of firms' tax treatments and the responsiveness to monetary policy.[34]

The role of firm characteristics This part explores the role of specific firm characteristics within the baseline findings. First, I group firms based on their cross-sectional characteristics using size (using number of employees), leverage and liquidity, and explore whether certain groups are more sensitive to indirect effects of taxes on monetary outcomes.

Figure 1.36 - 1.37, Figure 1.38 - 1.39 and Figure 1.40 - 1.41 provide estimates of employment, investment and sales by firm size. The results suggest that the average response observed in Figure

[34] As a robustness, I re-estimate the specification with quartiles of doses. Figure 1.33-1.35 show that the results are robust to alternative dose cutoffs.

1.6 is generally similar across firms of different sizes,[35] with the exception that small firms within contractionary regime respond most to monetary policy.

I also analyze responses based on liquidity and leverage rates of firms. Figure 1.43 shows that high liquidity firms facing tax changes have a large and significant response to monetary policy on employees. On the investment results, there is no clear distinction of results across groups. On sales, high liquidity firms have an interesting response. Even within the neutral regime, high liquidity firms respond to monetary policy more than low liquidity group. The group similarly responds most to indirect effects of taxes on monetary policy.

Last, I analyze firms with different leverage groups. Figure 1.48 - 1.49, Figure 1.50 - 1.51 and Figure 1.52 - 1.53 suggest that the response of monetary policy on employees, investment and sales is most significant in low leverage group.

Taken together, the relationship between tax changes and monetary effectiveness is largely common across different firm characteristics. However, I find characteristics such as being small, holding high liquidity and low leverage to play a role in enhancing indirect effects of taxes on monetary effectiveness.

Measurement Error There can be two measurement related concerns in this study. These are related to definition of taxable income and tax avoidance of firms. One of the key features of my research design is to use the taxable income variable to be able to sort the firms into tax brackets and employ the time-series variation of marginal tax rate changes across different income brackets. Hence, the exact measure of taxable income is less of a concern as long as the firm is approximately matched to the right tax bracket.

Second, it is well documented that firms may engage in a variety of behavioral responses to minimize the tax burden (Rego, 2003). This type of measurement error would be highest for taxpayers with higher income as they may have greater access to avoidance opportunities. This is a less of a concern for my analysis since the treatment effects are based on the taxable income brackets the firms are in. Hence, even though the firm might not have a taxable income of $14,000, but say $12,000, it will be allocated to the same tax regime as long as the numbers are within the same

[35]This is reassuring since a big portion of contractionary regime firms are large firms (See Figure 1.5).

taxable income brackets. In addition, since the taxable income brackets for large income firms are vast, large firms have less chance of mismeasured treatment effects.

1.4 Theoretical Model

This section builds a medium-size New Keynesian model featuring capital, corporate taxes and Rotemberg (1982) type price rigidities. The purpose of the model is to lay out how corporate tax shifts can effect the transmission channels of monetary policy. The model is populated by identical infinitely-lived households, continuum of monopolistically competitive intermediate goods firms, a final goods firm and government. The time is discrete and the planning horizon is infinite.

1.4.1 Households

Households purchase consumption goods, provide labor services to the productive sector and save with bonds. A representative infinitely-lived household is seeking to solve the following dynamic optimization problem:

$$\max_{C_t, N_t, B_{t+1}} E_t \sum_{t=0}^{\infty} \beta^t \left\{ lnC_t - \theta \frac{N_t^{1+\chi}}{1+\chi} \right\} \qquad \text{s.t.}$$

$$P_t C_t + Q_t B_t \leq B_{t-1} + W_t N_t + \Pi_t + T_t$$

where C_t is the final goods consumption. N_t denotes hours of employment[36], W_t is the nominal wage and P_t is the price of final good. The χ is the inverse of Frisch elasticity and B_t is the stock of one-period nominally riskless savings household purchases. Each bond pays one unit at maturity and its price is Q_t[37]. T_t is government transfers and Π_t is the dividend distribution from the ownership of firms.[38] The consumption index, C_t, is given by

$$C_t \equiv \left(\int_0^1 C_t(i)^{1-\frac{1}{\epsilon}} di \right)^{\frac{\epsilon}{\epsilon-1}}$$

[36]Note that N_t can be interpreted as the number of household members employed as in Galí (2009).

[37]Q_t is equal to inverse of gross nominal interest rate.

[38]The dividends received by households are just the sum of profits from the intermediate good producers. Since the final good firms is competitive, they earn no profit.

where $C_t(i)$ is the quantity of good i consumed by the household in period t. The representative household has to solve two problems: allocation of spending across goods and allocation of spending across time. The solution to households problem is standard and is provided in Appendix 1.8.3. The first order conditions bring the optimality conditions on consumption-savings and labor supply decision:

$$\theta N_t^{\chi} C_t = \frac{W_t}{P_t} = w_t \tag{1.4}$$

$$Q_t = \beta E_t \frac{C_t}{C_{t+1}} \frac{1}{1+\pi_{t+1}} \qquad \text{for } t = 0, 1, 2, ... \tag{1.5}$$

where $\pi_{t+1} = \frac{P_{t+1}}{P_t} - 1$ is the net inflation rate and $w_t = \frac{W_t}{P_t}$ is real wages. Equation 1.4 can be interpreted as the competitive labor supply condition, determining the quantity of labor supplied as a function of the real wage, given the marginal utility of consumption (which is a function of consumption only). Workers do not have any market power, hence they take the wage as given.[39]

1.4.2 Production

The production is split into two sectors: final and intermediate good sectors. There is a representative competitive final goods firm that aggregates intermediate inputs according to a CES technology. This generates a downward sloping demand for each intermediate good and grants the pricing power to intermediate producers. The intermediate firms produce output using capital and labor and are subject to price rigidities.

Final Good Producer

The final goods producers is perfectly competitive and aggregates the intermediate goods into a

[39] Last, equation 1.5 can also be used to determine the implied real interest rate in linear form as:

$$r_t \equiv i_t - E_t\{\pi_{t+1}\}$$

final good for consumption using a CES technology:

$$Y_t \equiv \left(\int_0^1 Y_t(i)^{1-\frac{1}{\epsilon}} di \right)^{\frac{\epsilon}{\epsilon-1}}$$

where ϵ is the constant elasticity of substitution and $\epsilon > 1$ between different intermediate goods, i $\in [0, 1]$.[40]

Intermediate Good Producer

The intermediate goods producers are a continuum of firms indexed by i $\in [0, 1]$. Each firm produces a differentiated good for which it sets the price. All firms use an identical technology, represented by a standard Cobb-Douglas production function:

$$Y_t(i) = A_t K_t(i)^{\alpha} N_t(i)^{1-\alpha} \tag{1.6}$$

where A_t represents the level of technology, assumed to be common to all firms and to evolve exogenously over time. All firms face an identical isoelastic demand schedule given, and take the aggregate price level P_t, aggregate consumption index C_t and wage level, W_t, as given. Following Rotemberg (1982), each monopolistic firm faces a quadratic cost of adjusting nominal prices, measured in terms of the final good:

$$Adj\_P_t(i) = \frac{\psi}{2} \left(\frac{P_t(i)}{P_{t-1}(i)} - 1 \right)^2 Y_t$$

where ψ is the degree of nominal price rigidities. Firms own the capital stock and generate more capital through investment. The capital accumulation process is subject to convex capital adjust-

[40] As standard, the cost minimization problem of the final good producers implies that the demand for the intermediate good i is given by:

$$Y_t(i) = \left(\frac{P_t(i)}{P_t} \right)^{-\epsilon} Y_t \text{ for all } i \in [0, 1]$$

where $P_t(i)$ is the price of the intermediate good i. This suggests that relative demand for good i is a function of its relative price, ϵ price elasticity of demand and aggregate output, Y_t. We can also reach the aggregate price index from the zero profit condition: $P_t \equiv \left(\int_0^1 P_t(i)^{1-\epsilon} di \right)^{\frac{1}{1-\epsilon}}$

ment costs. Capital accumulates according to:

$$K_{t+1}(i) = I_t(i) - \frac{\phi}{2}\left(\frac{I_t(i)}{I_{t-1}(i)} - 1\right)^2 + (1-\delta)K_t(i) \tag{1.7}$$

Investment is financed out of dividends, and for simplicity we assume that the firm issues no intertemporal debt. The dividend payout is then

$$\Pi_t = (Y_t - w_t N_t)(1-\tau_t) - I_t$$

Note that the corporate taxes is levied on the accounting profits of the firm which is the object explored in the empirical section. The firm i will aim to maximize the value of future profits discounted by the stochastic discount factor of the household, $E_t\beta^j \frac{\lambda_{t+1}P_t}{\lambda_t P_{t+1}} = Q_t$. Each period the firm maximizes profits subject to two constraints. q_t is the multiplier on the accumulation equation (since the units of the firm's problem are in real terms, q_t has the interpretation of reflecting how many goods the firms would give up for an additional unit of installed capital.). $MC_t^n(i)$ measures how much nominal costs change if the firm produces an additional unit of its good. First order conditions are:

$$N_t(i): \qquad W_t(1-\tau_t) = MC_t^n(i)[(1-\alpha)A_t K_t(i)^\alpha N_t(i)^{-\alpha}]$$

where $MC_t^n = MC_t * Price$ and MC_t is the real marginal cost and w is the real wage. Hence, the labor demand equation becomes:

$$w_t(1-\tau_t) = MC_t(i)[(1-\alpha)A_t K_t(i)^\alpha N_t(i)^{-\alpha}]$$

mapping real wage into the quantity of labor demanded, given the level of technology and capital.

$$I_t(i) \ : \qquad 1 \ = \ q_t\big[1 - \phi\big(\frac{I_t(i)}{I_{t-1}(i)} - 1\big)\frac{1}{I_{t-1}} + \beta\frac{\lambda_{t+1}}{\lambda_t}q_{t+1}\left[\phi\ \frac{I_{t+1}}{I_t^2}\big(\frac{I_{t+1}}{I_t} - 1\big)\right] \tag{1.8}$$

$$K_{t+1}(i): \qquad q_t = \beta E_t \frac{\lambda_{t+1}}{\lambda_t}\Big\{ q_{t+1}(1-\delta) + MC_{t+1}(i)(A_{t+1}\alpha K_{t+1}(i)^{\alpha-1} N_{t+1}(i)^{1-\alpha}\Big\}$$

$$P_t(i): \qquad \frac{1}{P_t}\Big[(1-\epsilon)(\frac{P_t}{P_t(i)})^{\epsilon}\, Y_t(1-\tau_t) - \psi(\pi_t(i))\frac{1}{P_{t-1}(i)}P_t Y_t\Big] - \frac{MC_t^n(i)}{P_t}\Big[\frac{-\epsilon P_t(i)^{-\epsilon-1}}{P_t^{-\epsilon}}Y_t\Big]$$
$$+ \beta E_t \frac{\lambda_{t+1}}{P_{t+1}\lambda_t}\big[\psi(\pi_{t+1}(i))(\pi_{t+1}(i)+1)\frac{P_{t+1}}{P_t(i)}\, Y_{t+1}\big] = 0$$

where $\pi_t(i) = \frac{P_t(i)}{P_{t-1}(i)} - 1$. By imposing symmetry and simplifying, we can reach the steady state markups:

$$(1-\epsilon)\; Y_t(1-\tau) - \psi\pi_t(\pi_t+1)Y_t + \beta E_t \frac{\lambda_{t+1}}{\lambda_t}\big[\psi\pi_{t+1}\;(\pi_{t+1}+1)\; Y_{t+1}\big] \;=\; -\epsilon\; MC_t(i)Y_t \qquad (1.9)$$

where equation 1.9 is the counterpart of a New Keynesian phillips curve relating nominal variables to the real economy. Under steady state:

$$P = MC^n \frac{\epsilon}{(\epsilon-1)}\frac{1}{(1-\tau)} \qquad (1.10)$$

where $\frac{\epsilon}{(\epsilon-1)}$ is the conventional flexible price markup, $\mathcal{M}$. The equation suggests that optimal price would be a fixed mark-up over nominal marginal cost and existence of taxes shows up like an increase in steady state mark up.

The market-clearing conditions are standard. Since the firm issues no intertemporal debt, then the household can not have any stock of savings. Note that from investment first order condition (equation 1.8) it is clear that q =1 in steady state, where there is no adjustment costs. Labor market clearing requires that the sum of labor used by firms equals the total labor supplied by households, e.g. $\int_0^1 N_t(i)di = N_t$. There is no market for capital, since firms own the capital stock. Goods market clearing conditions is:

$$Y_t = C_t + I_t + \frac{\psi}{2}(\pi_t)^2 Y_t \qquad (1.11)$$

1.4.3 Government

The government sets tax policy and an independent monetary authority, the Central Bank, conducts the monetary policy. The government is assumed to hold a balance budget and distribute the corporate tax returns to households as lump-sum transfers. Tax policy follows the following rule:

$$\tau_t = (1-\rho_\tau)\tau_{ss} + (\rho_\tau)\tau_{t-1} + s_\tau \epsilon_{\tau,t} \tag{1.12}$$

Monetary policy sets the nominal interest rate according to the following Taylor (1993) rule:

$$i_t = \rho i_{t-1} + (1-\rho)\phi_\pi \pi_t + s_i \epsilon_{i,t} \tag{1.13}$$

where $\phi_\pi > 1$ and $\epsilon_{i,t}$ is an exogenous stochastic disturbance in the nominal interest rate, which follows a white noise process with zero mean and finite variance. A positive realization of $\epsilon_{i,t}$ is interpreted as a contractionary monetary policy shock, leading to a rise in nominal interest rate. Note that this rule implies a countercyclical monetary policy where central bank increases the nominal interest rates when inflation is positive. Combining the above conditions and imposing symmetry leads the set of equilibrium conditions provided in Appendix 1.8.3.

1.4.4 Model Results

This section discusses a simulation exercise which compares the effects of monetary policy in two economies with differing preceding tax interventions. Output, investment, capital and hours worked are variables of interest and units are interpreted as percent deviations from the steady state. The calibration parameters closely follow Galí (2009), Burnside et al. (2004) and Miao and Ngo (2019) and are provided in Appendix Table A1.4. I use a second order approximation to capture the level effect of an initial tax shock on the subsequent monetary policy shock.[41] The aim is to test whether an economy with persistent preceding tax interventions shows a differentiated response to monetary policy.

Figure 1.54a plots the impulse responses of the blue and red economy in which the red economy

[41] In first order approximation, the initial condition, the sequence of shocks, their sign and size does not matter.

receives a 50 basis points increase in corporate tax rate at time $t = 1$, blue economy does not receive a tax shock. The responses to tax shocks start at time $t = 1$ and are presented with boxed lines. At time $t = 10$, both economies receive 25 basis point contractionary monetary policy shock.[42] The object of interest is the shaded area that is the difference between the boxed (τ) and the solid (MP+τ) lines, capturing the effects of monetary policy in each economy. Comparing shaded areas of the two economy shows that the economy that received a preceding tax increase has a larger response to monetary policy shock than the economy with stable taxes. In order to visualize the effect more clearly, Figure 1.54b plots the gap between solid line (MP+τ) and the boxed (τ) line which is the total effect of monetary policy in each economy. Figure 1.54b shows that the effects of monetary policy is larger in an economy that faces a preceding contractionary tax shock. The main mechanism follows as persistent contractionary tax policy lowers the net present value of projects and leads firms to downsize capital subject to adjustment costs. When monetary policy hits during this transition, firms re-optimize their investment path as a response to the transitory change in funding cost. However, due to decreasing returns to capital, the elasticity to monetary policy may be higher in transition with a lower level of capital.

Last, I compare the effects of monetary policy when an economy faces a preceding *expansionary* tax shock. Figure 1.55b bottom panel shows the impulse responses to monetary policy shocks where the dashed (red) economy receives an expansionary tax shock at time $t = 1$ and contractionary monetary shock at time $t = 10$. Similarly, the blue economy only receives a contractionary monetary shock at time $t = 10$. Comparison of the two impulse responses suggest that effectiveness of monetary policy is reduced if monetary innovations are preceded by a persistent tax cut.

Overall, these results support the empirical findings such that a preceding contractionary tax policy amplifies the impact of monetary shocks and a preceding expansionary tax policy reduces the impact of monetary shocks. Although the theoretical model does not lay out cross-sectional features of taxes, the output of the model rationalizes the empirical findings of this paper. As for robustness, the results suggest that amplification effect varies with the timing of the monetary policy shock. The sooner the monetary policy shock hits, the larger is the amplification. Similarly, the adjustment costs also have a role in propagating the effect on investment and capital, such that

[42] The choice of 10 quarter is guided by half life of 5 year regime length used in empirical section.

with lower adjustment costs the total effect of monetary policy is larger on investment and capital. (See Appendix Figure A1.6 and A1.7)

Alternative Mechanisms An alternative mechanism accounting for these results might be that changes in liability rates alter the tightness of financial constraints of firms. In an environment with capital market imperfections, borrowers' balance sheet conditions (Gertler and Gilchrist, 1994; Bernanke and Gertler, 1989; Kiyotaki and Moore, 1997; Calomiris and Hubbard, 1990) play a significant role in access to credit. As tax shifts changes the balance sheet conditions of firms, we may see tax policy to contribute to financial propagation mechanism. The impact of tax policy can be seen both directly and indirectly. First, an increase in tax rate would weaken the balance sheet by lowering cash flows. Second, it may also lower the value of collateral assets through the decline in net worth. As highlighted in Gertler and Gilchrist (1994), monetary policy already operates through these two effects. Hence, in a model with financial frictions, tax increases (decreases) can potentially amplify (weaken) these two channels. This paper does not focus on this mechanism since the heterogeneity results suggest that the average response is observed somewhat similarly across firms with alternative financial characteristics. The reason for this may be that Compustat disproportionately feature large companies, therefore may underrepresent small and financially constrained firms.

Also at a general equilibrium level, corporate tax changes can affect aggregate demand and savings through household balance sheet channels. By increasing corporate taxes, tax policy would lower dividend payouts and decrease asset prices which may affect household wealth. The fall in household wealth would dampen the aggregate demand and savings which can result in the fall of funds for firms. Hence, while not directly testable in my current setup, tax policy may influence the wealth effect based channels in monetary transmission.[43]

[43] Another mechanism can be through the changes in the inflation expectations in the economy. Following a tax increase, deflation is triggered, which leads firms to inherit positive price adjustment costs. Hence, when a monetary contraction hits in this transition, the corresponding changes in prices happen at a much lower rate, meaning firms lower their prices much less. In contrast, when monetary policy hits as a solo shock, deflation occurs more severely, suppressing the real interest rate.

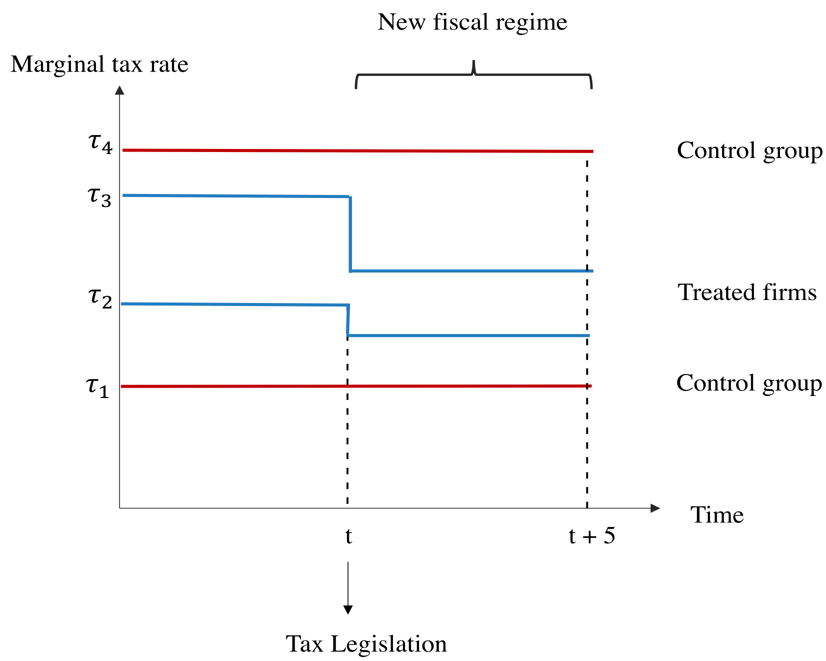
1.5 Conclusion

This paper provides the first empirical evidence on how tax policy dynamics influence the transmission of monetary policy. Using a unique feature of the US corporate tax code, tax narrative accounts and detailed firm-level data, I document that changes in tax policy alter the average impact of monetary policy shocks and lead to heterogeneous effects. Specifically, I estimate investment, employment and sales responses to monetary policy changes allowing the effects to vary based on the firm-level tax treatment. Overall, my findings document that accounting for the dynamics of tax system may explain sizable variation in monetary effectiveness.

The results of this paper are particularly important for three reasons. First, this study is the first paper evaluating the impact of monetary policy conditional on underlying tax structures. This is quite different than the earlier approaches adopted in the monetary transmission literature that mainly uses firm characteristics to explain heterogeneous effects of monetary policy. Second, this paper is the first attempt to evaluate the intersection of two main policy tools in an applied setting with micro-data. In this regard, these findings provide a practical rule-of-thumb to the design of stabilization policy and also contribute to our understanding of the scope of monetary policy within an underlying tax system. Last, the findings of this paper are particularly relevant to understand the muted effects of monetary policy observed in last decades (Boivin et al., 2010). Taken together, my analysis provides an insight on how the historical downward trend in corporate taxes may have contributed to weaker effects of monetary policy as documented in recent decades. Moreover, these results bring the possibility of future work questioning the role of downward trend in taxes on the gradual decline of interest rates in the US.

1.6 Figures

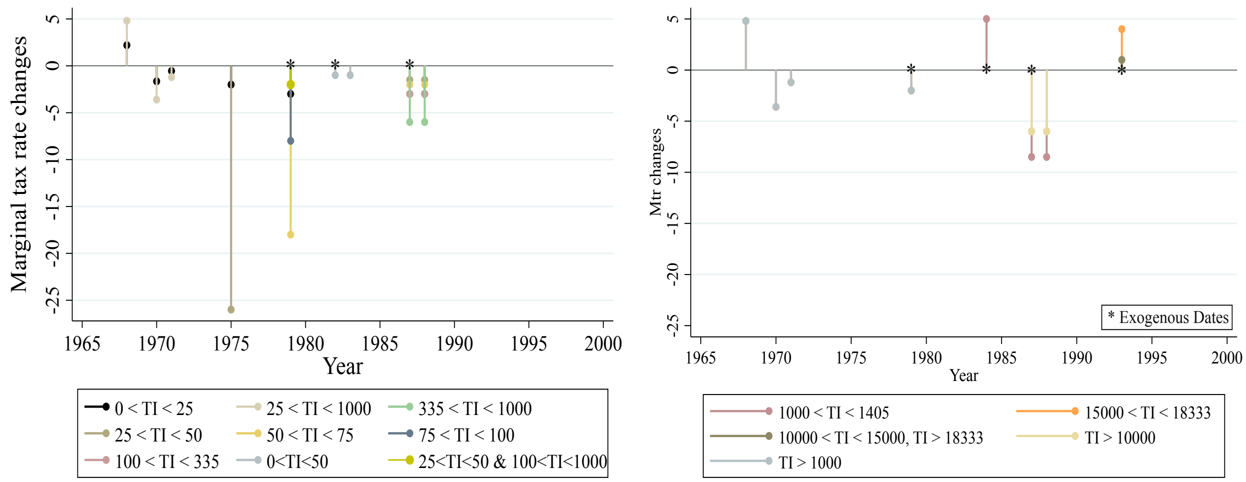
Figure 1.1: Sketch of tax treatment



Note: This chart plots the basic research design employed in this paper. At time t, a new legislation is exercuted which changes the marginal tax rate for some brackets and leaves it unchanged for some others. This feature of tax policy is used to form a quasi-experimental research design using variation in direction and the level of tax treatment across income brackets over time.

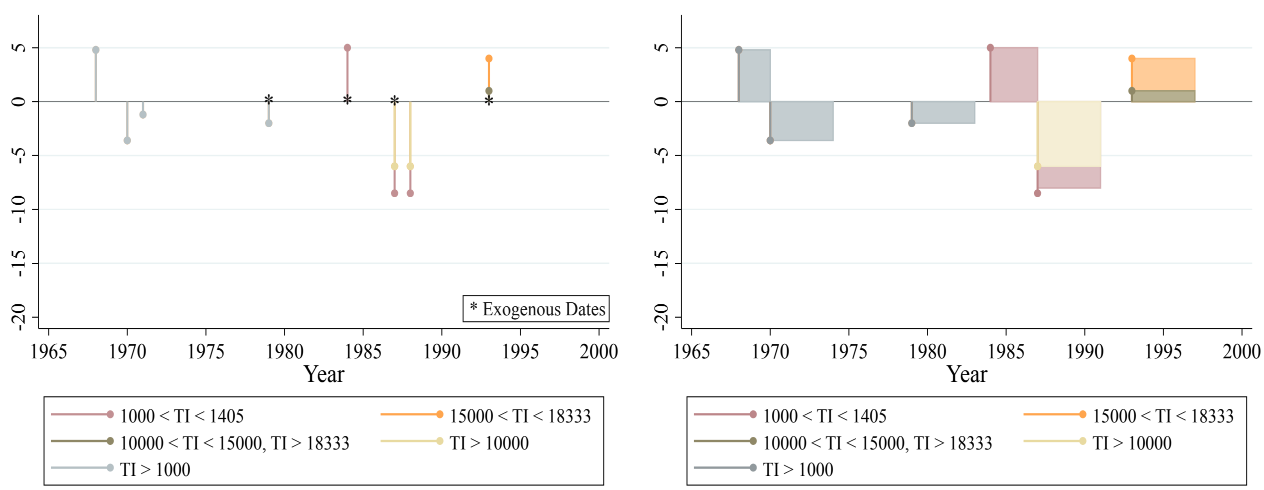
Figure 1.2: Corporate tax rate changes by alternative taxable income brackets

(a) Taxable Income ≤ \$1,000,000 **(b)** Taxable Income > \$1,000,000



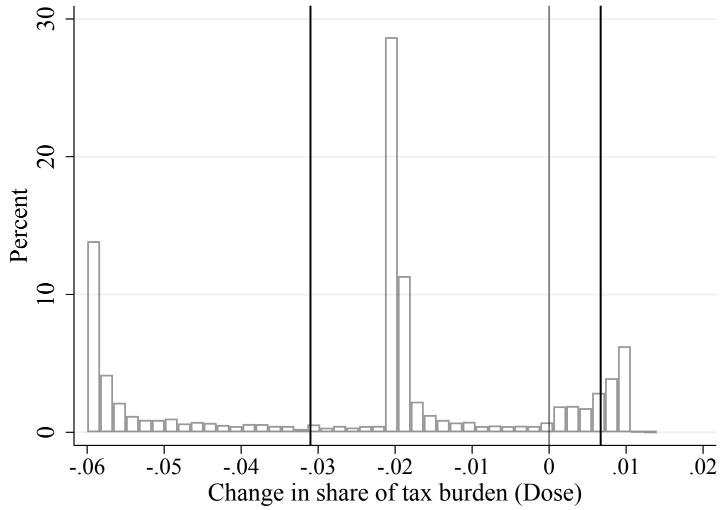
Note: The 1979, 1982, 1984, 1987 and 1993 are exogenous acts based on Romer and Romer (2009) and Mertens and Ravn (2013). All the tax changes are categorized as persistent in Mertens and Ravn (2012).

Figure 1.3: IRS corporate rate changes and fiscal regimes by taxable income groups.



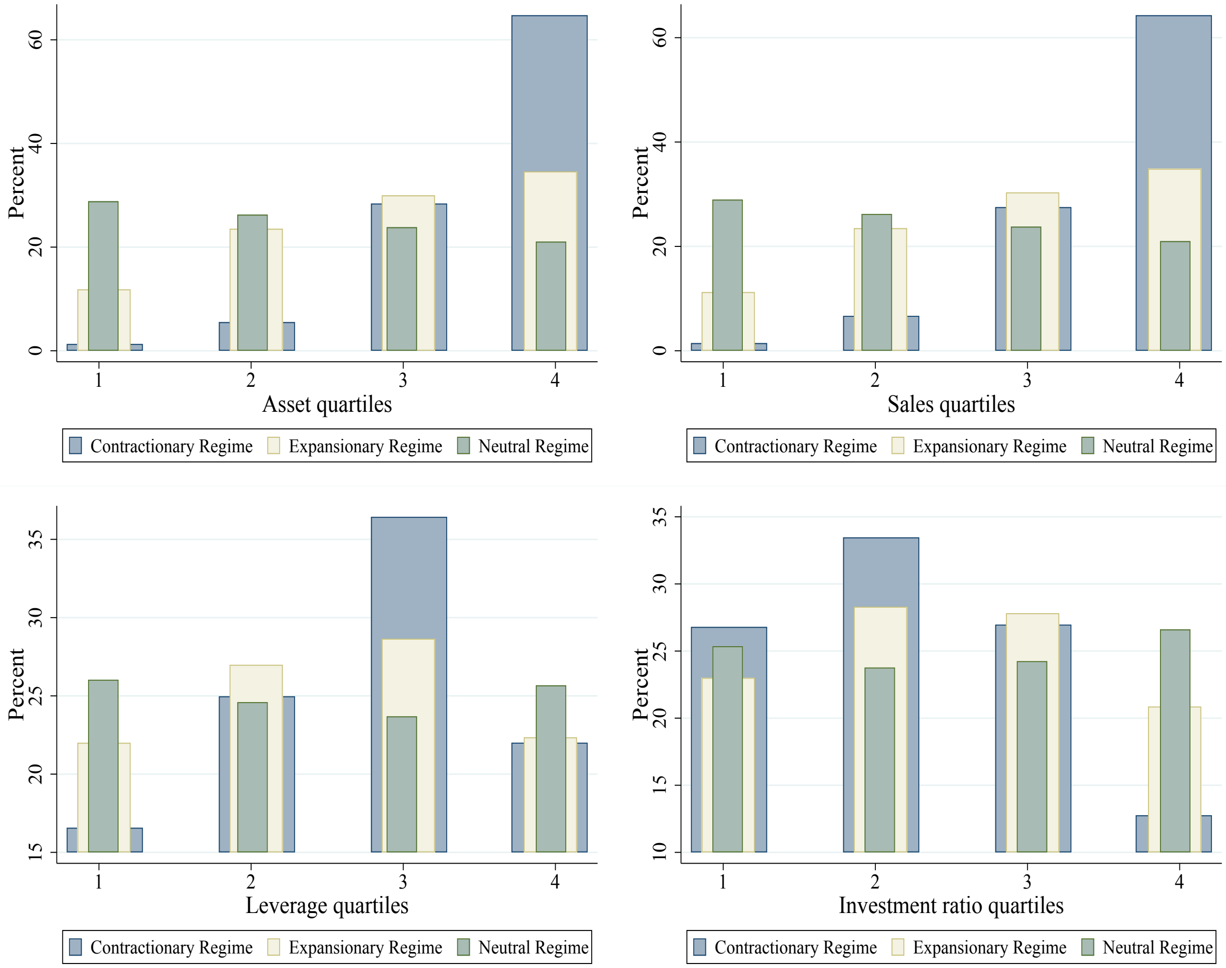
Note: Taxable Income (TI) is in \$1000. Shaded areas denotes the respective regimes. Source is IRS historical statutory corporate tax rates.

Figure 1.4: Histogram of changes in tax burden (dose)



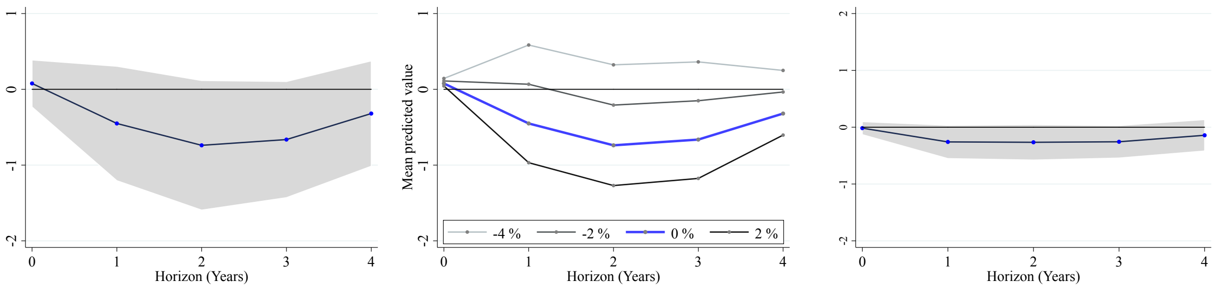
Note: This figure plots the histogram of tax burden changes across the sample. The dose is calculated as change in tax liability over taxable income. Negative doses are from expansionary tax changes and positive doses are from contractionary tax changes. The gray thresholds show the mean dose of each regime on points -0.031 and 0.0067. The highest expansionary tax shock changes a firm's share of tax liabilities by 6 percent.

Figure 1.5: Firm characteristic decomposition by each tax regime.



Note: The figure plots the quartiles of real asset, real sales, leverage and investment in each regime. The quartiles is generated by year and histogram spans observations from 1969 to 2006.

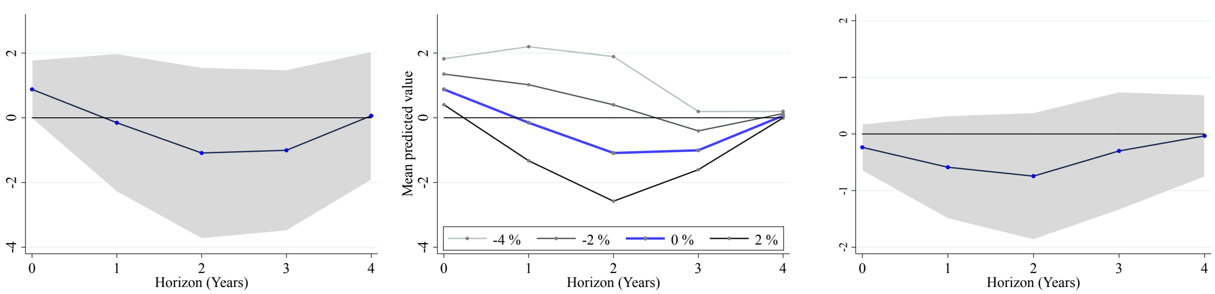
Figure 1.6: Impulse responses of number of employees using equation 1.3.



(a) Neutral Regime ($\hat{\beta^h}$) **(b)** Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$) **(c)** $\hat{\Gamma^h}$

Note: The plots show impulse responses of employees using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. *Dose* = 0% is equal to neutral regime plot. Shaded areas show 95% confidence intervals.

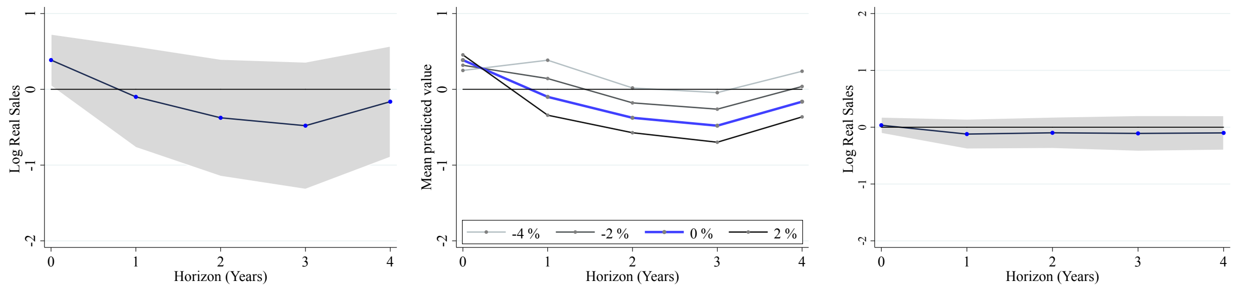
Figure 1.7: Impulse responses of investment using LP-IV specification in equation 1.3.



(a) Neutral Regime ($\hat{\beta^h}$) **(b)** Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$) **(c)** $\hat{\Gamma^h}$

Note: The plots show impulse responses of investment using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. *Dose* = 0% is equal to neutral regime plot. Shaded areas show 95% confidence intervals.

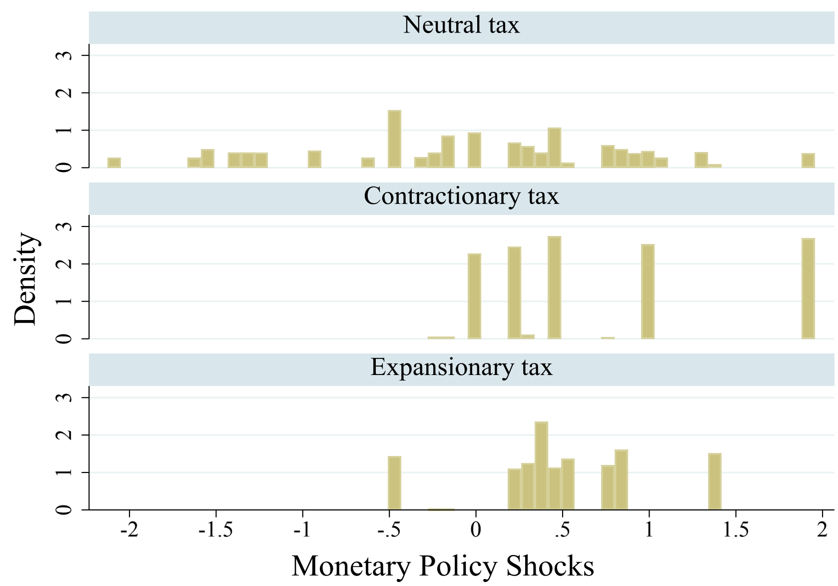
Figure 1.8: Impulse responses of log real sales using LP-IV specification in equation 1.3.



(a) Neutral Regime ($\hat{\beta^h}$) **(b)** Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$) **(c)** $\hat{\Gamma^h}$

Note: The plots show impulse responses of sales using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. *Dose* = 0% is equal to neutral regime plot. Shaded areas show 95% confidence intervals.

Figure 1.9: Histogram of monetary policy shocks across regimes (1969-2006)



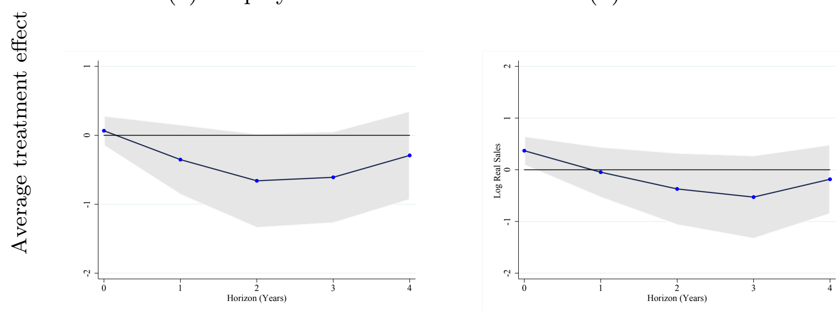
Note: The figure plots distribution of Wieland and Yang (2020) monetary policy shocks across different tax regimes from 1969 to 2006. Neutral regime confirms the randomly distributed monetary policy shocks. The shocks in contractionary and expansionary regimes are also comparable in terms of having a right skewed distribution. The -0.5 in the expansionary tax regime corresponds to year 1981 and is controlled in the regressions with a year dummy.

The average causal effects of monetary policy shocks estimated using local projection:

$$y_{j,t+h} - y_{j,t-1} = \alpha_j^h + \beta^h \Delta \mathrm{R}_t + \Omega'(L)\, Z_{j,t-1} + \epsilon_{j,t+h} \text{ where } h = 0, 1, ..., H\ , \tag{1.1}$$

Figure 1.10: Impulse responses to monetary policy shocks (1969-2006).

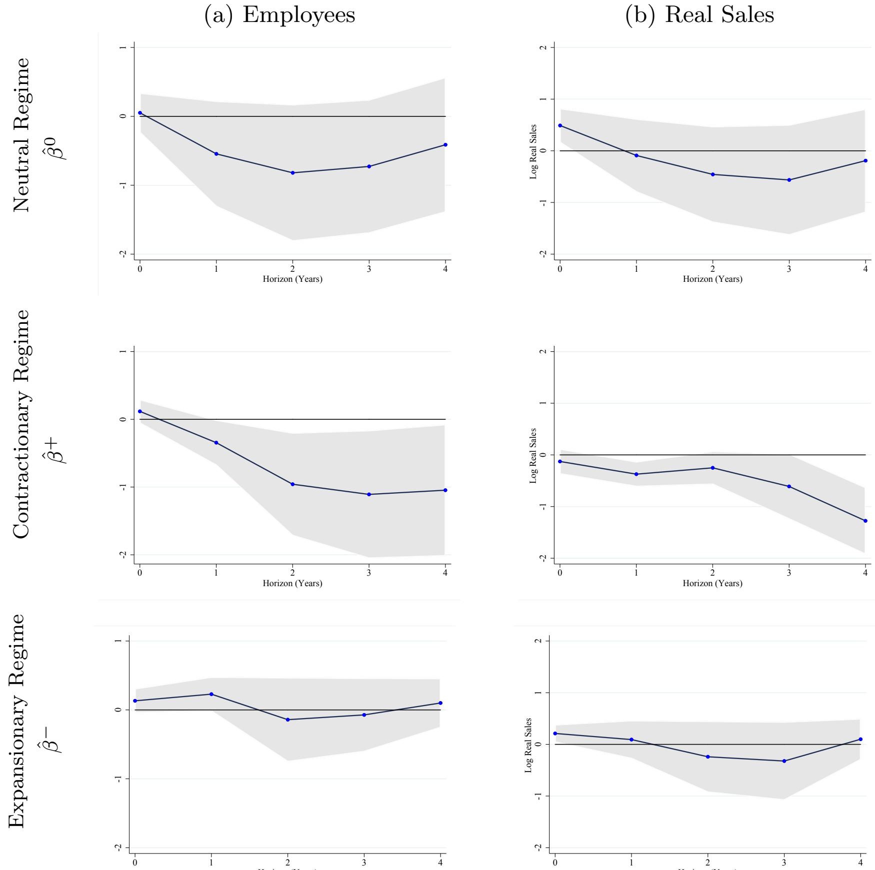
(a) Employees (b) Real Sales



Note: The plots show impulse responses of employees and sales using RR monetary policy shocks as instruments for one year treasury rate. Horizon is 4 years, lag is set to 2. Both specifications include a year dummy for 1981 and 2001. Standard errors are clustered by firm and year. Controls follow the baseline specification.

Figure 1.11: Impulse responses to monetary policy shocks by regime (1969-2006).

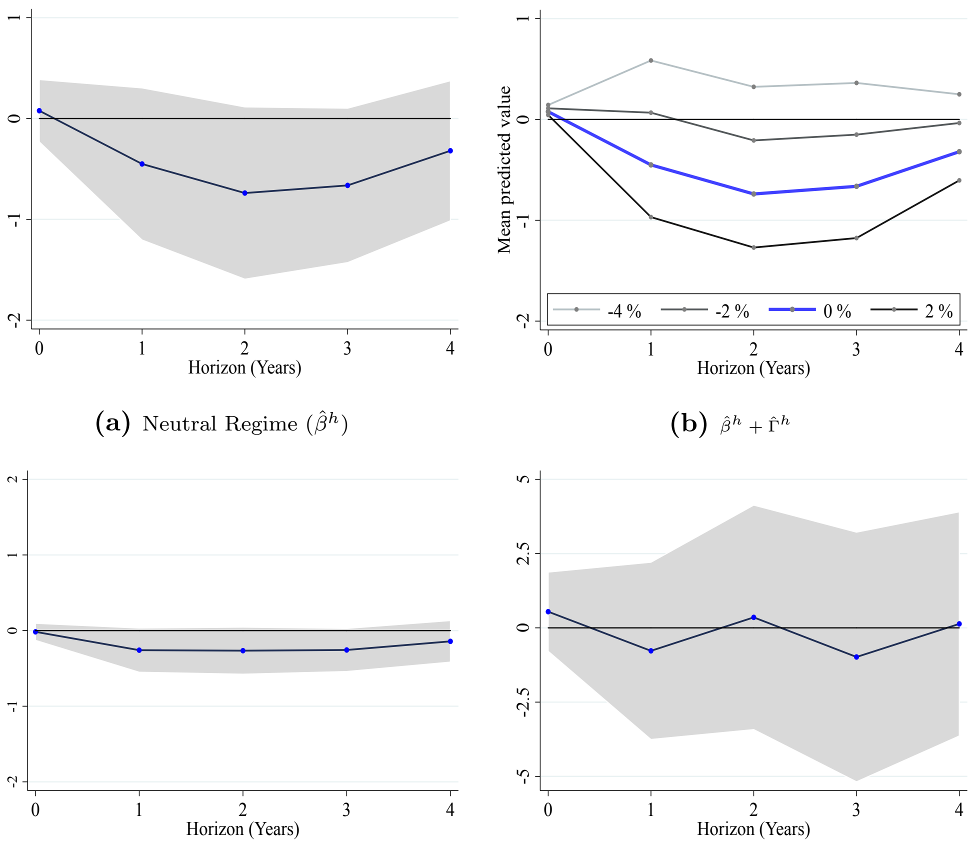
(a) Employees (b) Real Sales



Note: The plots show impulse responses to monetary policy shocks. Each figure is generated separately in the respective sample to capture sample average effects. Specifications include a year dummy for 1981 and 2001. Contractionary regime spans years 1984-1988 and 1993-1997. Expansionary regime spans years 1979-1991.

Baseline specification in details

Figure 1.12: Impulse responses of number of employees to monetary shocks.



(a) Neutral Regime ($\hat{\beta}^h$)

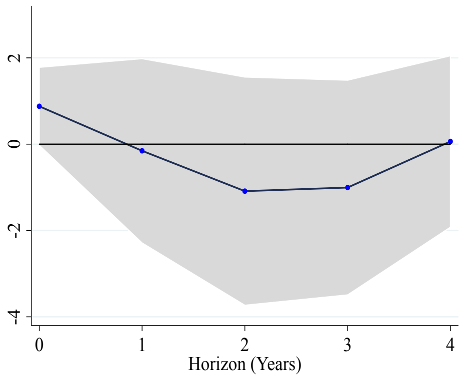
(b) $\hat{\beta}^h + \hat{\Gamma}^h$

(c) $\hat{\Gamma}^h$

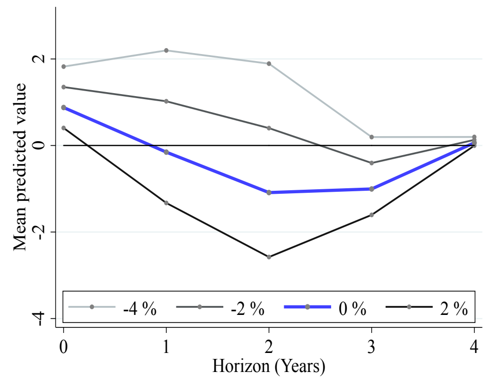
(d) $\hat{\theta}^h$

Note: The plots show impulse responses of employees using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. Standard errors are clustered by firm and year. *Dose* = 0% is equal to neutral regime plot. Shaded areas show 95% confidence intervals.

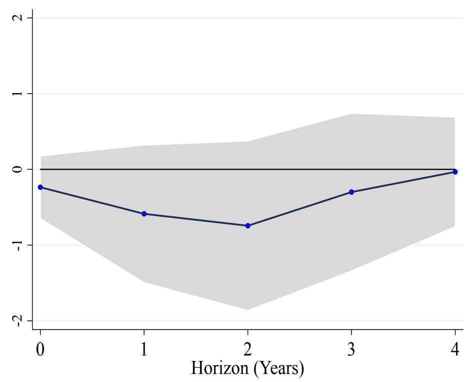
Figure 1.13: Impulse responses of log investment to monetary shocks.



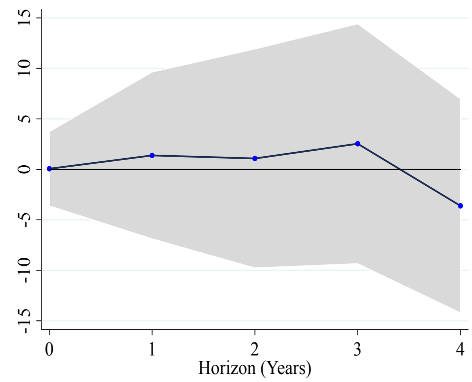
(a) Neutral Regime ($\hat{\beta}^h$)



(b) $\hat{\beta}^h + \hat{\Gamma}^h$



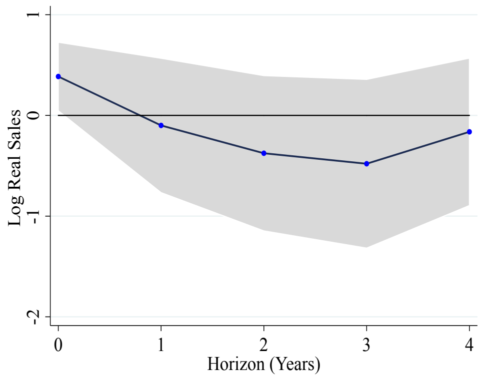
(c) $\hat{\Gamma}^h$



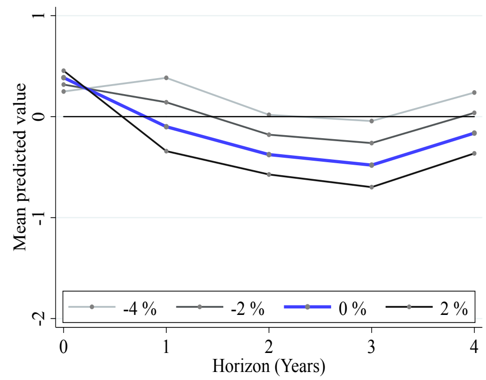
(d) $\hat{\theta}^h$

Note: The plots show impulse responses of investment using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. Standard errors are clustered by firm and year. $Dose = 0\%$ is equal to neutral regime plot. Shaded areas show 95% confidence intervals.

