Aggregation and Tax Shocks: Lessons from Firm-Level Data

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May 1, 2021

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Abstract

I investigate the cross-sectional impact of tax shocks on investment with aggregate and firm-level data sources and find two key results. First, current estimates of the impact of tax shocks on investment are biased because they fail to account for underlying heterogeneity in the responses of the components of investment and types of firms and shifts in the composition of investment over time. Second, I show that the firm-level corporate investment response to a one percentage point increase in the average corporate tax rate reaches a peak impact of -2% after two quarters and disappears within eight quarters. Additionally, I show that financially constrained firms exhibit a relatively smaller response to tax shocks than their unconstrained counterparts, which is consistent with the idea that the upward-sloping supply curve of capital dominates the flattening accelerator effect documented elsewhere. These results have important implications for future estimates of the impact of tax shocks.

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1 Introduction

A key question in the analysis of fiscal policy is the extent to which a particular tax shock may increase or decrease investment. The investment response to a tax shock may differ across types of investment and types of firms depending on various time-invariant characteristics. For example, investment in equipment may respond differently from investment in intangibles because of pre-existing differences in tax treatment, while the response of a highly productive firm may differ strongly from one which is financially constrained. With that in mind, I have two goals in this paper. First, I seek to show that current estimates of the effect on output and investment of a tax shock fail to consider the rich heterogeneity underlying the components of output and investment and are therefore biased. This finding motivates my second goal, which is to uncover idiosyncrasy in the cross-section using firm-level data.

I motivate the reason for examining heterogeneity in response by first discussing a fundamental issue with the literature, namely that current approaches are excessively aggregated and therefore do not pay close enough attention to heterogeneity in the components of aggregates.[1] This is not necessarily a problem if the relative shares of each component remain constant over time, but will bias estimates if there exist time trends associated with these components. To take a pertinent example, suppose we are interested in the effect of a tax shock on nonresidential investment over time because we aim to use this as an input into a policy discussion over a proposed tax reform. If our estimates of the effect are dependent on a period when tangible investment made up a much larger share of nonresidential investment than it does presently and tangible investment responds significantly differently to a tax shock than intangible investment, then my estimates of any kind of elasticity or multiplier for nonresidential investment (or even higher order variables like GDP) will necessarily be biased.[2] If firms are organized as pass-throughs, they may react differently to tax shocks

[1] Throughout this paper, I use phrases like "overly aggregated," "excessively aggregated," and "over-aggregated" to indicate that, given the goal of discovering the effect of a macroeconomic shock on some response variable, our current method of doing so masks too much heterogeneity. The problem is not that economists are summing components of investment or output incorrectly.

[2] The exact same tax change may have different impacts at different times due to nonlinearities in response

than c-corporations, something which again becomes relevant if the composition of firms organized as the latter decreases over time. These criticisms apply to much of the most influential papers on output and fiscal policy, including Romer and Romer (2010), Mertens and Ravn (2013), Blanchard and Perotti (2002), Mountford and Uhlig (2009), and Caldara and Kamps (2017), though I primarily rely on Mertens and Ravn (2013) and Romer and Romer (2010) to illustrate these points.

The aggregation issue in the literature leaves a hole for how to estimate the static and dynamic responses of investment to a macroeconomic shock. In principle, we can estimate these responses as long as we know the relevant cross-sectional (and time-invariant) responses to a tax shock and the share of total investment that each component makes up. If those conditions do not hold, we will once more be swimming blindly in policy space. Given clear differences in investment response by legal form, I address heterogeneity in response using firm-level corporate investment data and the vector of corporate income tax shocks identified by Mertens and Ravn (2013). This empirical approach combines a panel regression with firm fixed effects to capture permanent, idiosyncratic differences across firms with Jorda local projections to obtain static and dynamic responses to a tax shock.

I find that on average, the corporate firm-level investment response to a tax shock is similar in magnitude and impact to the aggregate effect on nonresidential investment (though shorter-lived), with physical investment declining by 1% immediately in response to a one percentage point increase in the average corporate tax rate and reaching a peak impact of -2.4% after six quarters and recovering after eight. This response is also less persistent than the average response to a monetary policy shock, though it is difficult to make a direct comparison.

To model how we might examine heterogeneity going forward, I use common indicators of financial constraint including firm size, firm age, high leverage firms, dividend payers,

to tax shocks and because the circumstances are different (see Gunter et al. (2021) on nonlinearities). For example, a prediction of the effect of the 2017 Tax Cuts and Jobs Act could not reasonably be made based on the effect of the Tax Reform Act of 1986 because the political circumstances were so divergent; the latter could be expected to be highly persistent in contrast to the former, which could not.

and the Hadlock-Pierce Index. In contradiction to some of the literature on monetary policy shocks, I show that financially constrained firms tend to respond *less* than their counterparts, though highly leveraged firms respond more significantly. I attribute this to the dominance of the "frictions" effect on financially constrained firms, which by definition face a steeply increasing marginal cost curve for investment.