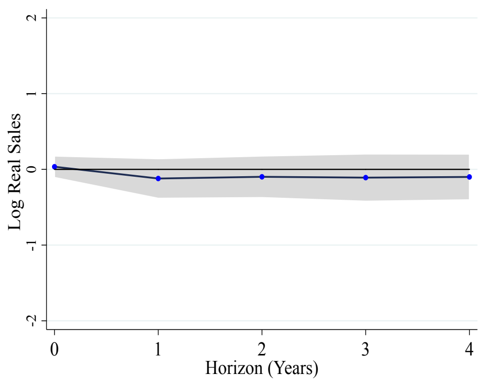
Figure 1.14: Impulse responses of log sales to monetary shocks.



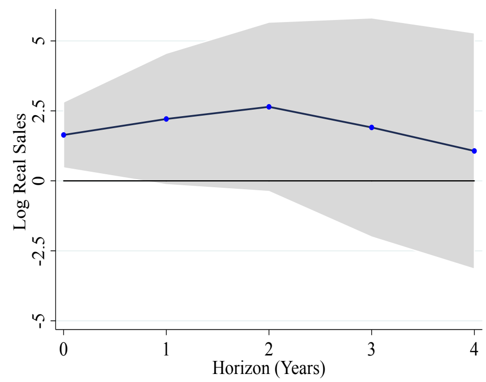
(a) Neutral Regime ($\hat{\beta}^h$)



(b) $\hat{\beta}^h + \hat{\Gamma}^h$



(c) $\hat{\Gamma}^h$



(d) $\hat{\theta}^h$

Note: The plots show impulse responses of employees using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. Standard errors are clustered by firm and year. *Dose* = 0% is equal to neutral regime plot. Shaded areas show 95% confidence intervals.

Robustness Figures

Less parametric specification:

$$\Delta_h log(y_{j,t+h}) = \alpha_j^{h,r} + \beta^h * \Delta\, \mathrm{R}_t + \beta^{h,r} * \Delta\, \mathrm{R}_t * \mathbb{1}\{\Delta\tau_{j,t}^r\} * |Dose_{j,t}| + \delta^{h,r} * |Dose_{j,t}^r| + \theta^{h,r} + \Omega'(L)^{h,r} * Z_{j,t-1}^r + \epsilon_{\,j,t+h} \tag{1.2}$$

Figure 1.15: Impulse responses of employees using specification 1.2.

(a) β **(b)** β^+ **(c)** β^-

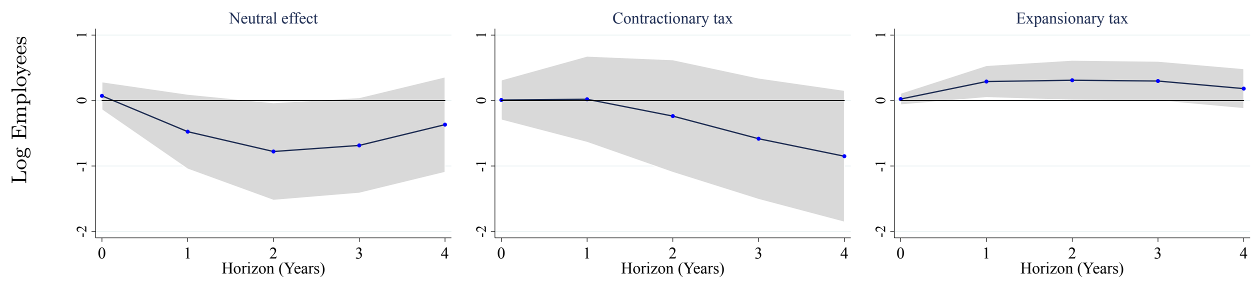


Figure 1.16: Impulse responses of capital expenditures using specification 1.2.

(a) β **(b)** β^+ **(c)** β^-

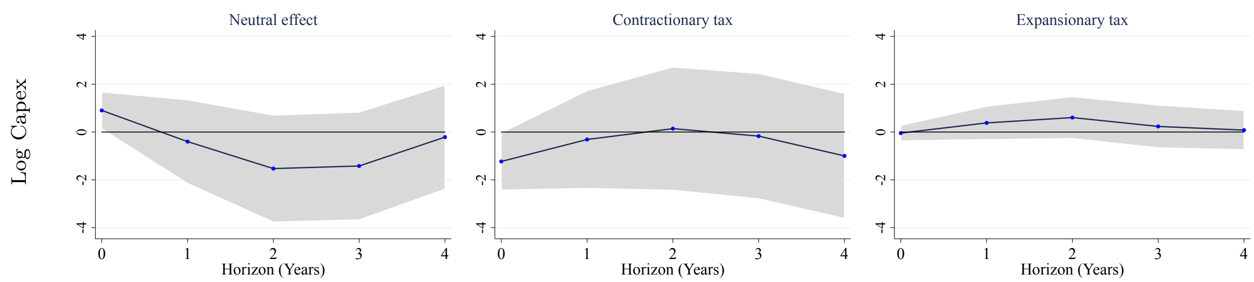


Figure 1.17: Impulse responses of sales using specification 1.2.

(a) β **(b)** β^+ **(c)** β^-

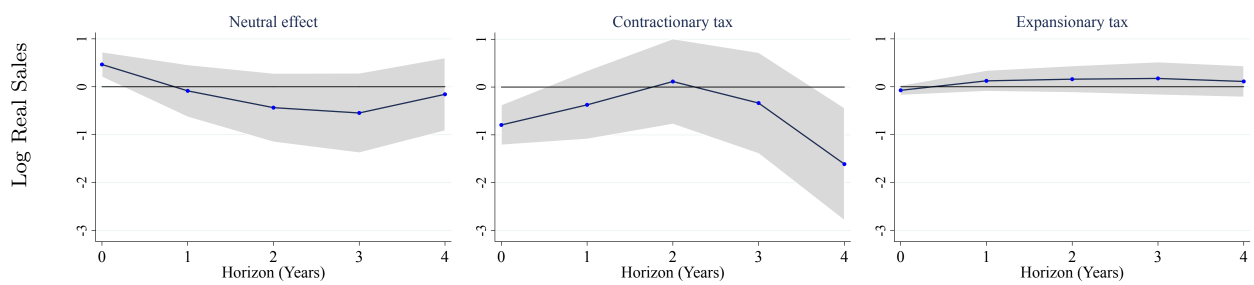
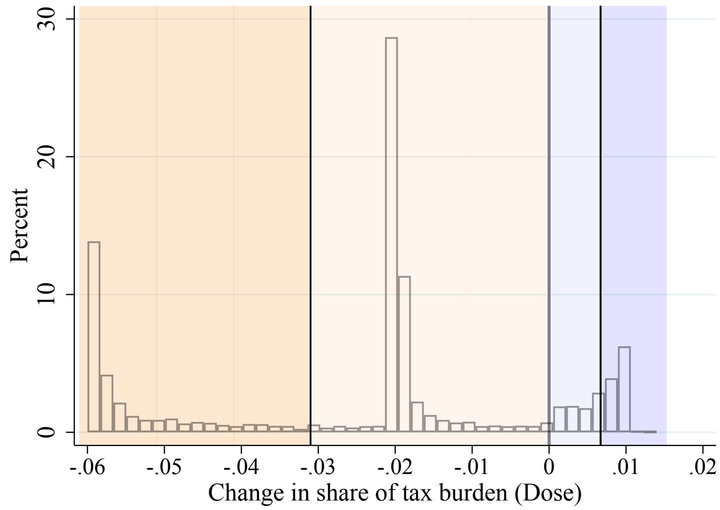
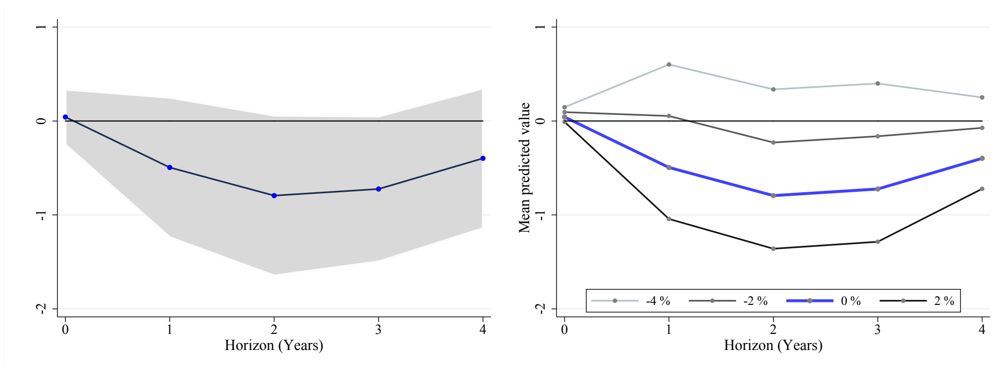


Figure 1.18: Histogram of changes in tax burden (dose) by regime bins



Note: This figure plots the histogram of tax burden changes across the sample. The dose is calculated as change in tax liability over lagged taxable income. Negative doses are from expansionary tax changes and positive doses are from contractionary tax changes. The dark shades reflect higher dose treatments. The gray thresholds show the mean dose of each regime on points -0.031 and 0.0067. For instance, if a firm receives tax dose lower than -0.031, it is labeled as receiving a high expansionary tax treatment.

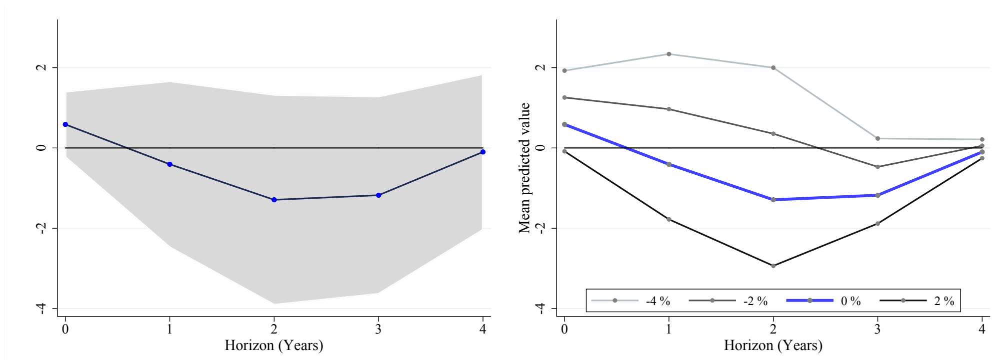
Figure 1.19: Impulse responses of employees using additional controls in equation 1.3.



(a) Neutral Regime ($\hat{\beta^h}$) **(b)** Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$)

Note: The plots show impulse responses of employees using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate, an indicator variable for dividends paid, leverage ratio and liquidity ratio. Standard errors are clustered by firm and year. *Dose* = 0% is equal to neutral regime plot. Shaded areas show 95% confidence intervals.

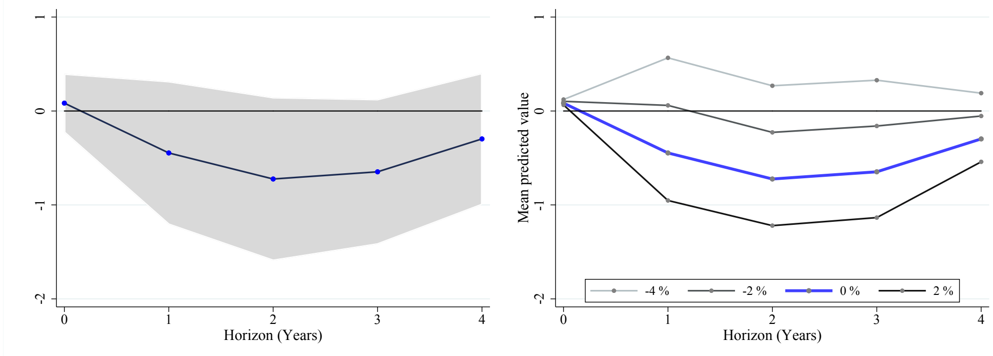
Figure 1.20: Impulse responses of investment using additional controls in equation 1.3.



(a) Neutral Regime ($\hat{\beta^h}$) **(b)** Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$)

Note: The plots show impulse responses of investment using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate, an indicator variable for dividends paid, leverage ratio and liquidity ratio. Standard errors are clustered by firm and year. *Dose* = 0% is equal to neutral regime plot. Shaded areas show 95% confidence intervals.

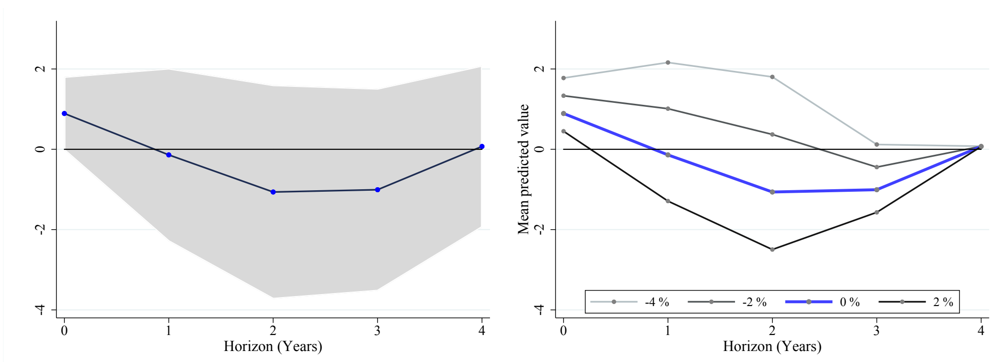
Figure 1.21: Impulse responses of employees using additional income restrictions in equation 1.3.



(a) Neutral Regime ($\hat{\beta^h}$) **(b)** Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$)

Note: The plots show impulse responses of employees using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. $Dose = 0\%$ is equal to neutral regime plot. Shaded area in panel (a) shows 95% confidence intervals.

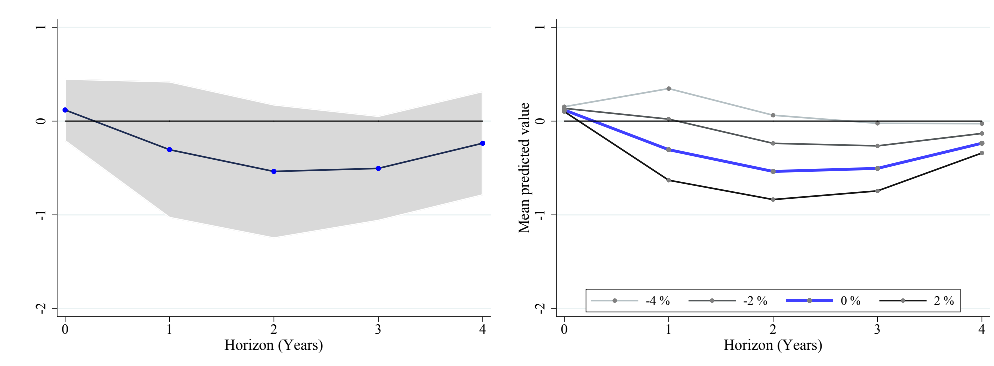
Figure 1.22: Impulse responses of investment using additional income restrictions in equation 1.3.



(a) Neutral Regime ($\hat{\beta^h}$) **(b)** Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$)

Note: The plots show impulse responses of investment using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. $Dose = 0\%$ is equal to neutral regime plot. Shaded area in panel (a) shows 95% confidence intervals.

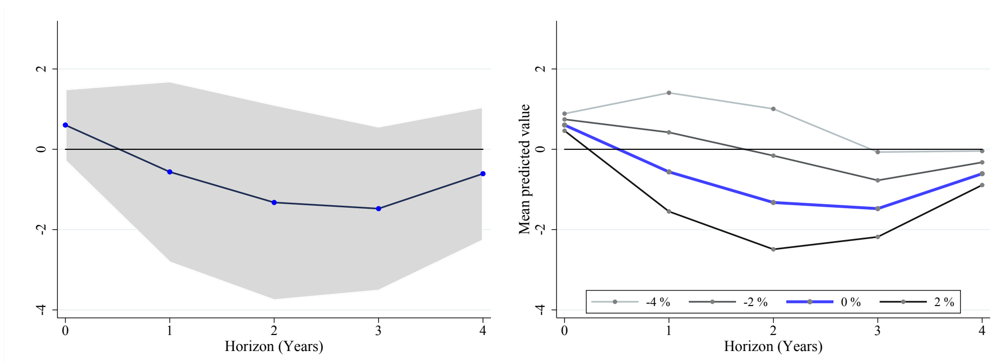
Figure 1.23: Impulse responses of employees using extended regime length in equation 1.3.



(a) Neutral Regime ($\hat{\beta^h}$) **(b)** Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$)

Note: The plots show impulse responses of employees using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. *Dose* = 0% is equal to neutral regime plot. Shaded area in panel (a) shows 95% confidence intervals.

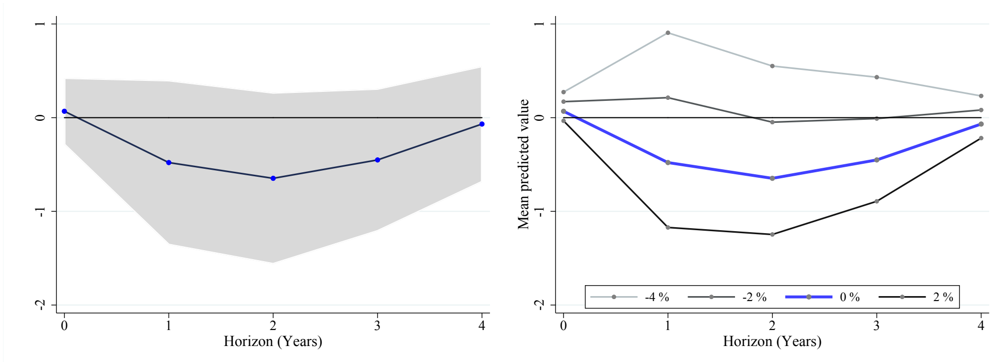
Figure 1.24: Impulse responses of investment using extended regime length in equation 1.3.



(a) Neutral Regime ($\hat{\beta^h}$) **(b)** Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$)

Note: The plots show impulse responses of investment using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. *Dose* = 0% is equal to neutral regime plot. Shaded area in panel (a) shows 95% confidence intervals.

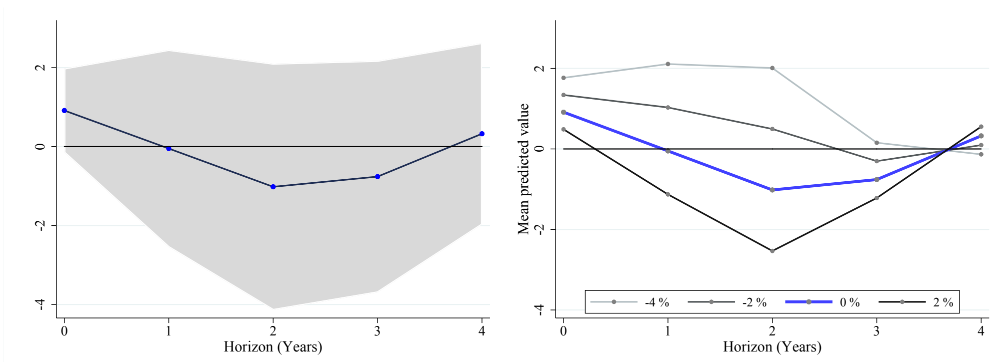
Figure 1.25: Impulse responses of employees using full set of tax reforms in equation 1.3.



(a) Neutral Regime ($\hat{\beta^h}$) **(b)** Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$)

Note: The plots show impulse responses of employees using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. Full set of tax reform (both endogenous and exogenous) are used in firm grouping. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. *Dose* = 0% is equal to neutral regime plot. Shaded area in panel (a) shows 95% confidence intervals.

Figure 1.26: Impulse responses of investment using full set of tax reforms in equation 1.3.



(a) Neutral Regime ($\hat{\beta^h}$) **(b)** Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$)

Note: The plots show impulse responses of investment using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. Full set of tax reform (both endogenous and exogenous) are used in firm grouping. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. *Dose* = 0% is equal to neutral regime plot. Shaded areas show 95% confidence intervals.

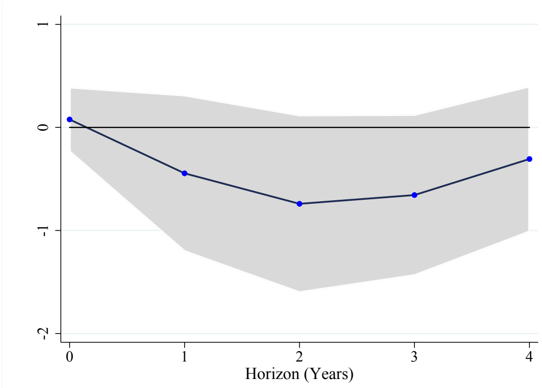
Heterogeneity in other observable firm characteristics

This part shows that the heterogeneous effects of monetary policy are not driven by firm characteristics such as sales growth, asset growth or leverage. I expand the baseline specification as:

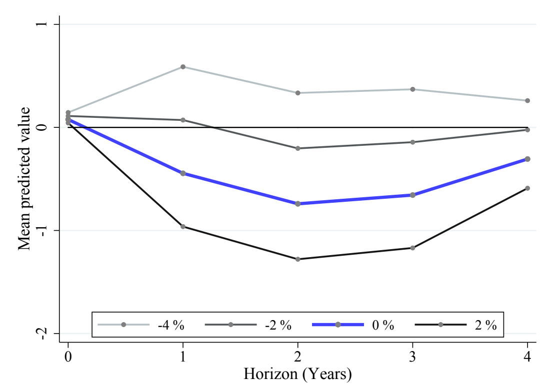
$$\Delta_h log(y_{j,t+h}) = \alpha_j^h + \beta^h \Delta \mathrm{R}_{\,t} + \Gamma^h \Delta \mathrm{R}_{\,t} \,\mathrm{Dose}_{j,t} + \beta_y^h \,\Delta \mathrm{R}_t \; s_{j,t} + \theta^h \mathrm{Dose}_{j,t} + \Omega'(L)\; Z_{j,t-1} + \epsilon_{\,j,t+h} \tag{1.3}$$

where s_{jt} is lagged real sales growth, lagged leverage and lagged real asset growth. In each case, the coefficient on monetary and tax regime interaction remains robust. Hence, firm characteristics that may be correlated with tax regimes or monetary effectiveness do not drive the heterogeneous responses in tax regimes.

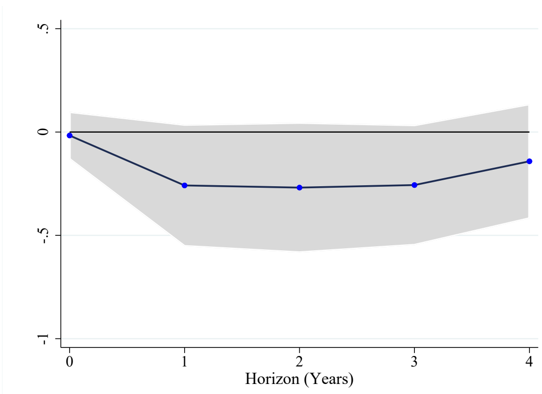
Figure 1.27: Impulse responses of employees using equation 1.3 with lagged asset growth.



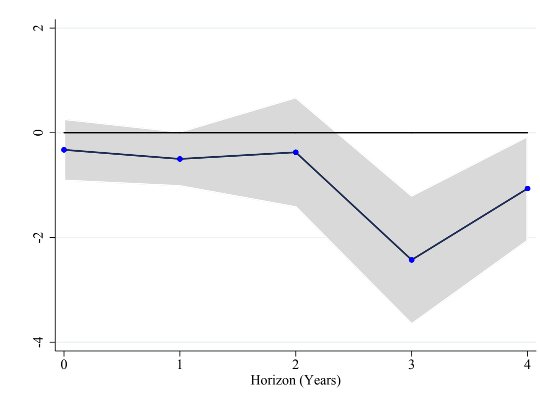
(a) Neutral Regime ($\hat{\beta^h}$)



(b) Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$)



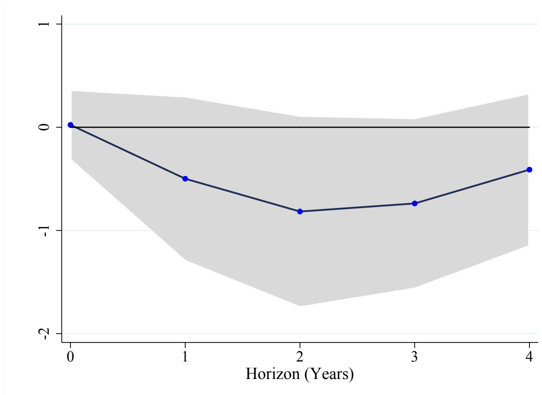
(c) $\hat{\Gamma^h}$



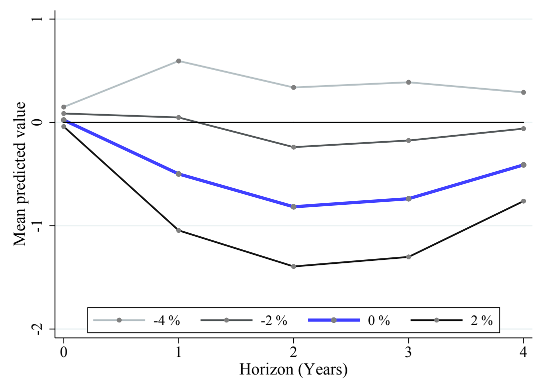
(d) $\hat{\beta_y^h}$

Note: The plots show impulse responses using equation 1.3 where s_t is lagged real asset growth. Firms are allowed to stay in a regime for a maximum of 5 years. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. The specification include a year dummy for 1981 and 2001 as dependent variable. Standard errors are clustered by firm and year.

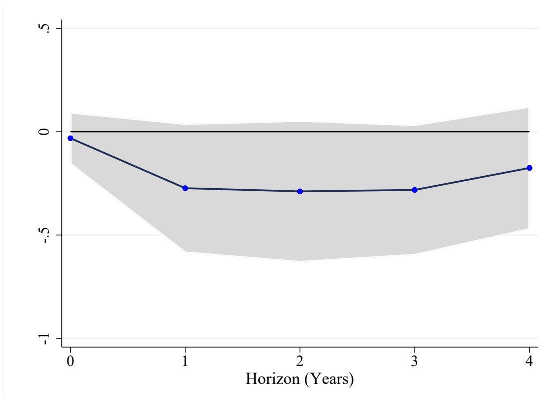
Figure 1.28: Impulse responses of employees using equation 1.3 with lagged sales growth.



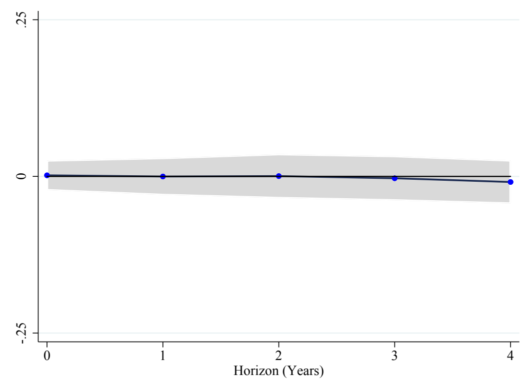
(a) Neutral Regime ($\hat{\beta^h}$)



(b) Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$)



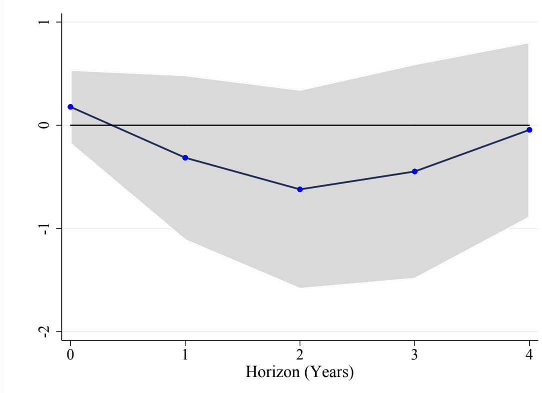
(c) $\hat{\Gamma^h}$



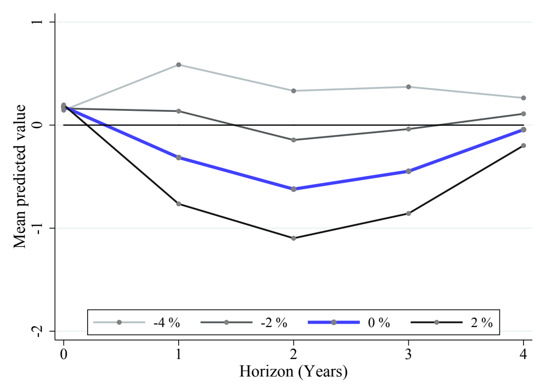
(d) $\hat{\beta^h_y}$

Note: The plots show impulse responses of equation 1.3 where s_t is lagged sales growth. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. The specification include a year dummy for 1981 and 2001 as dependent variable. Standard errors are clustered by firm and year.

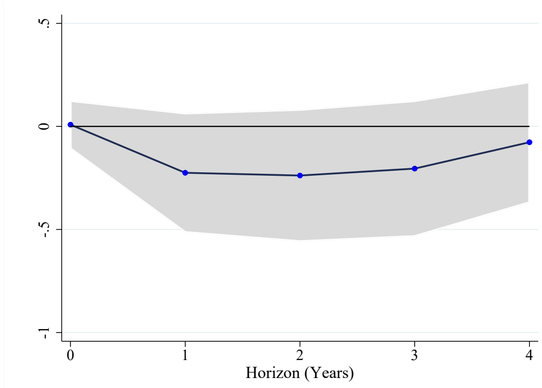
Figure 1.29: Impulse responses of employees using equation 1.3 with lagged leverage ratio.



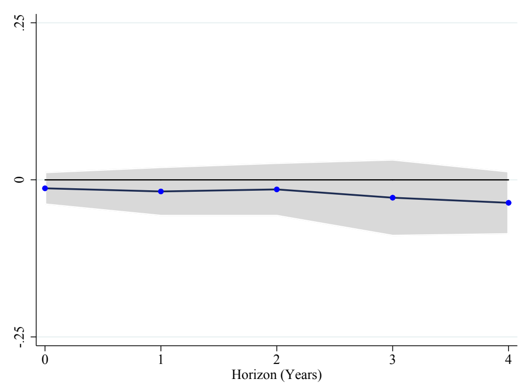
(a) Neutral Regime ($\hat{\beta^h}$)



(b) Total effects by dose ($\hat{\beta^h} + \hat{\Gamma^h}$)



(c) $\hat{\Gamma^h}$



(d) $\hat{\beta_y^h}$

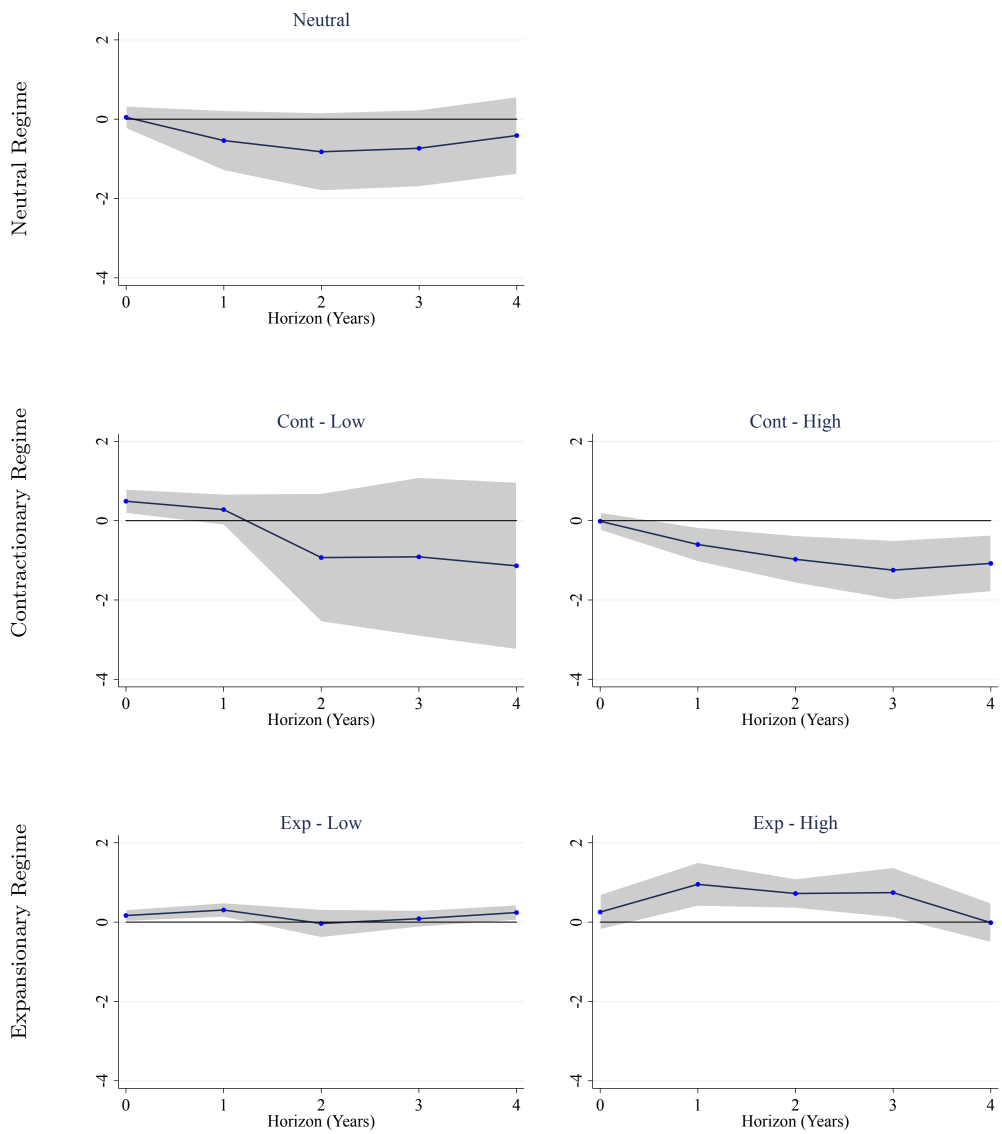
Note: The plots show impulse responses using IV local projection regressions with using equation 1.3 where s_t is lagged leverage ratio. Firms are allowed to stay in a regime for a maximum of 5 years. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. The specification includes a year dummy for 1981 and 2001 as dependent variable. Standard errors are clustered by firm and year.

Impact of liability changes on estimates

$$\Delta_h log(y_{j,t+h}) = \alpha_j^{h,r} + \theta^{h,r} + \beta^{h,r} * \Delta \mathrm{R}_t * \mathbb{1}\{\Delta\tau_{j,t}^r\} \; + \Omega'(L)^{h,r} * Z_{j,t-1}^r + \epsilon_{\,j,t+h} \tag{1.4}$$

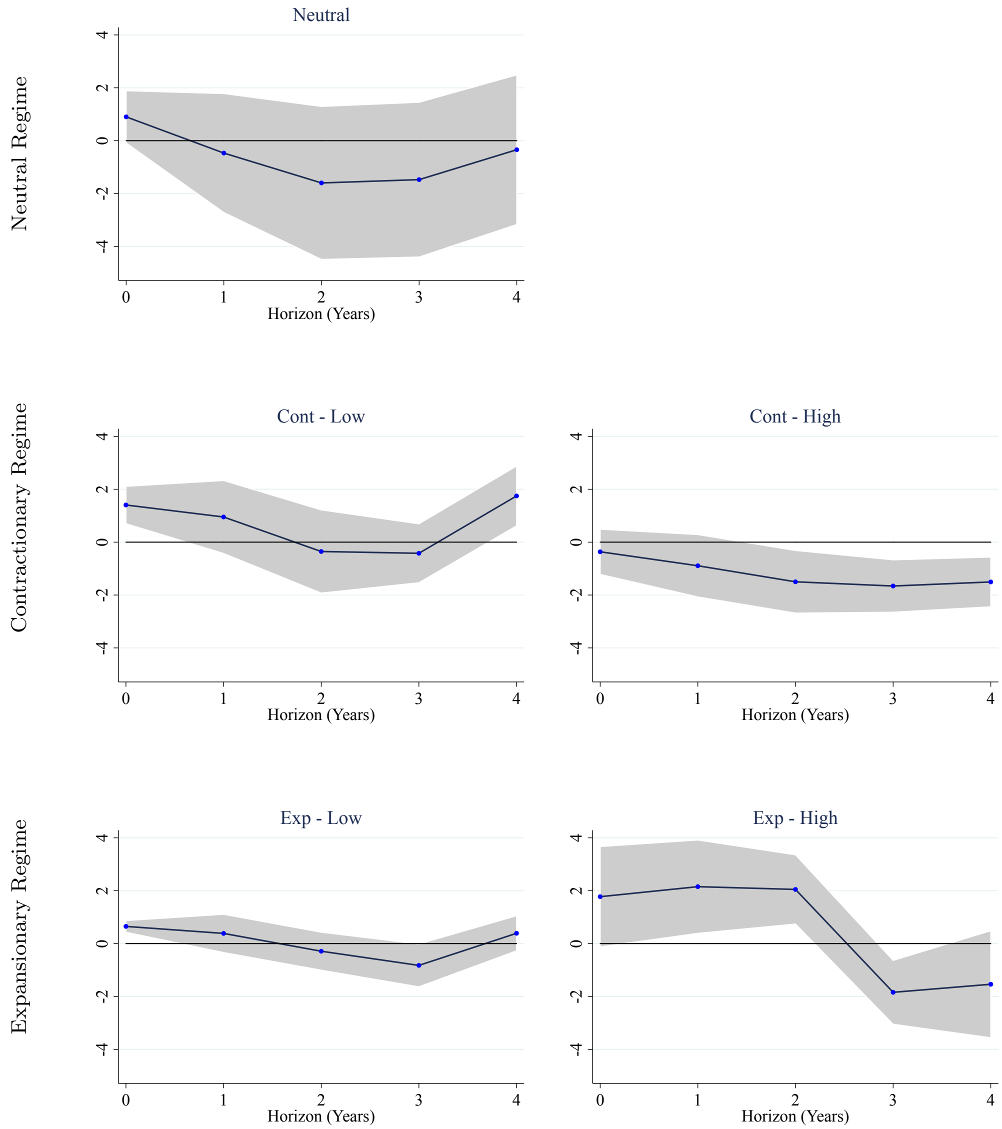
where r denotes regime-dose dummy bins: Expansionary low, Expansionary high, Contractionary low, Contractionary high and Neutral regime.

Figure 1.30: Impulse responses of number of employees using equation 1.4.



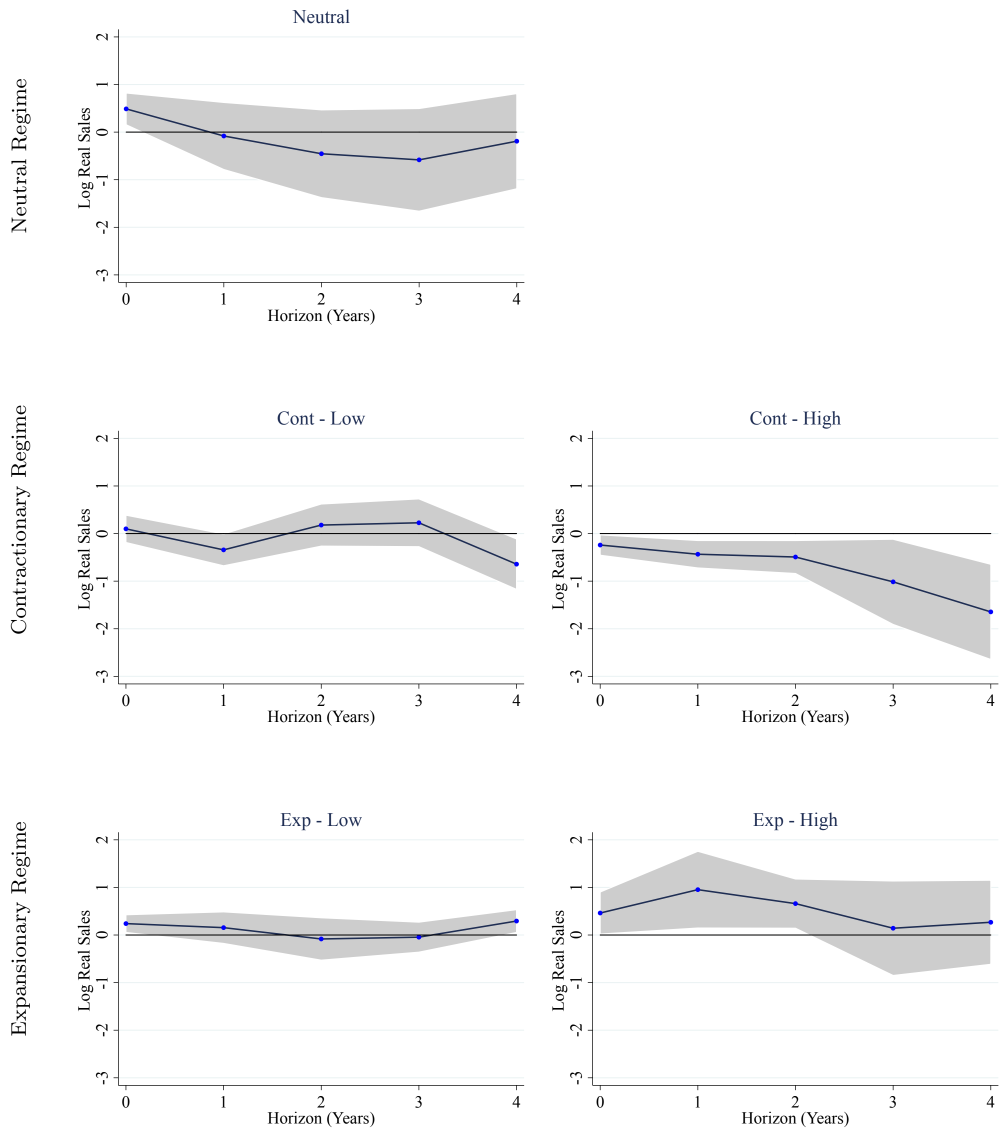
Note: The plots show impulse responses of number of employees using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid.

Figure 1.31: Impulse responses of investment using equation 1.4.



Note: The plots show impulse responses of investment using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid.

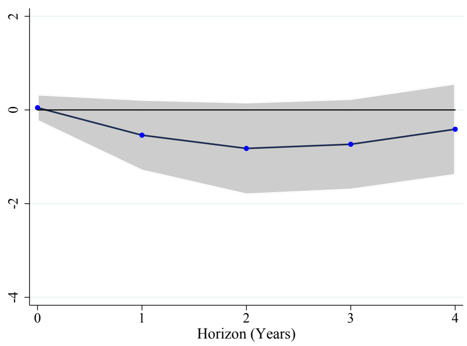
Figure 1.32: Impulse responses of log real sales using equation 1.4.



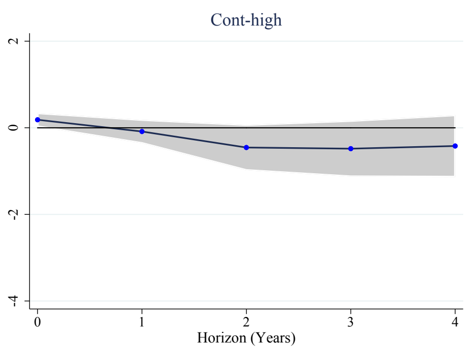
Note: The plots show impulse responses of log real sales using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid.

Impact of liability changes using quartiles of dose

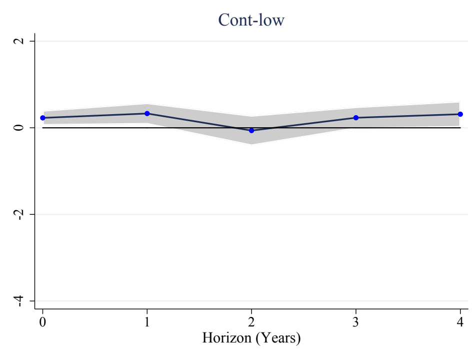
Figure 1.33: Impulse responses of number of employees using equation 1.4.



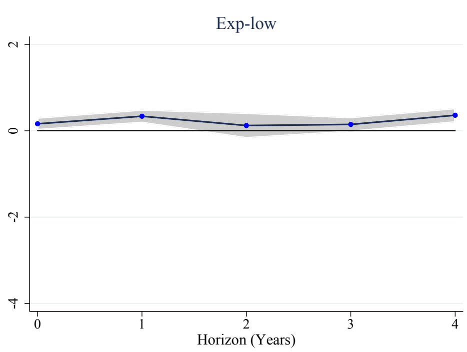
(a) Neutral regime



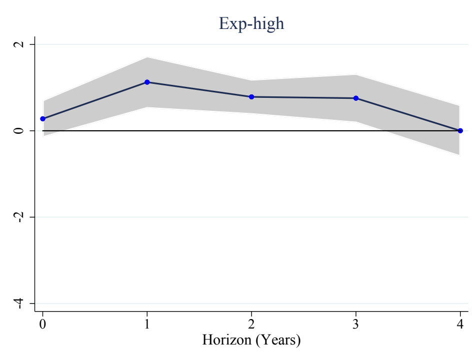
(b) Quartile 4



(c) Quartile 3



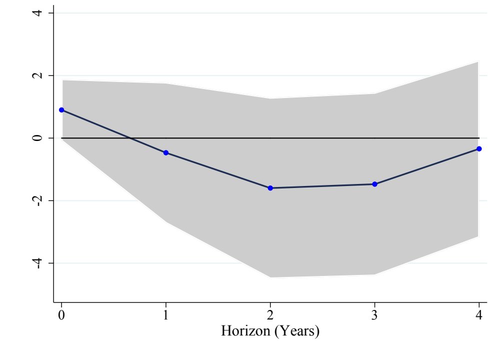
(d) Quartile 2



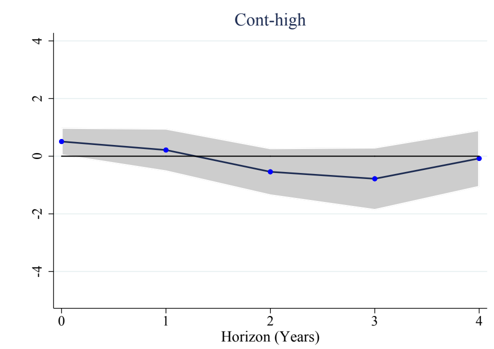
(e) Quartile 1

Note: The plots show impulse responses of number of employees using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. Quartile 1 corresponds to $-0.06 < dose < -0.028$ (e.g. high dose tax cut group), Quartile 2 corresponds to $-0.028 \leq dose < -0.013$, Quartile 3 corresponds to $-0.013 \leq dose < -0.003$, Quartile 4 corresponds to $-0.003 \leq dose$.

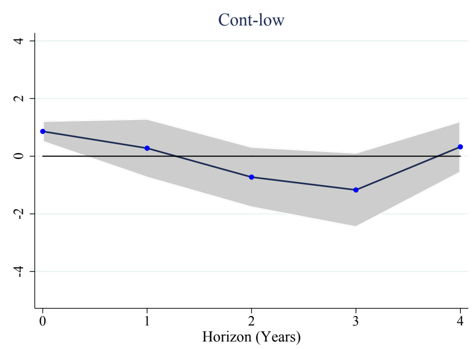
Figure 1.34: Impulse responses of investment using equation 1.4.



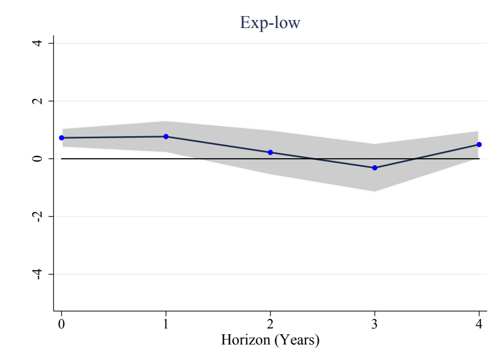
(a) Neutral regime



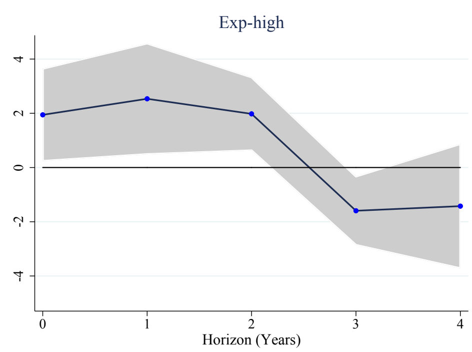
(b) Quartile 4



(c) Quartile 3



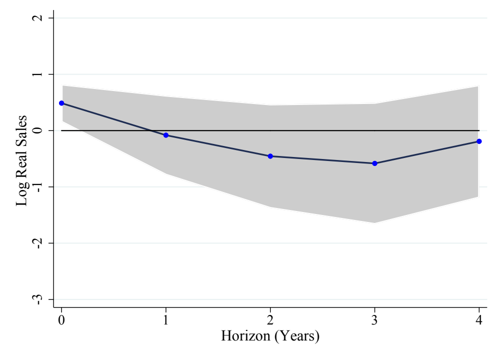
(d) Quartile 2



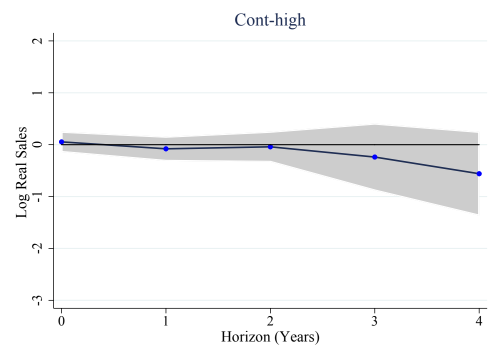
(e) Quartile 1

Note: The plots show impulse responses of investment using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 1. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. Quartile 1 corresponds to $-0.06 < dose < -0.028$ (e.g. high dose tax cut group), Quartile 2 corresponds to $-0.028 \leq dose < -0.013$, Quartile 3 corresponds to $-0.013 \leq dose < -0.003$, Quartile 4 corresponds to $-0.003 \leq dose$

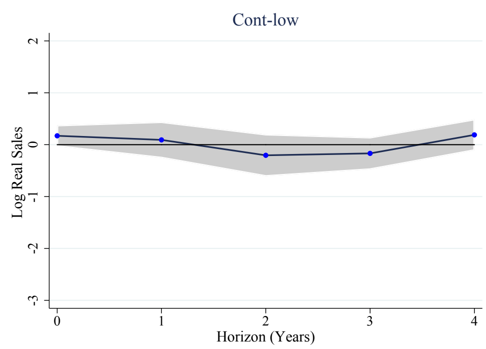
Figure 1.35: Impulse responses of sales using equation 1.4.



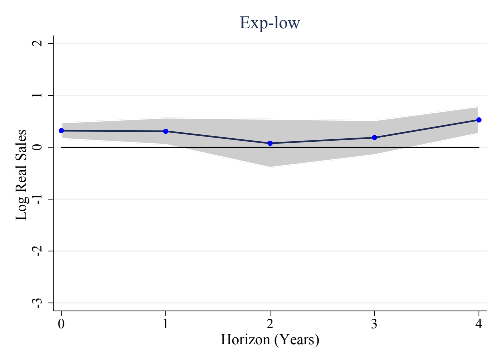
(a) Neutral regime



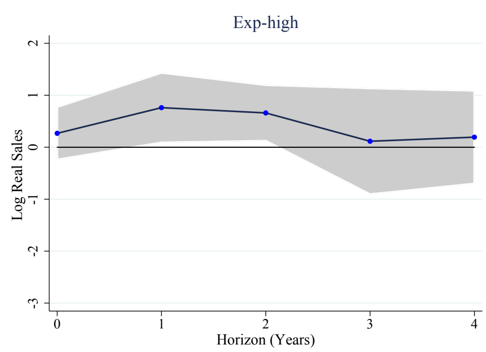
(b) Quartile 4



(c) Quartile 3



(d) Quartile 2



(e) Quartile 1

Note: The plots show impulse responses of investment using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. Horizon is 4 years, lag is set to 1. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. Quartile 1 corresponds to $-0.06 < dose < -0.028$ (e.g. high dose tax cut group), Quartile 2 corresponds to $-0.028 \leq dose < -0.013$, Quartile 3 corresponds to $-0.013 \leq dose < -0.003$, Quartile 4 corresponds to $-0.003 \leq dose$.

Heterogeneity Figures

Marginal tax rate estimates on employment based on firm size

Figure 1.36: Impulse responses of employees using LP-IV specification in equation 1.3.

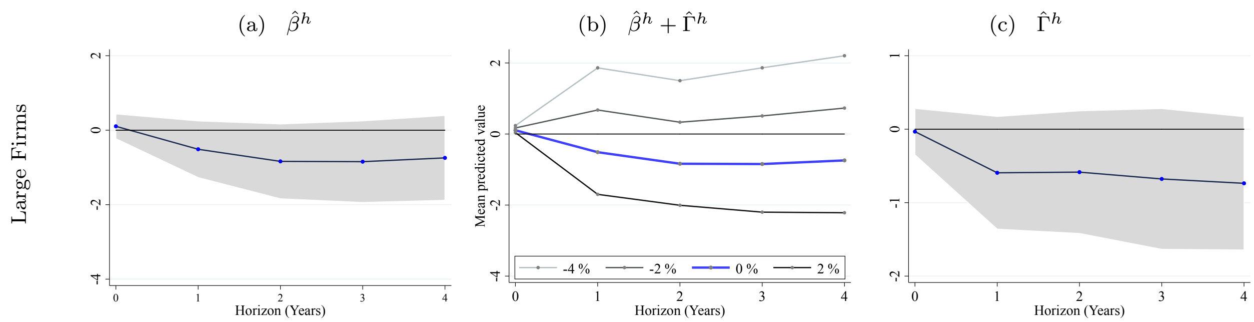
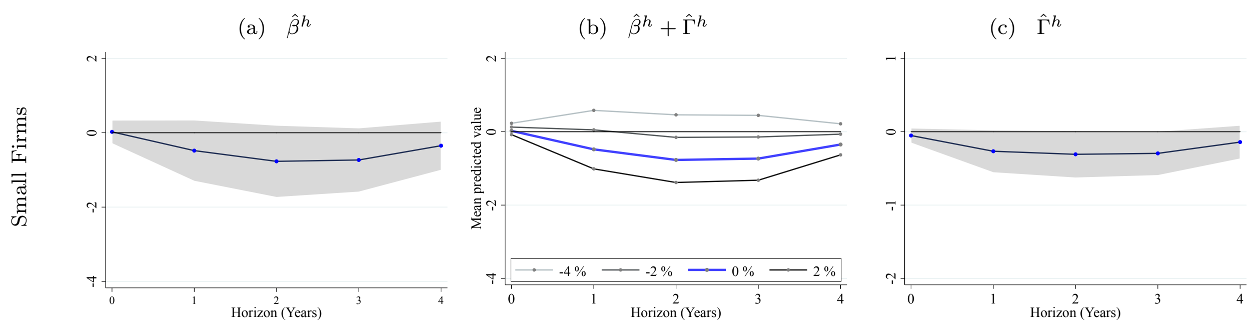


Figure 1.37: Impulse responses of employees using LP-IV specification in equation 1.3.



Note: The plots show impulse responses of employees using IV local projections. Horizon is 5 years, lag is set to 2. The time span is 1969-2006. Standard errors are clustered by firm and year. The left panel shows the impulse responses to monetary shock absent any tax intervention. The middle (right) panel shows the impulse responses to monetary shock for firms that received a tax hike (cut). Large (small) firms refers to firms with employment greater (less) than median company (906 employees).

Marginal tax rate estimates on investment based on firm size

Figure 1.38: Impulse responses of investment using LP-IV specification in equation 1.3.

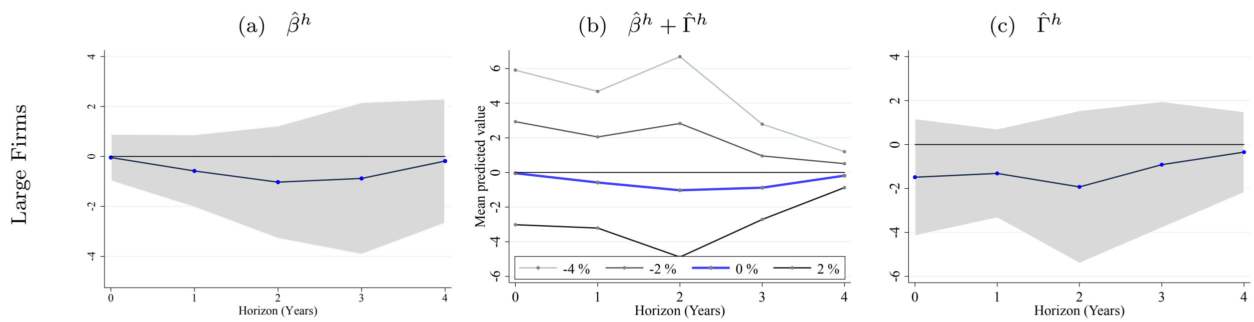
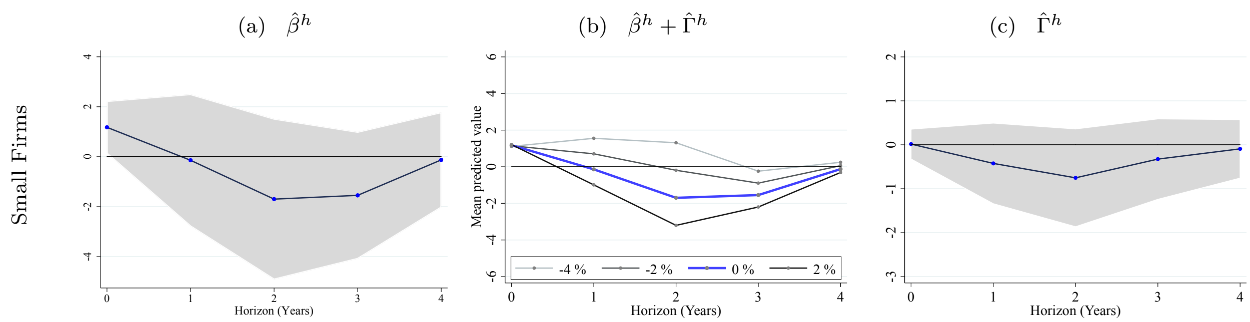


Figure 1.39: Impulse responses of investment using LP-IV specification in equation 1.3.



Note: The plots show impulse responses of investment using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. The time span is 1969-2006. Standard errors are clustered by firm and year. Large (small) firms refers to firms with employment greater (less) than median company (906 employees).

Marginal tax rate estimates on sales based on firm size

Figure 1.40: Impulse responses of log real sales using LP-IV specification in equation 1.3.

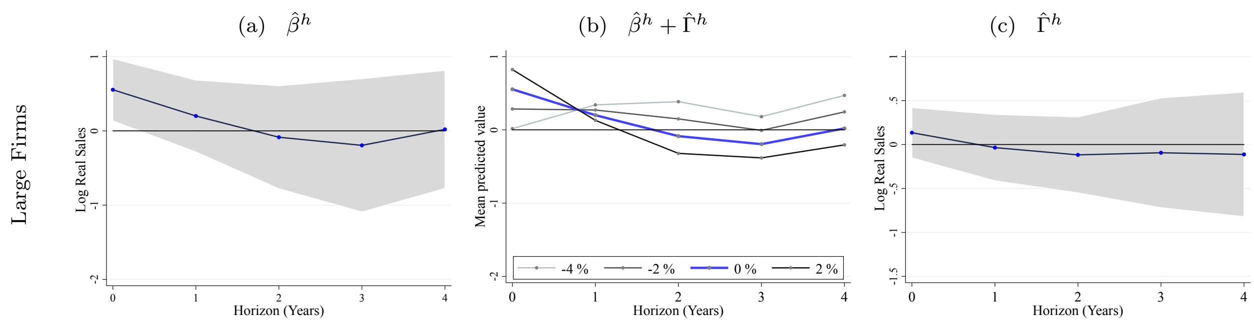
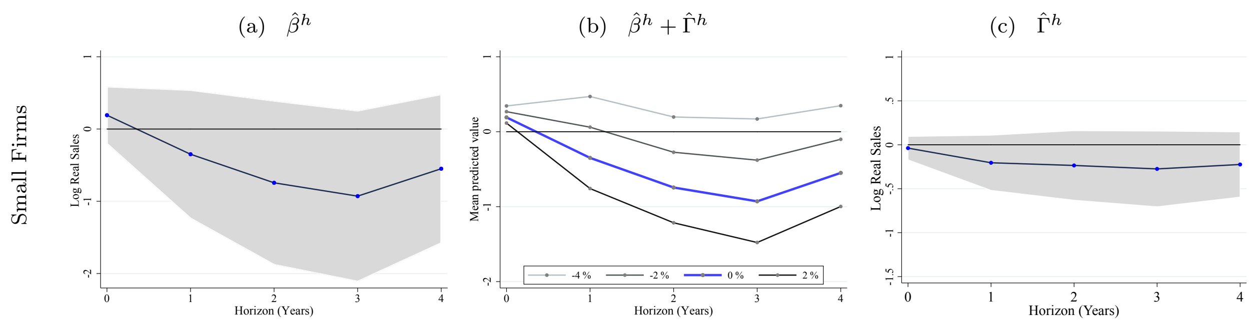


Figure 1.41: Impulse responses of log real sales using LP-IV specification in equation 1.3.



Note: The plots show impulse responses of log real sales using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. Large (small) firms refers to firms with employment greater (less) than median company (906 employees).

Marginal tax rate estimates on employment based on liquidity

Figure 1.42: Impulse responses of employees using LP-IV specification in equation 1.3.

(a) $\hat{\beta}^h$ (b) $\hat{\beta}^h + \hat{\Gamma}^h$ (c) $\hat{\Gamma}^h$

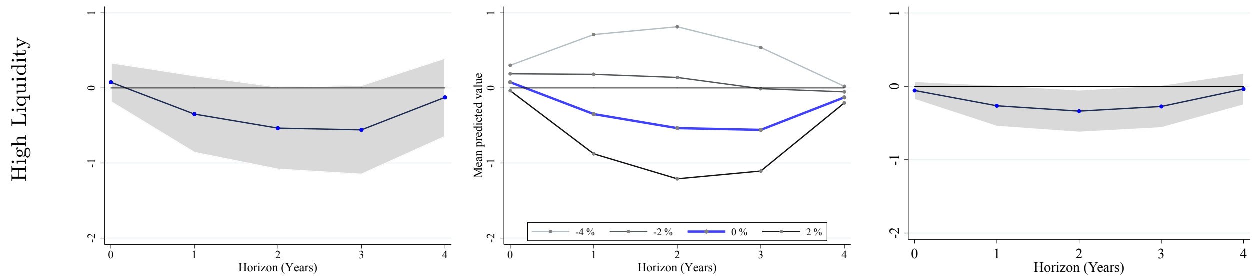
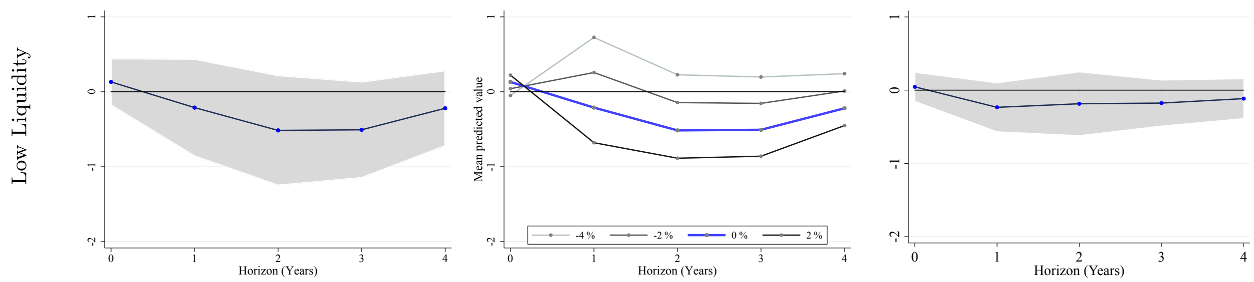


Figure 1.43: Impulse responses of employees using LP-IV specification in equation 1.3.

(a) $\hat{\beta}^h$ (b) $\hat{\beta}^h + \hat{\Gamma}^h$ (c) $\hat{\Gamma}^h$



Note: The plots show impulse responses of employees using IV local projections. Horizon is 5 years, lag is set to 2. The time span is 1969-2006. Standard errors are clustered by firm and year. The left panel shows the impulse responses to monetary shock absent any tax intervention. The middle (right) panel shows the impulse responses to monetary shock for firms that received a tax hike (cut). Large (small) firms refers to firms with employment greater (less) than median company (906 employees).

Marginal tax rate estimates on investment based on liquidity

Figure 1.44: Impulse responses of investment using LP-IV specification in equation 1.3.

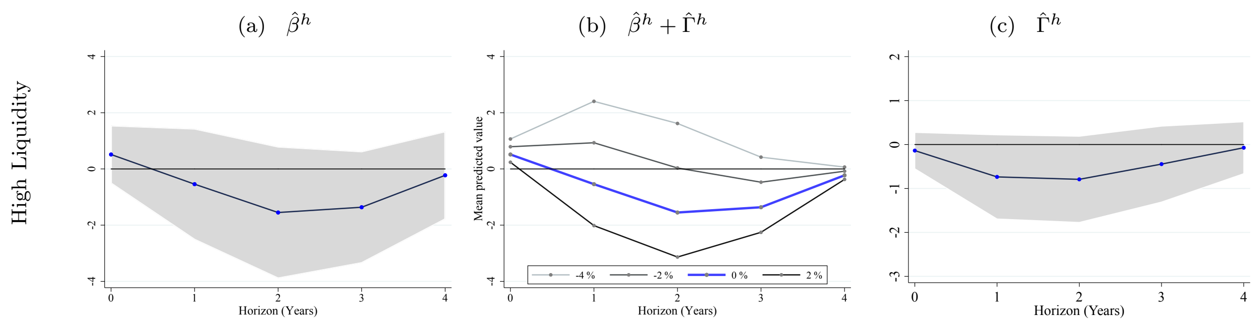
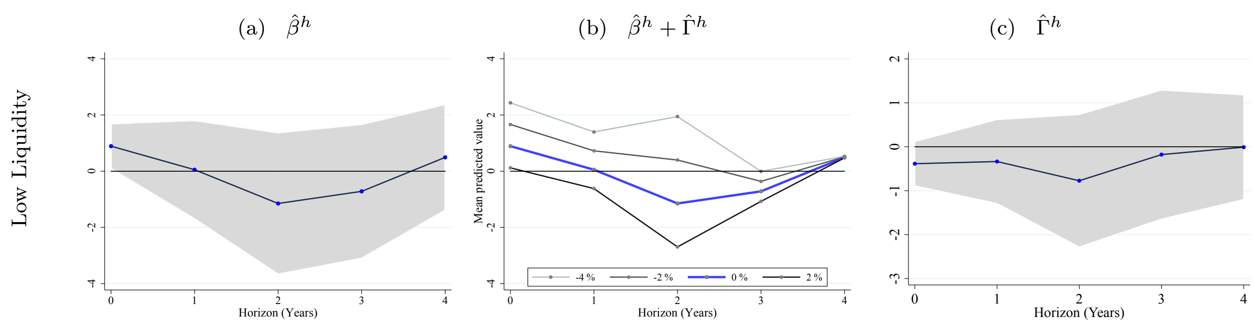


Figure 1.45: Impulse responses of investment using LP-IV specification in equation 1.3.



Note: The plots show impulse responses of investment using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. The time span is 1969-2006. Standard errors are clustered by firm and year. Large (small) firms refers to firms with employment greater (less) than median company (906 employees).

Marginal tax rate estimates on sales based on liquidity

Figure 1.46: Impulse responses of log real sales using LP-IV specification in equation 1.3.

(a) $\hat{\beta}^h$ (b) $\hat{\beta}^h + \hat{\Gamma}^h$ (c) $\hat{\Gamma}^h$

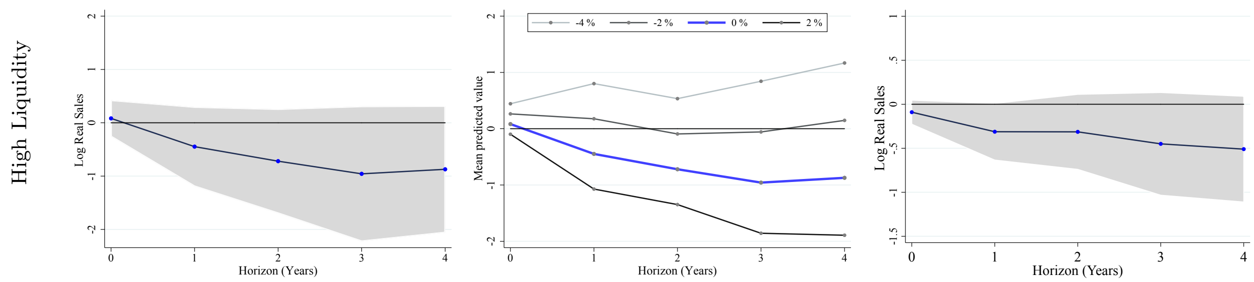
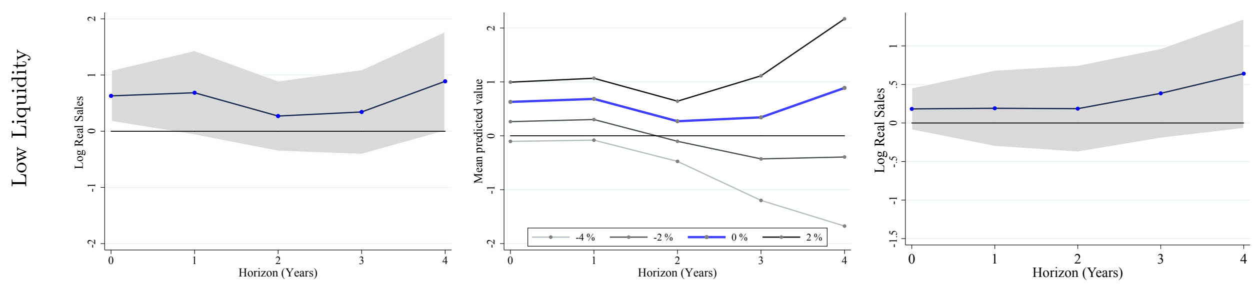


Figure 1.47: Impulse responses of log real sales using LP-IV specification in equation 1.3.

(a) $\hat{\beta}^h$ (b) $\hat{\beta}^h + \hat{\Gamma}^h$ (c) $\hat{\Gamma}^h$



Note: The plots show impulse responses of log real sales using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. Large (small) firms refers to firms with employment greater (less) than median company (906 employees).

Marginal tax rate estimates on employment based on leverage

Figure 1.48: Impulse responses of employees using LP-IV specification in equation 1.3.

(a) $\hat{\beta}^h$ (b) $\hat{\beta}^h + \hat{\Gamma}^h$ (c) $\hat{\Gamma}^h$

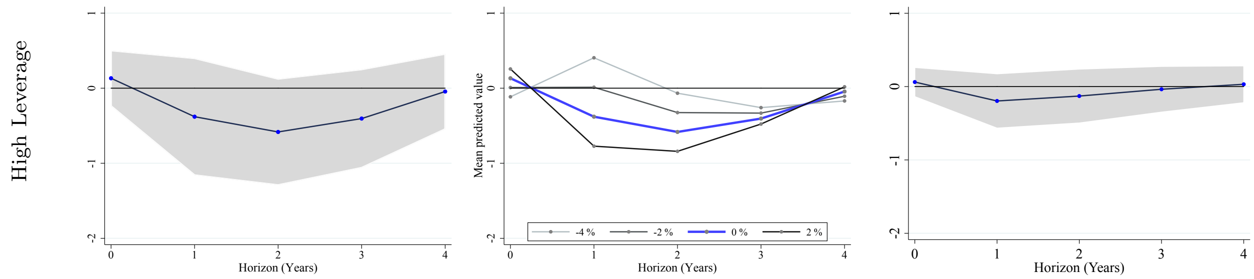
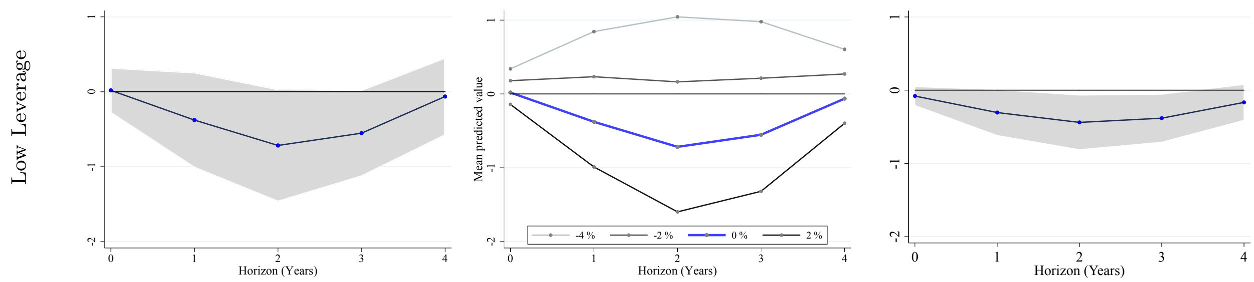


Figure 1.49: Impulse responses of employees using LP-IV specification in equation 1.3.

(a) $\hat{\beta}^h$ (b) $\hat{\beta}^h + \hat{\Gamma}^h$ (c) $\hat{\Gamma}^h$



Note: The plots show impulse responses of employees using IV local projections. Horizon is 5 years, lag is set to 2. The time span is 1969-2006. Standard errors are clustered by firm and year. The left panel shows the impulse responses to monetary shock absent any tax intervention. The middle (right) panel shows the impulse responses to monetary shock for firms that received a tax hike (cut). Large (small) firms refers to firms with employment greater (less) than median company (906 employees).

Marginal tax rate estimates on investment based on leverage

Figure 1.50: Impulse responses of investment using LP-IV specification in equation 1.3.

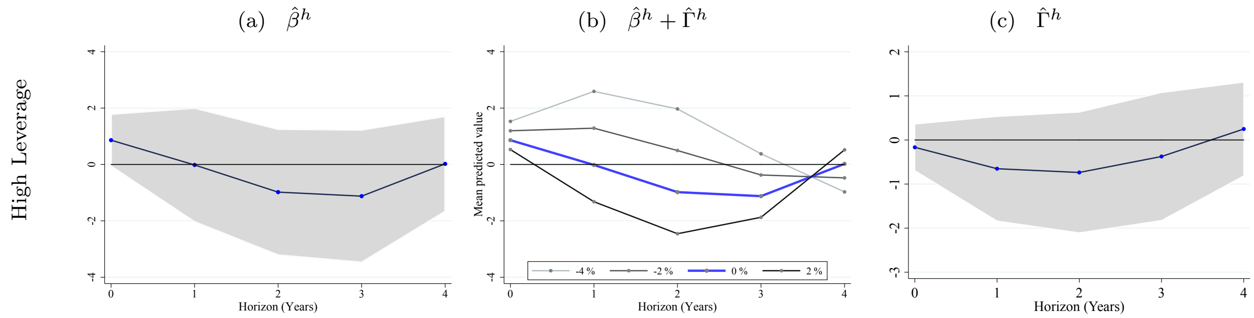
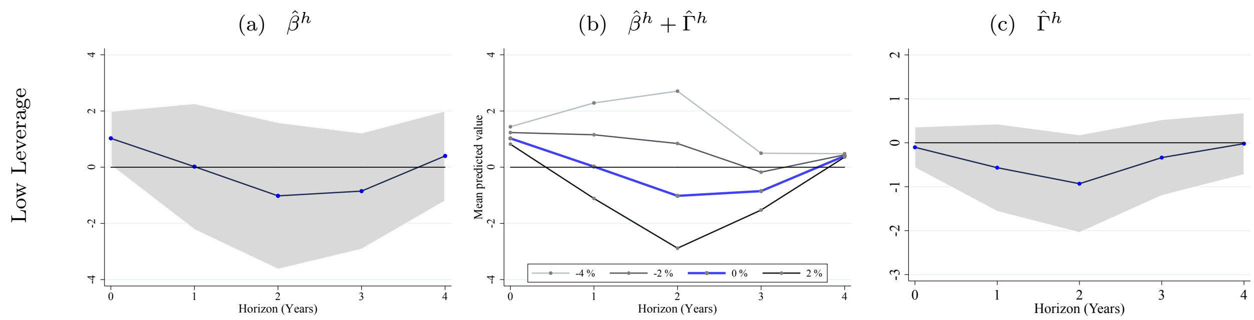


Figure 1.51: Impulse responses of investment using LP-IV specification in equation 1.3.



Note: The plots show impulse responses of investment using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. The time span is 1969-2006. Standard errors are clustered by firm and year. Large (small) firms refers to firms with employment greater (less) than median company (906 employees).

Marginal tax rate estimates on sales based on leverage

Figure 1.52: Impulse responses of log real sales using LP-IV specification in equation 1.3.

(a) $\hat{\beta}^h$ (b) $\hat{\beta}^h + \hat{\Gamma}^h$ (c) $\hat{\Gamma}^h$

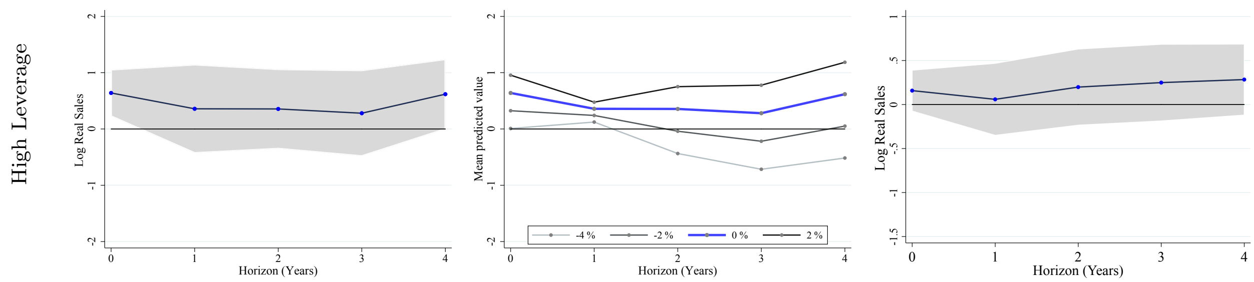
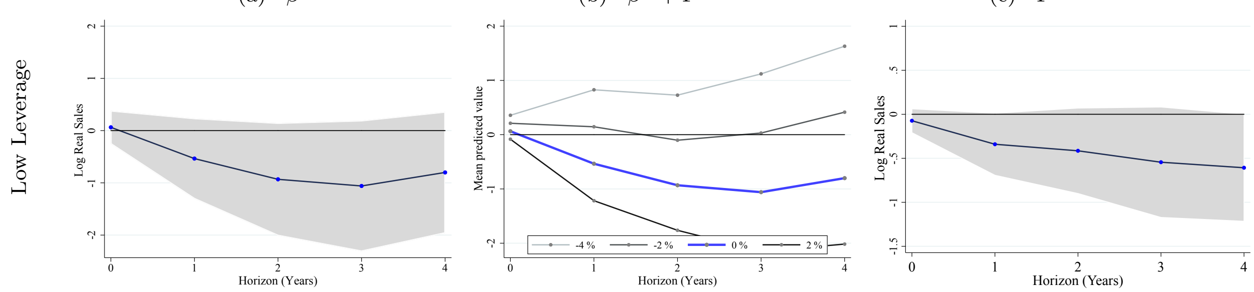


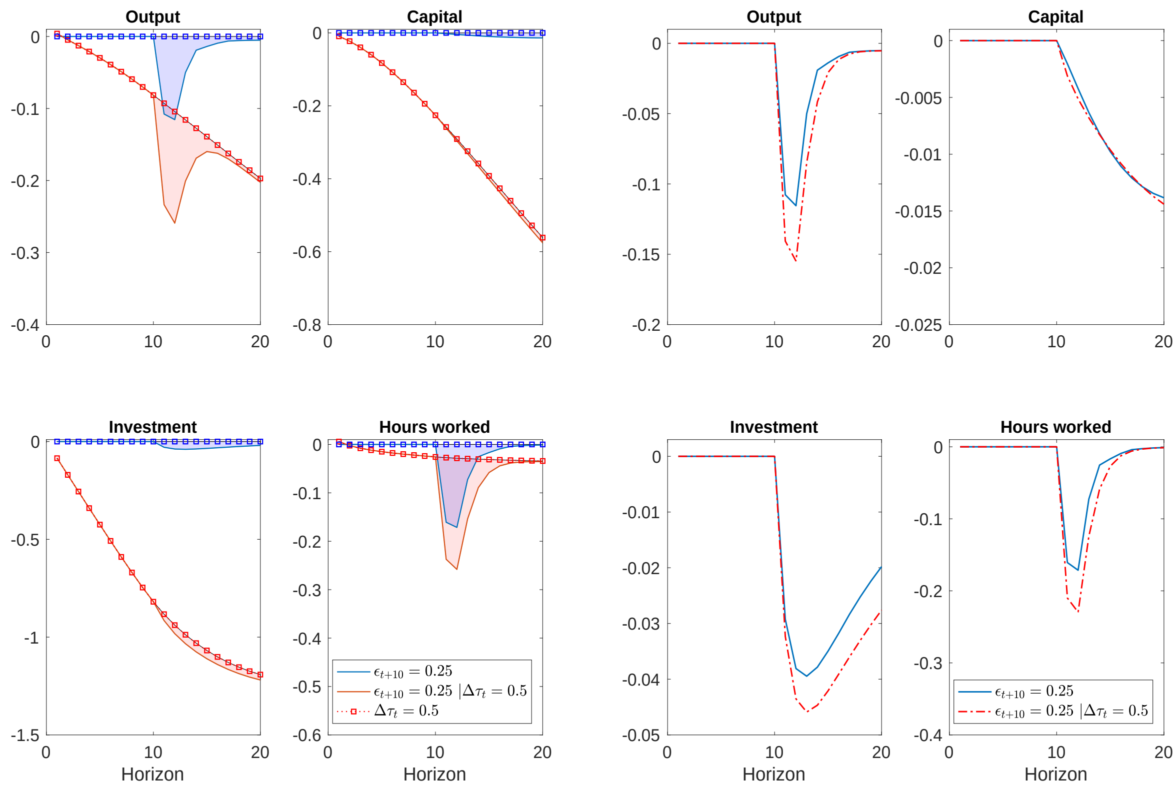
Figure 1.53: Impulse responses of log real sales using LP-IV specification in equation 1.3.

(a) $\hat{\beta}^h$ (b) $\hat{\beta}^h + \hat{\Gamma}^h$ (c) $\hat{\Gamma}^h$



Note: The plots show impulse responses of log real sales using IV local projection regressions with Romer and Romer (extended) monetary policy shocks instrumenting 1 year government bond rate. The time span is 1969-2006. The control variables are change in log employees, change in log sales, real asset growth, real investment growth, log real assets, top statutory tax rate and an indicator variable for dividends paid. Standard errors are clustered by firm and year. Large (small) firms refers to firms with employment greater (less) than median company (906 employees).

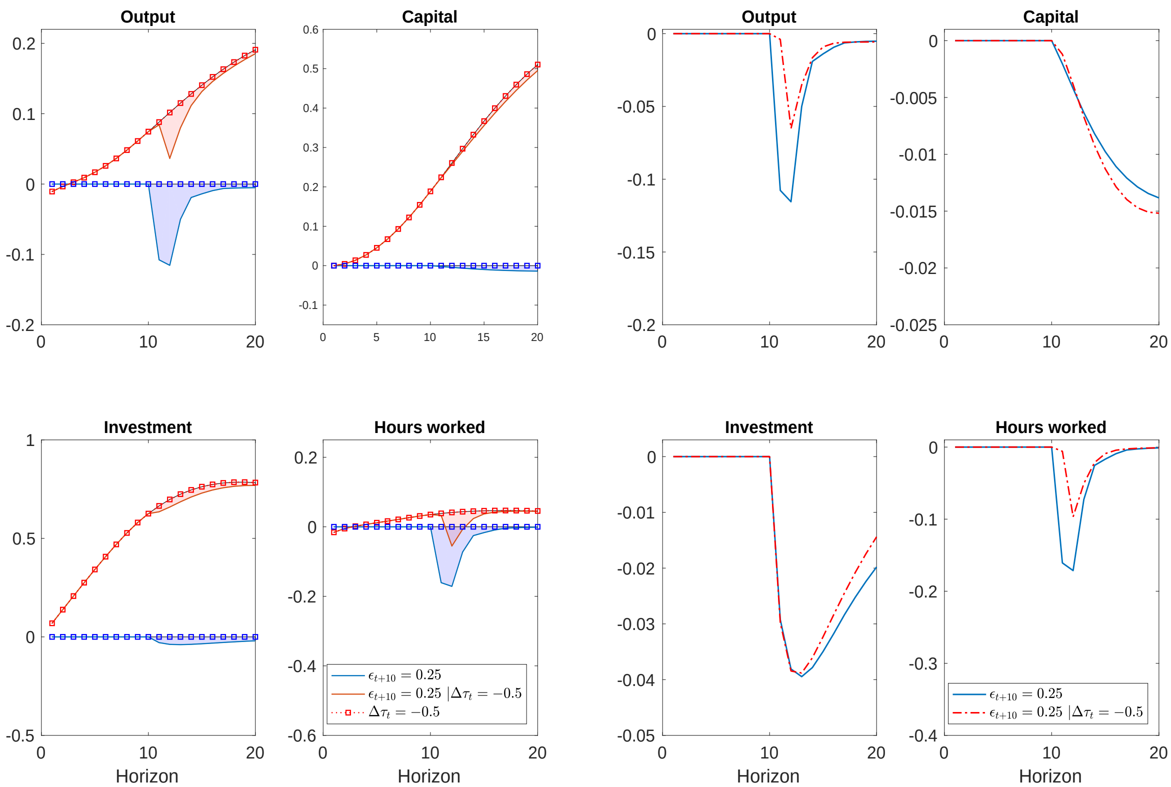
Figure 1.54: Impulse responses to monetary policy shock following contractionary tax shifts.



(a) Total effect (τ+MP) **(b)** Effect of monetary policy

Note: The monetary policy shocks are 25 basis points (0.25 ppt) increase to steady state nominal interest rate and 50 basis points (0.5 ppt) increase to steady state corporate tax rate ($\tau_{ss} = 0.20$).

Figure 1.55: Impulse responses to monetary policy shock following expansionary tax shifts.



(a) Total effect (τ+MP) **(b)** Effect of monetary policy

Note: The monetary policy shocks are 25 basis points (0.25 ppt) increase to steady state nominal interest rate and 50 basis points (0.5 ppt) increase to steady state corporate tax rate ($\tau_{ss} = 0.20$).

1.7 Tables

Table 1.1: List of tax reforms matching statutory rate changes

Exogenous Statutory Tax Rate Changes	Signed	Effective	Type	Persistence	Statutory rate changes	Exogenous	Years
1. Revenue Act of 1978	Nov-78	1979:1	Surprise	Permanent	[0, -2,-3,-8,-18]	Exogenous	1979-1981
2. Economic Recovery Tax Act of 1981	Aug-81	1982:3	Surprise	Permanent	[0,-1]	Exogenous	1982-1983
3. Economic Recovery Tax Act of 1981	Aug-81	1983:1	Anticipated	Permanent	[0,-1]	Exogenous	1983-1984
4. Deficit Reduction Act of 1984	Jul-84	1984:3	Surprise	Permanent	[0,5]	Exogenous	1984-1986
5. Tax Reform Act of 1986	Oct-86	1987:1	Surprise	Permanent	[0,-1.5,-2.5,-3,-3.5,-6,-8.5]	Exogenous	1987-1988
6. Tax Reform Act of 1986	Oct-86	1988:1	Anticipated	Permanent	[0,-1.5,-2.5,-3,-3.5,-6,-8.5]	Exogenous	1988-1992
7. Omnibus Budget Reconciliation Act of 1993	Aug-93	1993:3	Surprise	Permanent	[0,1,4]	Exogenous	1993-2006

Notes: The list covers exogenous corporate tax reforms with statutory rate changes from 1969 to 2006. Following Mertens and Ravn (2012) anticipated tax liability changes with more than 90 days difference between the signing of the legislation and their implementation are classified as anticipated tax reforms. The baseline specification only includes tax changes categorised as exogenous and unanticipated. The anticipated reforms will be used for robustness. Source: Romer and Romer (2009), Mertens and Ravn (2012, 2013), Joint Committee of Taxation and IRS SOI files.

Table 1.2: IRS Corporate Income Tax Brackets (1968-2016)

Years	Taxable Income	Rate
1968-1969	First $25,000	24.2
	Over - $25,000	52.80
1970	First $25,000	22.55
	Over - $25,000	49.20
1971-1974	First $25,000	22
	Over - $25,000	48
1975-1978	First $25,000	20
	$25,000-$50,000	22
	Over - $50,000	48
1979-1981	First $25,000	17
	$25,000-$50,000	20
	$50,000-$75,000	30
	$75,000-$100,000	40
	Over -$100,000	46
1982	First $25,000	16
	$25,000-$50,000	19
	$50,000-$75,000	30
	$75,000-$100,000	40
	Over -$100,000	46
1983	First $25,000	15
	$25,000-$50,000	18
	$50,000-$75,000	30
	$75,000-$100,000	40
	Over -$100,000	46
1984-1986	First $25,000	15
	$25,000-$50,000	18
	$50,000-$75,000	30
	$75,000-$100,000	40
	$100,000-$1,000,000	46
	$1,000,000-$1,405,000	51
	Over $1,405,000	46
1987	First $25,000	15
	$25,000-$50,000	16.5
	$50,000-$75,000	27.5
	$75,000-$100,000	37
	$100,000-$335,000	42.5
	$335,000-$1,000,000	40
	$1,000,000-$1,405,000	42.5
	Over $1,405,000	40
1988-1992	First $50,000	15
	$50,000-$75,000	25
	$75,000-$100,000	34
	$100,000-$335,000	39
	Over $335,000	34
1993-2016	First $50,000	15
	$50,000-$75,000	25
	$75,000-$100,000	34
	$100,000-$335,000	39
	$335,000 - $10,000,000	34
	$10,000,000-$15,000,000	35
	$15,000,000 -$18,333,000	38
	Over $18,333,000	35

Source: IRS historical Table 24.

Table 1.3: Descriptive Statistics

A. Total Sample	Employees	Taxable income	Marginal tax rate	Income taxes	Sales	Assets	Debt to asset
count	187552	205342	205334	204516	204502	205342	204530.0
min	0	-124111	0	-5878	-9	0	0.0
p5	0	-43	0	-1	0	1	0.0
p50	0.7	3	35	1	62	56	24.2
mean	6.8	105	27	30	987	1169	29.1
p95	29.1	429	48	105	3553	4200	74.4
max	1800	79671	53	27902	335086	750507	1140.0
sd	29.4	964	20	240	5775	7853	34.8
B. Neutral Tax Regime							
count	148183	164131	164126	163365	163346	164131	163491.0
min	0	-124111	0	-5878	-9	0	0.0
p5	0	-52	0	-1	0	1	0.0
p50	0.5	1	34	0	46	43	23.4
mean	5.8	86	25	25	850	1024	29.4
p95	24	319	48	77	2840	3315	77.6
max	1800	79671	53	27902	335086	750507	1140.0
sd	27.8	1004	21	241	5657	7983	37.5
C. Contractionary Tax Regime							
count	7907	8251	8251	8241	8242	8251	8205
min	0	-5802	0	-2213	0	0	0
p5	0.1	-28	0	-3	33	32	0
p50	3.5	71	35	17	677	711	26.3
mean	14.8	399	32	98	3104	3736	25.9
p95	62.0	1812	38	425	12636	16520	57.7
max	756.3	25566	51	7824	195805	304012	195.3
sd	40.8	1281	11	323	9527	11492	19.3
D. Expansionary Tax Regime							
count	31462.0	32960	32957	32910	32914	32960	32834.0
min	0	-9559	0	-1713	0	0	0.0
p5	0	-9	0	-1	4	4	0.0
p50	1.4	9	40	2	128	101	27.2
mean	9.2	126	35	37	1141	1249	28.5
p95	42.0	566	46	146	4562	5154	64.7
max	854.0	16471	51	8449	147848	184326	191.2
sd	32.6	580	16	205	4942	5661	20.3

1.8 Appendix

1.8.1 Data Construction

Firm level variables I use annual version of Compustat from 1969 to 2006 in all regressions. Compustat provides high-quality information on balance sheet and income statement components of publicly traded C corporations in North America. Detailed variable definitions of Compustat can be accessed through Wharton Research Data Services for the United States.

Table A1.1 provides the variable names and respective codes in Compustat. Leverage is the ratio of short and long term debt to total assets. Liquidity ratio is the ratio of cash and short-term investments (*che*) to total assets. Tobins'Q is defined as total assets at market value[44] over total assets at book value following Cloyne et al. (2019). Dividend variable is used as an indicator on whether the firm has paid cash dividends in the previous year. *aqc* represents the cash outflow or funds used to acquisition of a company. All variables in level are deflated using the aggregate GVA deflator. I explain the variables used in taxable income definition in the next page in detail.

Table A1.1: Variable Definitions

Variable	Compustat variable
Leverage	$(dlc + dltt) * 100/at$
Liquidity ratio	$che * 100/at$
Tobins' Q	$(at + prcc_f * csho - ceq + txditc)/at$
Employees	emp
Investment	capx
Total Assets (Book value)	at
Sales	sale
Dividend	dv
Acquisitions	aqc/at

I exclude firms within the finance, insurance, real estate (FIRE) and public administration sectors. Following Ottonello and Winberry (2020), I also exclude firms with acquisitions accounting for more than 5% of total assets. I drop firms which are in the panel for less than 5 years. I also drop the first and last year observations of firms as these years lead to natural build-up or depletion of companies assets and may lead to bias.

[44]Note that $prcc_f$ refers to closing price of the fiscal year. *csho* is common shares outstanding[45]. *ceq* is common/ordinary equity and *txditc* is deferred taxes and investment tax credit.

Sample Restrictions The baseline trimming excludes firms with i) top 1 percent of leverage ratio; ii) top and bottom 1% of real sales growth; iii) Tobin's Q ratio greater than 4; iv) acquisitions are more than 5% of total assets. Trimming is done by year. In order to address volatility of taxable income, I drop firms who jump more than one neighboring income bracket after a tax reform. I also drop firms who switch to non-treated income bracket after a tax reform.

Macro Time Series Data The one-year risk free is the 1-Year Treasury Contrant Maturity Rate from FRED series GS1. The GVA (gross value added) deflator series is Price Index (Business : Nonfarm) from FRED (data series is B358RG3Q086SBEA). Top statutory rate is from IRS historical Table 24.

Monetary Policy Shocks I use Romer and Romer (2004) monetary policy shocks that are extended by Wieland and Yang (2020) (Available here.)

Alternative definitions of taxable income

$$TI = \text{Net Income} - \text{Interest Paid} - \frac{\sum_{n=t-3}^{n=t-1} \text{Tax Loss Carryforward}_n}{3} - \text{Depreciation and Depletion Expense} + \text{Special items} + \frac{\text{Income from extraordinary items}}{(1 - mtr)} \quad \text{(A1.1)}$$

$$TI^{Graham} = \text{Net Income} - \frac{\text{Deferred tax expense}^{\text{CF}}}{mtr} + \text{Taxes paid} + \frac{\text{Minority interest}}{(1-mtr)} + \frac{\text{Income from extraordinary items}}{(1 - mtr)} \quad \text{(A1.2)}$$

$$TI^{Blouin} = \text{EBIT} + \text{Interest on leases} - \frac{\text{Deferred tax expense}^{\text{IS}}}{mtr} + \text{Special items} + \frac{\text{Income from extraordinary items}}{(1 - mtr)} \quad \text{(A1.3)}$$

TI is the main definition employed in this paper that is constructed using the 1984 IRS corporate tax filings instructions and the two main definitions in the literature from Blouin et al. (2010) and Graham (1996).[46]

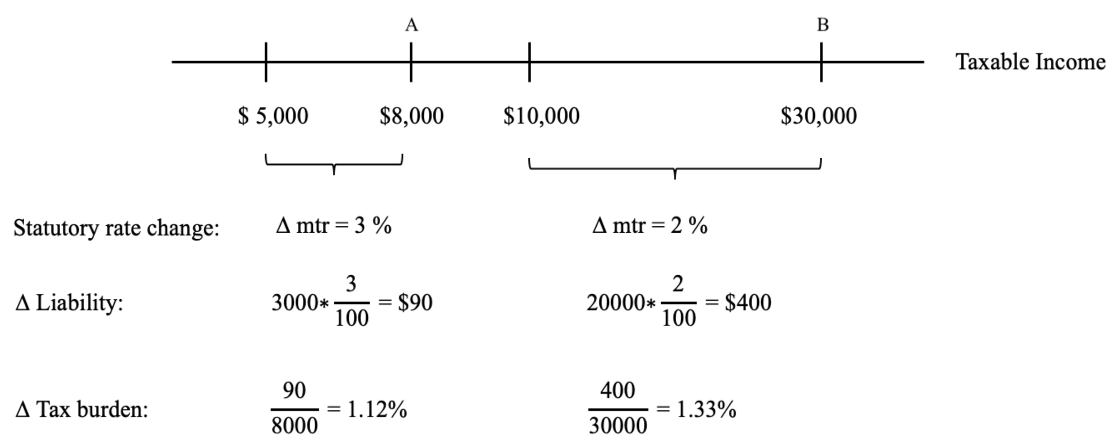
Net income is sum of operating (*ebit*) and nonoperating (*nopi*) income. Compustat *ebit* is sum of Sales (Net) minus Cost of Goods Sold (COGS) minus Selling, General and Administrative Expense (XSGA) minus Depreciation/Amortization (DP). It is also referred as Operating Income After Depreciation (OIADPQ). Income from extraordinary items is *xido*, this item is deflated by one minus the top statutory rate following Blouin et al. (2010) and Graham (1996). Special items is *spi*, depreciation expense is *xdp*, depletion expense *xdelp*, interest paid (net) is *intpn*. According to the IRS instructions, firms are allowed to carry tax losses up to three years. Hence, carryforwards are calculated as the average of last three years tax loss carry forward ($tlcf$). The capital gains are also part of the definition of taxable income, however since Compustat total capital gains (*cgti*) variable is genuinely missing, it is not part of the definition. Deferred tax liability is a tax that is assessed or is due for the current period but has not yet been paid, so I do not exclude it from the taxable income definition.

[46] See the 1984 IRS corporate income tax return form 1120-A.

Note that the differences in the taxable income definitions also reflects the relative weight of a static versus dynamic definition of taxable income. Blouin et al. (2010) and Graham (1996) both focus on dynamic properties of taxable income in order to forecast taxable income over long periods of time and to proxy for the marginal tax rates. The definition also accounts for firms' incentives to allocate income across time through carryforwards and allows for forward-looking behavior. However, the goal is to generate a taxable income definition closest to the actual reports, to the extent of data availability.

Calculating dose of tax treatment Figure A1.1 presents an example calculation of average tax burden. Suppose at year t, a new tax legislation has decreased the statutory tax rates by 3 percentage points for the ($5,000, $10,000) taxable income bracket and by 2 percentage points for the ($10,000, $30,000) taxable income bracket. Firm A has $8,000 taxable income and firm B has $30,000 taxable income. Using the closest taxable income thresholds at $5,000 and $10,000 of taxable income, respectively, we can calculate the changes in liability for firm A and B can be calculated as $90 and $400, respectively.[47] Next, in order to facilitate comparison across firms and time, I scale the liability changes with the lagged taxable income which leads 1.12 and 1.33 percent change in average tax burden of the firm A and B, respectively.

Figure A1.1: Sketch of change in tax burden calculation.