Related Literature My paper contributes to several different literatures. The first studies the aggregate response of output and investment to aggregate tax shocks. While there has been little theoretical dispute since Hall and Jorgenson (1967) that a positive tax shock will decrease investment, existing studies tend to treat investment as an afterthought to output, resulting in imprecision and disagreement over the extent to which a tax shock affects investment (Ramey, 2016). Blanchard and Perotti (2002) set the foundation for time series approaches, utilizing a SVAR with identification of tax shocks achieved by imposing the elasticity of net taxes to GDP from prior estimates and finding a peak impact of -0.36% in response to a 1% increase in net taxes.[3] Mountford and Uhlig (2009) estimate multipliers under different budgetary regimes by imposing sign restrictions on a VAR, most relevantly finding an impact investment multiplier of around 2% for a deficit-financed negative unit shock to taxation. Additionally, Barro and Redlick (2011) estimate an investment multiplier of around -0.35 for a 1% increase in the average marginal tax rate. Two additional studies utilize the vector of exogenous shocks compiled from narrative evidence by Romer and Romer (2010), both of which ultimately aim to estimate dynamic multipliers for output rather than investment. From a 1% increase in taxes as a share of output, Romer and Romer (2010) estimate that investment will decline by 9% after 10 quarters. Given a one percentage point decrease in the average corporate income tax rate, Mertens and Ravn (2013) find that investment will increase about 2.3% after six quarters, which is very close to my baseline result. While these studies differ in specification, their methodological approaches to study-

[3] Caldara and Kamps (2017) criticize their identification strategy and provide an alternative dynamic response for output, but not investment.

ing the question is uniform insofar as they all aggregate shocks and response variables over time. I show that this procedure biases estimates upward because they fail to account for underlying heterogeneity and time trends in the components of investment and output.

Second, I contribute to the literature on firm-level responses to policy shocks. Yagan (2015) exploits changes in depreciation allowances, while Zwick and Mahon (2017) and House and Shapiro (2008) study the effect on investment of changes in the investment tax credit and the dividend tax credit, respectively. Zidar (2019) studies a slightly different question—the distributional impact of tax policy—but likewise emphasizes the cross-section and disaggregation. Cloyne et al. (2018) and Ottonello and Winberry (2018) motivate much of this paper; they study similar questions in the context of monetary policy. Eskandari and Zamanian (2020) study a similar question but largely focus on implied credit constraints. My purpose is different. I seek to show not only that financial constraints may affect investment response in different ways, but also to show why taking account of heterogeneity is necessary.

Third, I engage with the literature on financial constraints. The response of a financially constrained firm to a tax shock is theoretically ambiguous; they may respond less because they have a steeply increasing marginal cost curve for investment or they may respond more due to a financial accelerator mechanism. The evidence is mixed. Bernanke and Gertler (1989) and Kiyotaki and Moore (1997) emphasize the importance of collateral and net worth for the financial accelerator mechanism on the theoretical side, while Cloyne et al. (2018) likewise emphasizes this on the empirical side. In contrast, Ottonello and Winberry (2018) finds that the effect of an increasing marginal cost curve dominates the financial accelerator effect, albeit for monetary policy. My results are in agreement with Ottonello and Winberry (2018) and Eskandari and Zamanian (2020) insofar as we find that unconstrained firms respond more than constrained firms. In contrast to Ottonello and Winberry (2018) and Eskandari and Zamanian (2020), I find that leverage and size are not good indicators of financial constraint because highly leveraged and small firms are both able to significantly increase long-term leverage following a positive tax shock, which suggests that they have access to debt markets and are therefore not constrained.

2 Aggregate Tax Shocks: Problems and Idiosyncrasies

In this section, I illuminate some of the issues with aggregation using the seminal Mertens and Ravn (2013) and Romer and Romer (2010) studies. I first discuss heterogeneity in the components of investment and next heterogeneity in response by legal form.

2.1 Overaggregation in Response Variable

In the short-run at least, it is generally agreed that a tax cut will stimulate investment and likely consumer spending, thereby increasing output. The clearest illustration of the investment channel comes from the canonical user cost of capital model (Hall and Jorgenson, 1967). In this framework, the user cost of capital is an increasing function of the tax rate, generating the result that the equilibrium quantity of investment will increase given a tax cut. To start, assume that there are no capital gains and the price of capital is equal to one so that a firm's user cost is given by

$$\Psi = (r+\delta)\left(\frac{(1-\tau_c)(1-\lambda\tau)}{1-\tau}\right), \tag{1}$$

where r is the real interest rate, δ is the depreciation rate, λ is the present value of depreciation allowances on a dollar of investment, τ is the tax rate, and τ_c is the investment tax credit. Thus the opportunity cost of holding capital is the opportunity cost r plus depreciation, adjusted by increases or decreases in the firm's marginal tax rate, depreciation allowance, and available investment tax credits. Then the elasticity of user cost with respect to the tax rate is

$$\frac{\tau}{\Psi}\frac{\partial \Psi}{\partial \tau} = \frac{\tau}{1-\tau}\frac{1-\lambda}{1-\lambda\tau}. \tag{2}$$

Given current (and previous) tax law, equation (2) has different implications for different types of capital. Physical capital follows rigid GAAP and tax depreciation schedules. In general, these stipulate $\lambda < 1$, so that a decrease in the tax rate τ will cause the user cost of capital to decrease and hence equilibrium investment to rise given the increase in the marginal product of capital. Thus, a negative tax shock should lead to greater investment and hence greater output. However, most types of intangible capital are immediately expensed, i.e., $\lambda = 1$. If $\lambda = 1$, the investment channel for tax shocks is not only muted, it is wholly inert, rendering the theory behind the investment channel for tax shocks completely moot. In fact, investment in internally developed intangibles infrequently appears on firm balance sheets. When firms invest \$1 to train employees, invest in a more efficient organizational structure, engage in goodwill, develop their brand, and other types of internal intangible investment, current accounting standards require that current profitability fall by \$1 and total assets fall by \$1 because it spent cash to carry out the operation. In contrast, an additional dollar spent on tangible assets does not affect total assets because the dollar of cash spent is offset by a dollar gain in fixed assets. In such cases, the user cost of capital is completely unresponsive to changes in the tax rate, which implies that shocks to tax policy will generally be ineffective at shifting spending on intangible investments. For some types of intangible investment, like R&D spending, the capital good is instead amortized over time. Note that even in these cases the effect will be quite small: the cut in user cost in the first term is muted by depreciation allowances in the second term. These differences are non-trivial; intangibles make up a large share of fixed investment (Peters and Taylor, 2017).

Hence a clear prediction is that tax shocks should affect intellectual property (intangible) investment less than other types of investment. That is precisely what happens in Figure 1, where intellectual property clearly responds less than structures and equipment to a one percentage point increase in the average corporate tax rate.

[Figure 1 here]

If the composition of investment remains similar over time, then variation in the components of investment to tax shocks would be perfectly captured by aggregate investment and estimates of investment and output multipliers would therefore be unbiased. But if the composition of investment changes over time to reflect a greater share of intangibles, then investment elasticities and output multipliers will be biased upward. Firm-level and aggregate evidence strongly suggest that this is the case. At the firm level, Crouzet and Eberly (2019), Falato et al. (2018), and Peters and Taylor (2017) all document the tremendous rise in intangible intensity, with the work of the latter two suggesting that the typical firm's intangible capital stock as a share of total capital stock has risen from 10% in the 1970s to well over 50% today. Similarly, if we take intellectual property investment as a proxy for intangible investment, then Figure 2 provides evidence that intangible investment as a share of total investment in fixed assets has risen, particularly in the post-1976 period, compensated for by declines in structures and equipment.

[Figure 2 here]

The evolution of Figure 2 is largely driven by two distinct factors. First, in 1975 the Financial Accounting Standards Board and the Securities and Exchange Commission banned capitalization for R&D spending and required that all corporations report annual R&D spending, starting in 1976. Second, the R&D tax credit was first enacted in 1981, giving tax incentives for spending on R&D. The first development gave rise to $\lambda = 1$ in the user cost framework, while changes in the R&D tax credit led to level changes in R&D spending. Both made R&D cheap relative to other types of investment, which is a major determinant of the rise of intangibles.[4]

Given the rise in intangibles in the post-1976 period, a natural experiment to test the hypothesis that a change in the share of intangibles biases investment and output multipliers in one direction or another is to split the sample in two and obtain separate impulse responses, with one period running from 1950Q1-1975Q4 and the second running from 1976Q1-2007Q1. With the rise in intangibles as a rival production technology to physical capital and the

[4]Figure 2 likely understates the extent to which intangibles have risen because it does not account for spending on many other types of intangible capital.

absolute growing importance of intangibles, it should be the case that the post-1976 sample shows a relatively smaller response to a tax shock. I run this experiment with a three-variable VAR in the Romer-Romer framework with the Romer-Romer tax shock ordered first, followed by log output, and a log-transformed investment component. This mirrors the Romer-Romer specification.[5]