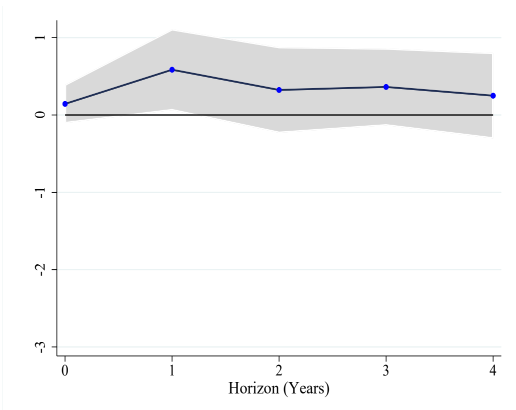


Note: $5,000 and $10,000 correspond to taxable income thresholds. $8,000 is the actual taxable income of firm A and $30,000 is the actual taxable income of firm B. Statutory rate change reflects the changes in statutory tax rate in the relevant taxable income bracket. Δ Liability shows the change in tax liability in the treated taxable income bracket. Δ Tax burden calculates the changes in share of tax burden within the treated taxable income bracket.

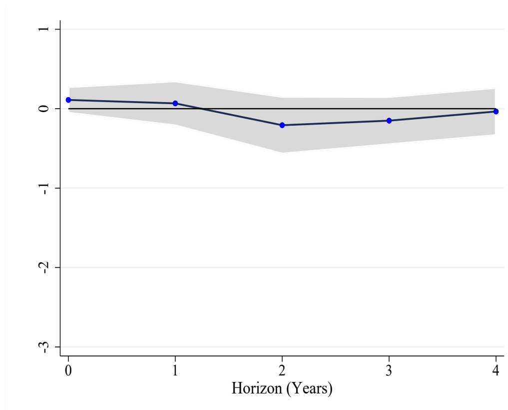
[47] The liability change of firm A is $(8000 - 5000) * 3/100 = 90$ and the liability change of firm B is $(30000 - 10000) * 2/100 = 400$

1.8.2 Additional Figures

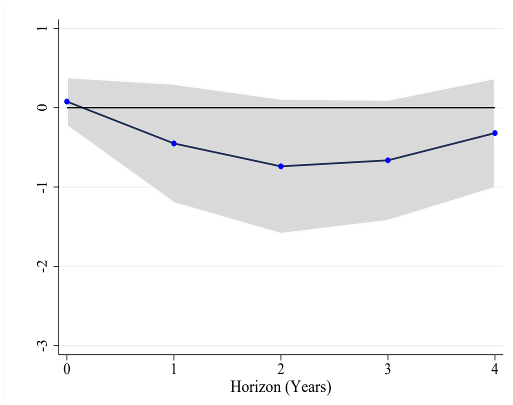
Figure A1.2: Impulse responses of number of employees to monetary shocks (by dose).



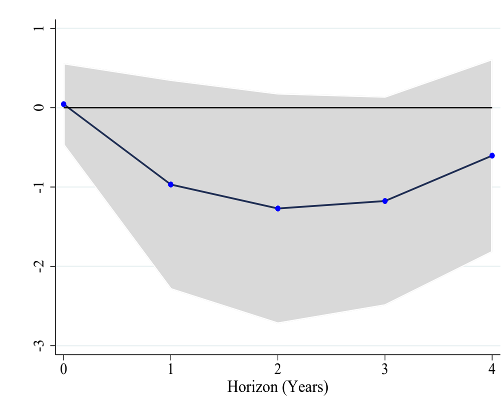
(a) Dose -4%



(b) Dose -2%



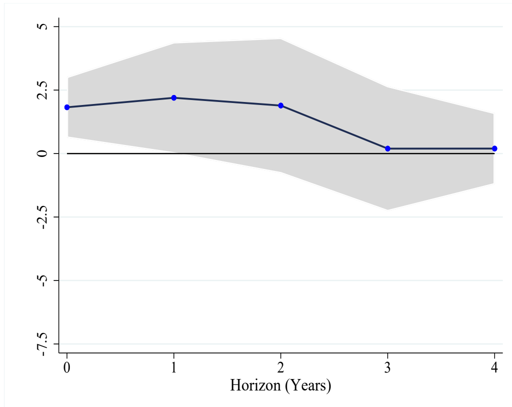
(c) Dose 0%



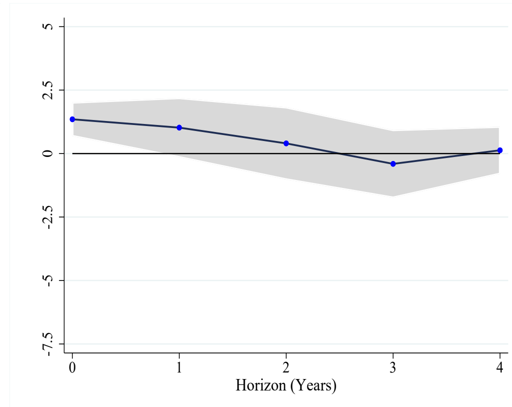
(d) Dose 2%

Note: The plots show impulse responses of employees using IV-LP, where the standard errors are plotted by dose. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. Standard errors are clustered by firm and year. $Dose = 0\%$ is equal to neutral regime plot. Shaded areas show 95% confidence intervals.

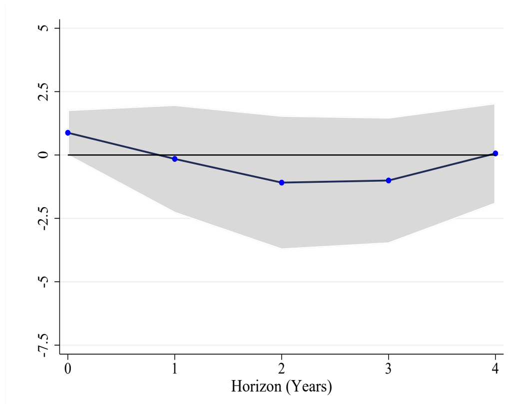
Figure A1.3: Impulse responses of number of investment to monetary shocks (by dose).



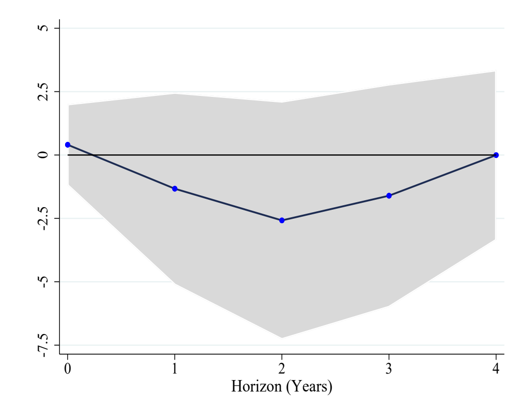
(a) Dose -4%



(b) Dose -2%



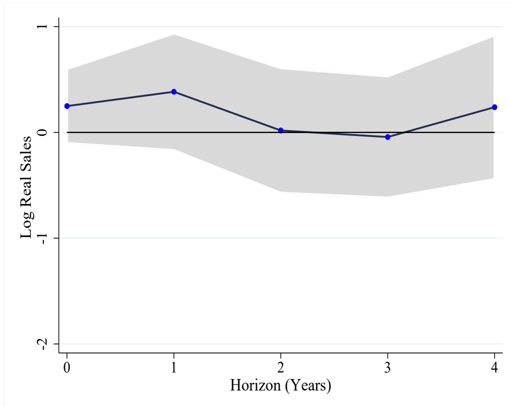
(c) Dose 0%



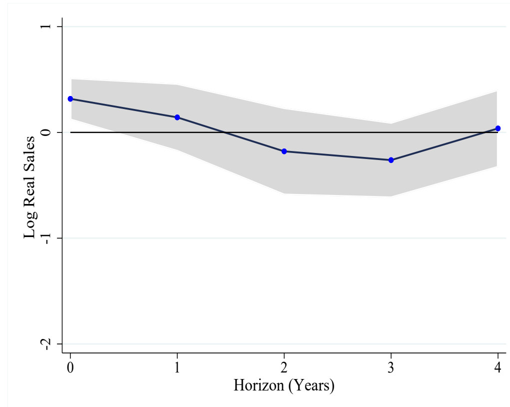
(d) Dose 2%

Note: The plots show impulse responses of investment using IV-LP, where the standard errors are plotted by dose. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. Standard errors are clustered by firm and year. $Dose = 0\%$ is equal to neutral regime plot. Shaded areas show 95% confidence intervals.

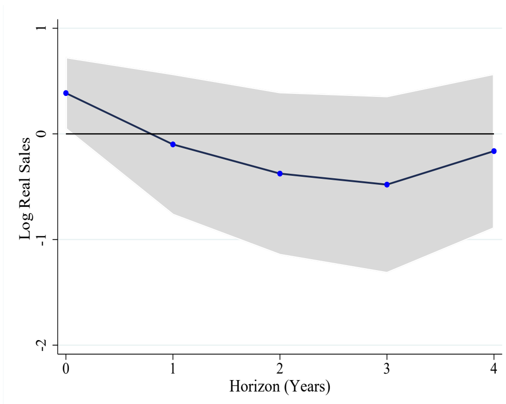
Figure A1.4: Impulse responses of number of sales to monetary shocks (by dose).



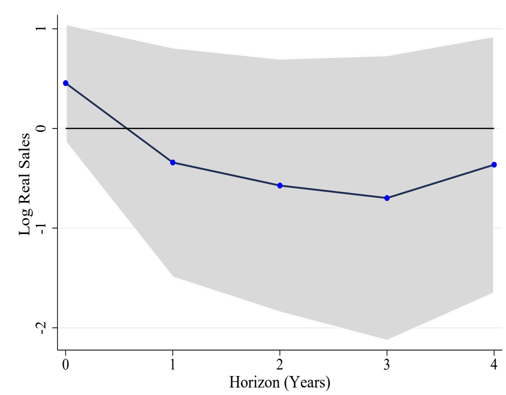
(a) Dose -4%



(b) Dose -2%



(c) Dose 0%



(d) Dose 2%

Note: The plots show impulse responses of sales using IV-LP, where the standard errors are plotted by dose. Horizon is 4 years, lag is set to 2. The time span is 1969-2006. Standard errors are clustered by firm and year. $Dose = 0\%$ is equal to neutral regime plot. Shaded areas show 95% confidence intervals.

Granger causality tests

This part provides a series of Granger causality tests confirming the Romer monetary shock series are uncorrelated to the exogenous tax reforms. Specifically, I regress annual monetary innovations on a set of lagged tax reform dates and aggregate variables including changes in real government spending, changes in total employees and changes in total public real debt. Lag length is 3 years.

$$\text{Romer shocks} = c + \sum_{i=1}^{L} \beta_i x_{t-i} + v_i \tag{A1.4}$$

The null hypothesis is that Romer shocks are not predictable from the exogenous tax reforms. Table A1.2 reports F statistics and p-values for the null hypothesis based on equation A1.4. All p-values are above 10 percent and mostly above 40 percent, hence we can not statistically reject the hypothesis of exogeneity of the monetary shocks to tax reforms. This suggests that studying tax changes around monetary policy innovations is suitable.

Table A1.2: Granger tests on monetary policy shocks

	(1)		(2)		(3)	
L.mtr_exo	0.842	(0.118)	0.859	(0.101)	1.365	(0.141)
L2.mtr_exo	-0.134	(0.793)	-0.139	(0.779)	1.144	(0.158)
L3.mtr_exo	-0.0297	(0.949)	-0.236	(0.619)	0.540	(0.444)
mtr_exo			0.836	(0.144)		
L.mtr_endo					-0.782	(0.262)
L2.mtr_endo					-1.236	(0.072)
L3.mtr_endo					-0.458	(0.518)
Cons	-0.783	(0.325)	-1.026	(0.197)	-0.553	(0.473)
F	1.12		1.27		1.28	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: All regressions spans from 1969 to 2006 and p-values are provided in parentheses. Specification (1) has exogenous tax reforms, changes in real GDP, changes in real government spending, changes in total employees and changes in total public real debt as left hand side variables. Specification (2) adds contemporaneous exogenous tax reforms to specification (1). Specification (3) has endogenous and exogenous tax reforms, changes in real GDP, changes in real government spending, changes in total employees and changes in total public real debt as left hand side variables.

In addition, I provide Granger causality tests confirming the exogenous tax reforms are uncorrelated to the monetary policy shocks. Specifically, I regress the tax reform dates on lagged annual monetary innovations and a set of aggregate variables including changes in real government spending and changes in real GDP.

$$\text{Tax Reforms} = c + \sum_{i=1}^{L} \beta_i x_{t-i} + v_i$$

Table A1.3: Granger tests on tax reforms

	(1)		(2)		(3)	
L.resid_full	-0.107	(0.188)	-0.0367	(0.624)	-0.00853	(0.924)
L2.resid_full	0.118	(0.144)	0.0882	(0.274)	0.0760	(0.403)
L3.resid_full	-0.0618	(0.383)	-0.0682	(0.314)	-0.0739	(0.337)
Cons	0.146*	(0.018)	0.237	(0.076)	0.276	(0.094)
F	0.966		2.771		1.765	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: All regressions spans from 1969 to 2006 and p-values are provided in parentheses. Specification (1) regresses exogenous tax reforms on lags of monetary shocks. Specification (2) regresses exogenous tax reforms on lags of monetary shocks and changes in real GDP. Specification (3) regresses exogenous tax reforms on lags of monetary shocks, changes in real GDP and changes in real government spending.

1.8.3 Model Appendix

Household's problem:

$$\mathcal{L} = E_t \sum_{t=0}^{\infty} \beta^t \Bigg\{ lnC_t - \theta \frac{N_t^{1+\chi}}{1+\chi} + \lambda_t \Bigg(\frac{W_t N_t}{P_t} + \frac{\Pi_t}{P_t} + \frac{T_t}{P_t} + \frac{B_{t-1}}{P_t} - \frac{P_t C_t}{P_t} - \frac{Q_t B_t}{P_t} \Bigg) \Bigg\}$$

where $P_t \equiv \left(\int_0^1 P_t(i)^{1-\epsilon} di \right)^{\frac{1}{1-\epsilon}}$ is the aggregate price index and C_t is the final goods consumption:

$$C_t(i) = \left(\frac{P_t(i)}{P_t} \right)^{-\epsilon} C_t \text{ for all } i \in [0,1]$$

Hence, consumption expenditures can be expressed as the product of the price index times the quantity index as follows:

$$P_t C_t = \int_0^1 P_t(i) C_t(i) di$$

Note that in addition to the flow budget constraint, the household is also subject to a solvency constraint that prevents it from engaging in Ponzi-type schemes.

$$\lim_{T \to \infty} E_t \{ \Lambda_{t,T} \frac{B_T}{P_T} \} \geq 0 \text{ for all } t, \text{ where } \Lambda_{t,T} \equiv \beta^{T-t} U_{c,T} / U_{c,t}$$

The first order conditions:

$$C_t : \quad \frac{1}{C_t} = \lambda_t \tag{A1.5}$$

$$N_t : \quad \theta N_t^{\chi} = \lambda_t \frac{W_t}{P_t} \tag{A1.6}$$

$$B_t : \quad \frac{\lambda_t Q_t}{P_t} = \beta E_t \left(\frac{\lambda_{t+1}}{P_{t+1}} \right) \tag{A1.7}$$

bring the optimality conditions on consumption-savings and labor supply decision:

$$\theta N_t^{\chi} C_t = \frac{W_t}{P_t} = w_t \tag{A1.8}$$

$$Q_t = \beta E_t \frac{C_t}{C_{t+1}} \frac{1}{1+\pi_{t+1}} \qquad \text{for } t = 0, 1, 2, ... \tag{A1.9}$$

where $\pi_{t+1} = \frac{P_{t+1}}{P_t} - 1$ is the net inflation rate and $w_t = \frac{W_t}{P_t}$ is real wages. Equation A1.8 can be interpreted as the competitive labor supply condition, determining the quantity of labor supplied as a function of the real wage, given the marginal utility of consumption (which is a function of consumption only). Workers do not have any market power, hence they take the wage as given.[48]

Firms' problem:

$$\mathcal{L} = E_t \sum_{j=0}^{\infty} \beta^j \frac{\lambda_{t+j}}{\lambda_t} \Bigg\{ \frac{1}{P_{t+j}} \bigg(P_{t+j}(i) Y_{t+j}(i) - W_{t+j} N_{t+j}(i) \big) (1-\tau_t) - \frac{\psi}{2} \big(\frac{P_{t+j}(i)}{P_{t+j-1}(i)} - 1 \big)^2 P_{t+j} Y_{t+j} \bigg) - I_{t+j}(i)$$
$$- \frac{MC^n_{t+j}(i)}{P_{t+j}} \bigg[\bigg(\frac{P_t(i)}{P_t} \bigg)^{-\epsilon} Y_t - A_t K_t(i)^{\alpha} N_t(i)^{1-\alpha} \big] + q_{t+j} \bigg[I_{t+j}(i) - \frac{\phi}{2} \big(\frac{I_t(i)}{I_{t-1}(i)} - 1 \big)^2 + (1-\delta) K_{t+j}(i) - K_{t+j+1}(i) \bigg] \Bigg\}$$

subject to demand function that follows the Dixit Stiglitz model of imperfect competition,

$$Y_t(i) = \left(\frac{P_t(i)}{P_t} \right)^{-\epsilon} Y_t = \left(\frac{P_t}{P_t(i)} \right)^{\epsilon} Y_t = \tilde{P}_t(i)^{\epsilon} \, Y_t$$

where $Y_t(i) = A_t K_t(i)^{\alpha} N_t(i)^{1-\alpha}$ and $\frac{P_t}{P_t(i)} = \tilde{P}_t(i)$.

[48]Last, equation A1.9 can also be used to determine the implied real interest rate in linear form as:

$$r_t \equiv i_t - E_t\{\pi_{t+1}\}$$

Equilibrium conditions:

$$\theta N_t^{\chi} C_t = w_t \tag{A1.10}$$

$$\frac{1}{C_t} Q_t E_t(1+\pi_{t+1}) = \beta E_t \frac{1}{C_{t+1}} \tag{A1.11}$$

$$MC_t\ (1-\alpha) A_t K_t^{\alpha} N_t^{-\alpha} = w_t(1-\tau_t) \tag{A1.12}$$

$$1 = q_t\big[1 - \phi(\frac{I_t(i)}{I_{t-1}(i)} - 1\big)\frac{1}{I_{t-1}} + \beta\frac{\lambda_{t+1}}{\lambda_t} q_{t+1}\left[\phi\ \frac{I_{t+1}}{I_t^2}(\frac{I_{t+1}}{I_t} - 1\big)\right] \tag{A1.13}$$

$$q_t = \beta E_t \frac{\lambda_{t+1}}{\lambda_t}\left\{ q_{t+1}(1-\delta) + MC_{t+1} A_{t+1} \alpha K_{t+1}^{\alpha-1} N_{t+1}^{1-\alpha} \right\} \tag{A1.14}$$

Phillips curve: $$(1-\epsilon)\,(1-\tau_t) - \psi\pi_t(\pi_t+1) + \beta E_t \frac{C_t}{C_{t+1}}\big[\psi\pi_{t+1}\,(\pi_{t+1}+1)\,\frac{Y_{t+1}}{Y_t}\big] = -\epsilon\, MC_t(i) \tag{A1.15}$$

Central Bank's interest rate rule: $$i_t = 0.7 i_{t-1} + 0.3 * \phi_{\pi}\pi_t + s_i \epsilon_{i,t} \tag{A1.16}$$

$$Y_t = A_t K_t^{\alpha} N_t^{1-\alpha} \tag{A1.17}$$

$$Y_t = C_t + I_t + \frac{\psi}{2}(\pi_t)^2 Y_t \tag{A1.18}$$

$$K_{t+1}(i) = I_t(i) - \frac{\phi}{2}\left(\frac{I_t(i)}{I_{t-1}(i)} - 1\right)^2 + (1-\delta)K_t(i) \tag{A1.19}$$

$$\tau_t = (1-\rho_{\tau})\tau_{ss} + (\rho_{\tau})\tau_{t-1} + s_{\tau}\epsilon_{\tau,t} \tag{A1.20}$$

$$Q_t = \frac{1}{1+i_t} \tag{A1.21}$$

Calibration Details

Table A1.4: Quarterly Parametrization

Parameter	Description	Value
β	Discount factor	0.96
δ	Capital depreciation rate	0.06
ϵ	Elasticity of substitution between goods	6
θ	Labor disutility parameter	1
α	Capital income share	0.33
ϕ	Capital adjustment parameter	4
χ	Inverse labor elasticity	2
ρ	Persistence of interest rate	0.7
ρ_τ	Persistence of tax shock	0.995
ϕ_π	Coefficient of inflation target in Taylor Rule	1.5
ψ	Rotemberg price adjustment cost parameter	107

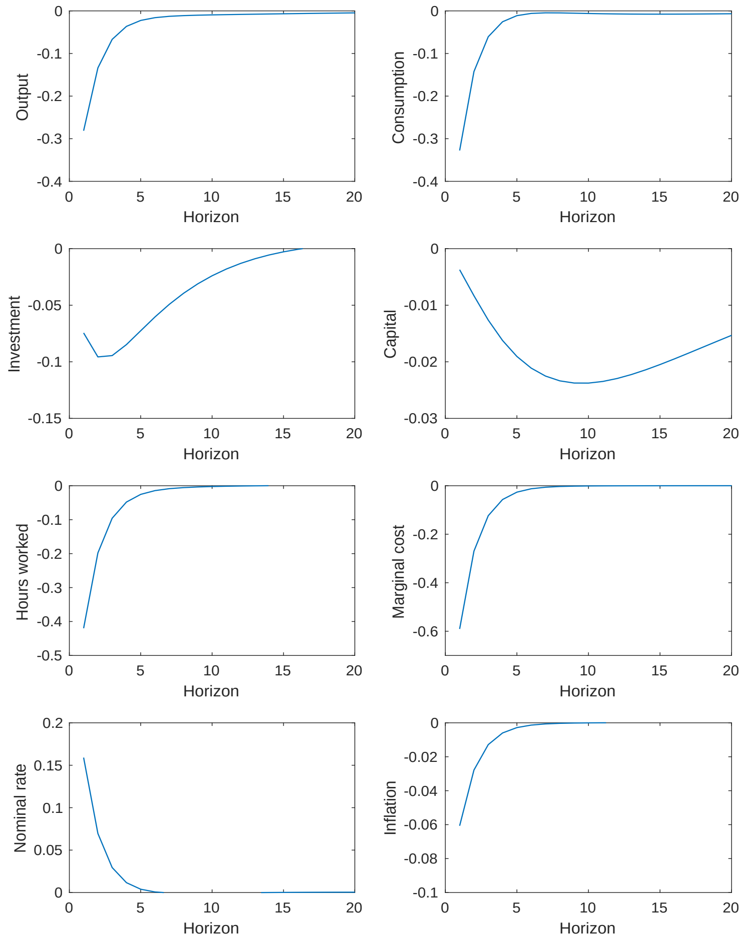
Note: The calibration parameters closely follow Galí (2009), Burnside et al. (2004) and Miao and Ngo (2019).

Simulation of monetary policy shocks

The figure A1.5 shows a basic contractionary monetary policy exercise in my model using quarterly calibrated parameters in Table A1.4. Figure A1.5 plots the impulse responses of key aggregate variables to a 25 basis points contractionary monetary policy shock. All variables are expressed in percent deviation from their steady state level. Following a 25 basis points positive shock to interest rate, nominal interest rate increases by 0.15 points. Since the prices are sticky, this also increases the real interest rate. Output falls by approximately 0.3 percent on impact and slowly transitions to steady state six periods after the shock. Through the Euler equation, as interest rate increases households reoptimize the consumption path, lowering consumption contemporaneously. Consumption falls about 0.34 percent on impact. By standard notion of arbitrage, a higher real interest rate increases the required return on capital which makes firms invest less. Both investment and capital show a hump-shaped response where the peak effect for investment occurs three periods after the shock with a -0.1 percent fall in steady state investment. Last, lower aggregate demand lowers hours worked.[49]

[49]In a model with intertemporal debt, we can also argue through the cost channel of monetary policy that the higher the interest rate, the more expensive it is to borrow.

Figure A1.5: Impulse response to 25 bps shocks to the nominal interest rate.



Comparison of capital adjustment costs

Figure A1.6: Impulse response to monetary policy with different capital adjustment costs ($\phi = 4$)

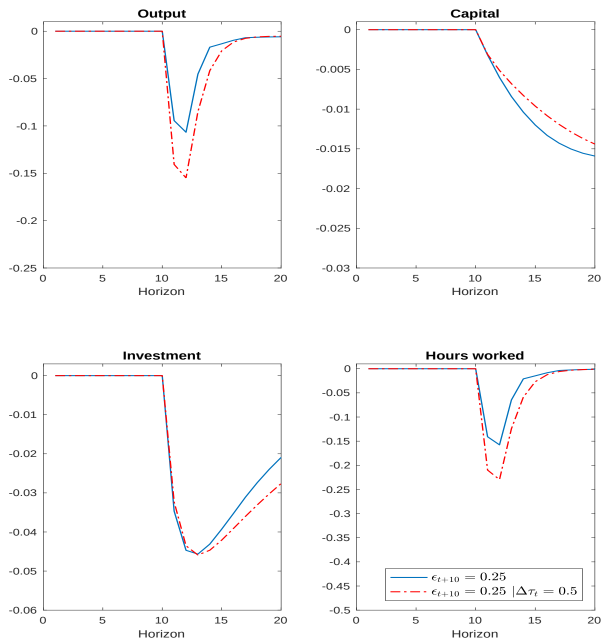
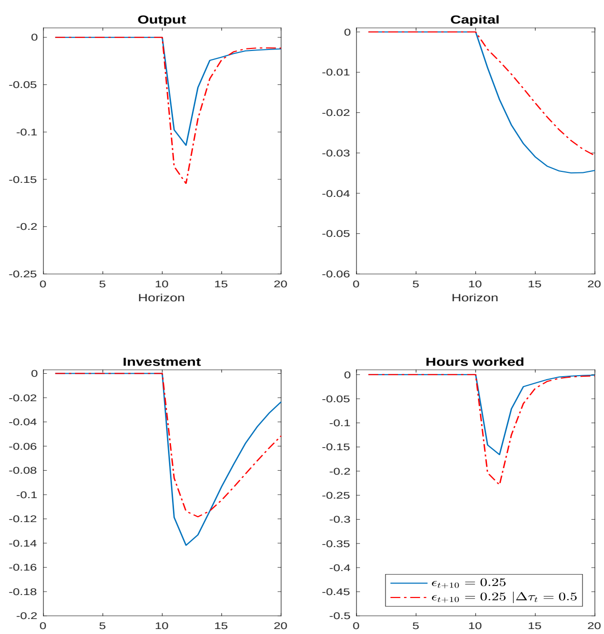


Figure A1.7: Impulse response to monetary policy with different capital adjustment costs ($\phi = 1$)

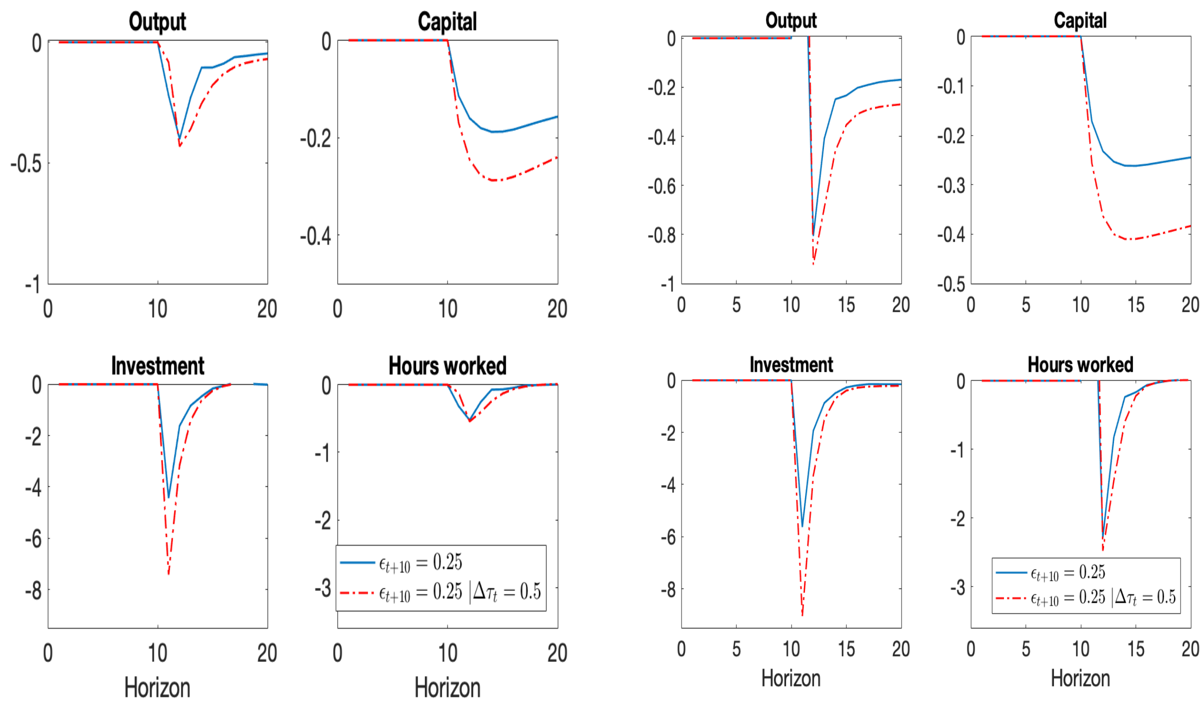


Note: The capital adjustment costs ($\phi = 4$) is the baseline version.

Role of share of capital

Figure A1.8: Impulse responses to monetary policy following contractionary tax shifts.

(a) $\alpha = 0.30$ **(b)** $\alpha = 0.70$



Note: The chart on the left uses 30 percent capital share, $\alpha = 0.30$. The chart on the right uses 70 percent as the capital share, $\alpha = 0.70$.

Chapter 2

Dynamic Effects of US Corporate Tax Policy: Firm-Level Evidence[1]

2.1 Introduction

There is a large literature and an ongoing debate on how tax policy affects investment. On the one hand, there are applied macroeconomic studies that use time-series identification methods to study the *aggregate* effects of postwar US tax changes.[2] These studies, by construction, provide a dynamic general equilibrium analysis of tax policy, yet have challenges in addressing issues such as targeted interventions, heterogeneous effects as well as the underlying transmission mechanisms. On the other hand, there is the applied-micro tradition that studies taxes through exploring *cross-sectional* variation within tax rules, firms, or locations.[3] However, despite the credible identification strategies provided by this literature, their conclusions are limited to case-specific, partial equilibrium analyses, hence cannot be generalized to the entire economy. Furthermore, these analyses may overlook general equilibrium effects that may be critical for policy implications.

What are the effects of different corporate tax policy changes? Which tax tool is better in stimulating investment? What type of firms benefits most from these tax tools? To address these questions

[1] This part of my thesis is part of joint work with James Cloyne and Paolo Surico. Suggested citation: "Cloyne et. al (2021). Dynamic Effects of US Corporate Tax Policy: Firm-Level Evidence." Unpublished Manuscript.

[2] See for example Romer and Romer (2010); Mertens and Ravn (2013); Barro and Redlick (2011); Blanchard and Perotti (2002); Favero and Giavazzi (2012), among others.

[3] See Zwick and Mahon (2017); Cummins et al. (1994); Goolsbee (1997); House and Shapiro (2008), among others.

in a stylized way, we need variation in corporate taxes at *both* time-series and cross-sectional level over time. Our paper achieves this by proposing a new approach that unites the strengths of these two strands of literature. First, we utilize the aggregate variation in US corporate tax reforms from 1950 to 2006. To do this, we employ the earlier time-series methods that allow us to capture dynamic causal effects of corporate tax changes. Second, we zoom in on two specific tax policy tools, marginal tax rates and the investment tax credit, and generate a micro-level measure of these tax tools since 1950. This step sets us apart from earlier approaches and allows us to attain micro-level variation in tax treatment over time. Hence, we build two novel measures of micro-level tax treatment with aggregate variation that allows us to provide new stylized insights on the comparative effectiveness of two main corporate tax tools in the US.

In addition to comparing the effectiveness of different tax tools, this paper aims to explore whether alternative tax tools affect firms differently. Assessing the role of heterogeneity in tax policy can provide insights on two main dimensions. First of all, exploring heterogeneity would allow us to quantify the responses of alternative firm types, and assess whether a certain type of firms drive the aggregate responses to tax policy. Second, exploring heterogeneity would contribute to the policy discussion on the effectiveness of targeted policy interventions across the firm distribution. To assess this, we bring together firm-level data and our two new measures of changes in marginal tax rates and investment tax credits. We use the annual Compustat dataset for the firm-level data. Compustat provides rich income statement and balance sheet information on 17,456 publicly traded US firms from 1950 to 2006. We focus on Compustat annual data as it offers the longest available firm-level panel on US firms. We supplement this dataset with two new measures capturing plausibly exogenous statutory rate and investment tax credit changes in the US. For the statutory rate change, we construct a dataset by tracking the marginal tax rate changes on different income brackets over time. For investment tax credit, we use the change in the rate of investment tax credit granted by the US government over time. In order to pin down the *plausibly exogenous* variation, we employ the dates of Romer and Romer (2010, 2009); Mertens and Ravn (2013) tax narrative accounts which provide historical records to isolate exogenous discretionary tax reforms in the US. Combining the narrative tax reforms with statutory rate changes allows us to explore both time-series and cross-sectional variation in statutory taxes across income brackets over time.

Lastly, the paper uses local projections (LP) (Jordà, 2005) that allow us to test the effectiveness of different tax policy tools conditional on firm characteristics using a variety of specifications.

Our findings can be summarized as follows. First, we show that both marginal tax rates and investment tax credits are genuinely effective tools to stimulate investment across the firm distribution. Yet, on average marginal tax rate changes are substantially more effective at raising firms' investment rates than investment tax credits (ITC). In particular, we find a one percentage point increase in the marginal tax rate decreases firm-level investment rates by 1.3 percentage points. In contrast, a one percentage point increase in ITC raises the investment rate by 0.7 percentage points. Second, we explore the role of firm characteristics in shaping the firms' responses to alternative tax policy changes. Our results provide evidence that small, low-leverage, and high-growth firms are substantially more responsive to corporate tax policy changes than firms with alternative characteristics. These findings highlight the role of firm heterogeneity in studying US corporate tax policy and provide historical insight on the relative scope of alternative corporate tax policy tools.

Related Literature This paper fits into the broad literature that studies the role of tax policy and corporate investment. Earlier debates on the implications of tax policy on corporations start with the seminal work of Modigliani and Miller (1958, 1963), which suggests taxes distort capital allocation decisions of firms. Subsequently, an extended literature covered the effects of business tax reforms on firm investment, exploiting cross-sectional variation (Hall and Jorgenson, 1967; Auerbach and Hassett, 1992; Cummins et al., 1994; Hassett and Glenn Hubbard, 2002; Goolsbee, 1997; House and Shapiro, 2008).[4] There is also a large literature of investment models using Q theory or user cost models that explain the role of tax policy on investment (Hassett and Hubbard, 1996; Tobin, 1969; Hayashi, 1982; Abel, 1980). Many of these models deliver a straightforward negative link between investment and tax policy. However, despite the agreement that an increase in taxes would reduce investment, the empirical literature is far from reaching a consensus and fails to provide robust stylized facts on the effects of different policy interventions (Caldara and Kamps, 2008).

[4]The Q theory of investment suggests the incentive to add new fixed capital depends on the market value of capital relative to its replacement cost. The user cost approach suggests a similar mechanism using the costs of adjusting the capital stock.

This paper contributes to the existing fiscal policy literature along several dimensions. First of all, our paper studies dynamic causal effects of alternative corporate tax instruments in the US. Earlier macroeconomic literature on tax policy - using SVAR, narrative accounts, or hybrid methodologies - has documented negative effects of *aggregate* tax increases on output and private investment. Blanchard and Perotti (2002) reports a multiplier of -0.3 (qtr 8) on investment and -1.33 on output; Mountford and Uhlig (2009) reports a 1.6 percent increase in nonresidential investment following a deficit-financed tax cut scenario; and Romer and Romer (2010) shows that an exogenous tax increase of one percent of GDP lowers real GDP by 3 percent. Using marginal tax rate data, Barro and Redlick (2011) also reports a multiplier of -0.3 for gross private domestic investment.[5] A common feature of this literature is to study tax policy as an aggregate object. In contrast, our approach allows us to assess the *individual, disaggregate* effects of marginal tax rates and investment tax credits, separately, while allowing general equilibrium effects to operate. This also allows us to contribute to the policy discussion by bringing evidence on targeted policy tools.

This paper also contributes to the tax narrative accounts literature by incorporating the narratives into new micro-level tax measures. Building on Romer and Romer (2010) tax narrative accounts, Mertens and Ravn (2012, 2013) have categorized US tax reforms into anticipated and unanticipated tax shocks, as well as to personal and corporate income tax rate categories. By providing detailed information on the legislated federal corporate income tax liability changes, Mertens and Ravn (2013) (henceforth MR) finds that a one percentage-point cut in average corporate income tax rate leads to a 2 percent peak effect in non-residential investment and 0.6 percent peak effect in output. Using narrative measures of marginal tax changes, Eskandari and Zamanian (2019) analyzes the impact of marginal tax cuts on manufacturing firms and finds large manufacturing firms to respond more to changes in the marginal tax rate. In this paper, we deviate from the liability-based measures since these cannot address different compositions of each tax reform.[6] Instead, we build two new measures exploring micro-level variation in marginal tax rates and the investment tax credit over time. We, then, supplement these new measures with the narratively-identified dates to pin down

[5]Overall, the literature estimates the tax multipliers in the US to be in between -0.3 to -3 (Ramey, 2016). Our paper, working with two new targeted tax measures, also approximates the range documented in the aggregate macro literature.

[6]For instance, the MR series with two different tax shocks generating the same level of change in revenue are reported as shocks of the same magnitudes. However, each of these tax changes may be composed of different tax components, bringing an underlying heterogeneity in the tax treatment.

the dynamic causal effects over time.[7]

Our paper also contributes to the literature by pinning down the role of firm heterogeneity in shaping differential responses to statutory rates and investment tax credits in panel data setup. There are extensive case studies that study specific tax stimulus programs using micro-data. Covering 1996-2004, Zwick and Mahon (2017) analyzes four different treatment years of temporary tax incentives on equipment investment. They highlight the role of financial frictions and show that small firms are substantially more responsive to the direct investment stimulus due to their liquidity-constrained, cash-poor nature. Yagan (2015) studies the 2003 dividend tax cut and finds that the tax cut caused zero change in corporate investment and employee compensation across all firm size distribution[8]. Rao (2016) studies R&D tax credit between 1981 and 1991 and show that a 10% reduction in the user cost of R&D leads average firm to increase its research intensity by about 20% percent in the short run. Desai and Goolsbee (2004) questions the capital overhang or excess investment hypothesis of the 1990s and 2000s and suggests that the tax policy may generically be impotent in stimulating investment. Most of these papers employ micro-data that allows them to explore heterogeneous effects of various tax reforms. However, despite the valuable insights of these detailed studies, their results can not be generalized due to their case-specific, partial equilibrium nature. This paper employs 56 years of micro-data and time-series/cross-sectionally identified tax measures that grant us more stylized, non-case-dependent findings on the US tax policy.

The rest of the paper is organized as follows. Section 2.2 describes the firm-level dataset, discusses the nature and history of corporate tax code, and presents our statutory tax and investment tax credit measures. Section 2.3 presents the empirical model, examines the results, and investigates the robustness of the results. Section 2.4 concludes.

[7]Specifically, marginal tax rates incorporate cross-sectional and time series variation, whereas investment tax credit (ITC) incorporates only time-series variation.

[8]A similar type of observation on the case of ineffective stimulus programs are shown on the household side by Mian and Sufi (2012) which suggests the effects of temporary stimulus programs (mainly on durable goods) are surprisingly ineffective and start to reverse its initial positive effects much sooner than expected.

2.2 Dataset

This section describes the datasets used in this paper. First, we provide details on the Compustat firm-level database, discuss the construction of the main variables, and present descriptive statistics. Next, we introduce the two new measures on the marginal tax rate and investment tax credits and discuss their identification properties. A detailed description of sources, definition, and sample selection are provided in Appendix 2.7.1.

2.2.1 Firm Level Variables

Compustat data This paper uses the annual Compustat database on the publicly traded C corporations in North America. Compustat provides high-quality information on the balance sheet and income statement components of active and inactive companies. The sample spans from 1950 to 2006 and consists of 220,609 firm-by-year observations from a total of 17,456 firms. Our main variable of interest is investment ratio, $\frac{i_{j,t}}{k_{j,t-1}}$, defined as capital expenditures of firm j in period t (*capx*) relative to the level of physical capital stock in the last period.[9] This measure allows us to focus on investment decisions controlling for the existing level of capital stock. Other variables we use throughout the analysis are book value of total assets (*at*), long term debt (*dltt*), liquidity ratio[10], leverage (total debt divided by the book value of total assets), number of employees (*emp*) and cash dividends paid (*dv*).[11]

Using Compustat data in this paper is advantageous for a couple of reasons. First, Compustat is a long enough panel to study within-firm variation. Second, Compustat has a rich cross-sectional dimension. The use of detailed balance sheet data allows us to construct a taxable income measure that determines the statutory rate treatment of firms. It also enhances the conduct of rich heterogeneity analysis across firms over time.

There are a few limitations of using Compustat data. First, Compustat only consists of publicly held companies; hence the estimates only represent the effects of the corporate tax code on the behavior

[9] See studies such as Chaney et al. (2012) and Cloyne et al. (2019) that use this measure.

[10] Liquidity ratio is calculated as the share of cash and short term investments (*che*, Compustat item 36) to total assets.

[11] Total debt is calculated as the sum of debt in current liabilities (*dlc*) and long term debt (*dltt*).

of publicly-traded C-corporations. Second, despite the good coverage across different sized firms, Compustat may disproportionately feature large companies, and therefore may underrepresent small firms. Last, Compustat is mainly a report of financial statements; hence the gross income and tax variables are not reported for the purposes of tax books. Although a growing public finance literature (Kleven et al., 2016) increasingly suggests the use of third-party information on business records in developed countries to be legitimate and accurate with little discrepancy between the tax reports and the third-party information[12], extracting the corporate tax variable out of Compustat (despite being second-party information) may still be subject to measurement error. Since taxable income is unobserved data in the generic financial statements in Compustat, we construct a taxable income measure using balance sheet variables. According to the definition, taxable income is generated using firms' profits net of allowable cost deductions. Appendix 2.7.1 details the construction of taxable income.

Table 2.1 provides the summary statistics of the key variables in Compustat covering the sample period 1950-2006. The sample contains 220,575 firm-by-year observations from 17,455 firms. The selected variables are investment ratio, real taxable income, marginal tax rate, real income taxes, total real assets, and leverage ratio. Since the sample consists of public firms, the median size (total real assets) is \$100 million, and the median taxable income is \$7 million. The mean and median marginal tax rate in the sample is 32 and 35 percent, respectively.[13] The average and median real income taxes paid are \$39 million and \$2 million, respectively. The mean of leverage ratio is 28 percent and the mean of investment ratio ($i_{j,t}/k_{j,t-1}$) is 36 percent. The right-skewed size distribution of firms motivates the use of log variables in regressions.

2.2.2 Corporate Tax Variables: Statutory Rate and Investment Tax Credit

This section describes the construction of two main corporate tax measures on the statutory tax rate and investment tax credits. First, we explain the features of tax narrative accounts, introduce the statutory rate proxy and discuss features of the corporate tax code. Next, we present the investment tax credit proxy and discuss its properties. Finally, we discuss identification assumptions behind

[12]Kleven et al. (2016) further suggests this transparency to be especially true for the large firms where the tax enforcement through auditing is strong.

[13]The firms with zero marginal tax rates lead mean marginal tax rate to be less than the median.

the corporate tax measures.

Statutory Tax Rate Constructing exogenous statutory corporate tax changes is a two-stage procedure. In the first stage, we utilize the Mertens and Ravn (2013) dataset of exogenous corporate federal tax changes in the United States. Narrative accounts use historical records to summarize policy intervention using descriptive details of the process such as size, timing, and particular motivation. According to Romer and Romer (2009) categorization, a tax reform is exogenous if its motivation is to address inherited budget deficits or achieve some long-run goal such as increasing fairness or changes in the philosophy of the government.[14] This paper discards the changes in tax liabilities related to the current state of the economy, hence fulfilling the required assumption on the orthogonality of tax reforms. We also focus on tax changes implemented within one-quarter of their legislation to avoid *anticipation* effects (Mertens and Ravn, 2012).[15] After the elimination of tax changes based on exogenous, unanticipated, and persistent ones, the selection procedure yields six tax reforms between 1950 and 2006 with significant and immediate impact on corporate statutory tax rates (See Figure 2.1).[16]

In the second stage, we use IRS statutory rate data and construct a measure of statutory rate changes across income brackets over time. Following the earlier methods in Mertens and Olea (2018) and Zidar (2019), we calculate the estimated statutory change in year t as the difference between a counterfactual statutory rate calculated using year $(t-1)$ taxable income and year t rates and the actual year $(t-1)$ statutory tax rate. The use of the previous year's taxable income is to strip away the behavioral responses of firms in adjusting their income.[17] Equation 2.1 formulates Δmtr_t measure, which is interpreted as the change in the statutory rate on an additional \$1 income earned today. This is a proxy for the change in statutory rate, with no income response.

$$\Delta mtr_t = \tau_t(TI_{t-1}) - \tau_{t-1}(TI_{t-1}) \tag{2.1}$$

[14] Similarly, a tax reform is considered endogenous if the tax reform is influenced by current economic conditions.

[15] This is crucial as many corporate reforms are implemented with either delay or have gradual multiyear phase-ins such as Economic Recovery Tax Act of 1981 and Tax Reform Act of 1986.

[16] The plausibly exogenous marginal tax rate change years are 1964, 1979, 1982, 1984, 1987 and 1993.

[17] On the corporate income side, the tax brackets are matched to brackets on a nominal basis at every year; hence I do not deflate the taxable income measure through time.

An important feature of the US corporate income tax code is that it is a piecewise linear system where the taxable income is divided into brackets where marginal tax rates are fixed within but vary across these brackets. Table 2.2 provides an overview of the IRS historical statutory marginal tax rates across different taxable income brackets from 1950 to 2016.[18] This table reveals several unique features of the corporate tax code. First, the corporate tax code has adhered to 12 historically *stable* taxable income brackets ranging from under 25,000 dollars to over 18.333 million dollars.[19] Second, the gradual rate structure has consistently been an important characteristic of the corporate tax code since the late 1960s. Specifically, the Revenue Act of 1978, the 1986 Tax Reform Act, and the Omnibus 1993 provide examples of substantial size-dependent tax rates.[20] Third, the changes in corporate statutory taxes are not uniform across brackets; in contrast, they incorporate a considerable variation in the tax treatment. For example, some acts changed rates for very high income levels, while others were more uniform.[21] As a result, the corporate rate schedule incorporates sizable time-series and cross-sectional variation in statutory rates. That is, the measure of statutory rate changes in equation 2.1 captures sizable variation in tax treatment across different income brackets.

Investment Tax Credit ITC provisions allow firms to deduct a certain percentage of the new investment expenditure as a tax credit. This paper generates a new time-series measure of the changes in the investment tax credit using historical data on ITC rates.[22]

Figure 2.2 plots the level of investment tax credit rate granted by IRS. ITC was introduced under the Revenue Act of 1962 and was in effect through the end of 1985, except for two short periods. Within this period, ITC was a commonly used tax policy tool that moved frequently and dramatically. In particular, ITC was suspended from October 1966 to March 1967, removed in April 1969, reintroduced in August 1971 and increased to 10 percent with the Tax Reduction Act of 1975. After 1982, the ITC rate was set to 8 percent on average.[23] Overall, Figure 2.2 displays that corporate

[18] See the complete historical data on IRS historical Table 13.

[19] These brackets are $0 - $25,000, $25,000 - $50,000, $50,000 - $75,000, $75,000 - $100,000, $100,000 - $335,000, $335,000 - $1,000,000, $1,000,000 - $1,405,000, $1,405,000 - $10,000,000, $10,000,000 - $15,000,000, $15,000,000 - $18,333,000 above $18,333,000.

[20] Note that 1986 Tax Reform Act implemented two step changes occurring in years 1987 and 1988.

[21] See, for instance, the 1993 tax reform that *only* affected large firms with income greater than $10 million.

[22] The ITC dataset is constructed using Auerbach and Summers (1979); Auerbach (1982); Auerbach et al. (1983), Rosacker, Robert E. and Metcalf (1992) and IRS historical resources such as David (1981).

[23] Note that the exact rates varied depending on the asset's life. For example, the investment tax credit was 6 percent of the purchasing price for an asset of 3-year life or 10 percent for assets of 5-10 years.

tax policy has incorporated generous capital-consumption allowances to stimulate investment. In addition, the rate of investment tax credit shows a sizable variation till 1986, when it was fully repealed under 1986 Tax Reform Act. After matching the changes in ITC to narratively identified dates, we focus on ITC changes legislated in the years 1962, 1967, 1971, 1982, and 1987.

2.3 Empirical Framework

This section provides the empirical framework used to analyze the effects of changes in marginal tax rates and investment tax credits on firms' investment rates. We first lay out the details of the empirical specification that tests the dynamic causal effects of alternative tax tools on investment rate. Next, we explore financial and non-financial proxies proposed in the literature and estimate whether a particular group of firms drives the main results of the paper. Finally, we provide robustness checks of the baseline results using a variety of different specifications and controls.