Running this experiment yields two results. First, in Figure 3a, the impact of a 1% increase in taxes as a share of total output is shorter and shallower in the post-1976 period, only going significantly below zero after 5 quarters with a peak impact of -2.5% after 10 quarters, whereas the pre-1976 period exhibits a quicker, deeper, and more persistent response to a tax shock with a peak impact of -3.5% after 10 quarters. The impact of a tax shock on the components of investment exhibits a similar difference between the pre-1976 and post-1976 periods. If the hypothesis is correct that the rise of intangible capital would effectively mute the response of investment to a tax shock, then nonresidential fixed investment in the post-1976 period should exhibit little response whereas the pre-1976 IRF should have a more persistent, negative, and significant response. This is precisely what happens in Figure 3b, in which nonresidential fixed investment is not significantly less than zero in response to a positive tax shock at any point for twenty quarters. In contrast, the pre-1976 sample demonstrates a substantially more persistent and highly negative response to a tax shock.[6]

[Figure 3 here]

The split-sample IRFs therefore indicate that the impact of tax shocks on output and the investment channel in particular are different and substantially weaker in the more recent period, and this is likely due to the rise of intangible capital and its unique tax treatment.

[5]Because our concern in this subsection is primarily about how aggregate investment and output respond when we consider heterogeneity, it makes more sense to use a total tax shock rather than a corporate or personal income tax shock. Results using the Mertens and Ravn (2013) specification are available upon request.

[6]These results are invariant to usage of the 2008 vintage of NIPA data that (Romer and Romer, 2010) use or an updated version which includes intellectual property as fixed investment.

Consequently, highly aggregated studies done in a similar vein as Romer and Romer (2010) and Mertens and Ravn (2013), including Blanchard and Perotti (2002), Mountford and Uhlig (2009), and Caldara and Kamps (2017), will tend to produce peak impact multipliers biased upward by a failure to acknowledge the changing composition of investment over time, making their relevance for current policy dubious at best.[7]

2.2 Heterogeneity Across Legal Form

One consideration in analyzing the investment response to a tax shock is not merely the type of investment but *who* is doing the investing. That is, it may matter whether or not a firm is organized as a c-corporation or as a pass-through. In the former case, corporate tax shocks will operate directly along the investment margin and for dividend-paying firms, a personal income tax shock may also affect investment. If the quality or quantity of investment is more or less muted in response to a tax shock from one legal type to another, perhaps for agency considerations, and if the tax shock is sufficiently large to induce switching from one type to another, then it matters to the aggregate analysis what the composition of these firms is. This aspect will be missed by aggregate analyses, especially if the composition of these firms changes over time.

To investigate heterogeneity by legal form, I construct quarterly estimates of fixed asset data from NIPA and then apply the Mertens-Ravn proxy SVAR. This is accomplished by adapting the method of McGrattan (2020). The general idea is to estimate a state space model with the annual data in combination with related quarterly series from NIPA and the

[7] I could levy similar criticism at work on monetary policy, which tends to also be heavily aggregated over time. Monetary policy typically affects investment in the user cost framework through the interest rate r. In this case, the response of the user cost to changes in r is given by

$$\frac{r}{\Psi}\frac{\partial \Psi}{\partial r} = \frac{r}{r+\delta}.$$

Depreciation rates for intangible capital are substantially higher for intangible capital than for physical capital (see also, Li and Hall (2016)), which implies that the elasticity of user cost to changes in interest rates is significantly less sensitive for intangible capital than tangible capital. This point receives scant attention in Cloyne et al. (2018) and Ottonello and Winberry (2018) and therefore remains an important area for further investigation given the rise of intangible capital.

Flow of Funds and apply a Kalman smoother. I construct quarterly estimates for Fixed Asset Table 4.7 Lines 37-44 (gross investment) and obtain impulse responses from these estimates.[8] Because in this case we are interested in a breakdown of investment by legal form, the shock variables are alternately a personal income tax shock and a corporate income tax shock, in each case taken from Mertens and Ravn (2013).

[Figure 5 here]

Results are in Figure 5 for non-financial corporations and Figure 6 for non-financial non-corporate entities. Interpretation is somewhat confounded by the fact that the corporate category includes both c-corporations and s-corporations and the latter is taxed as a pass-through. However, there are striking differences between these two types in response to both corporate and personal income tax shocks. Whereas corporations respond significantly to both personal and corporate income tax shocks, pass-throughs respond significantly only to personal income tax shocks. The corporate response to a personal income tax shock is more immediate than the corporate response to a corporate income tax shock, declining by three percent and recovering after seven quarters. Corporation investment declines by two percent in response to a one percentage point increase in the average corporate income tax rate and does not recover until nearly twenty quarters have passed. In contrast, non-corporate non-financial entities do not react at all to a corporate income tax shock, but react significantly to personal income tax shocks. Total investment reaches a peak impact of -8% after six quarters before recovering after twelve quarters.