2.3.1 Empirical Specification

This section specifies the empirical strategy that estimates the dynamic causal effects of statutory tax rate and investment tax credit changes on firm investment. The baseline panel regression specification estimates the impulse response functions using the following local projection (Jordà, 2005):

$$y_{j,t+h} - y_{j,t-1} = \alpha_j^h + \beta^h \, \Delta mtr_{j,t} + \delta^h \, \Delta ITC_t + \sum_{l=1}^{2} \theta_l^h X_{t-l} + \epsilon_{\, j,t+h} \tag{2.2}$$

where h = 0, 1,..., H and α_j is firm j fixed effect. $\Delta mtr_{j,t}$ is the changes in firm-level marginal tax rate, and ΔITC_t is the change in the rate of investment tax credit where both measures focus on changes occurring on the narratively-identified dates. The dependent variable is the investment rate which is constructed as the share of capital expenditures (*capx*) to physical capital at the beginning of the period measured by property, plant and equipment (net) (*ppent*). $Z_{j,t-1}$ is the vector of control variables that include lags of change in the average corporate income tax rate, change in log real gdp, change in log government spending, marginal tax rate and ITC rate, change in marginal tax rate, log real taxable income and lags of the dependent variable. Firm-level controls capture the impact of observable differences across firms over time and time-series controls help with the possible correlation of federal tax changes with other other aggregate variables (Mertens and Ravn, 2013).[24] The dependent variable is projected as the cumulative difference to interpret the parameters as impulse responses. Horizon limit is five years and lag length is set to two years.

There are two main parameters of interest: β^h and δ^h. β^h captures the effect of a one percentage point increase in the statutory tax rate of a firm at horizon h, and δ^h captures the impact of a one percentage point increase in the investment tax credit rate at horizon h. Firm fixed effects, captured by α_j^h, soak up permanent differences in investment behavior across firms and allows us to explore within-firm variation. Standard errors are two-way clustered by firm and year, where serial correlation adjustment is set to 2 years following Driscoll and Kraay (1998). This method adjusts standard errors for the possibility of correlation in the residuals across dates t and horizon h (Ramey and Zubairy, 2018; Tenreyro and Thwaites, 2016).

To prevent the results from being driven by outliers, the sample is 1 percent trimmed at both ends, based on the investment ratio. In addition, we trim the top 1 percent of the debt to asset ratio and drop firms with acquisition ratio exceeding five.[25] Firms observed less than five years are dropped as the impulse responses are estimated using at least five years of consecutive observations. Finally, the tax policy changes can influence other macroeconomic aggregates such as GDP and investment, which may impact firm-level investment. Our baseline specification does not control for time fixed effects, hence allows the general equilibrium effects to operate.

2.3.2 Baseline Results

Figure 2.3 presents OLS estimates for β^h and δ^h of equation 2.2 alongside the 95 percent confidence intervals over the horizon of five years. Figure 2.3a plots the coefficient β^h which captures the average effect of a one percentage point increase in marginal tax rate at horizon h and Figure 2.3b plots δ^h which captures the average effect of a one percentage point increase in the rate of

[24]The controls in the lags of dependent variables help capture time-varying changes in these two tax tools. See, for example, Figure 2.1 on the declining trend in the average corporate income tax rate.

[25]All trimming is done on a yearly basis. See Appendix for details on trimming and variable definitions.

investment tax credit at horizon h.

Figure 2.3a shows that the investment rate declines approximately 0.8 percentage points one year after a one percentage point increase in corporate marginal tax rates. The peak effect is -1.3 percentage points that is reached two years after the tax change. The effect of tax policy becomes significant one year after the shock and remains significant for about two years. Similarly, Figure 2.3b shows that investment rate increases approximately 0.4 percentage points one year after the rise in ITC by one percentage point. The peak effect is 0.7 percentage points that is reached two years after the ITC shock. Similar to marginal tax rate, the effect of investment tax credit becomes significant one year after the shock, and it remains significant about three years after the shock. Overall, Figure 2.3 shows that both marginal tax rates and investment tax credits significantly influence firms' investment decisions, yet marginal tax rates seem substantially more effective than investment tax credits. We use the average impulse response as a benchmark and move on to the heterogeneity results exploiting the role of certain firm characteristics in driving investment responses.

2.3.3 Heterogeneity Analysis

This section explores the heterogeneous effects of alternative tax tools. Exploring heterogeneity allows us to quantify the responses of alternative firm types and assess whether a certain type of firms drives the aggregate responses to tax policy. In addition, by exploring firm-level heterogeneity, we provide evidence on the effectiveness of targeted policy interventions across the firm distribution. In order to do this, we create indicators based on the financial and non-financial firm characteristics such as firm size (number of employees), leverage ratio, firm growth rate, and dividend status. The categories are created based on median cutoffs by year.

Figure 2.4 and 2.5 plot the OLS estimates of marginal tax rate and investment tax credits for firms of large and small sizes, respectively. Figure 2.4a suggests that following a one percentage point statutory tax increase, large firms start to lower their investment on impact and continue to do so for the next three years. The effect is significant for year one and year two and reaches its peak, -1 percentage point, two years after the shock. Similarly, Figure 2.5a shows that small firms start to lower their investment ratio one year after the shock. Their response is significant at years two and three, and the peak effect is -2 percentage points reached about two years after the statutory tax change. These findings suggest that following a one percentage point statutory tax rate increase, small firms lower their investment twice as much as large firms. Consistent with the marginal tax rate results, small firms also show much more responsiveness to investment tax credit changes. Figure 2.5b suggests that following a one percentage point increase in the investment tax credit rate, small firms increase their investment rate by about one percentage point (peak effect) two years after the shock. In contrast, large firms have a much weaker response to investment tax credit incentives; their peak response is 0.5 percentage points occurring two years after the shock (See Figure 2.4b). The effects are significant starting from year one till year three. Overall, investment tax credits lead both small and large firms to boost their capital stock in the following years, yet the effect, in general, is much more pronounced for small firms.

Next, Figure 2.6 and 2.7 show baseline results for firms with high and low leverage. Comparison of Figure 2.6 and 2.7 shows that firms with lower debt to asset positions prior to a tax shock lower their investment rate twice more than firms with high debt to asset positions. Figure 2.6a shows that the effect of a statutory rate change on high leverage firms is significant for two years following the shock and reaches its peak impact of -0.9 percentage points two years after the tax change. In contrast, firms with low leverage have a peak response of -1.6 percentage points occurring two years after the tax change (Figure 2.7a). Comparing Figure 2.6b and 2.7b also shows that a similar amplification is seen for responses to the investment tax credit. Although both types of firms utilize the temporary ITC incentives, firms with low leverage respond 0.4 percentage points more than firms with high leverage. The peak effect for low leverage firms is 0.8 percentage points increase in investment rate, occurring two years after the ITC change. In contrast, the peak effect for high leverage firms is 0.4 percentage points occuring two years after the ITC change. Both groups of firms show significant responses for about three years after the tax credit change.

Next, we test specification 2.2 using the dividend status of firms. Figure 2.8 presents the OLS estimates for firms with positive dividend payouts prior to tax reform. Figure 2.8a suggests that dividend-paying firms start to lower their investment ratio one year after the tax reform. The peak effect is a fall of around one percentage points, which occurs two years after the tax reform. The response is significant at years 1 and 2. In contrast, Figure 2.9a plots the impulse responses of firms that do not pay dividends. These firms start to lower their investment ratio on impact, and their peak response is a 1.8 percentage point fall in investment ratio occurring at year 2. The effect on non-dividend paying firms is significant at year 2. Like marginal tax rate results, firms that are not paying dividends show a more considerable investment boost following the ITC provisions. The peak effect on non-dividend payer firms is about 1.2 percentage points, e.g. much larger than the response of dividend payer firms (See Figure 2.8b). Overall, these results suggest that firms that are not paying dividends respond to both tax incentives substantially more than the firms that are paying dividends.

Lastly, we explore whether firms with different growth performances respond differently to marginal tax rate and investment tax credit provisions. Figure 2.10 and 2.11 show the impulse responses of firms with low and high asset growth prior to the tax reforms. Firms with high asset growth show a two percentage points fall in the investment ratio one year after a one percentage point increase in marginal tax rate. In contrast, firms with low asset growth show a 0.9 percentage point decline in the investment ratio occurring two years after the tax reform. Similarly, for the investment tax credit, firms on a high growth trajectory show a much larger response (1 percentage point increase) to investment tax credit provisions than low asset firms (0.4 percentage point increase).

Overall, we find that having low leverage, being small, not paying dividends, or being in a high growth phase is associated with a larger response to tax incentives from both statutory taxes and investment tax credits. These results stress the role of firm characteristics in determining the differential responses of firms to corporate tax policy changes. Our preferred interpretation follows a Bernanke et al. (1996) type "financial accelerator" mechanism. In an environment with capital market imperfections, tax shifts can change the balance sheet conditions of firms and impact their access to credit (Gertler and Gilchrist, 1994; Bernanke and Gertler, 1989; Kiyotaki and Moore, 1997). First of all, an increase in the tax rate would weaken firms' balance sheets by lowering the expected future cash flows. Second, taxes may reduce the value of collateral assets through the decline in firms' net worth. Recent evidence in Cloyne et al. (2020) also shows that these characteristics are consistent with financial frictions in the context of monetary policy. In line with Cloyne et al. (2020), our paper also concludes that small firms, firms with low leverage, firms not paying dividends, or firms in a growth trajectory are more likely to be financially constrained and show a higher sensitivity to tax policy.

2.3.4 Robustness

In this section, we supplement our baseline findings with some robustness checks. In particular, we confirm that the main results are robust to (i) controlling for additional firm characteristics, (ii) using a more recent time period, and (iii) sub-sample instability across sectors. Sensitivity results reassure that there are few differences across the specifications.

First, we begin by repeating the baseline exercise for investment using a richer set of control variables. Figure 2.13 shows that adding new control variables (debt to asset ratio and the number of employees) gives a very similar result to our baseline figures. The peak impact of a change in marginal tax rates is 1.4 percentage points occurring in year two and the peak impact of a change in the ITC is 0.8 percentage points, also occurring in year two. Both results suggest that the baseline results are robust to adding alternative control variables.

Second, we test our baseline regression using a more recent time span, 1970-2006. Our original time period covers 1950 to 2006, which provides an extensive timespan to study both tax policies. However, it is important to show that the effective power of these tax tools is also present as we analyze the more recent decades. Figure 2.12 provides the results estimated using data from 1970 to 2006. The impact of marginal tax rate and ITC changes is still present, where marginal tax rates change investment rate by 1.7 percentage points and ITCs change investment by one percentage point (peak effect). Overall, either period confirms that both of these tax tools were effective in stimulating investment of an average firm.

Next, we compare our results to Eskandari and Zamanian (2019)'s findings on US manufacturing firms. Using disaggregate sector-level data from 1956 to 2008, Eskandari and Zamanian (2019) finds that large manufacturing firms are twice as sensitive to changes in marginal tax rates than small manufacturing firms. We test their predictions using manufacturing firms in our sample, and our results corroborate their findings. As shown in Figure 2.15 and 2.16, when estimated purely on manufacturing firms, the marginal tax rates seem more effective on large firms. However, when we repeat the analysis on the full sample of firms, we find that marginal tax changes are more effective on small firms than large firms. Finally, Figure 2.14 also provides the average responses of manufacturing firms to alternative tax tools. The effects are very similar to the main findings estimated using the full sample.

2.4 Conclusion

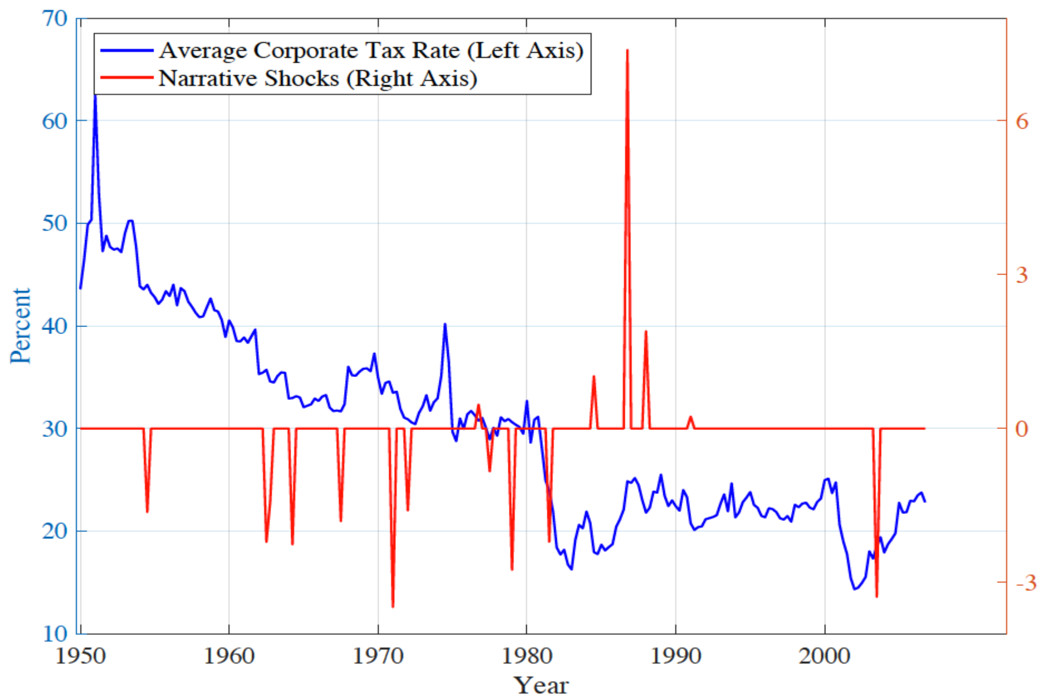
This paper contributes to the literature by providing a unique dataset on the two primary tools used in corporate tax policy: marginal tax rates and investment tax credits. We exploit 56 years of rich firm-level data and provide firm-level evidence on the dynamic causal effects of alternative corporate tax tools on business investment in the US.

The key results can be summarized as follows. First, we find that both changes in marginal tax rates and investment tax credits are effective tools to steer firms' investment, yet on average marginal tax rate changes are more effective than investment tax credits. Second, we pin down the role of financial and non-financial positions of the firms in determining the differential responses of firms to corporate tax policy changes. Our results highlight that characteristics such as being small, not paying dividend, having high-growth and low leverage amplify firms' responses to corporate tax policy changes. In contrast, large, dividend-paying, slow-growth firms or firms with a high leverage ratio at the time of the tax shock tend to reduce their investment much less over a period of two years following an increase in the corporate statutory taxes. These results are consistent with the responses to ITC provisions, as well.

This paper provides a comparison of the two most common tools used in corporate tax policy and helps us elucidate the potential fiscal policy transmission mechanisms. Our findings also show that these tax tools have heterogeneous effects. This provides a useful rule of thumb on the comparative effectiveness of aggregate tax policy changes across the firm distribution.

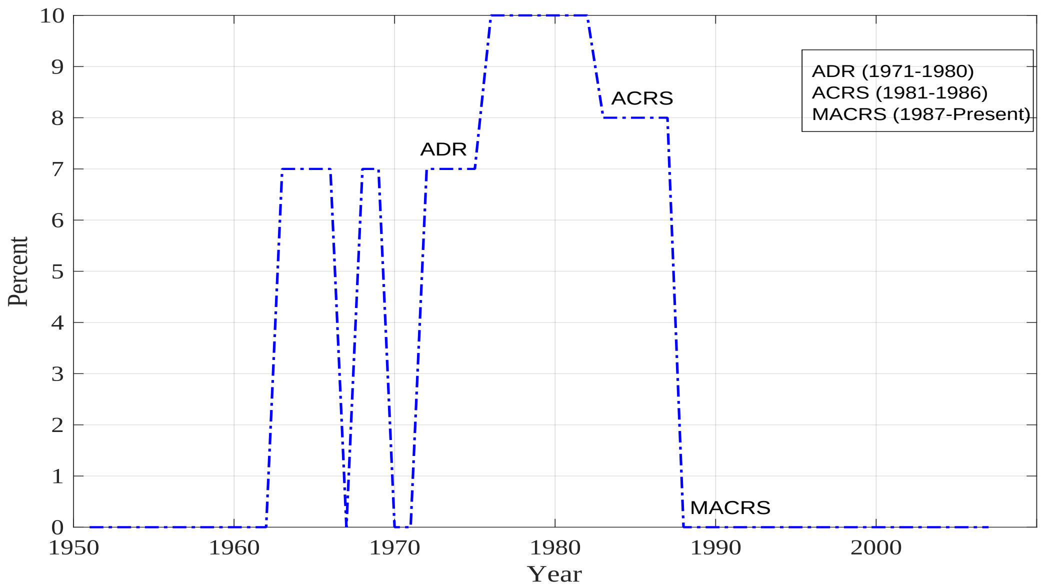
2.5 Figures

Figure 2.1: Mertens and Ravn (2013) Average Corporate Income Tax Rates and Narrative Shocks



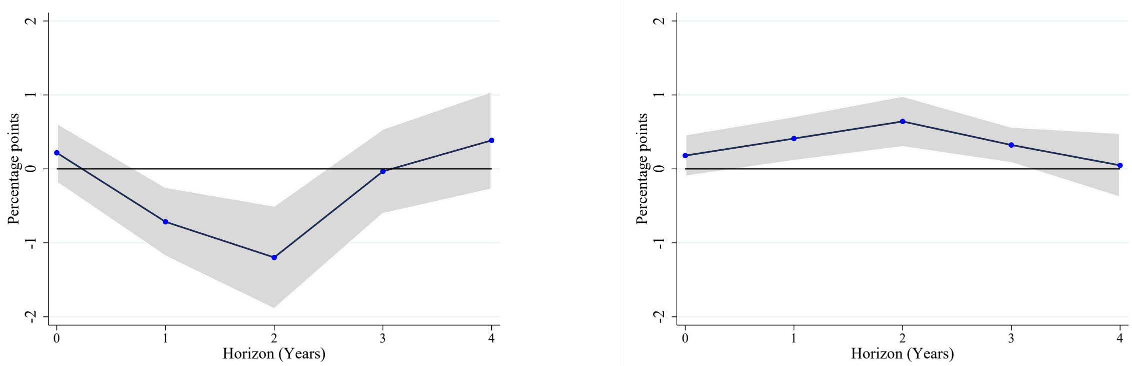
Note: Average Corporate Income Tax Rate is calculated as the federal corporate income tax revenues divided by corporate income tax base. Average tax rate changes are calculated from tax liability changes scaled by previous quarter taxable incomes, using national income and product accounts (NIPA). The data spans 1950Q1-2006Q4. $ACITR_t = \frac{\text{Taxes on Corporate Profits}_t}{\text{Corporate Profits}_t}$ and $\Delta T_t^{CI,narr} = \frac{\text{CI tax liability change}_t}{\text{Corporate Taxable Income}_{t-1}}$. The plausibly exogenous tax reforms years are 1964, 1979, 1982, 1984, 1987 and 1993.

Figure 2.2: Investment Tax Credit (ITC) Rate in the US (1950-2006)



Note: ITC dataset is constructed using Auerbach and Summers (1979); Auerbach (1982); Auerbach et al. (1983), Rosacker, Robert E. and Metcalf (1992) and various IRS historical resources such as David (1981). The ITC was introduced under the Revenue Act of 1962 (January 1, 1962) and was in effect through the end of 1985, except for two short periods. In particular, ITC was suspended from October 10, 1966 to March 9, 1967, removed in April 19, 1969, reintroduced in August 15, 1971 and increased to 10 percent with the Tax Reduction Act of 1975. After 1982 (under Accelerated Cost Recovery System), the ITC rate was set to 8 percent on average. The ITC was fully repealed under 1986 Tax Reform Act. After matching the changes in ITC to narratively identified dates, we focus on ITC changes legislated in years 1962, 1967, 1971, 1982 and 1987.

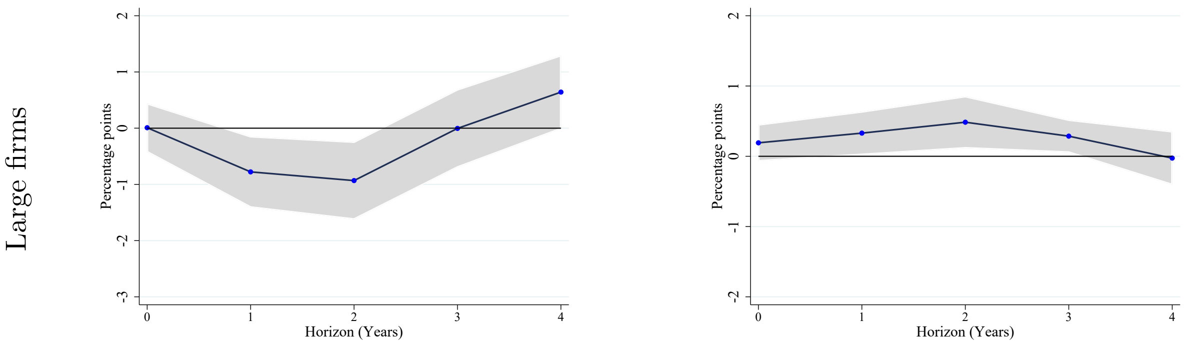
Figure 2.3: Effects of corporate tax changes on investment ratio.



(a) Marginal Tax Rate (β^h) **(b)** Investment Tax Credit (δ^h)

Note: Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate, log real taxable income and lags of dependent variable. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

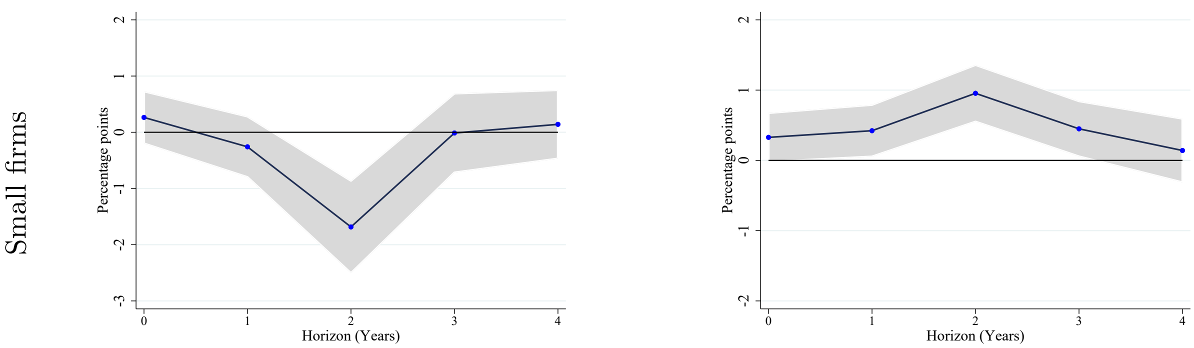
Figure 2.4: Effect of corporate tax changes on investment ratio for large firms.



(a) Marginal Tax Rate

(b) Investment Tax Credit

Figure 2.5: Effect of corporate tax changes on investment ratio for small firms.

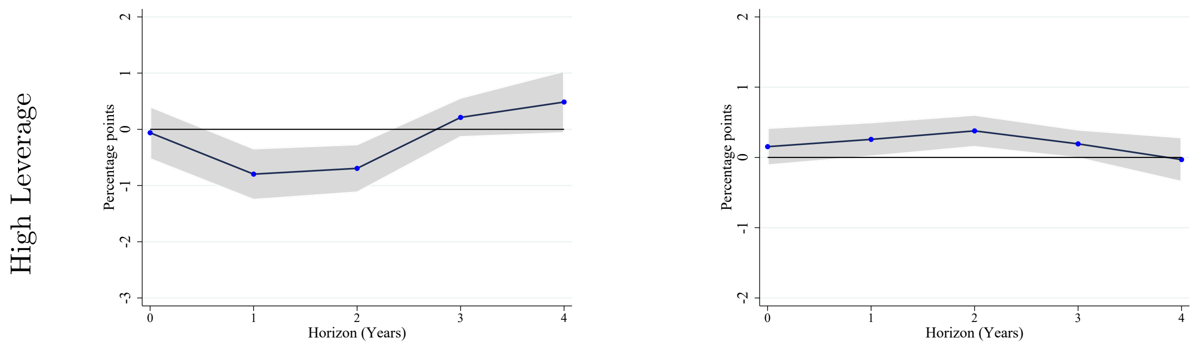


(a) Marginal Tax Rate

(b) Investment Tax Credit

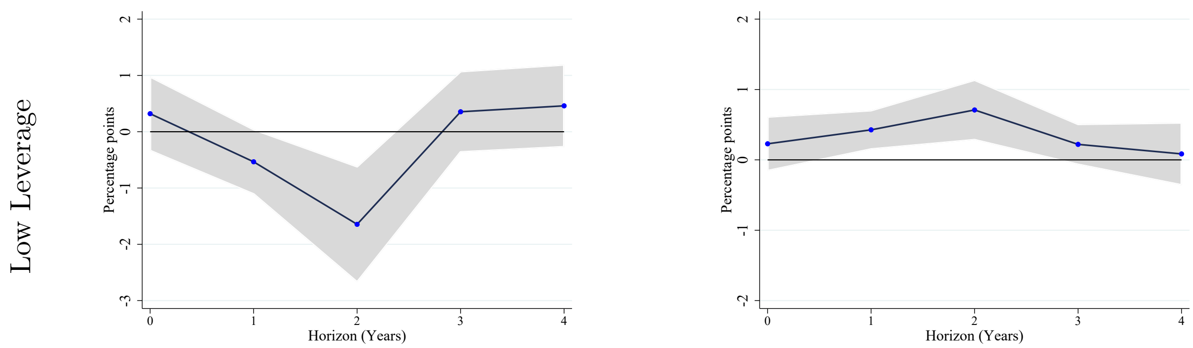
Note: Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate (the independent variable), log real taxable income and lags of dependent variable. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

Figure 2.6: Effect of corporate tax changes on investment ratio for high leverage firms.



(a) Marginal Tax Rate **(b)** Investment Tax Credit

Figure 2.7: Effect of corporate tax changes on investment ratio for low leverage firms.

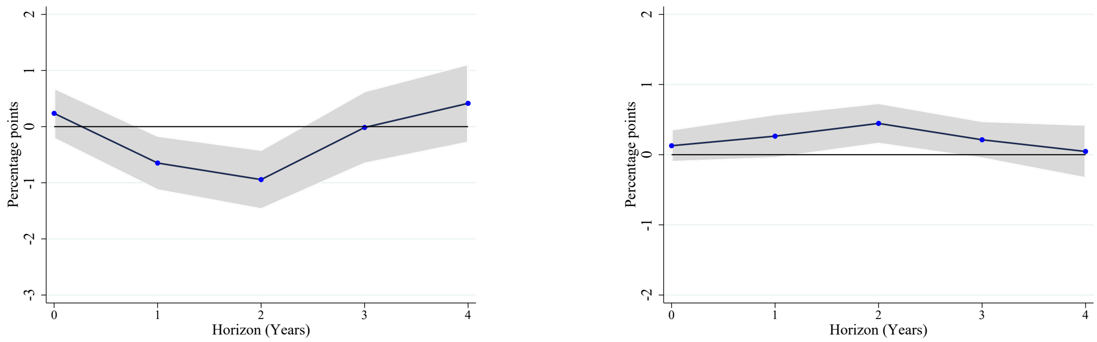


(a) Marginal Tax Rate **(b)** Investment Tax Credit

Note: Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate (the independent variable), log real taxable income and lags of dependent variable. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

Figure 2.8: Effect of corporate tax changes on investment ratio for dividend payer firms.

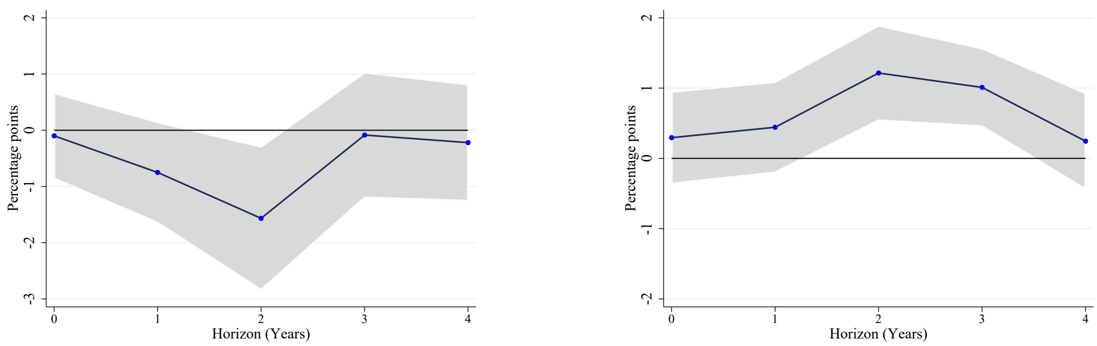
Positive dividend



(a) Marginal Tax Rate **(b)** Investment Tax Credit

Figure 2.9: Effect of corporate tax changes on investment ratio for dividend non-payer firms.

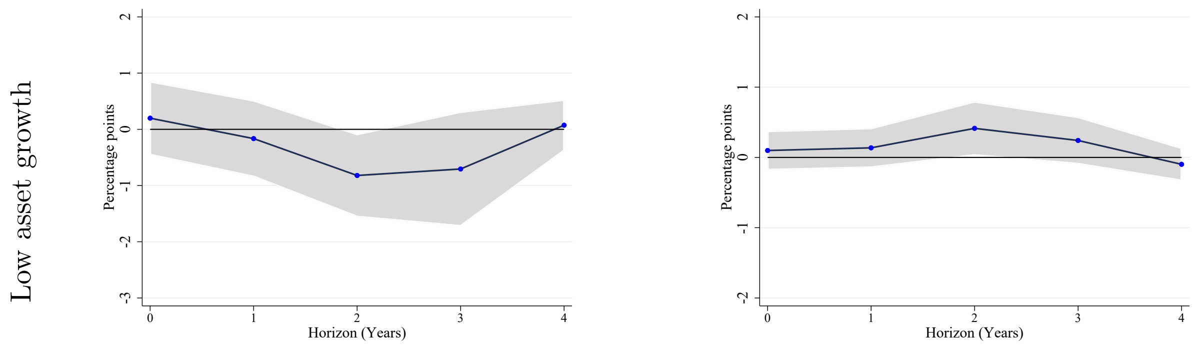
Zero dividend



(a) Marginal Tax Rate **(b)** Investment Tax Credit

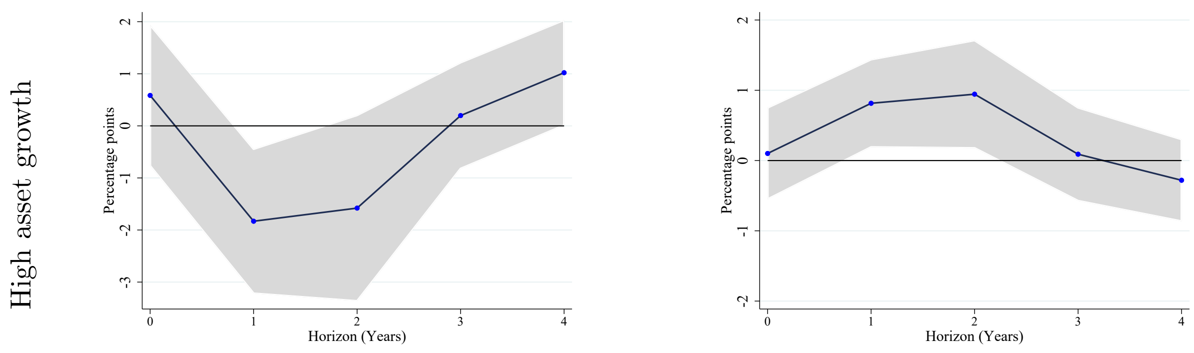
Note: Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate (the independent variable), log real taxable income and lags of dependent variable. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

Figure 2.10: Effect of corporate tax changes on investment ratio for low asset growth.



(a) Marginal Tax Rate **(b)** Investment Tax Credit

Figure 2.11: Effect of corporate tax changes on investment ratio for high asset growth firms.

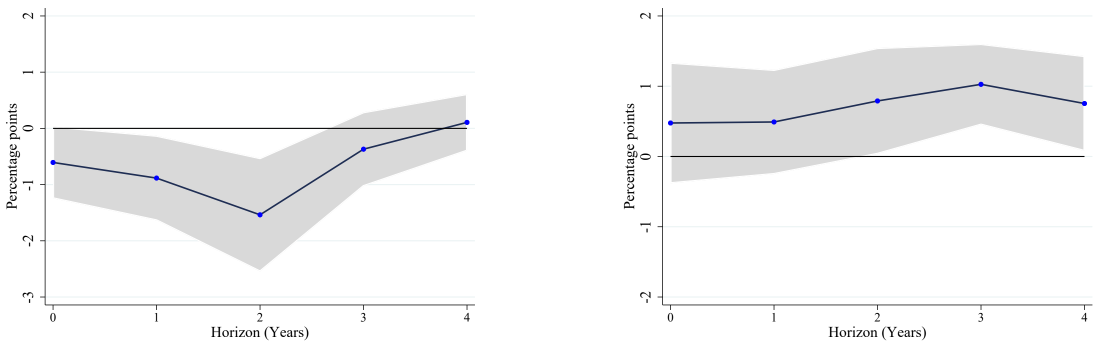


(a) Marginal Tax Rate **(b)** Investment Tax Credit

Note: Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate (the independent variable), log real taxable income and lags of dependent variable. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

Robustness Figures

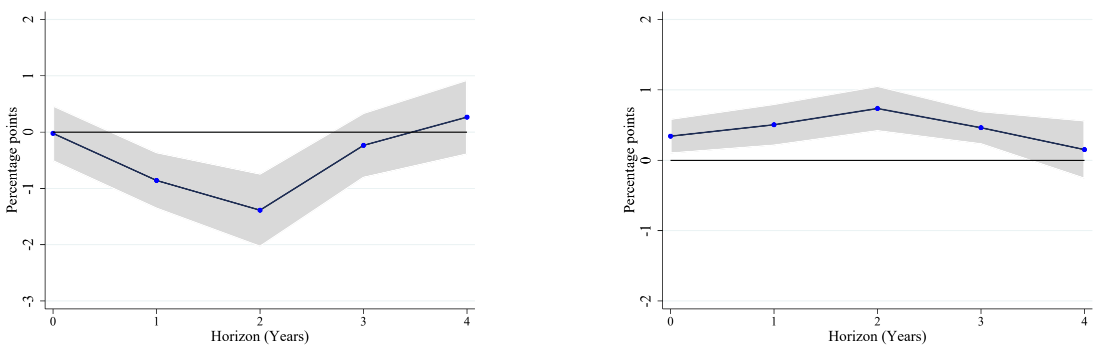
Figure 2.12: Effect of corporate tax changes on investment ratio (1970-2006).



(a) Marginal Tax Rate **(b)** Investment Tax Credit

Note: The short sample consists of 172,825 observations from 11,931 firms. Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate (the independent variable), log real taxable income and lags of dependent variable. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

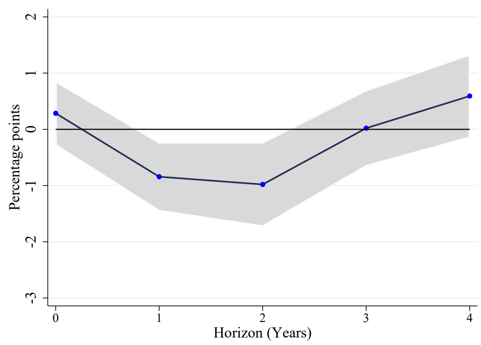
Figure 2.13: Effect of corporate tax changes on investment ratio (1950-2006).



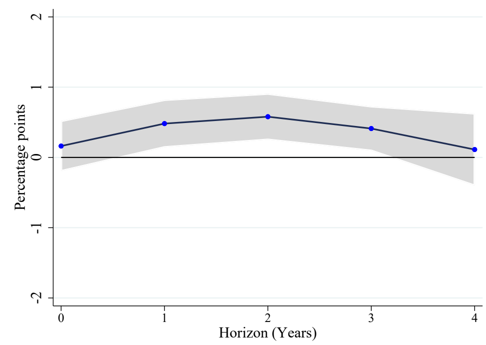
(a) Marginal Tax Rate **(b)** Investment Tax Credit

Note: The short sample consists of 172,825 observations from 11,931 firms. Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate (the independent variable), log real taxable income, lags of dependent variable, lags of debt to asset ratio and lags of log employees. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

Figure 2.14: Effect of corporate tax changes on investment ratio for manufacturing.



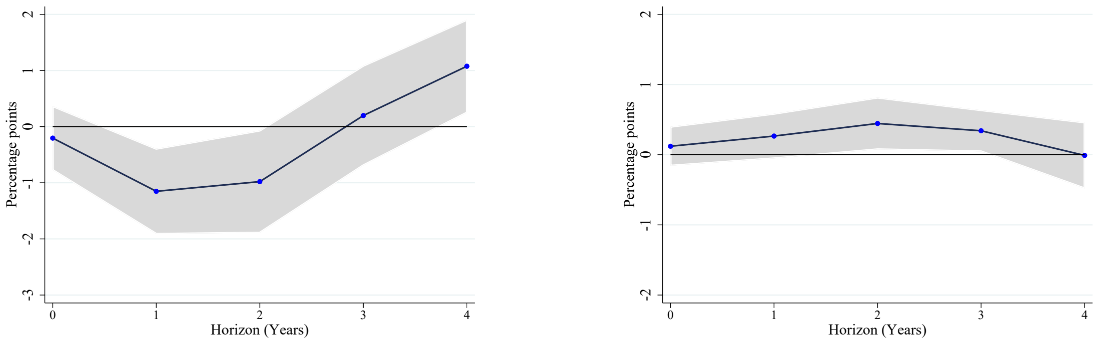
(a) Marginal Tax Rate



(b) Investment Tax Credit

Note: Manufacturing firms sample consists of 103,078 observations from 5,949 firms. Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate (the independent variable), log real taxable income and lags of dependent variable. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

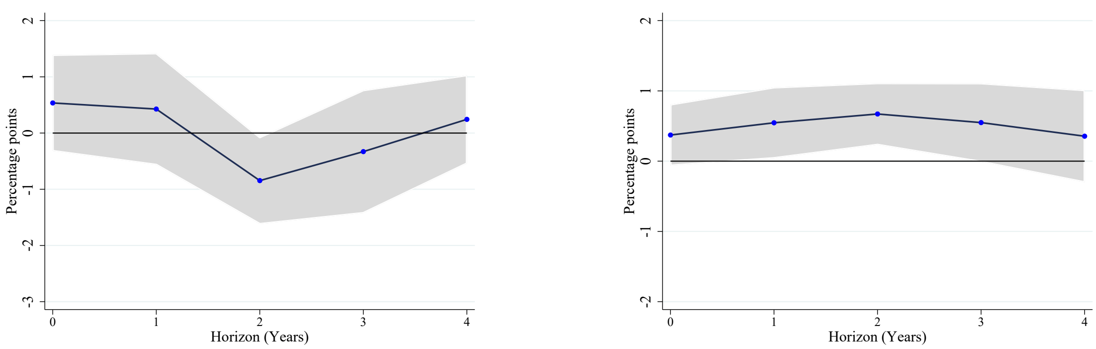
Figure 2.15: Effect of corporate tax changes on investment ratio for *large* manufacturing firms.



(a) Marginal Tax Rate **(b)** Investment Tax Credit

Note: Large manufacturing firms sample consists of 53,104 observations from 3,049 firms. Size split of the sample is done by median by year. Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate (the independent variable), log real taxable income and lags of dependent variable. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

Figure 2.16: Effect of corporate tax changes on investment ratio for *small* manufacturing firms.



(a) Marginal Tax Rate **(b)** Investment Tax Credit

Note: Small manufacturing firms sample consists of 53,696 observations from 4,302 firms. Size split of the sample is done by median by year. Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate (the independent variable), log real taxable income and lags of dependent variable. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

2.6 Tables

Table 2.1: Descriptive Statistics

	Investment Ratio	Taxable Income	Mtr	Income Taxes	Real Assets	Debt to Asset
Obs #	193574	220575	220572	219945	220575	215719
Bottom 5%	2.6	0	0	-1	3	0
p25	10.8	0.1	22	0	22	7.1
Median	20.5	7	35	2	100	23.6
mean	36.1	130.5	32	39	1312	28.3
p75	39.0	44.7	48	14	495	40.1
Top 5%	118.9	490.2	52	151	5150	71.8
Std dev	55.3	793.4	19	279	8300	34.2

Note: Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. All the trim is done on a yearly basis. Taxable income and income taxes are in real million dollars. See Appendix 2.7.1 for details on trimming and variable definitions.

Table 2.2: IRS Corporate Income Tax Rates and Brackets (1950-2016)

1950	
First $25,000	23.00
Over - $25,000	42.00
1951	
First $25,000	28.75
Over - $25,000	50.75
1952-1963	
First $25,000	30.00
Over - $25,000	52.00
1964	
First $25,000	22.00
Over - $25,000	50.00
1965-67	
First $25,000	22.00
Over - $25,000	48.00
1968-69	
First $25,000	24.20
Over - $25,000	52.80
1970	
First $25,000	22.55
Over - $25,000	49.20
1971-1974	
First $25,000	22.00
Over - $25,000	48.00
1975-1978	
First $25,000	20.00
$25,000 - $50,000	22.00
Over - $50,000	48.00
1979-1981	
First $25,000	17.00
$25,000-$50,000	20.00
$50,000-$75,000	30.00
$75,000-$100,000	40.00
Over -$100,000	46.00
1982	
First $25,000	16.00
$25,000-$50,000	19.00
$50,000-$75,000	30.00
$75,000-$100,000	40.00
Over -$100,000	46.00
1983	
First $25,000	15.00
$25,000-$50,000	18.00
$50,000-$75,000	30.00
$75,000-$100,000	40.00
Over -$100,000	46.00
1984-1986	
First $25,000	15.00
$25,000-$50,000	18.00
$50,000-$75,000	30.00
$75,000-$100,000	40.00
$100,000-$1,000,000	46.00
$1,000,000-$1,405,000	51.00
Over $1,405,000	46.00
1987	
First $25,000	15.00
$25,000-$50,000	16.50
$50,000-$75,000	27.50
$75,000-$100,000	37.00
$100,000-$335,000	42.50
$335,000-$1,000,000	40.00
$1,000,000-$1,405,000	42.50
Over $1,405,000	40.00
1988-1992	
First $50,000	15.00
$50,000-$75,000	25.00
$75,000-$100,000	34.00
$100,000-$335,000	39.00
Over $335,000	34.00
1993-2016	
First $50,000	15.00
$50,000-$75,000	25.00
$75,000-$100,000	34.00
$100,000-$335,000	39.00
$335,000-$10,000,000	34.00
$10,000,000-$15,000,000	35.00
$15,000,000-$18,333,333	38.00
Over $18,333,333	35.00

Source: IRS historical Table 24.

2.7 Appendix

2.7.1 Data Definitions

Compustat

Table A2.1 provides the variable names and respective codes in Compustat. Leverage is the ratio of short and long term debt to total assets. Liquidity ratio is the ratio of cash and short-term investments (*che*) to total assets. Dividend variable is used as an indicator on whether the firm has paid cash dividends in the previous year. *aqc* represents the cash outflow or funds used to acquisition of a company. All variables in level are deflated using the aggregate GVA deflator. I explain the variables used in taxable income definition in the next page in detail.

Table A2.1: Variable Definitions

Variable	Compustat variable
Leverage	$(dlc + dltt) * 100/at$
Liquidity ratio	$che * 100/at$
Employees	emp
Investment	capx
Total Assets (Book value)	at
Sales	sale
Dividend	dv
Acquisitions	aqc/at

I drop firms in finance, insurance, real estate and public administration sectors. Following Ottonello and Winberry (2020), I also exclude firms with acquisitions accounting for more than 5% of total assets. I drop firms which are in the panel for less than 5 years. I exclude firms within the finance, insurance, real estate (FIRE) and public administration sectors.

Sample Restrictions The baseline trimming excludes firms with i) top 1 percent of leverage ratio; ii) top and bottom 1% of real sales growth; iii) Tobin's Q ratio greater than 4; iv) acquisitions are more than 5% of total assets. Trimming is done by year.

Macro Time Series Data The GVA (gross value added) deflator series is Price Index (Business : Nonfarm) from FRED (data series is B358RG3Q086SBEA).The statutory rate data is from IRS historical Table 24. The data on real GDP and government spending is from Mertens and Ravn (2013).

Taxable income data definition

Taxable income is calculated as follows:

$$\text{Taxable Income} = \text{EBIT} + \text{interest on leases} - \text{timing differences estimated using deferred tax expense} \tag{A2.1}$$

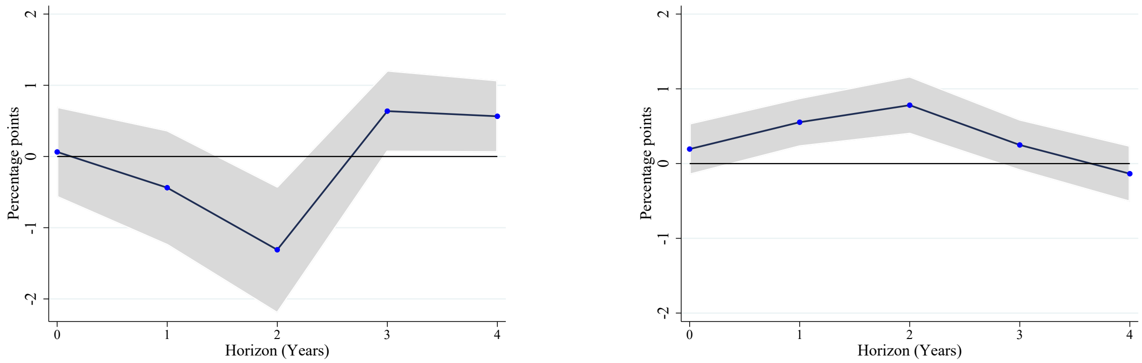
where EBIT is earnings before interest and taxes (*ebit* + *nopi*, CRSP & Compustat annual item), interest on leases is interest income is *tiiq* (Compustat quarterly item 62), deferred taxes is *txdiq* (Compustat quarterly, item 35).[26] Following Blouin et al. (2010)'s calculations, the timing differences estimated using deferred tax expense reported on the income statement is used as (*txdiq*/max statutory rate).

[26]Taxable income series includes ebit and nopi (nonoperating income) that is reported separately in Compustat.

2.7.2 Additional Figures

A. Additional Heterogeneous Effects of Corporate Taxes

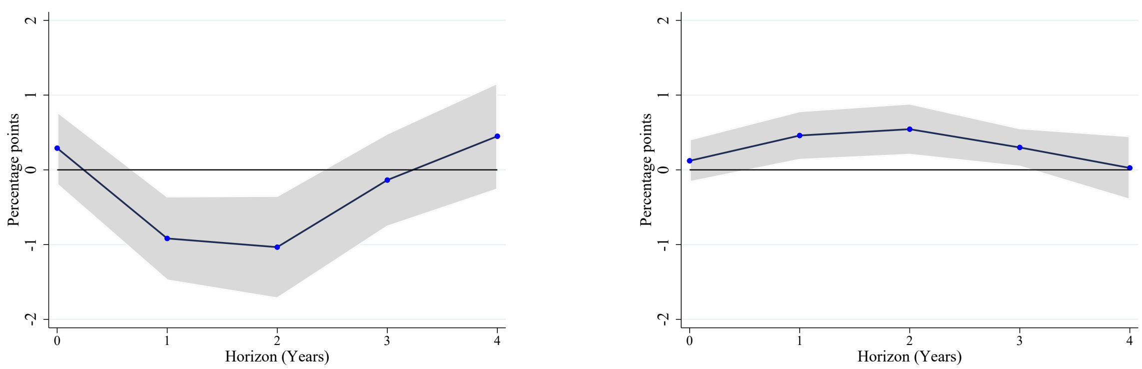
Figure A2.1: Effect of corporate tax changes on investment ratio for young firms.



(a) Marginal Tax Rate

(b) Investment Tax Credit

Figure A2.2: Effect of corporate tax changes on investment ratio for old firms.

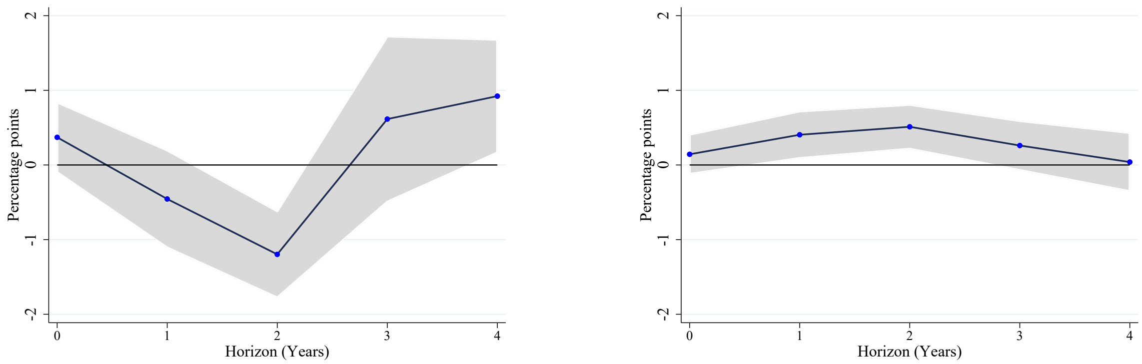


(a) Marginal Tax Rate

(b) Investment Tax Credit

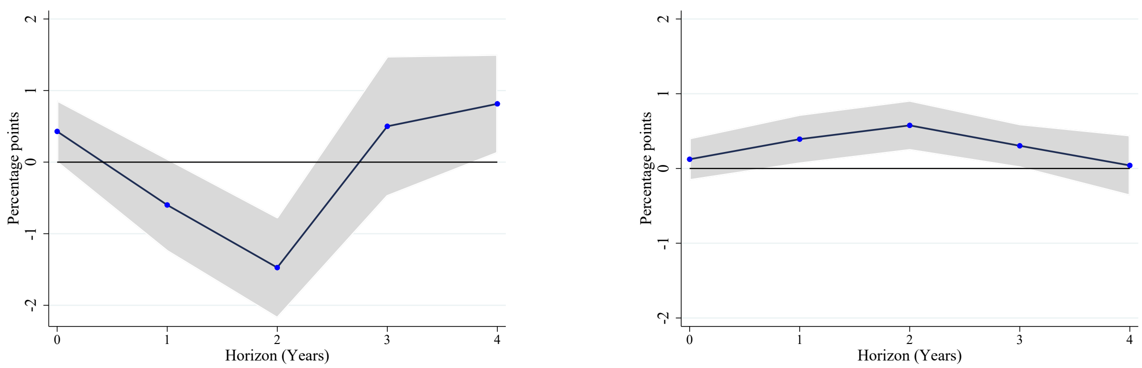
Note: Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate (the independent variable), log real taxable income and lags of dependent variable. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

Figure A2.3: Effect of corporate tax changes on investment ratio for high liquidity firms.



(a) Marginal Tax Rate **(b)** Investment Tax Credit

Figure A2.4: Effect of corporate tax changes on investment ratio for low liquidity firms.

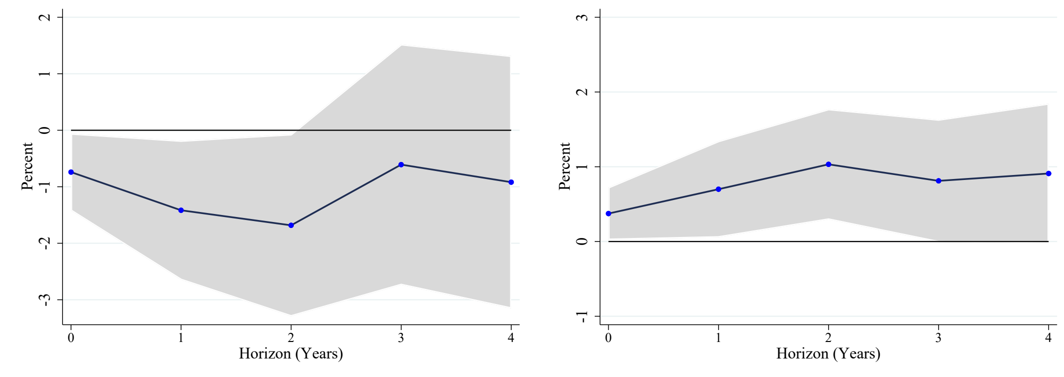


(a) Marginal Tax Rate **(b)** Investment Tax Credit

Note: Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate (the independent variable), log real taxable income and lags of dependent variable. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

B. Additional Effects on Employment

Figure A2.5: Effect of corporate tax changes on the number of employees.



Note: Control variables include lags of change in average corporate income tax rate, change in log real gdp, change in log government spending, lags of marginal tax rate, lags of ITC, change in marginal tax rate (the independent variable), log real taxable income and lags of dependent variable. Horizon is 5 years, lag is 2 years. Baseline trimming includes top and bottom 1 percent cut to investment ratio, top 1 percent cut to debt to assets ratio and trimming for acquisition ratio exceeding five. The standard errors are two-way clustered by firm and year.

Chapter 3

Asymmetric Effects of Monetary Policy in Micro-data

3.1 Introduction

The literature on monetary transmission has documented non-linear effects of monetary policy at the aggregate level. However, sign-dependent effects of monetary policy at the disaggregate level still remain relatively underexplored. This paper uses US firm-level data and examines firms' employment, investment and sales sensitivity to sign-dependent monetary policy innovations from 1980q6 to 2016q6. The paper attempts to advance the existing literature by (i) providing the first micro-level evidence on asymmetric effects of monetary policy in the US; and (ii) exploring the role of financial characteristics in propagating the asymmetric effects of monetary policy.

What causes asymmetry in monetary effectiveness? In recent years, the literature has proposed various mechanisms such as financial frictions and downward wage rigidity to model the asymmetric effects of monetary policy.[1] In this paper, I explore the role of financial frictions in explaining the asymmetric effects of monetary policy empirically. According to the financial frictions literature, a monetary tightening (e.g. an increase in interest rates) may result in (i) weaker firm and bank balance sheets, (ii) lower expected future value of collateral assets, which in turn, would lead to more

[1] A non-exhaustive list of papers; Lin (2020); Barnichon et al. (2017); Kandil (1995); Gorodnichenko and Weber (2016); Guerrieri and Iacoviello (2017).

binding borrowing constraints on firms that are at the very margin.[2] The main idea is that in times of tight monetary policy, credit constraints tend to bind, leading to larger effects on firms that are financially constrained. This type of amplification during contractions can be associated with banks engaging in credit rationing or incorporating higher risk premium to the financial contracts of high-risk firms to mitigate adverse selection and moral hazard problems.[3] In contrast, during monetary expansions borrowing constraints tend to relax, weakening the amplification mechanism.

Studying the role of financial frictions in the context of monetary asymmetry requires (i) time-series identified sign-dependent monetary policy innovations and (ii) an indicator capturing variation in financial constraints at the disaggregate level. To do this, I employ Gertler and Karadi (2015) monetary policy surprises and explore sign-dependent asymmetries in monetary policy. I also use quarterly Compustat firm-level data from 1980q3 to 2016q3 that provides rich balance sheet information and allows me to consider alternative financial proxies proposed in the literature. Through a micro-data analysis, I am able to test whether firms respond differently to monetary contractions and expansions and whether financial characteristics play a role in propagating the asymmetric effects of monetary policy.

This paper provides the first firm-level evidence, to the best of my knowledge, on the asymmetric effects of monetary policy in the US. By analyzing 36 years of micro-data, I show that monetary tightenings have larger effects on firms' employment, investment and sales than monetary expansions. These results are robust to various checks such as (i) controlling for additional firm characteristics, (ii) having a more restrictive sample, (iii) using an alternative time-span of monetary policy shocks, and (iv) sub-sample instability across sectors.

Second, I consider common financial constraint proxies used in the literature in the context of monetary asymmetry and provide evidence that financial frictions play a role in propagating sign-dependent effects of monetary policy. My findings suggest that characteristics such as being small,

[2] Gertler and Gilchrist (1994); Bernanke and Gertler (1989); Kiyotaki and Moore (1997); Calomiris and Hubbard (1990) show that in an environment with capital market imperfections, borrowers' balance sheet conditions may play a significant role in access to credit.

[3] See the literature on credit channels and market imperfections Gertler and Karadi (2015); Bordo et al. (2016); Bayoumi and Melander (2008); Aron et al. (2012); Jiménez et al. (2012) and Ciccarelli et al. (2015). Bernanke and Gertler (1990) and Calomiris and Hubbard (1990) studies the reallocation of credit in downturns from low-net-worth to high-net-worth borrowers. Besides, Barnichon et al. (2017) argue that banks may also change the overall pass-through of interest rate changes in an asymmetric way depending on the sign of the monetary policy intervention.

holding low leverage, holding high liquidity, or not paying dividends lead firms' employment and investment to be more sensitive to monetary tightenings. I also find the impact of monetary expansions are larger for firms with low leverage or high liquidity, but these occur at a much lower rate than monetary contractions. Besides, the role of firm characteristics on the expansionary monetary policy is mainly present on firm-level employment, - not on investment-.

This paper connects to the literature studying the asymmetric effects of monetary policy. Exploring sign-dependent money supply shocks in the US from 1951:1 to 1987:4, Cover (1992) documents that negative money supply shocks affect output while positive money supply shocks do not.[4] Using data from 1954q3 to 2002q4, Lo and Piger (2005) find a policy contraction to be more effective than a policy stimulus. Using aggregate data from 1989:8 through 2007:7, Angrist et al. (2018) show that monetary tightenings have more pronounced effects than monetary expansions on inflation, industrial production, and unemployment in the US. Last, using inflation and output data from 1969:I-2002:IV, Tenreyro and Thwaites (2016) document monetary tightenings to have a bigger impact on output -but not on inflation- than monetary expansions. A common feature of this literature is a focus on time-series data to study sign-dependent effects of monetary policy. My paper contributes to this literature by providing the first US firm-level evidence on the asymmetric effects of monetary policy, which, unlike earlier findings, allows for a rich analysis of transmission mechanisms in the context of monetary non-linearities. My empirical strategy allows for testing a variety of firm characteristics and can answer questions like whether certain firm characteristics propagate the non-linear effects of monetary policy.

Next, there is also a literature studying how the effectiveness of monetary policy depends on the state of the economy (Thoma, 1994; Weise, 1999; Garcia R. and Schaller H., 2002; Lo and Piger, 2005; Peersman and Smets, 2009).[5] Although this literature is mainly interested in sensitivity to interest rates at different points in business cycles, some interesting studies explore the sign-dependence *jointly* with size and business cycle related effects. Among these, Ravn and Sola (2004)

[4]Similar to Cover (1992), de Long et al. (1988) and Thoma (1994) find negative monetary policy shocks to have greater effect on real GNP and industrial output, respectively. In contrast, Weise (1999), using data from 1960q2 to 1995q2, finds no evidence to support differential effectiveness of positive and negative shocks. Note that most of the earlier studies work with money-based indicators of monetary policy rather than interest-rate-based measures.

[5]For example, Garcia R. and Schaller H. (2002) find asymmetry in the effects of monetary policy, with stronger effects during recessions than during expansions. Using data from 1969q1 till 2002q4, Tenreyro and Thwaites (2016) show that the effects of monetary policy are less powerful in recessions for durables expenditure and business investment.

analyze the *size* and direction of monetary policy shocks jointly and conclude that *small* contractionary monetary policy shocks have real effects on output.[6] In addition, Tenreyro and Thwaites (2016) explore asymmetric effects of monetary policy depending on the state of business cycle and document that the sign-dependent effects do not have a pattern with the regime-dependent effects. Their results show that monetary tightenings are inherently different from monetary expansions, and the contractions do not preponderate in booms (Tenreyro and Thwaites (2016), p.59). This suggests that the two sets of asymmetries in monetary effectiveness (sign-dependence and regime-dependence) co-exist.

This paper also connects to the literature studying heterogeneity of monetary policy on firm-level data. These papers provide evidence on how financial and non-financial factors like balance sheet conditions (Gertler and Gilchrist, 1994; Ottonello and Winberry, 2020; Kudlyak and Sánchez, 2017), dividend payments (Fazzari et al., 1988; Farre-Mensa and Ljungqvist, 2016), firm age-dividend (Cloyne et al., 2019), liquidity conditions (Jeenas, 2019; Fazzari et al., 1988; Kashyap et al., 1994; Gilchrist and Himmelberg, 1995) and collateral assets (Bahaj et al., 2020) play a role in the transmission of monetary policy. Although these studies investigate a wide variety of monetary policy transmission mechanisms, a common feature of these papers is to form a direct link between firm finance and transmission of monetary policy. Among these, Cloyne et al. (2019) provide a comparison of all these proxies and show that age-dividend characteristics are correlated with being small, holding low leverage and high liquidity and provide a better proxy for financial constraints at the firm-level. In this paper, my contribution is to analyze some of these alternative financial proxies studied in this literature in the context of monetary asymmetries. My results contribute to this literature by examining how financial characteristics play a role when we allow for *non-linear* effects of monetary policy.

Taken together, this paper makes two contributions to the literature. First, the results of this paper present the first micro-level empirical evidence, to the best of my knowledge, quantifying the sign-dependent effectiveness of monetary policy in the US. Specifically, I find that monetary contractions have more effect on firms' employment, investment, and sales than expansions. Second, with the use of micro-data, I'm able to consider alternative financial proxies debated in the literature. My

[6] Note that Ravn and Sola (2004) finds asymmetric effects only on the estimates using federal funds rate.

findings document that certain firm characteristics, such as being small, having low leverage, high liquidity, or no-dividend paying status, trigger a larger amount of variation in the sign-dependent monetary policy effectiveness. These results are consistent with the evidence in Cloyne et al. (2019) which shows that firms with these characteristics are more likely to be financially constrained. In sum, the results of this paper provide a practical rule-of-thumb for the design of stabilization policy and contribute to our understanding of the scope of monetary policy when there are non-linearities.

The rest of the paper is organized as follows. Section (3.2) discusses firm-level data and monetary policy shocks. Section (3.3) presents the empirical strategy, discusses the main results and robustness checks. Section (3.4) concludes.

3.2 Dataset

In this section, I describe the datasets used in this paper. First, I lay out the Compustat firm-level database, discuss the construction of the main variables and present descriptive statistics. Next, I discuss the monetary policy shocks used in this paper and illustrate its basic properties. A detailed description of sources, definitions and sample selection are discussed in Appendix 3.7.

Firm level variables

This paper employs the quarterly Compustat database on the universe of publicly traded C corporations. Compustat North America offers high-quality information on balance sheet and income statement components of active and inactive publicly held companies. The total sample covers the period between 1980q3 and 2016q3 and consists observation from 13,051 firms. Since the monetary policy shocks start at 1980q3, I take 1980q3 as the starting date. The sample ends in 2016, which is the last available observation on the excess bond premium data from Gilchrist and Zakrajšek (2012).

The main variables of interest are number of employees (emp), investment ratio ($\frac{i_{j,t}}{k_{j,t-1}}$), defined as capital expenditures of firm j in period t relative to the level of physical capital stock in the last period, sales (*saleq*), book value of total assets (*atq*), long-term debt (*dlttq*), liquidity ratio[7] and leverage (total debt divided by the book value of total assets).[8]

Using Compustat data in this paper is advantageous for a couple of reasons. First, Compustat is a long enough panel to study within-firm variation. I analyze thirty-six years of quarterly firm-level data where the average firm is observed for about 13 years.[9] Second, Compustat has a rich cross-sectional dimension which allows me to test alternative hypotheses and conduct heterogeneity analysis. Finally, Compustat is a high-frequency firm-level dataset that allows monetary policy analysis at a quarterly frequency. One limitation of using Compustat data is that despite the good coverage across different sized firms, Compustat data may disproportionately feature large companies, and therefore may underrepresent small firms. In addition, Compustat estimates represent only the behavior of publicly traded C-corporations.[10]

Summary statistics

Table 3.1 presents summary statistics of key variables of interest in the firm-level data for the period 1980q3-2016q3. The sample contains 687,043 firm-by-quarter observations from 13,051 firms. Since the sample consists of public firms, the average size (total assets) is \$1,9 million, and average firm sales is \$0.4 million over the sample. The investment ratio ($i_{j,t}/k_{j,t-1}$) is on average 8 percent with a standard deviation of 10.8. The median number of employees in the sample is 800, and the average number of employees is 7,600. The average leverage ratio is 32.2 percent, and the average liquidity rate is 17.3 percent. The right-skewed size distribution of firms motivates the use of log variables in regressions.

Next, I provide a comparison of different firm characteristics across the size distribution, which allows me to assess the correlations across cross-sectional characteristics. Figure A3.1 in Appendix 3.7 presents leverage, liquidity, investment ratio, dividend, and asset growth profiles of firms across different deciles of size. Size deciles are generated using the number of employees firms have. Figure

[7]Liquidity ratio is calculated as the share of cash and short term investments (*cheq*) to total assets.

[8]Total debt is calculated as the sum of debt in current liabilities (*dlcq*) and long term debt (*dlttq*). I provide further details on sample selection and data construction in Appendix 3.7.

[9]The average firm is observed for 52 quarters, e.g. 13 years.

[10]Since private firms often have harder financial constraints, the estimates incorporating financial proxies in Compustat should be taken as a lower bound.

A3.1 suggests that firms in the first decile of employment distribution (e.g. very small firms) tend to hold the highest leverage level in the sample. However, a comparison of medium-small, medium and large sizes reveals that leverage also tends to increase positively with size. In terms of liquidity, small firms seem to hold the largest liquidity in their portfolios, with rates as high as 33 percent. The cash holdings of the firms decrease from 33 to 8 percent gradually as the firms grow. Similarly, small firms also seem to show higher investment rates than large firms. Finally, the dividend payouts increase only for firms in the highest deciles, and the high asset growth seems present mainly for very small firms, e.g. firms at the first decile of size distribution. These correlations reveal underlying patterns in firm characteristics in the Compustat and are consistent with the statistics in Cloyne et al. (2019). I will revisit these correlations in Section 3.3.4 when I explore the role of alternative firm-characteristics in the context of financial constraints.

Monetary Policy Shocks

A common challenge in the identification of monetary policy surprises is the concern of endogeneity. Since interest rate movements can both react to prevailing economic conditions and influence them, it is challenging to isolate causes and effects of monetary policy innovations.[11]

In this paper, I identify the exogenous movements in monetary policy by using the external instrument-VAR approach of Gertler and Karadi (2015) that is developed by Stock and Watson (2012) and Mertens and Ravn (2013). Gertler and Karadi (2015) uses high-frequency surprises on interest rate futures around FOMC meetings as external instruments in VARs to identify the effects of monetary policy shocks. Building on Gurkaynak et al. (2005), high-frequency surprises capture changes in fed funds futures that are measured within a thirty-minute window around the FOMC decisions. The tight window deals with the endogeneity problem and helps identify monetary surprises that are due to purely exogenous policy shifts.[12] Note that the main identifying assumption of Gertler and Karadi (2015) is that they measure the surprises in the futures rate within the 30-minute window of FOMC meeting. Hence, any surprise movements in fed funds

[11] See Nakamura and Steinsson (2018) for a review of the literature on the causal estimation of monetary policy.

[12] As discussed in Gertler and Karadi (2015), within a period, policy shifts may not only affect financial variables, they may also be responding to them. By using daily data of surprise movements in fed funds future around a tight window, HFI approach addresses the simultaneity issue.

futures during this time period are contemporaneously exogenous to within period movements in both economic and financial variables (Gertler and Karadi (2015), p.46). This leads to consistent estimates of monetary innovations.

Similar to Gertler and Karadi (2015), I first estimate a monthly VAR using one-year government bond rate, log industrial production, log consumer price index, Gilchrist and Zakrajšek (2012) excess bond premium, employment rate, and debt to GDP.[13] The reduced form of the proxy VAR is given by

$$\mathbf{Y_t} = \sum_{j=1}^{J} \mathbf{B_j} \mathbf{Y_{t-j}} + \mathbf{u_t} \tag{3.1}$$

where $u_t = \mathbf{S}\epsilon_t$ is the reduced form shock and $\mathbf{S}$ is the structural impact matrix that maps latent structural shocks into reduced form shocks. Data on fed funds futures is available since 1991 and the VAR data spans 1979m6 to 2016m6. An advantage of proxy-VAR approach is that VAR can be estimated over a much longer time span than the instrument available (Cloyne et al. (2019), p.14). Following Cloyne et al. (2019), I extract the latent monetary policy shocks from the implied residuals of their SVAR-IV by inverting the structural VAR impact matrix.[14] This yields a time series of monetary policy innovations from 1980m6 to 2016m6. I aggregate these innovations from monthly to quarterly frequency by summing. Figure A3.2 plots the implied monetary policy structural shocks that are employed in the empirical section.

3.3 Empirical Framework

This section provides the empirical framework used to explore the asymmetric effects of monetary policy in micro-data. First, I study the sensitivity of three sets of variables: number of employees, the investment rate and sales to sign-dependent effects of monetary policy. Second, I provide robustness checks of the baseline results using a variety of different specifications and controls. Last, I explore whether a financial frictions channel propagates the sign-dependent effects of monetary

[13]The VAR is estimated using 12 lags. Similar to Gertler and Karadi (2015), I use shocks to instrument changes in one-year treasury rate. This is advantageous as movements in one-year rate not only incorporate surprises in the current funds rate but also shifts in expectations about the future path of the funds rate through forward guidance.

[14]Using $u_t = \mathbf{S}\epsilon_t$, we can write $E(u_t u_t') = E(\mathbf{S}\epsilon_t\epsilon_t'\mathbf{S}')$ where $E(u_t u_t') \equiv \Sigma$. Next, $\Sigma = E(\mathbf{SS}')$ requires $\mathbf{S}$ to be identified as the Cholesky factor of Σ. Since $u_t = \mathbf{S}\epsilon_t$, $\mathbf{S}^{-1}u_t = \epsilon_t$ would yield the latent shocks.

policy. In order to do this, I examine the role of alternative financial proxies proposed in the literature. My results provide evidence that financial frictions have a role in propagating asymmetric effects of monetary policy.

3.3.1 Empirical Specification

I estimate the asymmetric effects of monetary policy shocks in the spirit of Tenreyro and Thwaites (2016) using the following local projection (Jordà, 2005) instrumental variable (LP-IV) specification,

$$y_{j,t+h} - y_{j,t-1} = \alpha_j^h + \beta_h^+ \, max[0, \Delta R_t] + \beta_h^- \, min[0, \Delta R_t] + \Omega'(L) \, Z_{j,t-1} + \epsilon_{\, j,t+h} \tag{3.2}$$

where horizon is h = 0, 1,..., H. α_j^h is firm-level fixed effects, and the ΔR is the change in one-year government bond yield instrumented with the monetary policy shocks following Gertler and Karadi (2015). R_t^+ and R_t^- respectively stand for contractionary and expansionary monetary policy changes. The monetary policy shocks instrument the increases (or decreases) in the one-year government bond yields depending on the sign of movements in that particular quarter in the one-year treasury rate. That is, I pin down the increases and decreases in the one-year treasury rate for each quarter and instrument them with the monetary innovations that occurred in these quarters. This approach applies the sign-restriction *only* to the movements of the one-year treasury rate, and not to the monetary policy instruments. The reason is that monetary policy shocks reflect deviations from pre-FOMC meeting expectations of financial markets; hence its sign is not necessarily informative about whether the monetary policy is contractionary or expansionary.[15] For this reason, I only use the sign restriction on the one-year treasury rate and use the instrument that occurred *on* these dates. Finally, the policy rate is scaled such that the shock raises the one-year policy rate by 25 basis points on impact.

There are three main dependent variables: number of employees[16], real sales and the investment

[15] I thank Professor Òscar Jordà for this valuable suggestion.

[16] Since employment is reported at an annual frequency in Compustat. I linearly interpolate the within-year movements in the number of employees by firm.

rate.[17] The dependent variable is projected as the cumulative difference to interpret the parameters as impulse responses. β_h^+ captures the effect of a 25 bp increase in interest rate across different horizons. β_h^- captures the impact of a 25 bp decrease in interest rate across different horizons. The estimation is done up to horizon of $H = 20$ quarters and the lag structure for the control variables is 4 quarters.

Firm fixed effects, α_j^h, soak up permanent differences in investment behavior across firms and allows us to explore within-firm variation. $Z_{j,t-1}$ is the vector of control variables that include lags of real asset growth, lags of firm-level employment growth, and lags of log real GDP. Firm-level controls help control for differences in cross-sectional characteristics across firms. The aggregate variables help capture the aggregate outlook of the economy.[18] All the control variables in $Z_{j,t-1}$ are measured at the end of last year before the monetary shocks to ensure exogeneity with respect to the shock. Standard errors are two-way clustered by firm and quarter (calendar) where serial correlation adjustment is set to 4 quarters using Driscoll and Kraay (1998) methodology. This is a standard method to account for serial correlation at the firm level and through time.[19] In order to prevent results driven by outliers, the sample is 1% trimmed at both ends based on the investment ratio, sales growth and debt to equity ratio. I also trim the top 1% of the leverage ratio.[20] Firms which live less than 20 quarters are dropped as the impulse responses are estimated using at least five years of consecutive data. Lastly, since I test the implications of an aggregate shock on micro data, the analysis does not suffer from reverse causality which would imply firm-level variables to affect aggregate shocks.

3.3.2 Baseline Results

This section presents the impulse responses of specification 3.2 on employment, the investment ratio, and sales, respectively. These results pin down *average* sign-dependent effectiveness of monetary policy in the firm-level data and show that monetary contractions are substantially more effective

[17] Investment ratio is defined as the share of capital expenditures (*capx*) to physical capital at the beginning of the period (*ppent*).

[18] Controlling for lags of log GDP is motivated by Tenreyro and Thwaites (2016) which discusses whether positive and negative monetary policy shocks may be correlated with the expansions and recessions in the economy.

[19] See Cloyne et al. (2019), Bahaj et al. (2020).

[20] See Appendix 3.7 for the sample selection procedure.

than monetary expansions.

Figure 3.1 plots the impulse response of the number of employees to a monetary expansion (β_h^-) and monetary contraction (β_h^+), respectively. The shaded area displays 90 percent confidence intervals, and the impulse response functions are scaled so that shock results in a 25 basis points change in one-year treasure rate. The first column of Figure 3.1 (Figure 3.1a) shows that firms lower the number of employees gradually for about two years after a contractionary monetary policy shock. The peak effect is -1.8 percent and it occurs about ten quarters after the shock. Employment starts to recover about three years after the shock. Overall, the effect is significant from quarter four till quarter 11. In contrast, Figure 3.1b shows that a monetary expansion increases employment by a maximum of 0.5 percent. The effect is only significant at quarter 5. The effects on firm-level employment are visibly smaller and less pronounced for the monetary expansion as compared to the monetary contraction.

Next, monetary tightenings and expansions may not only alter firms' scale of production, but also impact the investment rate. Figure 3.2 shows that the empirical model delivers a similar picture on firms' investment rates. Following a 25 basis points monetary contraction, the investment ratio falls by 0.05 percentage points on impact, with the -0.5 percentage points peak effect occurring 11 quarter after the shock. The response of investment starts to drop after quarter three and becomes significant at quarter 11. The effect dies off about four years after the monetary shock. On the other hand, monetary expansions have much weaker effects that are not statistically significant across the horizon. Figure 3.2b shows that firms respond to a 25 basis point monetary expansion by increasing the investment ratio by a maximum of 0.1 percentage points. Overall, Figure 3.2 suggests that monetary tightenings are more effective on investment than monetary expansions.

Next, I present the effects of monetary expansions and contractions on firms' sales. Figure 3.3 shows that firms experience a decline in sales for about two years following a contractionary monetary policy shock. The peak effect is -1.8 percent, and it occurs about ten quarters after the shock. The effect is significant from quarter six till quarter 14. Sales start to recover about three years after the shock. In contrast, a monetary expansion increases sales by about 1 percent at maximum. The effect is significant only at quarters 5 and 7. In line with the earlier results, the impact on sales is smaller and less significant for monetary expansions than monetary contractions (Figure

3.3a).

Lastly, I present a formal asymmetry test for the hypothesis that positive and negative monetary policy shocks have alternative effects on firm-level variables. I test the following hypothesis:

$$H_0 : \beta^+ = \beta^-$$

$$H_1 : \beta^+ \neq \beta^-$$

using horizon 10 estimates as they are the half-life of the dynamic estimation window. The test results confirm that monetary contractions and expansions have significantly different effects on firms' employment and sales at a 10 percent significance level. The asymmetric effects on investment is found to be statistically insignificant.[21]

This section showed that firms' employment, investment, and sales respond substantially more to monetary contractions than monetary expansions. Overall, these dynamics line up with the earlier literature findings that analyze this type of non-linearities using aggregate data.[22] Both aggregate and micro-data estimates suggest that monetary tightenings have a larger impact on the economy than monetary expansions. In the next sections, my goal is to (i) confirm the robustness of these results across a range of specifications and (ii) disaggregate this average effect to see whether firms' financial characteristics propagate the observed non-linearity. This exercise will help disentangle the role of financial frictions within the asymmetric effects of monetary policy.

3.3.3 Robustness

In this section, I show that the main results capturing non-linearity of monetary policy on firm-level employment, investment, and sales are robust to a range of alternative specifications. In particular, I confirm that the main findings are robust to (i) controlling for additional firm characteristics, (ii) having a more restrictive sample, (iii) using the original Gertler and Karadi (2015) time-span of monetary policy shocks, and (iv) sub-sample instability across sectors.

[21]The p-value of the asymmetry test on employment and sales is 0.06. The p-value on investment results is 0.5.

[22]See Tenreyro and Thwaites (2016); Cover (1992); Angrist et al. (2018) among others.

First, the main findings of this paper use control variables: lags of real asset growth, firm-level employment growth, and log real GDP. In the following specification, I extend the control set with firms' leverage ratio, real sales growth and log real assets. These variables would capture even more cross-sectional variation in the estimates. Figure 3.4 shows the results that mirror the baseline findings: adding new control variables to the specifications does not change the non-linearity results. Second, I also add a specification where I control for the lags of the independent variable in addition to baseline controls. Figure 3.5 plots the impulse responses. The results are very similar to baseline findings and are slightly more significant than the baseline.

Next, I trim the sample with a more restrictive criteria and re-test the baseline specification. In addition to the baseline trim (See Appendix 3.7 for details), I trim the sample based on firms' sales growth and leverage ratio.[23] Figure 3.6 shows that the results look remarkably similar to baseline impulse response functions in Section 3.3.2. Both specifications support the main findings that monetary policy has substantially non-linear impacts on firm-level employment, investment and sales.

I also check the robustness of the sample to monetary policy shocks. The original financial market surprises used in Gertler and Karadi (2015) spans 1991 to 2012. In the baseline specification, I feed the VAR with data from 1979 to 2016, which allows me to also extract monetary policy shocks from 1980 to 2016. In the following specification, I test the alternative set of monetary policy shocks using VAR data from 1979 to 2012.[24] Figure 3.7 displays the baseline findings with new monetary policy shocks. The results corroborate the baseline findings that the effectiveness of monetary policy is significantly non-linear across all three variables.

Finally, I consider whether my estimates are driven by a particular sector in the sample. In order to do this, I focus on the two largest sectors in my sample: manufacturing and services. Manufacturing consists of 48 percent of the sample, and services consist of 18 percent. I also form a third group consisting of companies belonging to all remaining sectors.[25] The first and

[23]In particular, I trim top and bottom 1 percent of leverage and sales growth. All the trimming is done by year.

[24]The 6 by 6 VAR still uses the same variables: one-year government bond rate, log industrial production, log consumer price index, Gilchrist and Zakrajšek (2012) excess bond premium, employment rate and debt to GDP with data from 1979 to 2012.

[25]The largest sectors in the final group consist of construction (7 percent), transportation and communications services (13 percent), and wholesale trade (11 percent).

second row of Figure 3.8 show the dynamic effects of sign-dependent monetary policy on firm-level employment for manufacturing and service firms, respectively. The last row shows the effects on the rest of the industries. In all three figures, the baseline results of asymmetry are visible across all major sectors.

3.3.4 Heterogeneity Analysis

This section explores the role of financial frictions in the context of monetary asymmetries. Assessing the role of financial frictions requires identifying firms that are "financially constrained". Since financial constraints are not directly observable, empirical macroeconomics literature has used indirect measures as proxies of financial constraints.[26] This section will study a group of these proxies and explore whether some of these observables predict a larger sensitivity to sign-dependent effects of monetary policy.

First, earlier contributions in Bernanke et al. (1996), Oliner and Rudebusch (1996) and Gertler and Gilchrist (1994) propose firm size as a proxy for access to credit markets. According to financial accelerator literature, the effects of changes in the financial conditions of firms that are close to the margin would be much larger than the firms that are above the standard requirements, e.g. less constrained. Figure 3.9 and 3.10 show the dynamic responses of firm-level employment to monetary policy for the large and small firms, respectively. The small versus large binning is done using the median number of employees by year. Comparing full dynamic responses of Figure 3.9a and Figure 3.10a suggests that small firms reduce their employment capacity at about 2 percent three years after a monetary contraction. In contrast, large firms contract only by 1.2 percent. The effect is significant for both types of firms for about two years. These results partially corroborate the financial accelerator view: small firms show greater downturns in the aftermath of monetary contractions than large firms. However, the results comparing monetary expansions is less clear. Both large (Figure 3.9b) and small firms (Figure 3.10b) respond to monetary expansions with similar magnitudes. I also re-estimate the baseline specification on investment ratio across different size bins. Figure 3.11 and 3.12 show the impulse responses of firms' investment ratio to monetary policy shocks across large and small firms, respectively. The results mirror the employment findings:

[26] See Cloyne et al. (2019) that provides a thorough assessment of these alternative proxies in the literature.

small firms reduce investment rates more than large firms after monetary contractions. In contrast, the sensitivity to monetary expansions does not differ significantly by firm size.

Following Fazzari et al. (1988), Farre-Mensa and Ljungqvist (2016) and Cloyne et al. (2019), I also test the role of firms' dividend status in the baseline specification. In order to test this in the context of monetary asymmetry, I separate the sample of firms that are not dividend payers and firms that are positive dividend-payers. Figure 3.13 and 3.14 show the impulse responses of employment based on dividend groups. For the monetary contractions, I find firms that are not dividend payers reduce their employment by 4 percent (peak effect), yet firms that are dividend payers to reduce only by 2 percent. Hence, firms that are not paying dividends show a larger response to a monetary contraction. In contrast, I compare the responses to monetary expansions and find no significant difference based on the dividend status of firms. Similar to employment, the investment results also point to a similar type of asymmetry. Figure 3.15 and 3.16 show the impulse responses of the investment ratio based on dividend groups. The results suggest that firms that are not dividend payers have a slightly larger downturn following monetary contractions. In contrast, dividend status does not predict a clear-cut heterogeneity for the responses to monetary expansions. These results suggest that much of the earlier evidence on heterogeneity aligns more with monetary contractions than monetary expansions. That is, to the extent that dividend status reflects firms' financial constraints, it seems to only amplify the monetary contractions, and not the expansions.

Next, I consider whether heterogeneity in leverage can drive the baseline results. The evidence on whether leverage is an indicator of financial constraints is quite mixed. On the one hand, Ottonello and Winberry (2020) shows that firms with high leverage are associated with high default risk, and they respond the *least* to monetary policy due to high marginal costs of external finance. On the other hand, high leverage can be a proxy of a firm's financial contraints which would have opposite predictions in the financial accelerator framework. For example, Ippolito et al. (2017) finds that firms that use more bank debt and do not hedge it display a stronger sensitivity of their stock price, cash holdings, sales, inventory and fixed capital investment to monetary policy. In addition, Cloyne et al. (2019) shows that heterogeneity based on leverage, liquidity and size tend to suffer from endogeneity and selection issues. Figure 3.17 and Figure 3.18 re-estimate the baseline results using leverage binning of firms. In this categorization, firms are separated into high vs low leverage groups using median cutoff by year. Figure 3.18a shows that low leverage firms lower their employment by 2.2 percent three years following a contractionary monetary policy. The effect is significant at years 2 and 3. In contrast, the response of high leverage firms to a monetary contraction is about a 0.8 percent cut in employment, which is much smaller than low leverage groups. For monetary expansions, low leverage firms (Figure 3.18b) also show a slightly larger response than the high leverage firms (Figure 3.17b). These results show that employment of low leverage firms shows a larger swing in *both* contractions and expansions than high leverage firms. I also analyze these results on the firm-level investment. Similar to Ottonello and Winberry (2020) and Cloyne et al. (2019), I find low leverage firms to cut their investment ratio about twice more than the high leverage firms in times of monetary contractions. Unlike the employment results, the investment responses to monetary expansions do not reveal any heterogeneity in the leverage conditions of firms.

Finally, I test the liquidity conditions of firms in the context of monetary asymmetry.[27] This specification tests the role of firms' cash holdings on firm-level employment and investment, allowing for monetary non-linearities. Figure 3.21 and Figure 3.22 re-estimate the baseline results on employment using liquidity binning of firms. The findings on employment provide evidence that firms with high liquidity conditions show a much larger sensitivity to both monetary contractions and expansions. The results on the investment ratio also suggest that high liquidity firms cut back investment more in response to a monetary contraction. As with leverage, the cross-sectional effects of liquidity conditions on monetary expansions are mildly present in employment but not in investment.

So far, my results document that four main factors - having a small size, holding low leverage, holding high liquidity, and not paying dividends - contribute firm-level employment and investment to respond more to monetary tightenings. Among these variables, being small and not paying dividends are coherent with a financial accelerator type mechanism. That is, small firms and firms not paying dividend are likely to be more financially constrained as they are more likely to face

[27]In this literature, Kashyap et al. (1994) shows that firms with low liquid asset holdings contract their inventories more following a contractionary monetary policy. Jeenas (2019) shows that low cash holdings predict a stronger contraction of capital and argues that the firms with the least internal wealth are the ones with high marginal productivity of capital and most to gain from raising debt.

external finance premium.[28] However, the characteristics of holding low leverage and high liquidity seem a bit counterintuitive to a financial accelerator type mechanism at the first glance. In order to reconcile these findings, I follow Cloyne et al. (2019) and analyze simple correlations among these main firm-level observables to help see the stylized connections between these variables within the Compustat sample. According to Figure A3.1, the findings on the heterogeneous effects in this paper are consistent with correlations across observables. Figure A3.1 shows that small firms tend to hold high liquidity and pay no dividend. For the leverage, Figure A3.1 also shows that small firms generally have less leverage except for the first decile of firms that seem to hold an outlier amount of debt in their portfolios. These correlations imply that the heterogeneity findings of this paper seem coherent with the underlying links between these variables. In addition, small firms tend to pay no dividends, be less leveraged and have high liquidity in their portfolios. Bates et al. (2009)

Can these findings be reconciled with financial frictions? Yes, in fact, these findings are consistent with earlier evidence on financial frictions in the literature. In particular, Cloyne et al. (2019) suggests that leverage and liquidity conditions are weak proxies to identify which firms are relatively more exposed to a financial accelerator mechanism and may suffer from endogeneity issues. They also show that on average, younger firms are smaller, hold more cash and low leverage and have a lower probability of paying dividends, which are consistent with the findings of this paper. In addition, Bates et al. (2009) shows that firms tend to have precautionary motives and accumulate sizable cash holdings to hedge against any future risk. Relatedly, Lian and Ma (2021) and Begenau and Salomao (2019) show that many older and larger firms have high leverage ratio because they have good access to capital markets. All these findings suggest that leverage and liquidity characteristics may not be the best proxies to pin down firms' financial constraints as is. Overall, the findings of this paper corroborate the financial accelerator view that small firms and firms not paying dividends may be subject to higher information frictions and risk premiums, resulting in a much larger deterioration in access to credit after a monetary contraction. Hence, as monetary tightening makes credit constraints bind, contractionary monetary policy may result in larger effects in this particular group of firms.

[28]See Cloyne et al. (2019).

As a next step, I follow Cloyne et al. (2019) and analyze different cross-group bins to pin down if some of these characteristics are of second order for monetary contractions and expansions. In order to do this, I analyze size and leverage dimensions jointly by splitting the sample based on small vs. large and high vs. low leverage firms. Figure A3.3 suggests small and low leverage firms respond most to monetary contractions and expansions. Moreover, controlling for size, Figure A3.3 still accounts for some marginal effect from having low leverage. Both monetary contractions and expansions have a larger effect on small-low leverage firms than small-high leverage firms.

Next, Figure A3.5 analyzes size and liquidity conditions jointly and suggests that small and high liquidity firms respond most to both monetary contraction and expansions. Controlling for size, Figure A3.5 still shows that liquidity conditions are still relevant for amplifying the responses. Hence, these two observables jointly account for the amplified effects of monetary contraction and expansions.

Similarly, Figure A3.7 analyzes leverage and liquidity conditions jointly and suggests that low leverage and high liquidity firms have the largest response to monetary policy. Finally, Figure A3.9 compares size and dividend conditions jointly and suggests that the dividend status is more relevant for amplifying large firms' response than the small firms.

Note that these last specifications use a less-parametric approach by dividing the data into groups. The advantage of this approach is that it doesn't impose any restrictions on the underlying distribution of data. Next, I test these predictions using a more parametric approach. Figure A3.11 shows the joint estimation results using a local projection given in equation A3.1. In this estimation, I test the marginal effect of firms' size, leverage, and liquidity conditions in altering the sign-dependent effectiveness of monetary shocks. The variables on size, leverage, and liquidity are normalized by standard deviations around the mean, e.g., the impulse responses should be interpreted as the marginal effect of having one standard deviation *higher* size, leverage or liquidity ratio. Figure A3.11 suggests that the larger is the firm size, the more dampened is the effectiveness of monetary contractions and expansions. Similarly, I find high leverage also to dampen the effectiveness of both types of monetary shocks. These results are also confirmed for the investment responses (See Figure A3.12). The only result that is different from less parametric specification is the liquidity results.[29] This change in liquidity results stems from the differences in the underlying assumptions of the two models. The parametric approach makes assumptions about the parameters of the population distribution, whereas the semi-parametric approach allows for a less-restrictive estimation.[30] Overall, these results also show that when controlling for all three characteristics, size, leverage and liquidity all seem significant in amplifying employment responses. For investment, the parametric results suggest liquidity and size seem to be slightly more effective than leverage.

Finally, I do one final robustness check on liquidity to clarify the difference in the findings with Jeenas (2019). First of all, my results use investment ratio (i/k) as the independent variable, whereas Jeenas (2019) uses capital stock ($log\ k$) as the independent variable. To align the heterogeneity findings, I re-estimate equation A3.1 using Jeenas (2019) specification and find the exact same results that: low liquidity firms have the largest contraction in the stock of *capital* following a monetary contraction.(See Figure A3.13) This suggests that the differences in the liquidity findings are due to different outcome variables of interest.

To sum up, this section provides three main results. First, the baseline findings on monetary asymmetry are present across alternative sub-samples and are robust to alternative estimation strategies, control sets and underlying assumptions. In all specifications, I show that conditioning the results on various observables do not change the average asymmetry results. Second, I trace alternative proxies on financial frictions and find a considerable degree of heterogeneity in firms' responses to sign-dependent monetary innovations. In particular, I find that characteristics such as having a small size, holding low leverage, holding high liquidity and not paying dividends, contribute firms to be more sensitive to monetary tightenings on both employment and investment. I also find that monetary expansions are also amplified by low leverage and high liquidity characteristics, although the magnitudes are much dampened compared to contractions. Utilizing the recent evidence in Cloyne et al. (2019), I argue that these results highlight a financial frictions type channel on the asymmetric effects of monetary policy. Overall, these findings point that financial characteristics are important to understand the sensitivity of firms to non-linear effects of monetary policy. Fi-

[29]For employment, I find high liquidity firms to have a weaker response to either types of shocks, which is different than the findings in the less-parametric case. On the other hand, for investment, I find high liquidity firms to have a larger response which is in line with the less-parametric results.

[30]Moreover, the split sample approach requires the controls to be interacted only with the selected group observations, rather than the whole sample. This makes direct comparisons between two models inconsistent.

nally, I document that most of the amplification is seen more clearly in employment results than investment ratio results.

3.4 Conclusion

This paper documents how unanticipated exogenous monetary policy shocks generate strong asymmetric effects using detailed firm-level data. Specifically, I study firm-level employment, investment, and sales responses to monetary policy changes allowing the effects to vary based on the sign of the monetary policy change. My findings document that accounting for the non-linearities in monetary policy may explain sizable variation in monetary effectiveness.

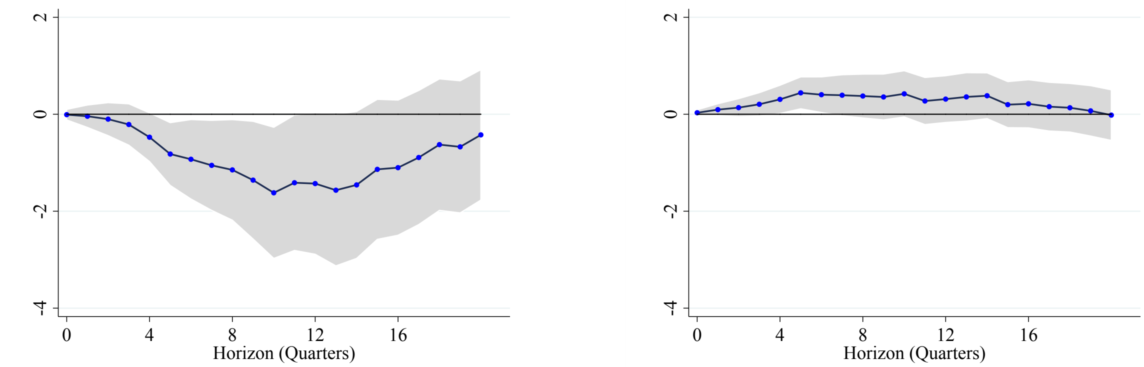
The main results of this paper can be summarized as follows. First, monetary policy shocks generate fairly asymmetric effects on the firms depending on the direction of the monetary action. In particular, contractionary monetary policy generates substantially larger response in firm-level employment, investment and sales than the expansionary monetary policy. These findings are robust to various checks such as controlling for additional firm characteristics, having a more restrictive sample, using an alternative time-span of monetary policy shocks, and sub-sample instability across sectors. Second, I study the heterogeneity in the context of monetary asymmetries and show that firms that are small in size, holding low leverage or high liquidity, or not paying dividends are more sensitive to monetary contractions than other firm groups. The impact of monetary expansions are also amplified for firms with low leverage or high liquidity, but at a much lower rate than monetary contractions. The heterogeneity in firms' responses across contractions and expansions can be reconciled with the financial accelerator literature.

The results of this paper are particularly important for two reasons. First, this study is the first paper evaluating asymmetric effects of monetary policy within micro-data in the US. This is quite different than the earlier approaches adopted in the monetary transmission literature that mainly uses time-series variables to test monetary non-linearities. Second, with the advantage of rich micro-data, this paper explores the role of financial frictions channel in the context of monetary asymmetries. These results highlight the role of firm-level observables on monetary asymmetries which may be an important input for future modeling efforts. Overall, these findings provide a practical rule-of-thumb to the design of stabilization policy and also contribute to our understanding of the scope of monetary policy when there are non-linearities.

3.5 Figures

Main Results

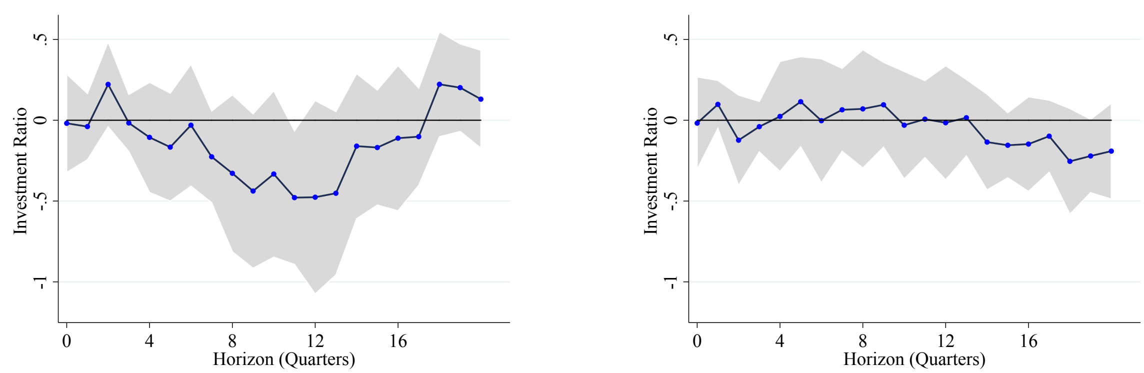
Figure 3.1: Asymmetric effects of monetary policy on log employees.



(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Note: The first (second) column shows the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Control variables are real asset growth, log GDP and growth in employees. The shaded areas show 90 percent confidence intervals.

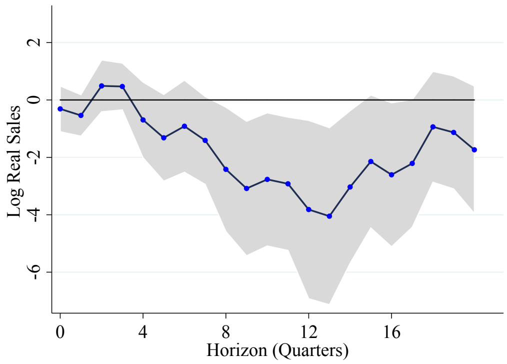
Figure 3.2: Asymmetric effects of monetary policy on investment ratio.



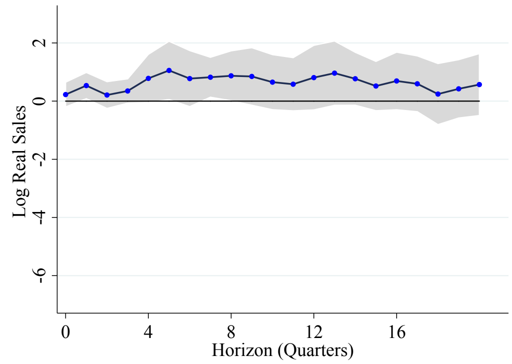
(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Note: The first (second) column shows the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Control variables are real asset growth, log GDP and growth in employees. The shaded areas show 90 percent confidence intervals.

Figure 3.3: Asymmetric effects of monetary policy on log real sales.



(a) Monetary Contraction (β_h^+)

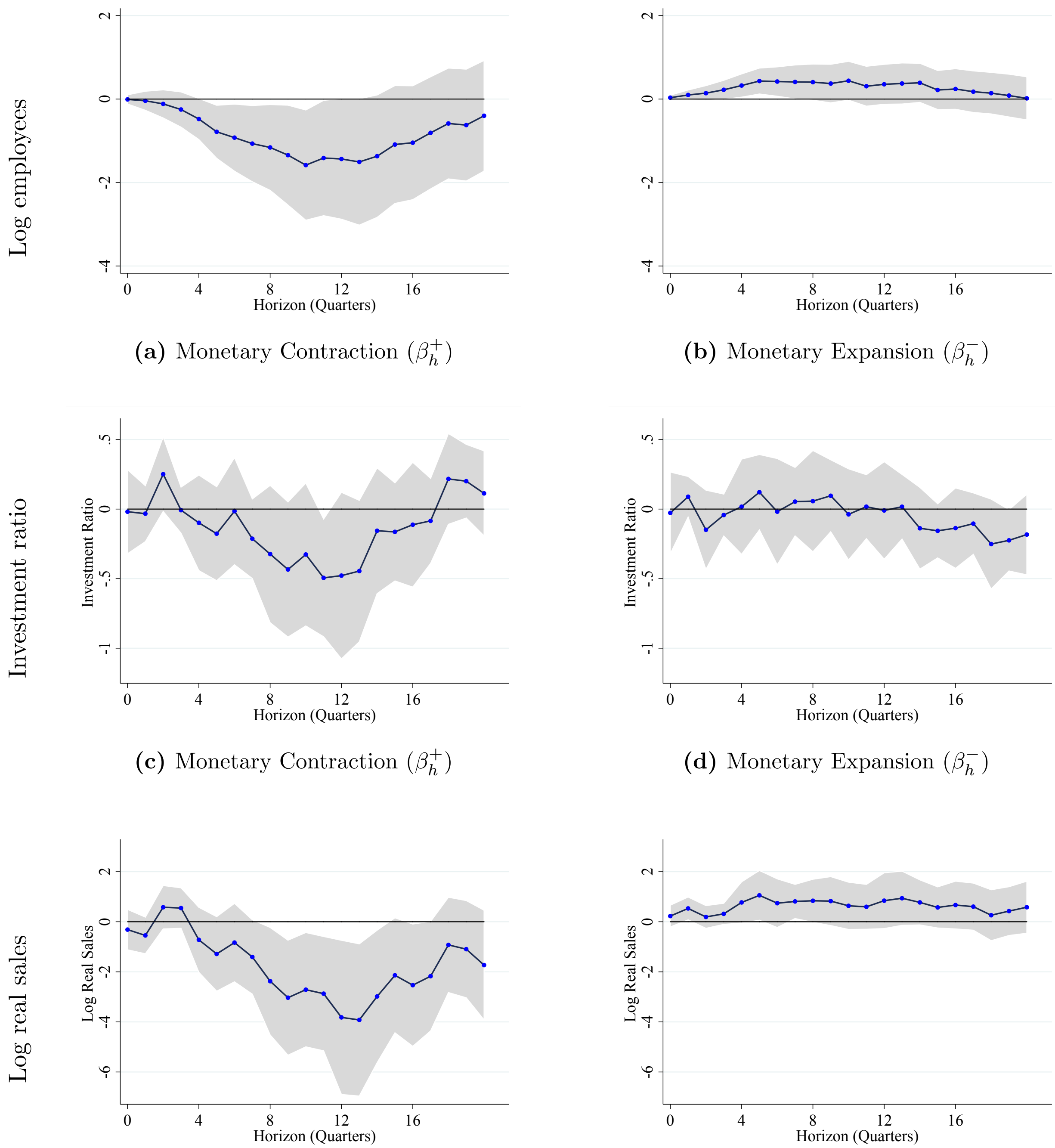


(b) Monetary Expansion (β_h^-)

Note: The first (second) column shows the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Control variables are real asset growth, log GDP and growth in employees. The shaded areas show 90 percent confidence intervals.