[Figure 6 here]

The results present two puzzles: why do corporations respond significantly to personal income tax shocks as well as corporate income tax shocks and why is the response of pass-throughs to a PI shock substantially more negative than the corporate response to a CI

[8] See Appendix C for details on the procedure.

shock? The first makes sense if we consider two facts. First, the corporate sample also includes s-corporations which are taxed as pass-throughs, so the PI shock is more relevant for them at the margin. Second, corporations may choose to invest profits rather than pay out dividends given a personal income tax shock because dividends become expensive relative to investment for shareholders following a PI shock. A major explanatory factor for the more negative response of pass-throughs is the fact that pass-throughs tend to be domestically constrained in comparison to corporations, which can optimize their tax burden more easily across borders. Hence a tax shock would have a relatively smaller effect. Relatedly, corporations can marshal the resources to engage in highly sophisticated tax planning whereas pass-throughs typically are less able to, though they can more easily mask income from tax authorities due to less stringent disclosure requirements (especially in this sample, which groups s-corporations with c-corporations). Additionally, there may be an agency consideration here: corporations are run by managers whereas pass-throughs tend to be run directly by their owners, which may make them more sensitive to changes in user cost.

There are other reasons that investment done by corporations may be systematically different from investment undertaken by pass-throughs. Historically, there has been a large tax wedge between the corporate form and other ways to organize. Barro and Wheaton (2019) suggest that the very fact that c-corporations exist despite this tax penalty is de facto evidence of a productivity benefit to incorporation and give several reasons to back up this claim. Perhaps the most crucial is that c-corporations can retain funds internally and have greater access to external funding than privately organized firms, which also tend to be much smaller. Size becomes relevant for concerns over intangible investment, which tends to be highly scalable and nonrivalrous relative to physical investment. In an age of rising intangible intensity, it may therefore be the case that c-corporations, which tend to be much larger, are better able to optimally invest in intangibles without fear of losing market power (Doidge et al., 2018). Indeed, Kopp et al. (2019) argue that market power was a major factor in the weak investment response to TCJA 2017, but this would not be inconsistent with the hypothesis that intangibles are driving concentration, which in turn does not necessarily imply a competition problem. Yet another possibility exists that agency problems related to managerial handling of investment may mean that pass-throughs are, in some sense, more efficient investing bodies even if they have reduced access to external funds (Stein, 2001).

[Figure 7 here]

Given the evidence that legal type non-trivially affects a firm's approach to investment, then the dynamics of the aggregate investment and output responses will be affected by the composition of firms by legal type, something which will not get picked up by more aggregated studies. Figure 7 provides some evidence that there has been a substantial shift toward pass-throughs and away from c-corporations in the last thirty years, something which can likely be attributed to the increase in the tax wedge induced by the 1986 Tax Reform Act (Plesko and Toder, 2013). Moreover, the share of firms organized as c-corporations and the absolute number of c-corporations has declined since the 1980s (Auerbach, 2018; Doidge et al., 2018). Finally, there is substantial evidence that tax changes induce switching between forms for firms at the tax wedge-productivity margin (Clarke and Kopczuk, 2017). This is troubling because it suggests that the vector of tax shocks compiled by Mertens and Ravn (2013) may not be orthogonal to other variables affecting corporate investment. If switching is significant, then aggregate corporate investment will fall given a tax shock not only because corporations are reducing their investment due to an increase in the user cost of capital, but also because the quantity of corporations falls in response.

In any case, it is clear that investment undertaken by different types of firms in response to a tax shock is categorically different in form and magnitude, something which becomes especially relevant when the composition of these firms also changes over time, thereby introducing a bias to investment and output multipliers. The bias introduced in this case would to some extent offset the bias examined in 2.1, but it is exceedingly difficult to discover the net effect given current methods.

2.3 Conceptual Problems

There is a deeper conceptual problem with the tax shock literature as it currently exists. Measuring tax shocks as changes in revenues masks substantial underlying heterogeneity in actual changes in tax law. There is not an obvious way around this other than being more specific about what exactly is meant by a tax shock. This is the key problem with bringing a concept—policy shocks—that had relevance initially for monetary policy into fiscal policy. The federal funds rate is the federal funds rate and approximates to some extent a ∂r but in practice a tax shock is not a $\partial \tau$. A change in tax revenue today does not mean the same thing as a change in tax revenue yesterday because the underlying law changes are not the same. An alteration in the R&D credit may have the same revenue effect as a change in the statutory tax rate, but that does not imply they will have the same effect on user cost. Δr is the fundamental element in a monetary policy shock but ΔT is the sum of heterogeneous components generating heterogeneous responses; it is *not* fundamental.