Robustness Figures

Figure 3.4: Impulse responses to expansionary and contractionary monetary policy



(a) Monetary Contraction (β_h^+)

(b) Monetary Expansion (β_h^-)

(c) Monetary Contraction (β_h^+)

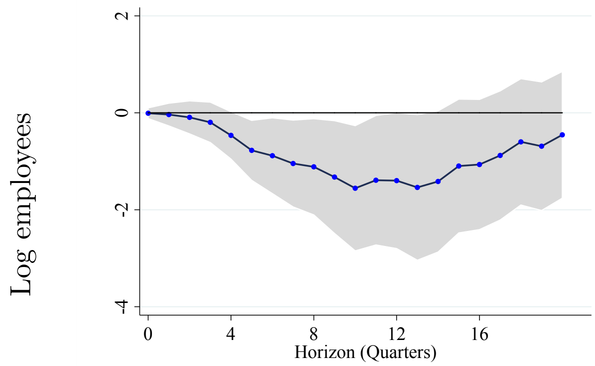
(d) Monetary Expansion (β_h^-)

(e) Monetary Contraction (β_h^+)

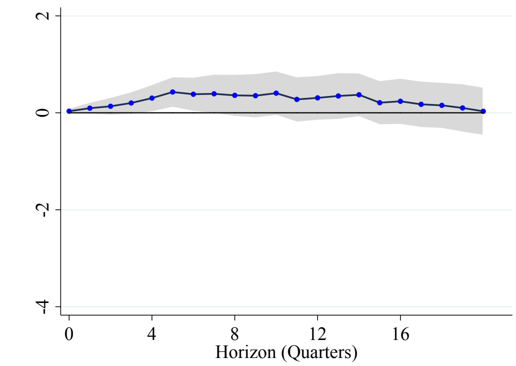
(f) Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Each row is estimated separately. Control variables are real asset growth, log gdp, growth in firm-level employees, firms' leverage rate, real sales growth and log real asset. The shaded areas show 90 percent confidence intervals.

Figure 3.5: Impulse responses to expansionary and contractionary monetary policy

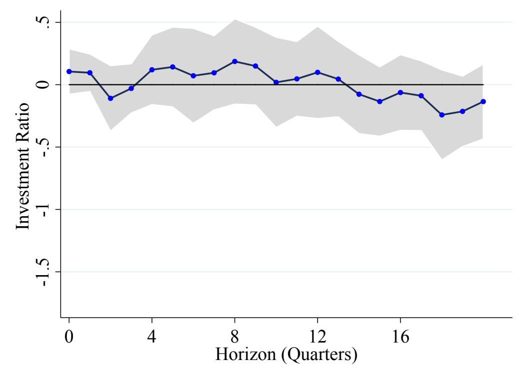


(a) Monetary Contraction (β_h^+)

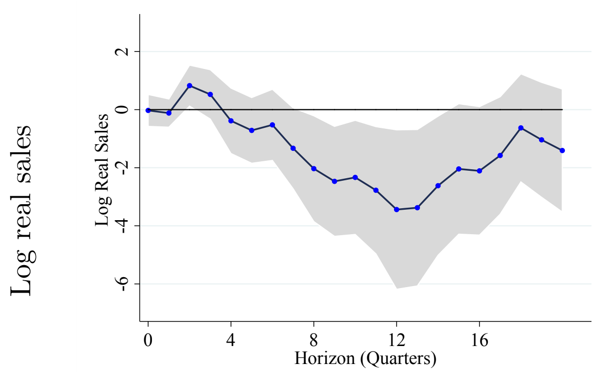


(b) Monetary Expansion (β_h^-)

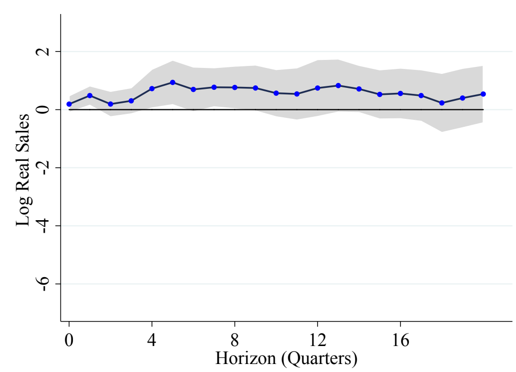
(c) Monetary Contraction (β_h^+)



(d) Monetary Expansion (β_h^-)



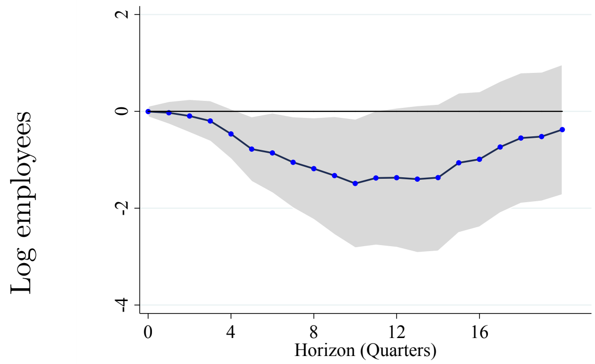
(e) Monetary Contraction (β_h^+)



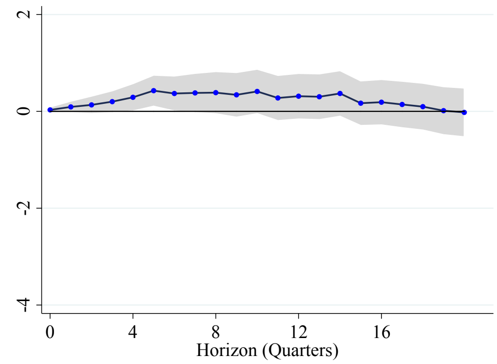
(f) Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Each row is estimated separately. Control variables are real asset growth, log gdp, growth in firm-level employees and lags of dependent variable. The shaded areas show 90 percent confidence intervals.

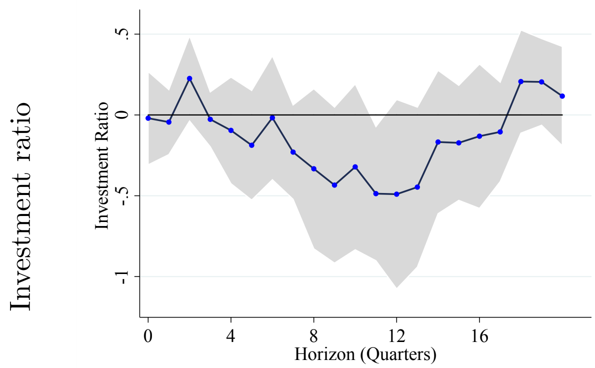
Figure 3.6: Impulse responses to expansionary and contractionary monetary policy



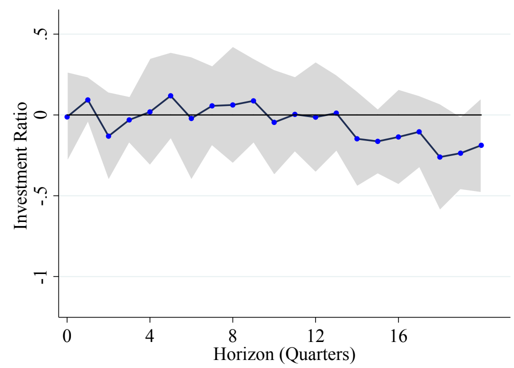
(a) Monetary Contraction (β_h^+)



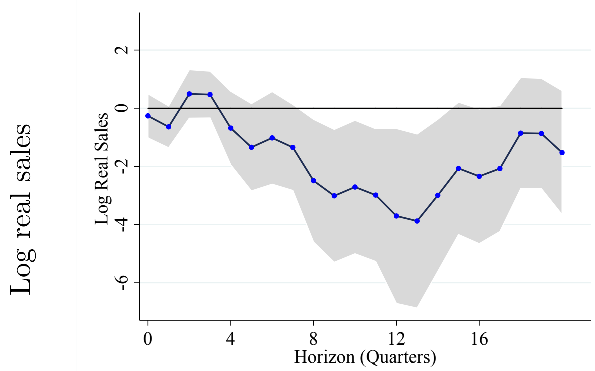
(b) Monetary Expansion (β_h^-)



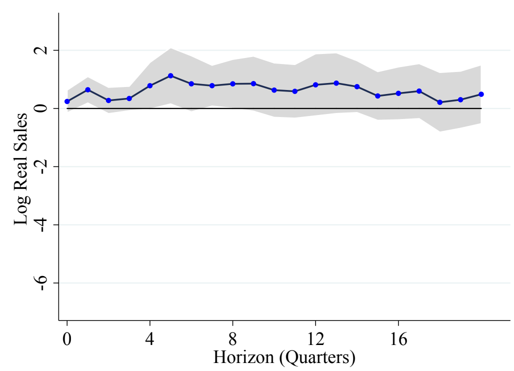
(c) Monetary Contraction (β_h^+)



(d) Monetary Expansion (β_h^-)



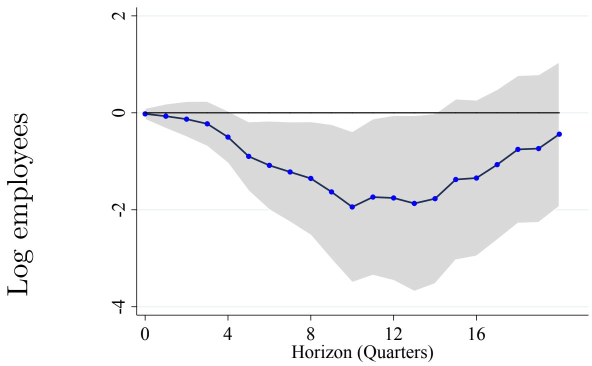
(e) Monetary Contraction (β_h^+)



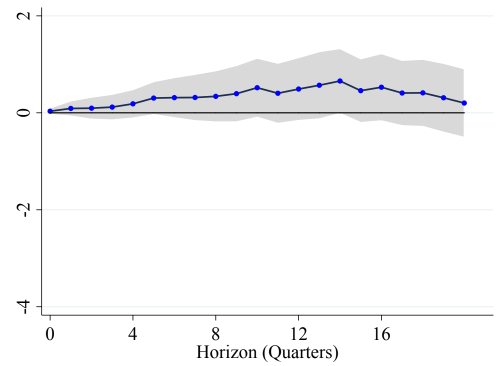
(f) Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Each row is estimated separately. Control variables are real asset growth, log gdp and growth in firm-level employees. The shaded areas show 90 percent confidence intervals. The sample is further trimmed based on top and bottom 1 percent of leverage and sales growth. The trimming is done by year.

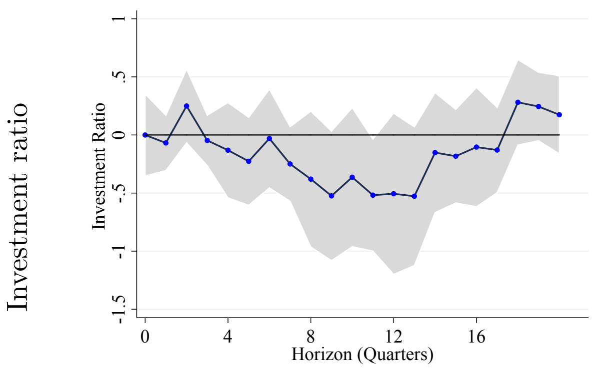
Figure 3.7: Impulse responses to expansionary and contractionary monetary policy



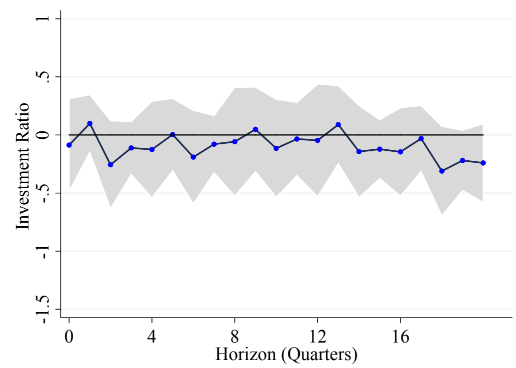
(a) Monetary Contraction (β_h^+)



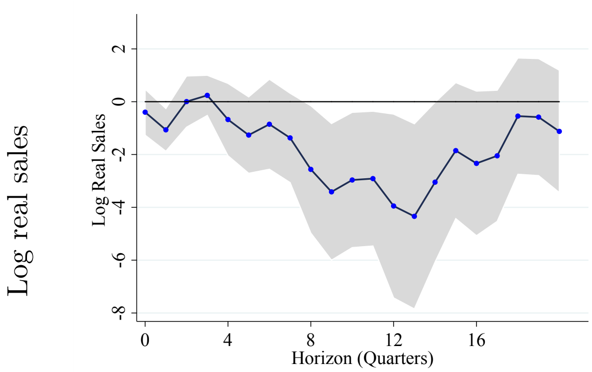
(b) Monetary Expansion (β_h^-)



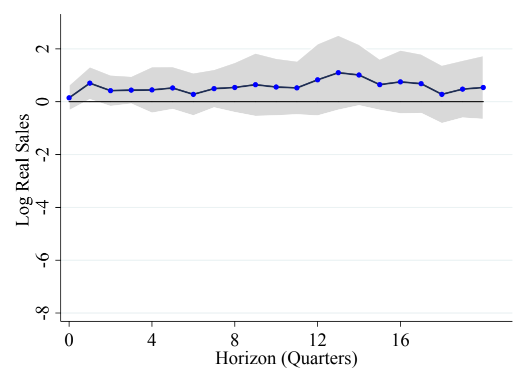
(c) Monetary Contraction (β_h^+)



(d) Monetary Expansion (β_h^-)



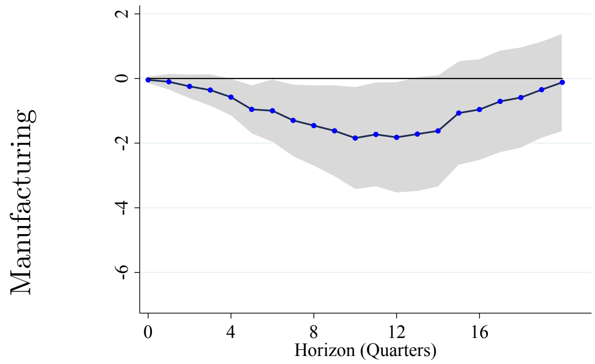
(e) Monetary Contraction (β_h^+)



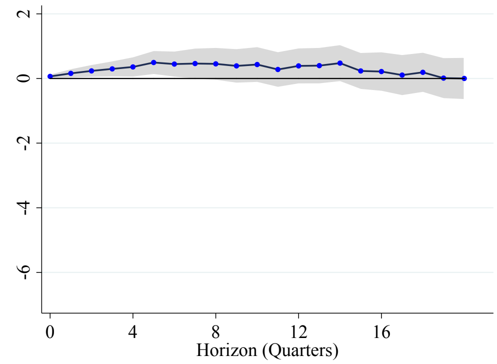
(f) Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Each row is estimated separately. Control variables are real asset growth, log gdp and growth in firm-level employees. The shaded areas show 90 percent confidence intervals. The specification uses original sample period of Gertler and Karadi (2015), e.g. 1980-2012.

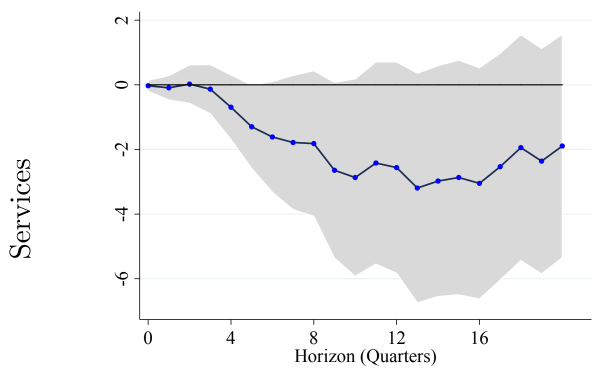
Figure 3.8: Impulse responses to expansionary and contractionary monetary policy on employment



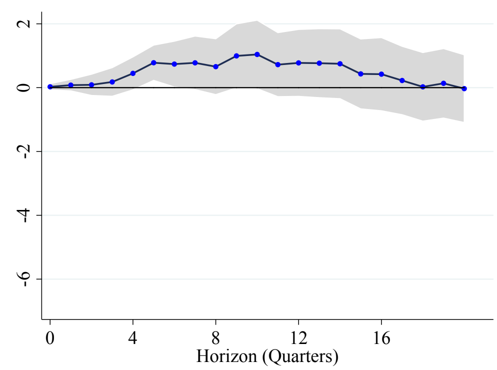
(a) Monetary Contraction (β_h^+)



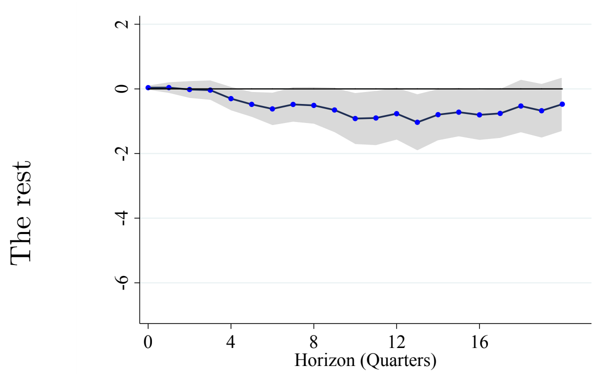
(b) Monetary Expansion (β_h^-)



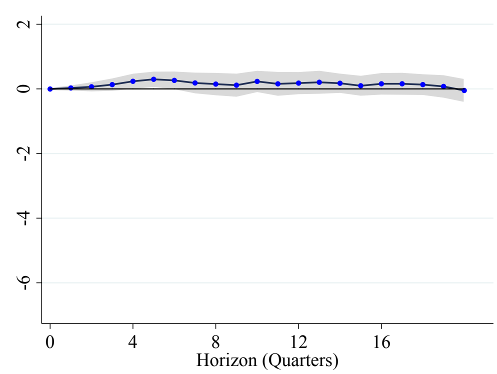
(c) Monetary Contraction (β_h^+)



(d) Monetary Expansion (β_h^-)



(e) Monetary Contraction (β_h^+)



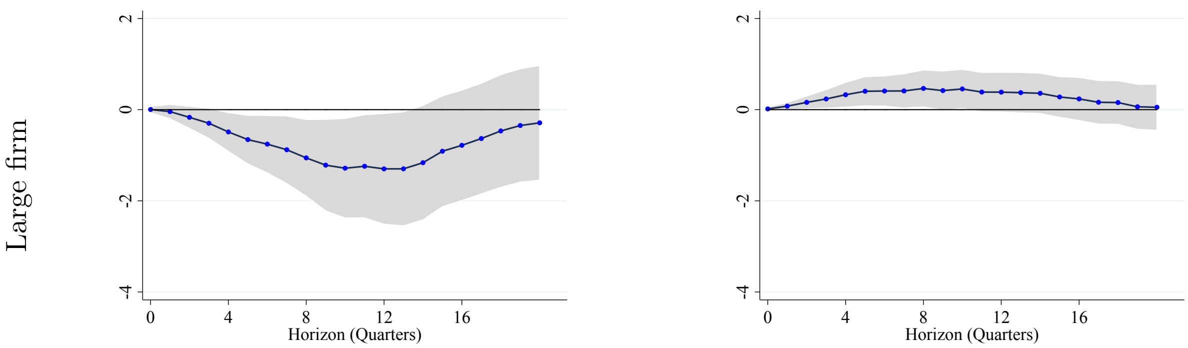
(f) Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Each row is estimated separately for different sectors. Control variables are real asset growth, log gdp and growth in firm-level employees. The shaded areas show 90 percent confidence intervals.

Heterogeneous Effects

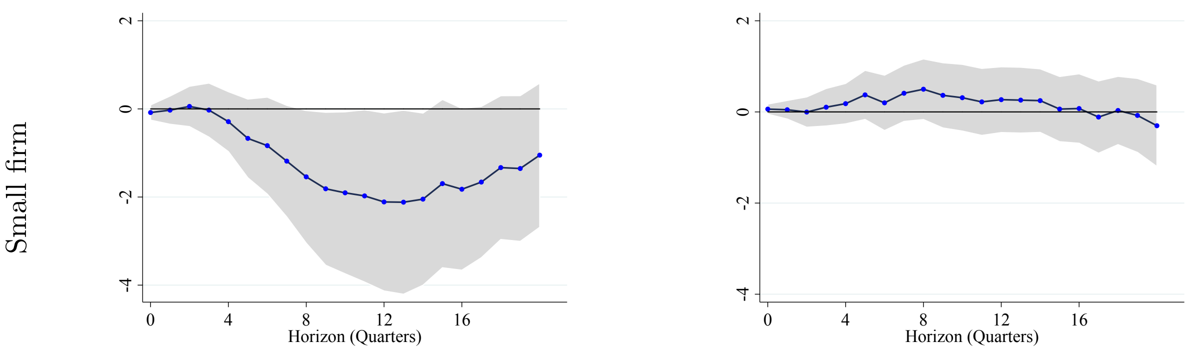
$$y_{j,t+h} - y_{j,t-1} = \alpha_j^h + \beta_h^+ \, max[0, \Delta R_t] + \beta_h^- \, min[0, \Delta R_t] + \Omega'(L) \, Z_{j,t-1} + \epsilon_{\,j,t+h}$$

Figure 3.9: Asymmetric effects of monetary policy on log employees.



(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Figure 3.10: Asymmetric effects of monetary policy on log employees.



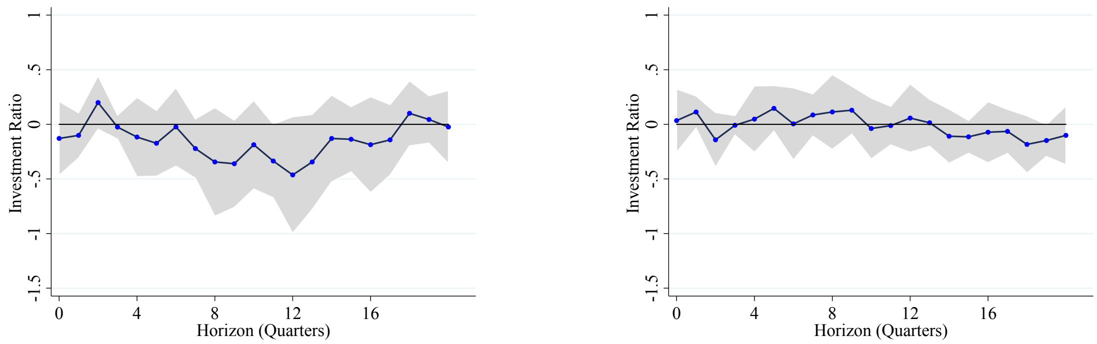
(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Control variables are real asset growth, log gdp and growth in employees. The shaded areas show 90 percent confidence intervals.

$$\frac{i_{j,t+h}}{k_{j,t+h-1}} - \frac{i_{j,t-1}}{k_{j,t-2}} = \alpha_j^h + \beta_h^+ \, max[0, \Delta R_t] + \beta_h^- \, min[0, \Delta R_t] + \Omega'(L) \, Z_{j,t-1} + \epsilon_{\, j,t+h}$$

Figure 3.11: Asymmetric effects of monetary policy on investment ratio.

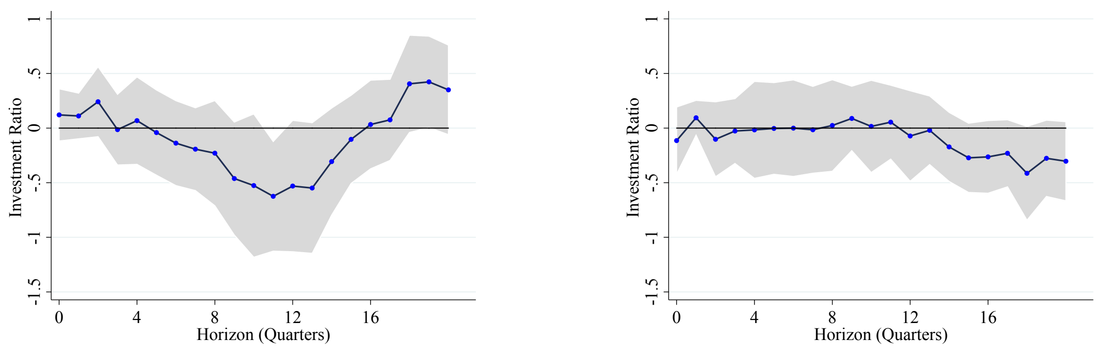
Large firm



(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Figure 3.12: Asymmetric effects of monetary policy on investment ratio.

Small firm

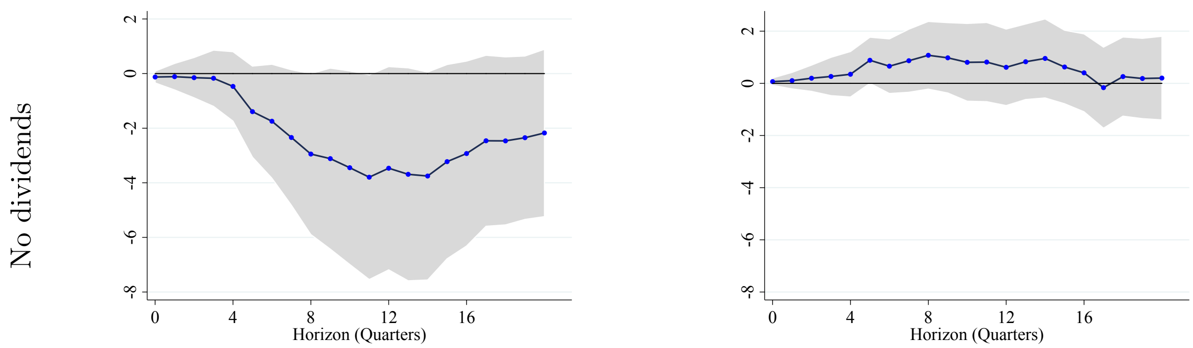


(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Control variables are real asset growth, log gdp and growth in employees. The shaded areas show 90 percent confidence intervals.

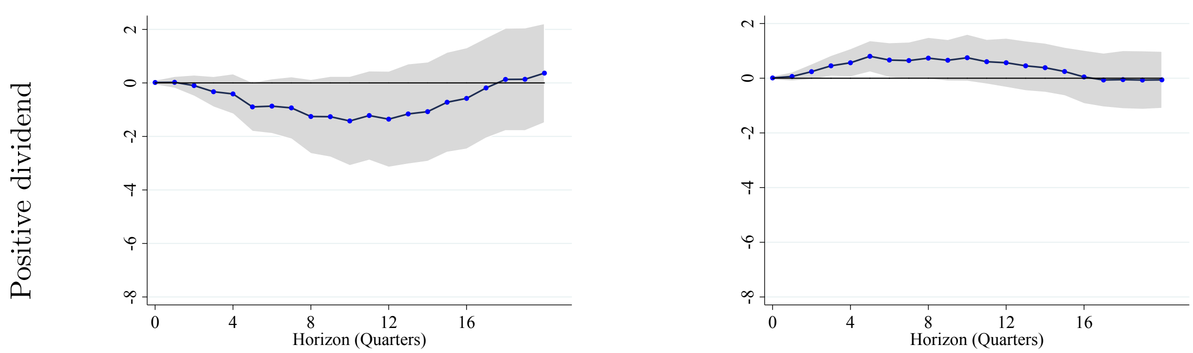
$$y_{j,t+h} - y_{j,t-1} = \alpha_j^h + \beta_h^+ \; max[0, \Delta R_t] + \beta_h^- \; min[0, \Delta R_t] + \Omega'(L) \; Z_{j,t-1} + \epsilon_{\,j,t+h}$$

Figure 3.13: Asymmetric effects of monetary policy on log employees.



(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Figure 3.14: Asymmetric effects of monetary policy on log employees.

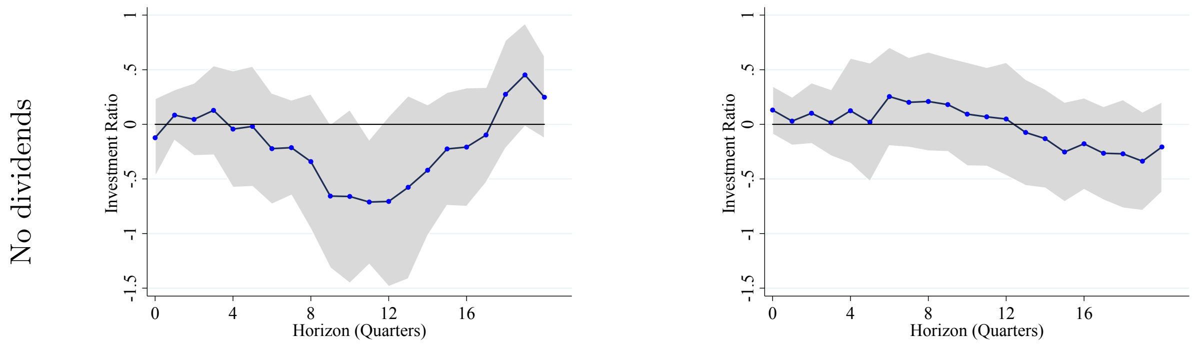


(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Control variables are real asset growth, log gdp and growth in employees. The shaded areas show 90 percent confidence intervals.

$$\frac{i_{j,t+h}}{k_{j,t+h-1}} - \frac{i_{j,t-1}}{k_{j,t-2}} = \alpha_j^h + \beta_h^+ \, max[0, \Delta R_t] + \beta_h^- \, min[0, \Delta R_t] + \Omega'(L) \, Z_{j,t-1} + \epsilon_{\,j,t+h}$$

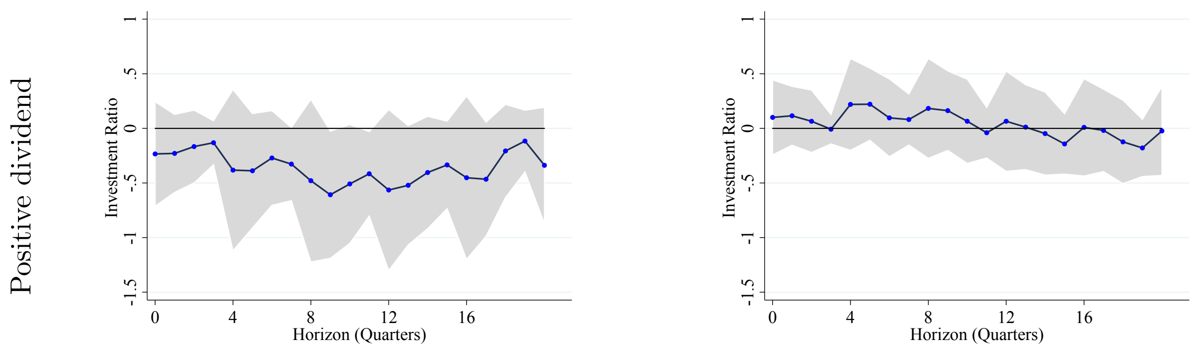
Figure 3.15: Asymmetric effects of monetary policy on investment ratio.



(a) Monetary Contraction (β_h^+)

(b) Monetary Expansion (β_h^-)

Figure 3.16: Asymmetric effects of monetary policy on investment ratio.



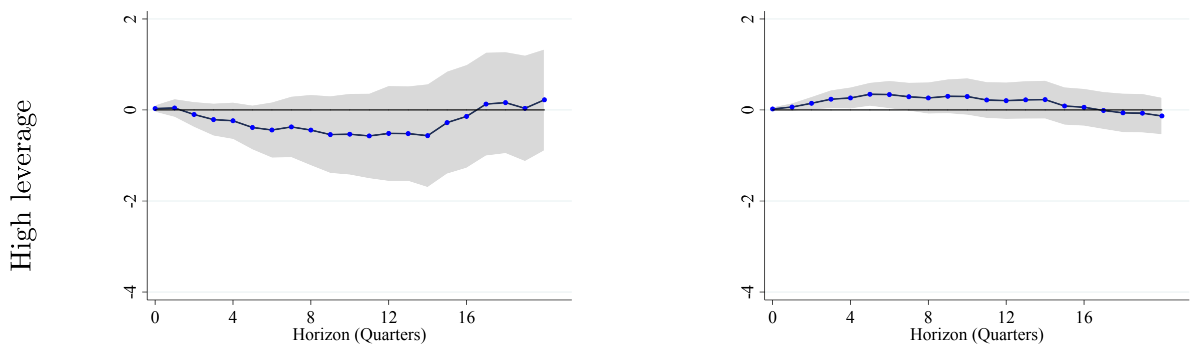
(a) Monetary Contraction (β_h^+)

(b) Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Control variables are real asset growth, log gdp and growth in employees. The shaded areas show 90 percent confidence intervals.

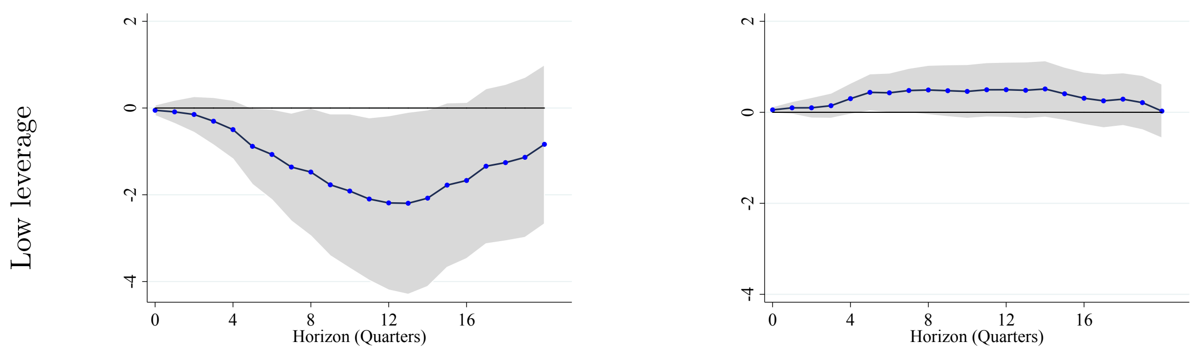
$$y_{j,t+h} - y_{j,t-1} = \alpha_j^h + \beta_h^+ \, max[0, \Delta R_t] + \beta_h^- \, min[0, \Delta R_t] + \Omega'(L)\, Z_{j,t-1} + \epsilon_{\,j,t+h} \tag{3.1}$$

Figure 3.17: Asymmetric effects of monetary policy on log employees.



(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Figure 3.18: Asymmetric effects of monetary policy on log employees.

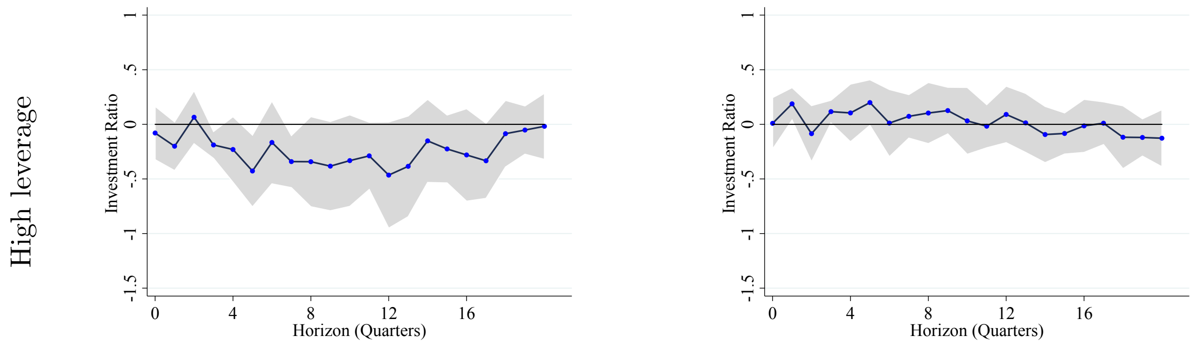


(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Control variables are real asset growth, log gdp and growth in employees. The shaded areas show 90 percent confidence intervals.

$$\frac{i_{j,t+h}}{k_{j,t+h-1}} - \frac{i_{j,t-1}}{k_{j,t-2}} = \alpha_j^h + \beta_h^+ \, max[0, \Delta R_t] + \beta_h^- \, min[0, \Delta R_t] + \Omega'(L) \, Z_{j,t-1} + \epsilon_{\,j,t+h}$$

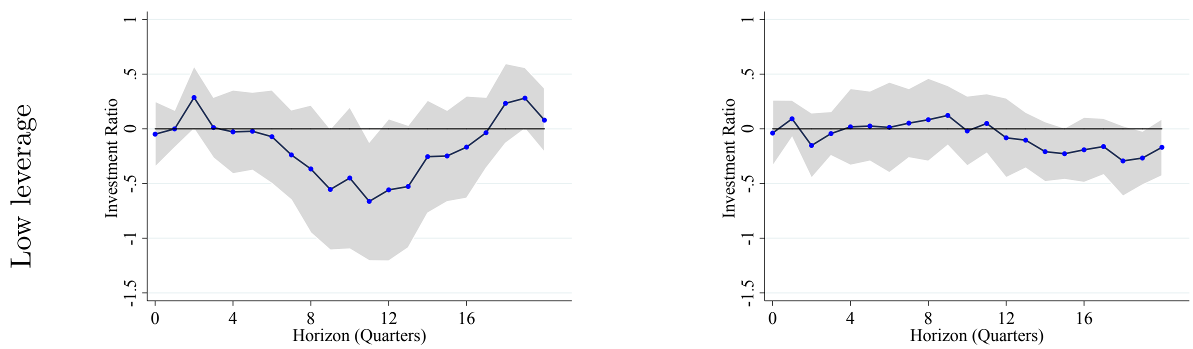
Figure 3.19: Asymmetric effects of monetary policy on investment ratio.



(a) Monetary Contraction (β_h^+)

(b) Monetary Expansion (β_h^-)

Figure 3.20: Asymmetric effects of monetary policy on investment ratio.



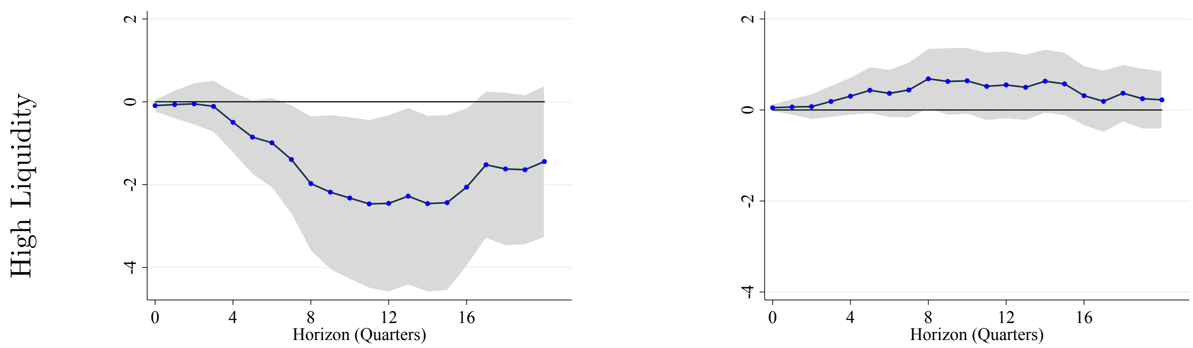
(a) Monetary Contraction (β_h^+)

(b) Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Control variables are real asset growth, log gdp and growth in employees. The shaded areas show 90 percent confidence intervals.

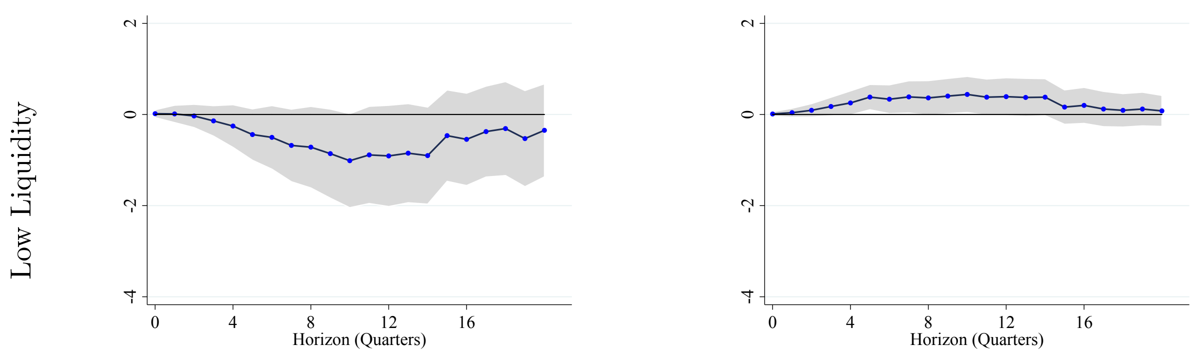
$$y_{j,t+h} - y_{j,t-1} = \alpha_j^h + \beta_h^+ \, max[0, \Delta R_t] + \beta_h^- \, min[0, \Delta R_t] + \Omega'(L) \, Z_{j,t-1} + \epsilon_{\,j,t+h}$$

Figure 3.21: Asymmetric effects of monetary policy on log employees.



(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Figure 3.22: Asymmetric effects of monetary policy on log employees.

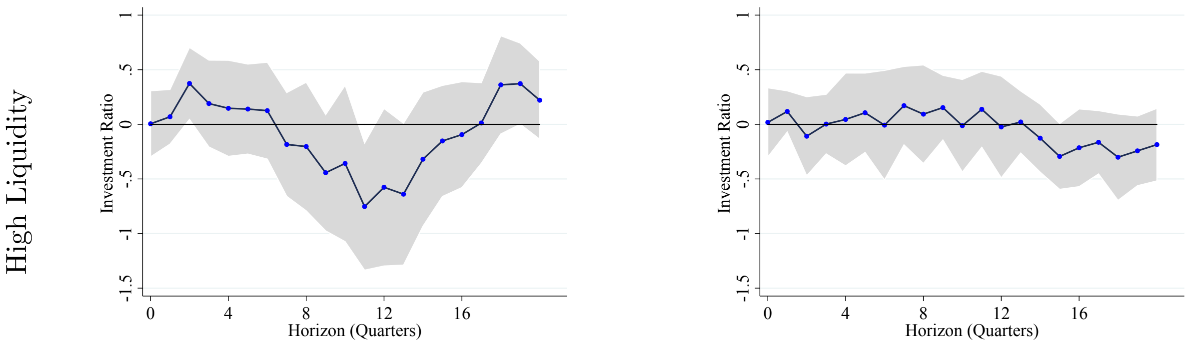


(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Control variables are real asset growth, log gdp and growth in employees. The shaded areas show 90 percent confidence intervals.

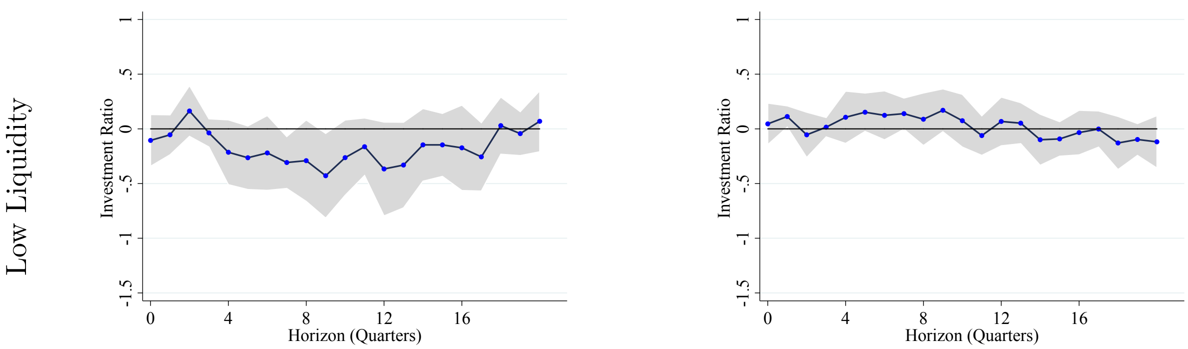
$$\frac{i_{j,t+h}}{k_{j,t+h-1}} - \frac{i_{j,t-1}}{k_{j,t-2}} = \alpha_j^h + \beta_h^+ \; max[0, \Delta R_t] + \beta_h^- \; min[0, \Delta R_t] + \Omega'(L)\; Z_{j,t-1} + \epsilon_{\,j,t+h}$$

Figure 3.23: Asymmetric effects of monetary policy on investment ratio.



(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Figure 3.24: Asymmetric effects of monetary policy on investment ratio.



(a) Monetary Contraction (β_h^+) **(b)** Monetary Expansion (β_h^-)

Note: The first (second) column show the impulse response to a monetary policy shock that increases (decrease) the one-year treasury rate by 25 basis points on impact. Control variables are real asset growth, log gdp and growth in employees. The shaded areas show 90 percent confidence intervals.

3.6 Tables

Table 3.1: Descriptive Statistics

	Employees	Investment Rate	Sales	Assets	Debt to Assets	Liquidity Rate
Obs #	594173	572650	675309	676488	672447	676190
Botton 5%	0.0	0.0	0.0	1.6	0.0	0.2
Median	0.8	4.5	29.5	108.5	23.1	7.0
Average	7.6	7.8	390.0	1898.1	32.2	17.3
Top 5%	31.5	26.7	1471.4	7166.0	81.1	72.0
Std dev	35.5	10.8	2193.8	12024.6	70.0	23.0

3.7 Appendix

Firm level variables I use quarterly Compustat firm-level from 1980 to 2016. Compustat provides high-quality information on balance sheet and income statement components of publicly traded C corporations in North America. Detailed variable definitions of Compustat can be accessed through Wharton Research Data Services for the United States.

Table A3.1 provides the variable names and respective codes in Compustat. Leverage is the ratio of short and long term debt to total assets. Liquidity ratio is the ratio of cash and short-term investments (*cheq*) to total assets. Dividend variable is used as an indicator on whether the firm has paid cash dividends in the previous year. *aqcq* represents the cash outflow or funds used to acquisition of a company. Employment data is pulled from yearly data and linearly interpolated across quarters within the year. All nominal variables in level are deflated using the aggregate GVA deflator.

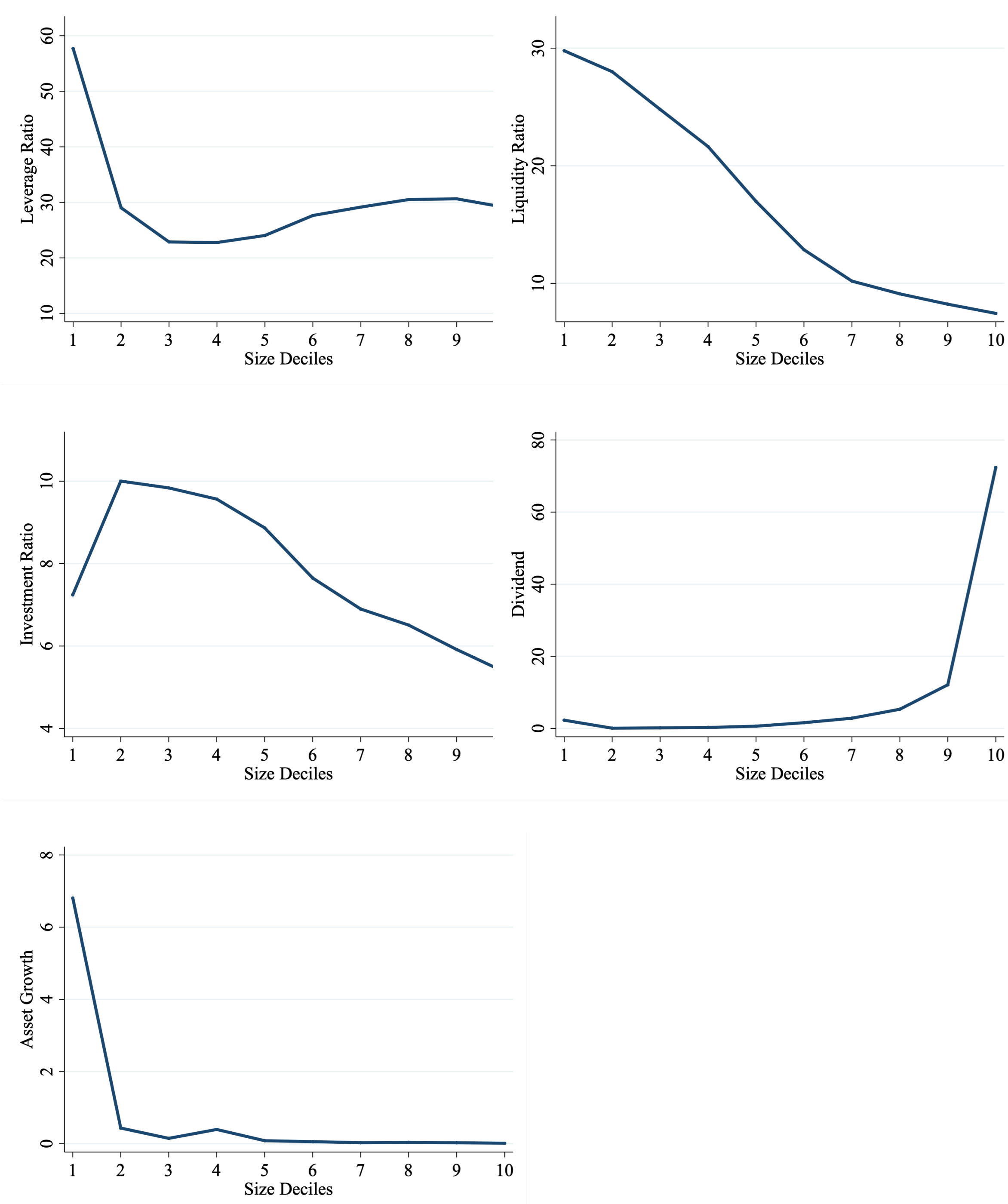
Table A3.1: Variable Definitions

Variable	Compustat variable
Leverage	$(dlcq + dlttq) * 100/atq$
Liquidity ratio	$cheq * 100/atq$
Employees	emp
Investment ratio	$capxq/L.ppentq$
Total Assets (Book value)	atq
Debt to Equity ratio	$(dlcq + dlttq) * 100/ceqq$
Sales	$saleq$
Dividend	dvq
Acquisitions	$aqcq/atq$

Sample Restrictions I drop firms in finance, insurance, real estate and public administration sectors. Following Ottonello and Winberry (2020), I also exclude firms with acquisitions accounting for more than 5% of total assets. I drop firms which are in the panel for less than 5 years. The baseline trimming excludes firms with top and bottom 1% of investment ratio, sales growth and debt to equity ratio. I also trim top 1% of leverage ratio. Trimming is done by year.