In the limit, the shocks themselves may be excessively aggregated such that it is inappropriate to draw inferences from them. Changes to tax law often contain offsetting incentives. A look at the comprehensive Tax Reform Act of 1986 is instructive. Even though the statutory corporate tax rate fell substantially from 46% to 34% and became significantly less progressive (thereby reducing the cost of capital), the elongation of depreciation schedules and the repeal of the investment tax credit increased the cost of capital. On net, per the analysis of Romer and Romer (2010) and Mertens and Ravn (2013), the reform was a large increase in corporate taxation. While certainly true, this masks the effect that different provisions have on different industries and on firms at different stages in the lifecycle. For example, young, unprofitable firms with high net operating losses would be substantially unaffected by the drop in the corporate tax rate. The 2017 Tax Cuts and Jobs Act also illustrates this clearly. Whereas the law increased the user cost of capital for R&D investment, it decreased user cost for equipment and structures (Barro and Furman, 2018, p. 276). A danger of aggregating these changes into a net shock value is that it will overlook key aspects of investment dynamics within the economy and will cease to provide value at even first-order levels of disaggregation.

2.4 Aggregates from Disaggregates

The proper response to the problems discussed in 2.1, 2.2, and 2.3 is not nihilism or dismissal of previous studies, but to ask how we can build a roadmap for constructing bias-corrected estimates going forward. What we have done historically and what is currently in vogue, namely to assume that there are not relevant trends in the components of aggregates and we can therefore sample across time without issue, is not optimal given what has been discussed above. An alternative is to assume instead that the components of an aggregate can be sufficiently separated into different categories based on time-invariant properties. If we know how these relevant properties cause a component to respond, then at any point in time we can apply a previously obtained statistical estimate of the response to each component to get an accurate forecast. Because time trends are encapsulated in these estimates, an unbiased estimate will emerge.

For example, suppose I want to know the effect on corporate investment of an exogenous tax cut of 1%. In the simplest case, I can estimate the immediate impact by taking

$$\Delta I_t = \sum_{j}^{N} \gamma_j \alpha_{j,t-1}, \tag{3}$$

where ΔI_t is the change in corporate investment at time t, γ_j is the semi-elasticity of investment to tax shock for time-invariant property j, and $\alpha_{j,t-1}$ is the real value of corporate investment with time-invariant property j at time $t-1$. To avoid double-counting, the estimator would have to pick mutually exclusive properties (e.g., there is overlap between multinationals and intangible-intensive firms, but we could isolate intangible-intensive multinationals from their counterparts). For example, suppose I think the leverage distinction is most relevant. Suppose that investment by high leverage firms in the previous quarter was

$100B and investment by their counterparts was $200B. Then given a positive corporate income tax shock of 1% and my estimates in Table 2, total corporate investment will decrease by $2.72B, but only by 0.9% overall. To illustrate the difference between this method and what is currently popular, suppose instead that a semi-elasticity for all investment was obtained over a period when high leverage firms conducted 2/3 of investment rather than 1/3. My method would still predict an overall drop of 0.9%, but an aggregated approach would instead predict a drop of 1.8% because it would be unable to account for time trends in the relative components. My work on firm-level data in the following section is motivated by this insight.

This procedure could then be carried out iteratively for each mutually exclusive aggregate to obtain estimates at each level for investment by legal form, private investment, total investment, and finally output (after following the same procedure for consumption, net exports, and government expenditures). The work of Zidar (2019) is a potentially critical input in this regard.

There are two problems with this method. First, the exact level of disaggregation and the categories used remains far from clear. Holding constant the level of aggregation for the vector of tax shocks, a greater level of disaggregation in the response variable leads to noisier estimates. It therefore becomes necessary to properly specify the shock variable at the proper level of aggregation given the level of aggregation in the response variable. Consequently, the reliability of our estimates using this method are dependent on the quality of shock variable identification and specificity. Second, there is a data availability issue for cross-sectional analysis of many private businesses. There is a mechanical correspondence between aggregate and disaggregate data in the sense that the latter must necessarily sum to the former. But the ease with which we can draw inferences from one about the other is limited by the quality of available data and until more complete research comes to light on the tax response of privately held business, it will be nearly impossible to actually carry out this exercise.

3 Further Lessons from Firm-Level Data

I use firm-level data rather than aggregates for several reasons beyond what is discussed previously. First, aggregate quarterly measures of investment are the result of investment by all legal forms, not just corporations. While it is true that a corporate tax cut may have general equilibrium effects and therefore could affect investment by other legal forms in significant ways, it makes more sense to look at panel data for individual firms because the number of corporate firms has declined precipitously in recent years (Doidge et al., 2018) so that aggregate corporate data is less reliable as a possible indicator of firm-level investment response. Additionally, the share of firms organized in corporate form is substantially lower than in the past (Auerbach, 2018), which means that a corporate tax cut will tend to be less effective on investment aggregates than previously. Moreover, the evidence on switching suggests that firm-level data is more reliable because switching generates two undesirable effects on aggregate data which may not be desirable to measure if the goal is to isolate the effect of tax shocks on investment. First, aggregate investment by legal form will change not only because of the induced tax shock, but because firms may be switching between forms. As a result, measures of aggregate investment will measure the relative attractiveness of legal forms in addition to the increase attractiveness of investment. Relatedly, if there is substantial switching between forms following a tax shock, then a dynamic measurement of a tax shock on aggregate investment will capture the dynamic effects of firms switching legal form over time as well. Moreover, firm-level data allows a closer look at the cross-section. Consequently, I run panel regressions and compute Jorda local projections to examine heterogeneity in response.

Given a vector of shocks to firm i at time t, a panel regression with time and individual fixed effects and a Jorda local projection can be carried out. I use the vector of corporate tax shocks compiled by Mertens and Ravn (2013). Each shock consists of an increase or decrease in the average corporate income tax rate. For the closest comparison with aggregate literature, I capture gross investment as

$$I_{i,t} = K_{i,t} - K_{i,t-1} + \delta_{i,t},$$

where $K_{i,t}$ is the book value of net property, plant, and equipment of firm i at the end of quarter t and $\delta_{i,t}$ is total depreciation expense. I choose this measure rather than more popular measures like capital expenditures because there is a growing awareness that capital expenditures as measured in Compustat frequently violates the law of motion of capital (Bai et al., 2019). Optimally, I would also get a measure of quarterly gross investment in intangible capital, but quarterly R&D spend reporting does not become widespread until 1989 in Compustat. One limitation to my analysis is that it does not begin until 1975q1 and ends in 2007q1 (the latter date follows the Romer-Romer sample). Hence I am unable to utilize 25 years worth of corporate tax shocks made available by Mertens and Ravn (2013). Additionally, because the sample ends in 2007, we miss out on fifteen years of new developments documented by Crouzet and Eberly (2019), Doidge et al. (2018), and Gutiérrez and Philippon (2016), including the continued rise of intangibles, the continually shrinking quantity of publicly traded firms, a weakening of physical investment, and the rise in markups.

3.1 Firm-Level Investment Dynamics

I use two methods to get a measure of firm-level investment response. My primary tool—and the one most comparable to aggregate methods—is a Jorda (2005) local projection of the form

$$\log I_{i,t+h} - \log I_{i,t} = \alpha_{i,h} + \beta_h \Delta T_{t-1} + \zeta_h Q_q + \epsilon_{i,t,h} \tag{4}$$

where $\log I_{i,t+h}$ is log gross investment for firm i at time t plus horizon h, $\alpha_{i,h}$ is a firm fixed effect, ΔT_{t-1} is a shock occurring in the prior quarter, Q_q is a quarterly control, and $\epsilon_{i,t,h}$ is a residual. In principle, if the shock is truly exogenous, then it is unnecessary to provide any further controls. On the other hand, the shock itself is not firm-specific and there could be some firm-level idiosyncrasy affecting investment decisions. Failure to control for that could result in a biased estimator β. Consequently, I also report results for a specification with a vector of firm-level controls:

$$\log I_{i,t+h} - \log I_{i,t} = \alpha_{i,h} + \beta_h \Delta T_{t-1} + \Gamma'_h Z_{i,t-1} + \epsilon_{i,t,h}, \tag{5}$$

where Equation 5 uses a vector of firm controls Z estimated at each horizon h. These controls are lagged liquidity, leverage, gross investment, size (log total assets), capital stock, and sales growth. The empirical and theoretical literature on the firm has documented that these all have significant effects on firm-level investment.[9] A second method—and one I find useful for discussing static effects and tax elasticities—is a panel regression with firm fixed-effects. Again, I give results without firm controls:

$$\log I_{i,t} = \alpha_i + \beta \Delta T_{t-1} + \zeta Q_q + \epsilon_{i,t}, \tag{6}$$

and with firm controls:

$$\log I_{i,t} = \alpha_i + \beta \Delta T_{t-1} + \zeta Q_q + \Gamma' Z_{i,t-1} + \epsilon_{i,t}, \tag{7}$$

where $Z_{i,t-1}$ is a vector of firm controls. Robust White standard errors clustered by group are used for all specifications. I choose this particular specification not only because it reflects those in Ottonello and Winberry (2018), but also because the quarter immediately following a tax shock tends to be the peak impact. Heterogeneity in these specifications unsurprisingly directly reflects that in the impulse responses from the Jorda local projections. A key difference is that the response variable for the local projections is not the log-level of investment, but rather log differences.[10] In all specifications, the magnitude of the regression

[9] See Appendix B for details on construction and summary statistics.