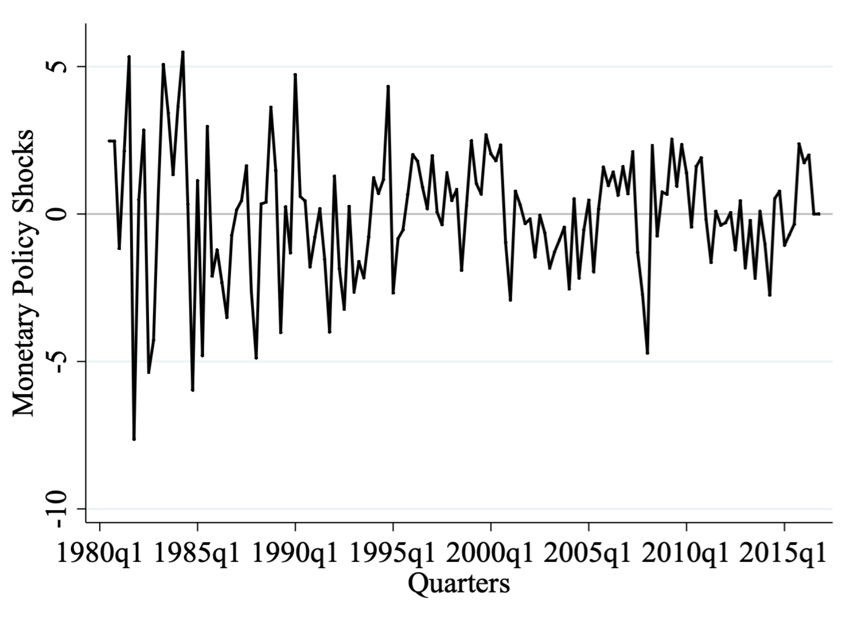
Macro Time Series Data The one-year risk free is the 1-Year Treasury Constant Maturity Rate (Monthly, Not Seasonally Adjusted) from FRED series GS1. The excess bond premium is complied by Gilchrist and Zakrajšek (2012), EBP_OA, available at author's website. Employment rate is available at FRED as the seasonally adjusted employment rate of all persons aged 15:64 in the United States (LREM64TTUSM156S). CPI is the seasonally adjusted consumer prices index computed for total items in the United States by FRED (CPALTT01USM661S). Debt to GDP is from provided by macro trends, available here. PPI is the producer prices index computed for total items in the United States by FRED (PPIACO). The GVA (gross value added) deflator series (B358RG3Q086SBEA) is Price Index (Business : Nonfarm) from FRED.

Figure A3.1: Characteristics of alternative firm size deciles.



Note: Size deciles are generated using the number of employees. The figure uses data from 1980q3 to 2016q3. See Appendix 3.7 for data construction and trimming. The figure is generated after the trims. Replicating this figure using firm assets deciles yield very similar results.

Figure A3.2: Time-series of monetary policy shocks (1980q3-2016q3)



Note: The figure plots implied monetary policy shocks derived from Gertler and Karadi (2015)'s structural VAR impact matrix. See the text in section 3.2 for details.

Figure A3.3: Asymmetric effects of monetary policy on log employees for size leverage groups.

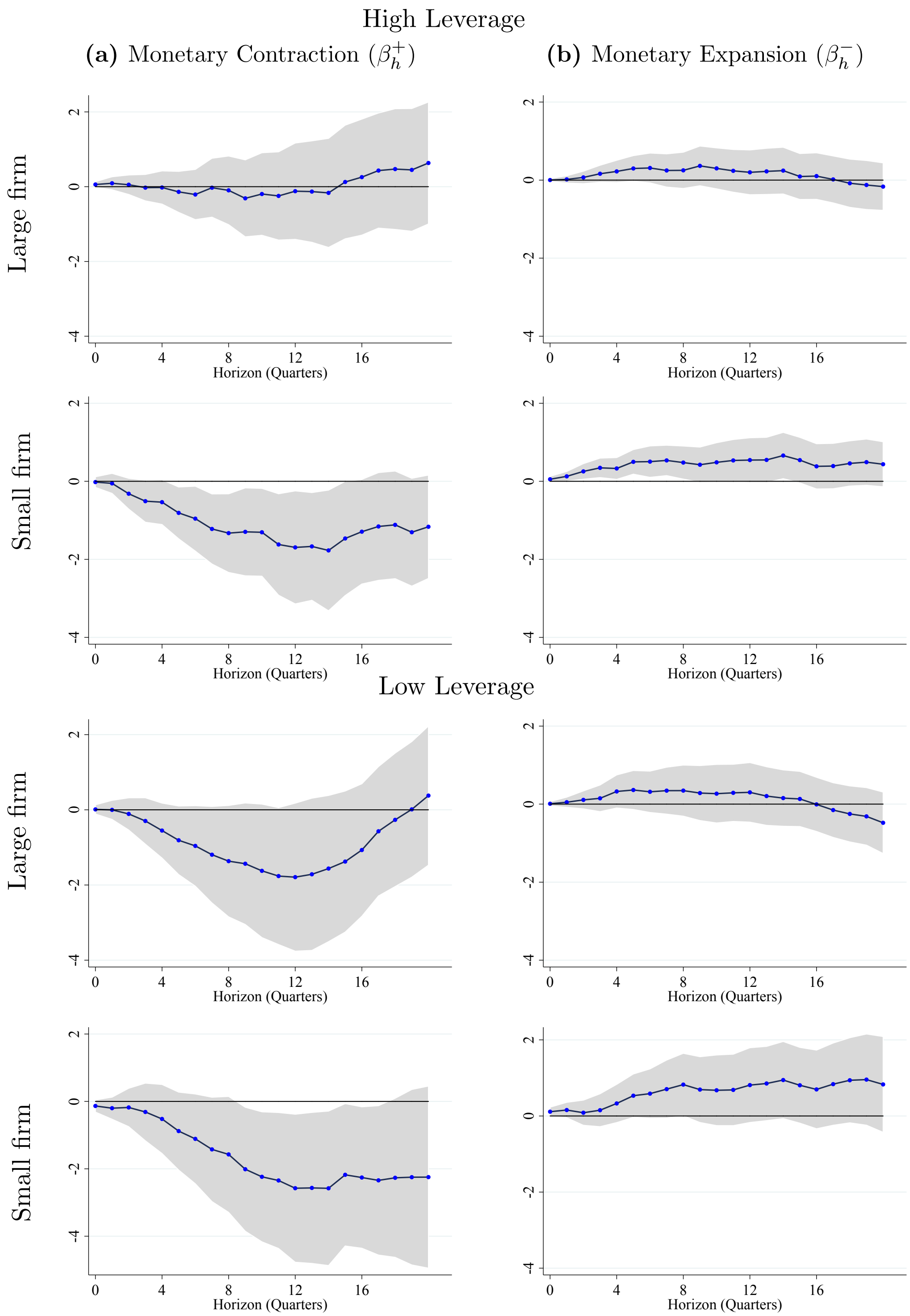


Figure A3.4: Asymmetric effects of monetary policy on investment ratio for size leverage groups.

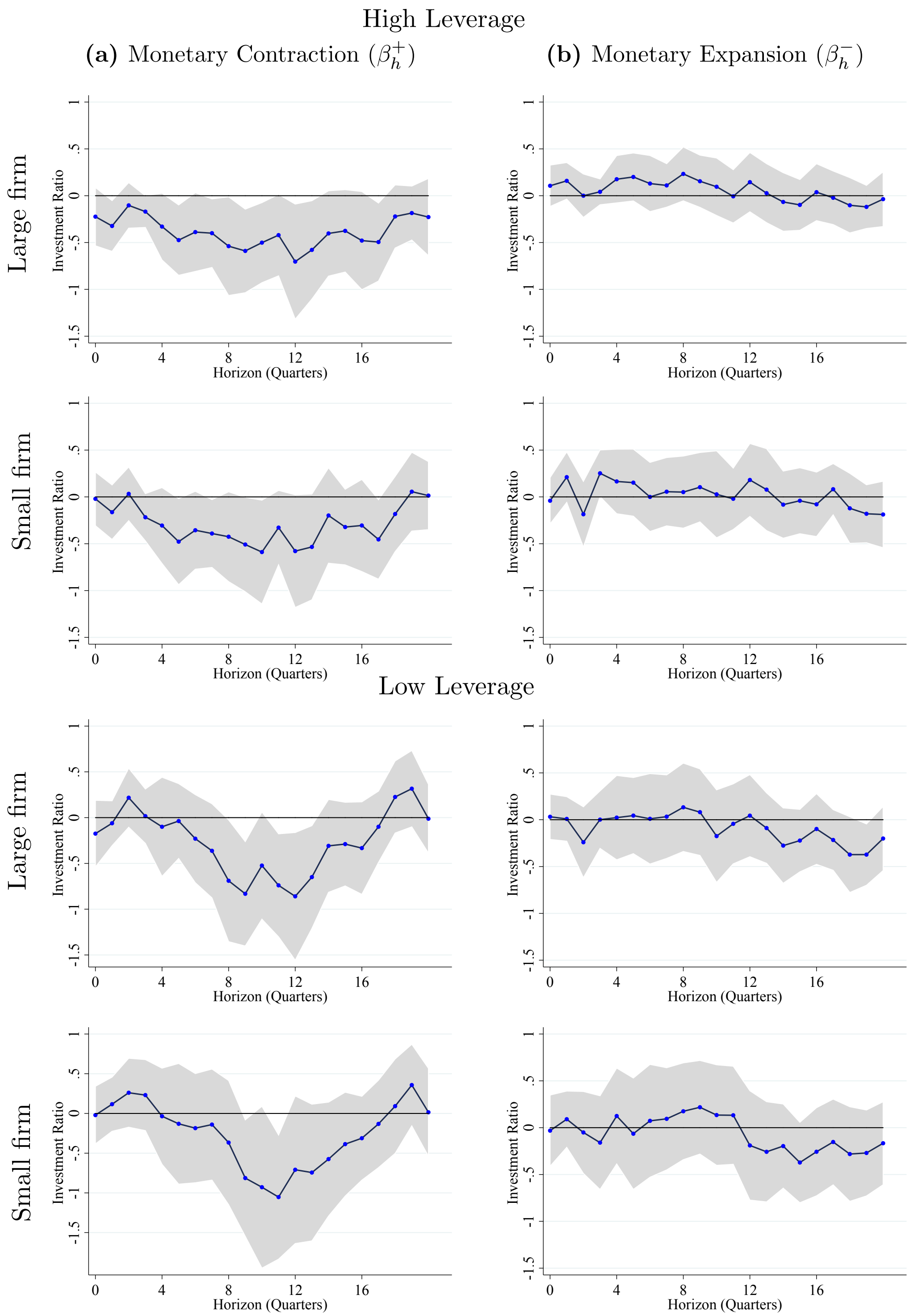


Figure A3.5: Asymmetric effects of monetary policy on log employees for size liquidity groups.

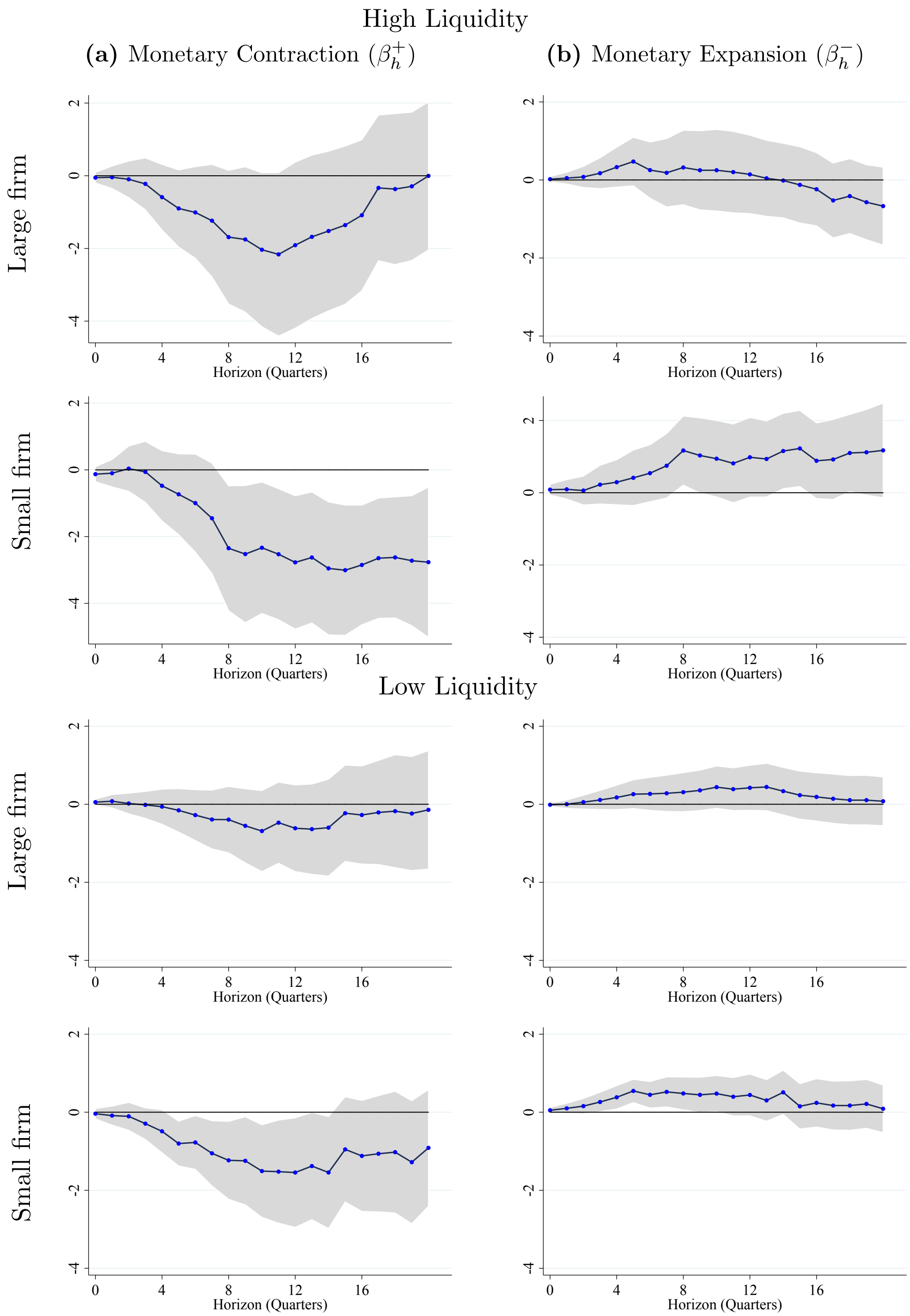


Figure A3.6: Asymmetric effects of monetary policy on investment ratio for size liquidity groups.

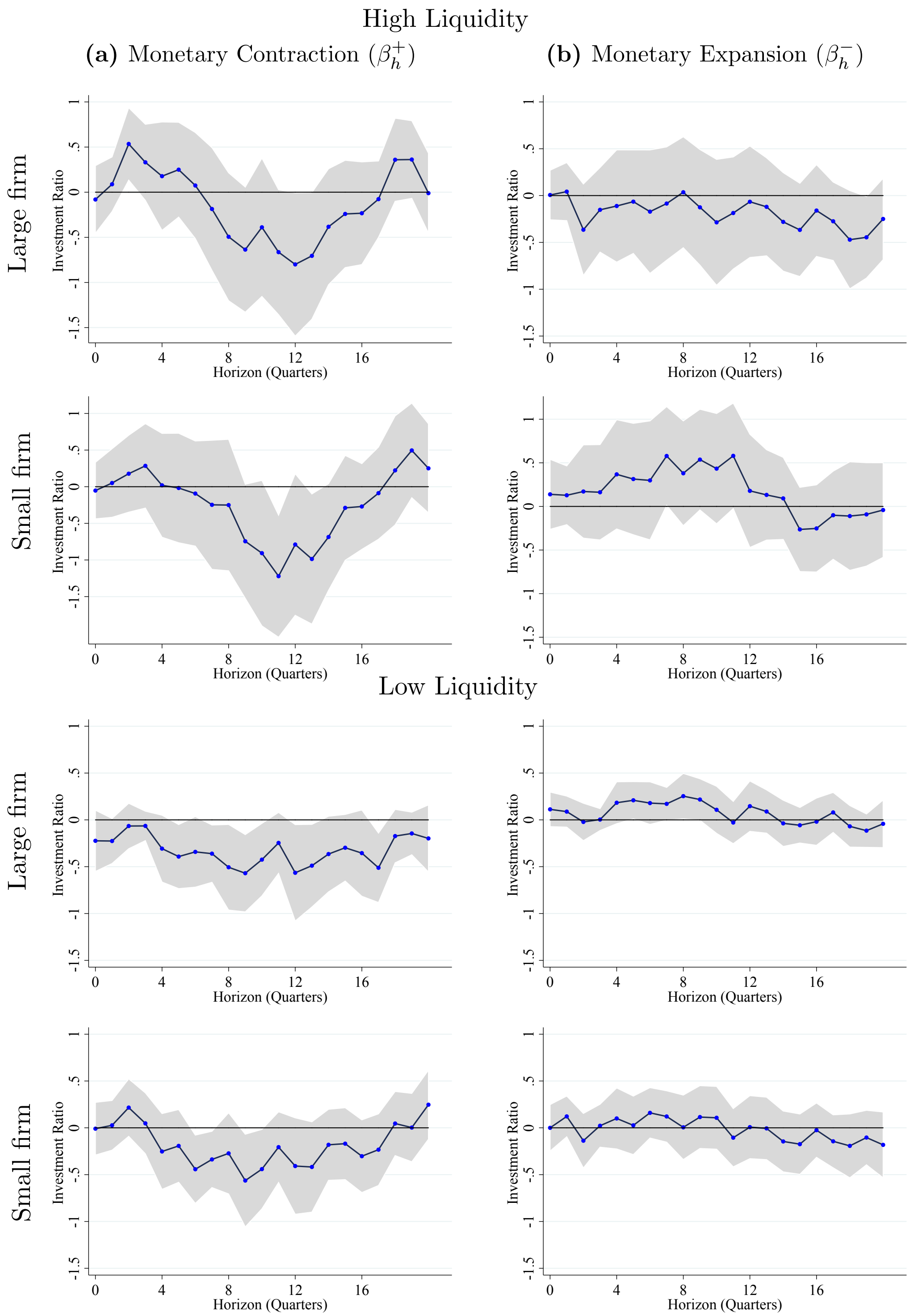


Figure A3.7: Asymmetric effects of monetary policy on log employees for leverage liquidity groups.

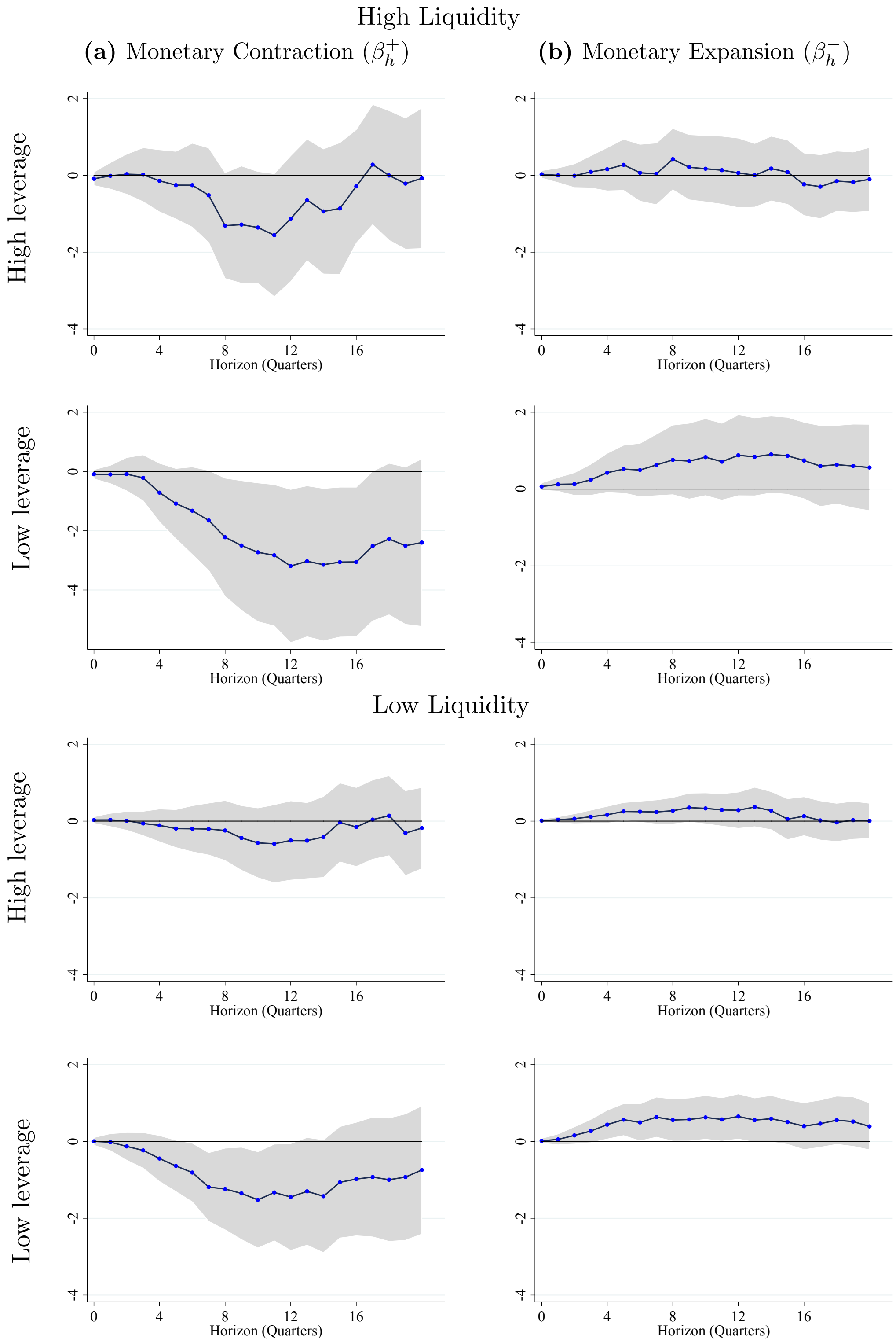


Figure A3.8: Asymmetric effects of monetary policy on investment ratio for leverage liquidity groups.

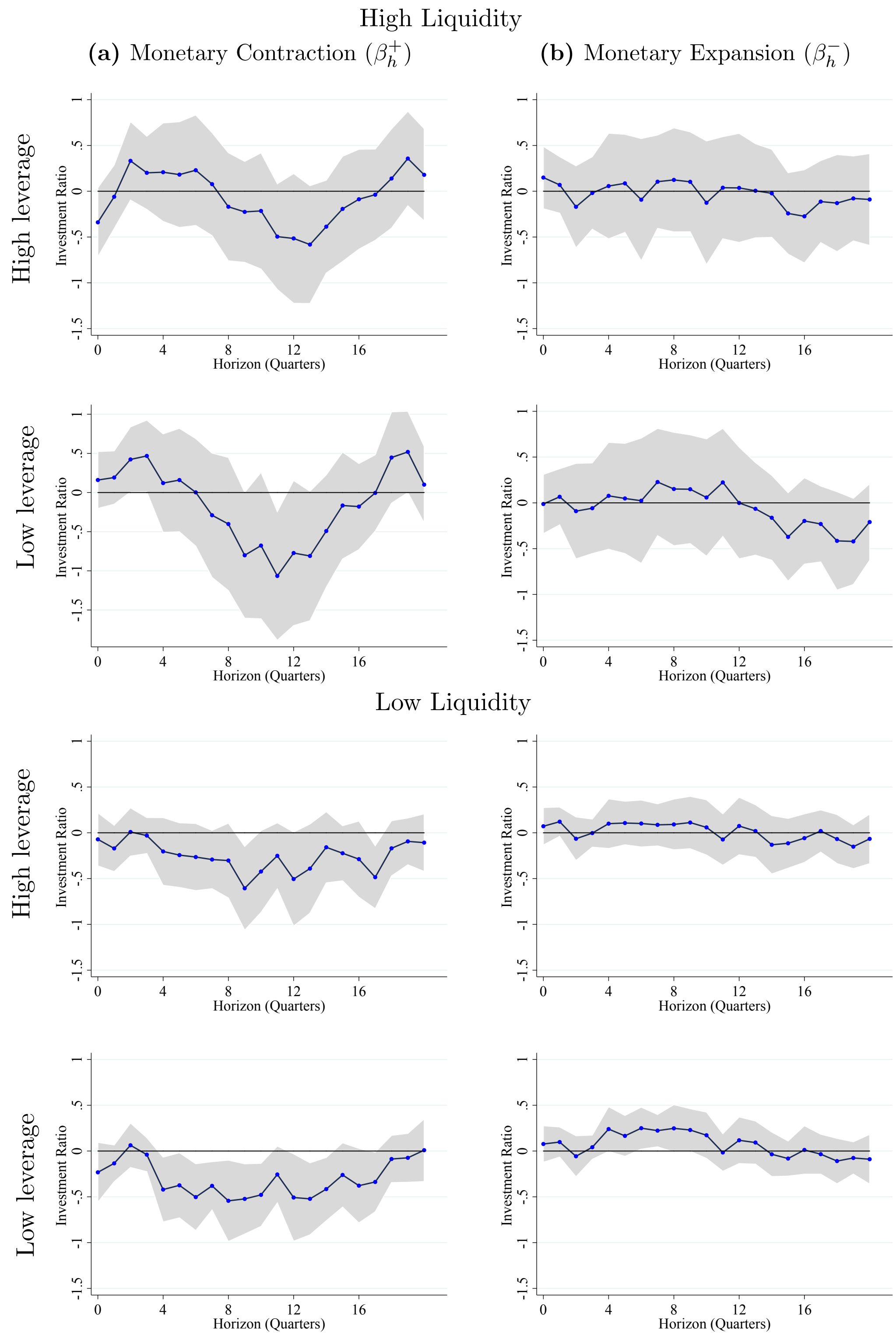


Figure A3.9: Asymmetric effects of monetary policy on log employees for dividend size groups.

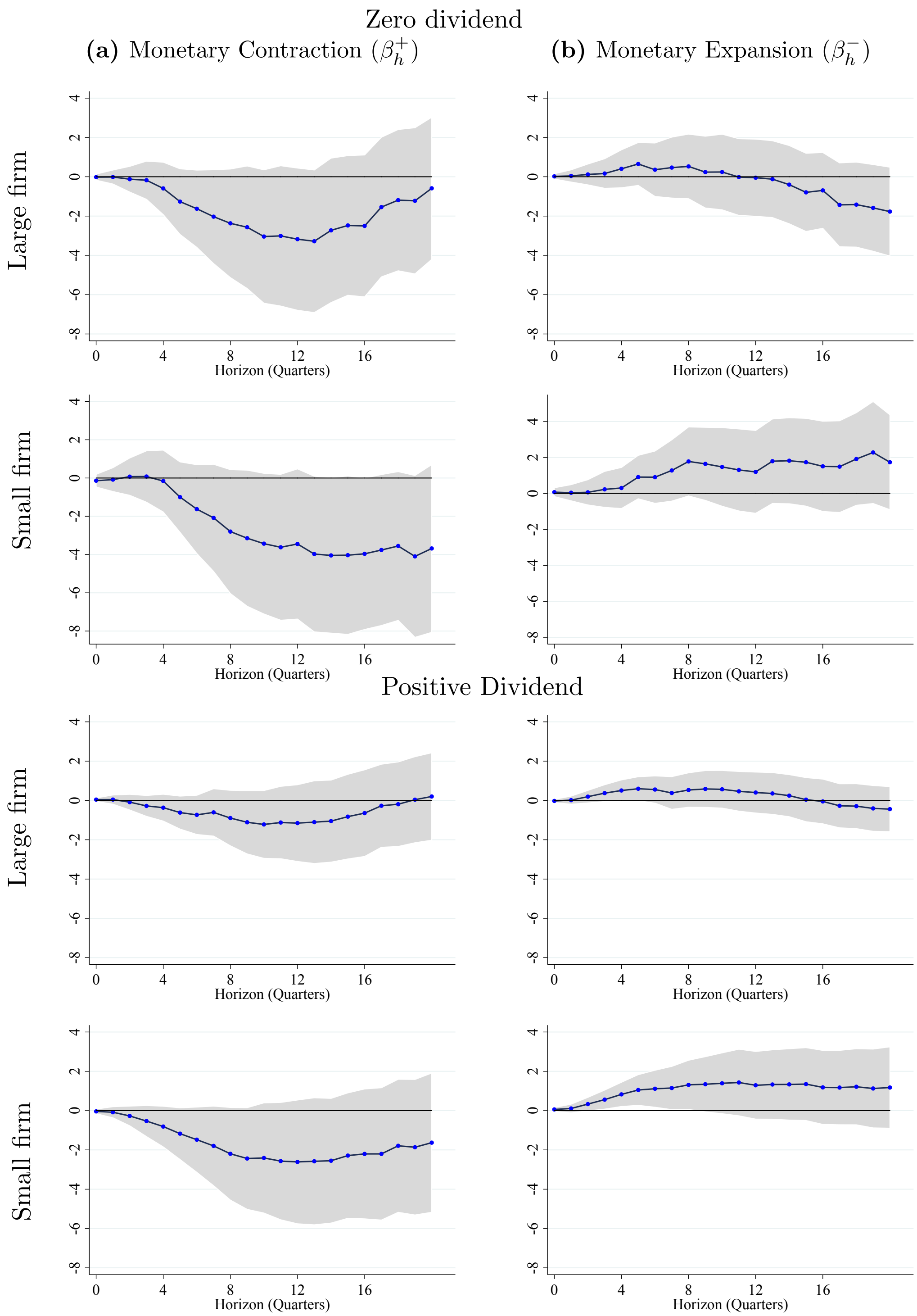
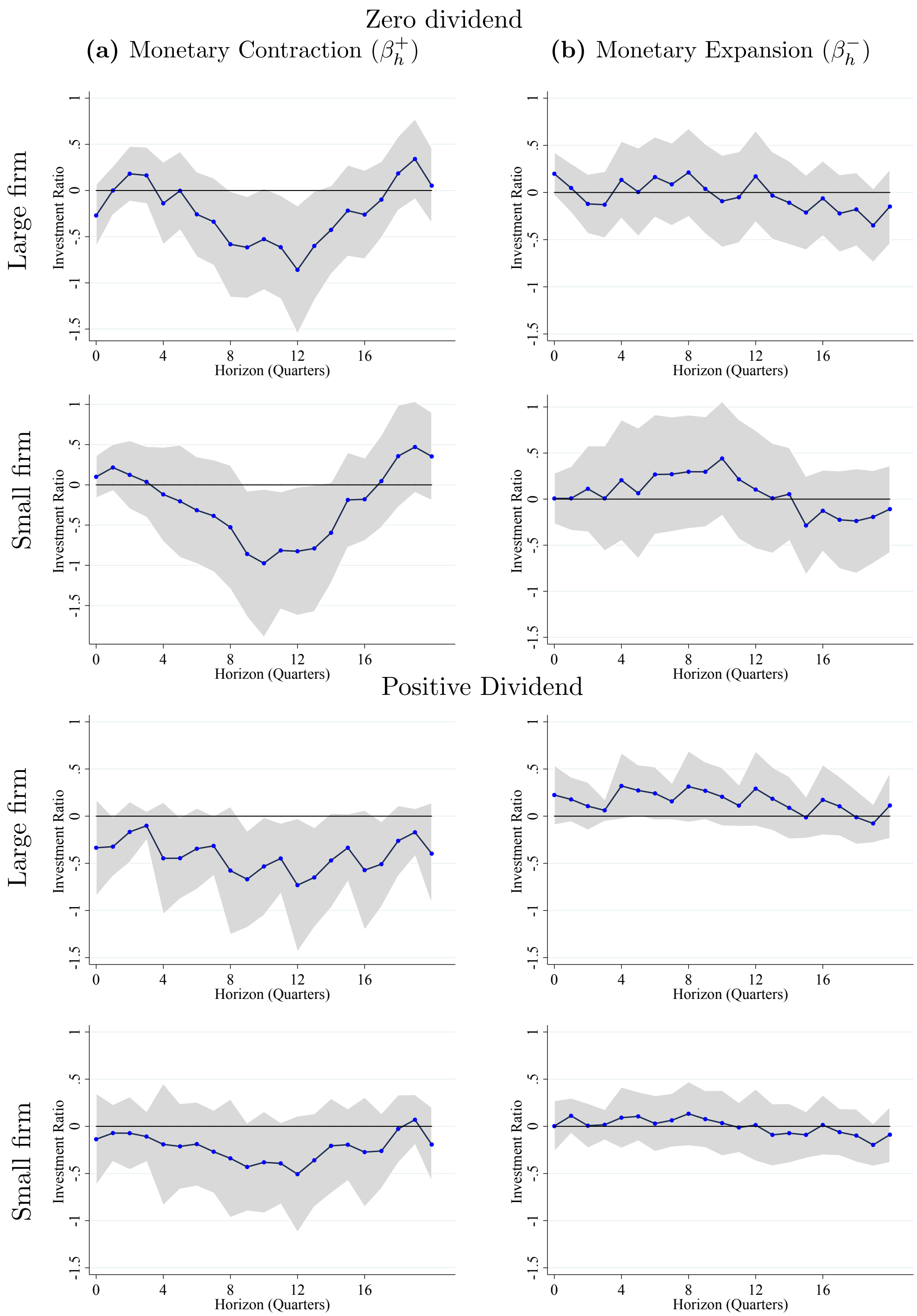
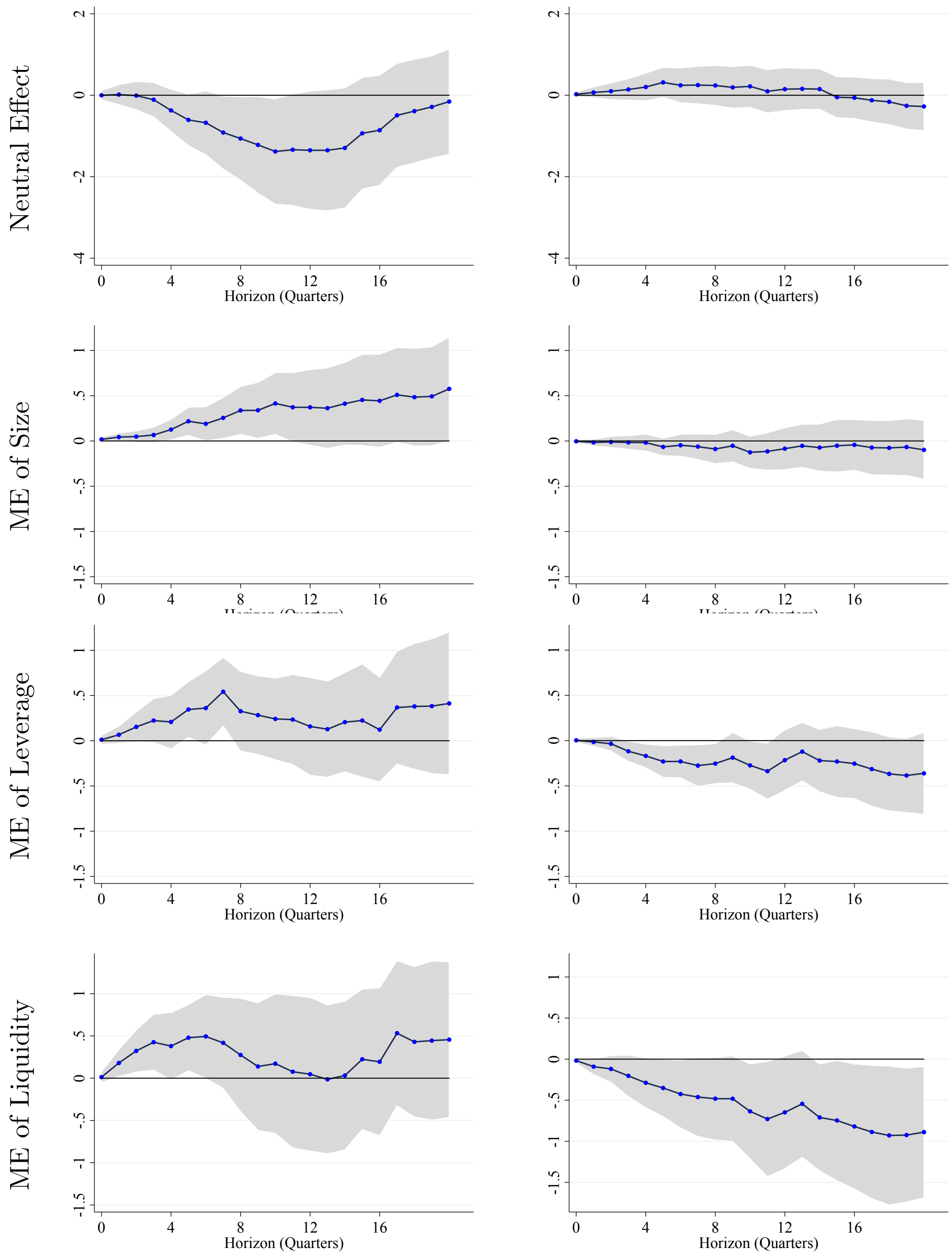


Figure A3.10: Asymmetric effects of monetary policy on investment ratio for dividend size groups.



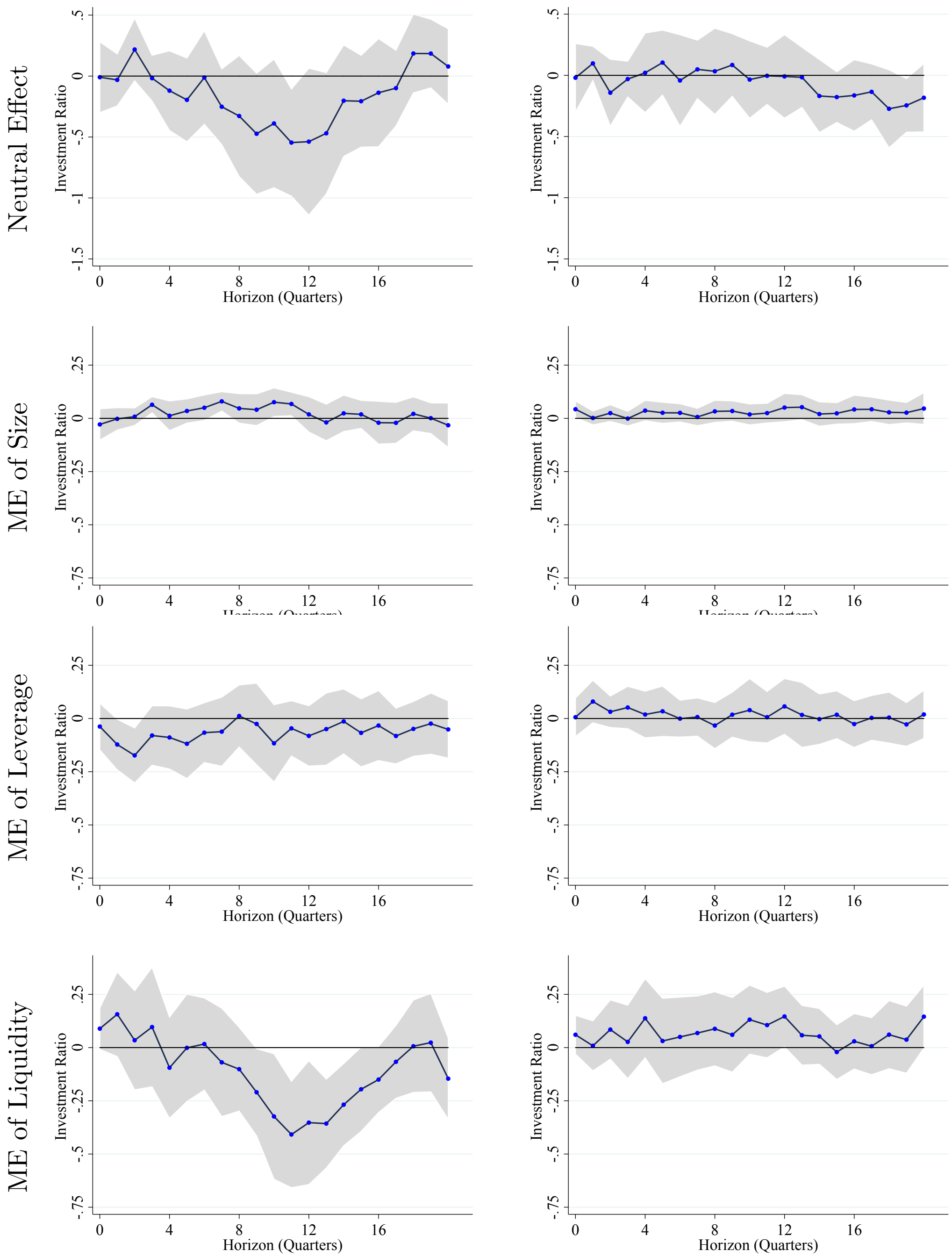
$$
\begin{aligned}
y_{j,t+h} - y_{j,t-1} = \alpha_j^h &+ \beta_h^+ \, \Delta R_t^+ + \beta_h^+ \, \Delta R_t^+ \, \text{lev}_{j,t-1} + \beta_h^+ \, \Delta R_t^+ \, \text{liq}_{j,t-1} + \beta_h^+ \, \Delta R_t^+ \, \text{emp}_{j,t-1} \\
&+ \beta_h^- \, \Delta R_t^- + \beta_h^- \, \Delta R_t^- \, \text{lev}_{j,t-1} + \beta_h^- \, \Delta R_t^- \, \text{liq}_{j,t-1} + \beta_h^- \, \Delta R_t^- \, \text{emp}_{j,t-1} \\
&+ \Omega'(L) \, Z_{j,t-1} + \theta_h^1 \text{lev}_{j,t-1} + \theta_h^2 \text{liq}_{j,t-1} + \theta_h^3 \text{emp}_{j,t-1} + \epsilon_{\,j,t+h}
\end{aligned}
\tag{A3.1}
$$

Figure A3.11: Impact of monetary policy on number of employees



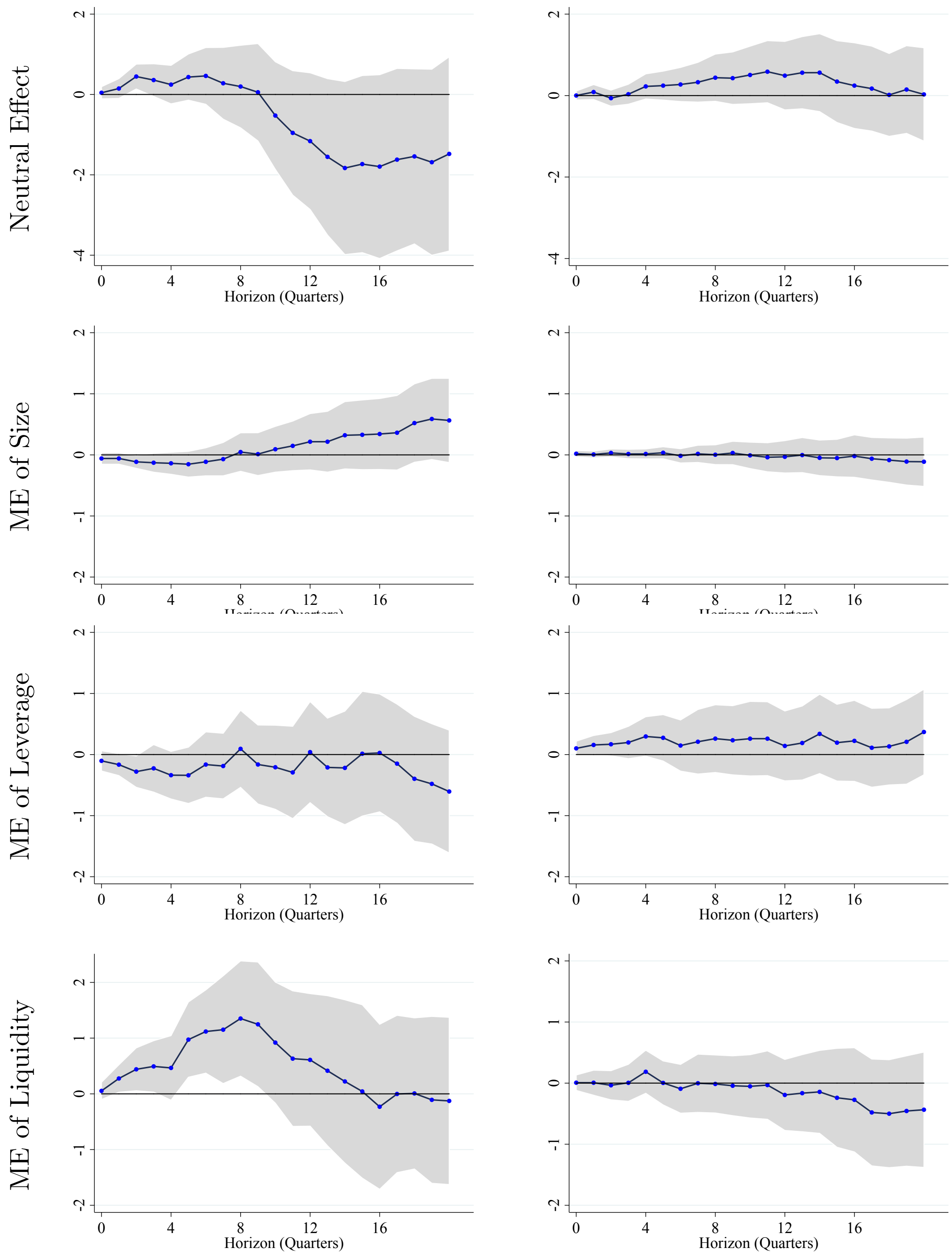
$$y_{j,t+h} - y_{j,t-1} = \alpha_j^h + \beta_h^+ \, \Delta R_t^+ + \beta_h^+ \, \Delta R_t^+ \, \text{lev}_{j,t-1} + \beta_h^+ \, \Delta R_t^+ \, \text{liq}_{j,t-1} + \beta_h^+ \, \Delta R_t^+ \, \text{emp}_{j,t-1}$$
$$+\beta_h^- \, \Delta R_t^- + \beta_h^- \, \Delta R_t^- \, \text{lev}_{j,t-1} + \beta_h^- \, \Delta R_t^- \, \text{liq}_{j,t-1} + \beta_h^- \, \Delta R_t^- \, \text{emp}_{j,t-1}$$
$$+\Omega'(L) \, Z_{j,t-1} + \theta_h^1 \text{lev}_{j,t-1} + \theta_h^2 \text{liq}_{j,t-1} + \theta_h^3 \text{emp}_{j,t-1} + \epsilon_{j,t+h}$$

Figure A3.12: Impact of monetary policy on number of investment ratio



$$
\begin{aligned}
y_{j,t+h} - y_{j,t-1} = \alpha_j^h &+ \beta_h^+ \, \Delta R_t^+ + \beta_h^+ \, \Delta R_t^+ \, \text{lev}_{j,t-1} + \beta_h^+ \, \Delta R_t^+ \, \text{liq}_{j,t-1} + \beta_h^+ \, \Delta R_t^+ \, \text{emp}_{j,t-1} \\
&+ \beta_h^- \, \Delta R_t^- + \beta_h^- \, \Delta R_t^- \, \text{lev}_{j,t-1} + \beta_h^- \, \Delta R_t^- \, \text{liq}_{j,t-1} + \beta_h^- \, \Delta R_t^- \, \text{emp}_{j,t-1} \\
&+ \Omega'(L) \, Z_{j,t-1} + \theta_h^1 \text{lev}_{j,t-1} + \theta_h^2 \text{liq}_{j,t-1} + \theta_h^3 \text{emp}_{j,t-1} + \epsilon_{\,j,t+h}
\end{aligned}
$$

Figure A3.13: Impact of monetary policy on number of log capital stock



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