[10] Results are also available for log investment as the dependent variable rather than log differences.

coefficient is driven by variation within each firm over time because I control for firm fixed effects. I exclude sectoral controls but their inclusion does not change the results.

Interpretation of dynamic responses changes slightly whether the specification with firm controls is used or without. In both cases, the peak impact is reached after one quarter with the growth rate of investment declining by between 1.5 and 2%, but the specification without firm controls recovers after three quarters and the specification with controls takes eight quarters. Despite that difference, the baseline response is similar to the aggregate response by nonresidential investment to a corporate income tax shock, though the aggregate response takes longer to recover. However, it is difficult to make a comparison between these firm-level results and the aggregate response by legal type because, as noted above, Figure 5 includes both s-corporations and c-corporations. See Figure 9 for full results.

[Figure 9a here]

It is also worthwhile to compare my results to the most recent studies on monetary policy shocks and investment. My baseline results differ substantially from Cloyne et al. (2018). In response to a 25bps increase in interest rates, the average firm decreases investment by significantly less (peak impact of .6%) but this effect lasts for several more quarters. These are not quite comparable in the sense that a 25bps shift in monetary policy does not have an equivalent effect on the user cost of capital as a tax shock. Additionally, my average investment semi-elasticity and peak impact are roughly similar to (Ottonello and Winberry, 2018), though the effect of a monetary policy shock is more persistent.

Table 1 shows results without firm controls and Table 2 shows results with firm controls for panel regressions. The results are substantively similar regardless of whether firm controls are imposed. The baseline result with and without firm controls is that a one percentage point increase in the average effective corporate tax rate leads, on average, to a 0.99% and 1.25% decrease in gross physical investment, respectively. Coefficients are statistically significant at the .05 level.

Next, I investigate heterogeneity with respect to financial constraint using several dummy variables common in the literature. Coefficients and dynamic trends are similar for panel regressions and local projections regardless of whether sector controls are imposed; these results are available upon request.

3.2 Financial Constraints

The impact of policy shocks on firms facing financial frictions has been studied more in the context of monetary policy (Bernanke and Gertler, 1989; Cloyne et al., 2018; Ottonello and Winberry, 2018) than fiscal policy, but the latter has received some attention (Fernández-Villaverde, 2010). The impact of a tax shock is theoretically similar to the impact of a monetary policy shock; an increase in taxes causes a reduction in net worth for firms and lowers profitability, thereby widening frictions. However, it is not immediately clear whether a tax shock would cause financially constrained firms—those firms facing financial frictions—to react more or less than their counterparts in response to a shock.

There are two ways to think of how financial constraints should translate into a particular response to a shock. First, it is possible that financially constrained firms respond less because they do not have access to requisite funding to take advantage of a negative tax shock. A downward shift in the user cost of capital would otherwise result in a higher equilibrium investment, but constrained firms do not have requisite internal or external funding to take advantage of this because they face an increasing marginal cost of investment. This would be in line with Ottonello and Winberry (2018). Second, financially constrained firms may respond *more* due to a financial accelerator mechanism. A tax shock could flatten out the marginal cost curve for investment through changes in asset prices that likewise change collateral values and net worth.[11] Unconstrained firms are thought to face a flatter marginal cost curve but tend to have more collateral, higher net worth, and rely on a more diverse array of funding sources, which allows them to mute the effect of a tax shock and therefore react optimally. I call these the "frictions" effect and the "accelerator" effect.

[11] See McGrattan and Prescott (2010) for a demonstration of how changes in taxation can alter the value of shareholder equity.

Cooley and Quadrini (2001); Cooper and Haltiwanger (2006); Bai et al. (2019) have all modelled or shown evidence noting the importance of the life-cycle of the firm with respect to financial constraint, particularly in relation to irreversibilities in investment. For these models, age is a key indicator of financial constraint; all else equal, younger firms tend to be more financially constrained and would therefore change their borrowing or investment relatively more following a shock. This is partially a reflection of the increased risk of younger firms and the lack of collateral—this latter point matters particularly for younger firms in the modern era, which tend to invest more in intangible capital than physical plant. In these models, younger firms finance their early investments with debt until the efficient level of investment is reached and debt is paid off; a standard prediction is that leverage decreases with age.

Empirically, proxies for financial constraints have not performed particularly well. Popular measures like the Kaplan-Zingales Index (Kaplan and Zingales, 1997; Lamont et al., 2001), the Hadlock-Pierce Index (Hadlock and Pierce, 2010), dividend-paying status, and credit rating all fail a battery of tests conducted by Farre-Mensa and Ljungqvist (2016) to determine whether these proxies actually measure financial constraint. On the other hand, Haltiwanger et al. (2013) and Cloyne et al. (2018) provide some evidence that dividend-paying status and age are adequate proxies for financial constraint. Indeed, most relevant for us, Cloyne et al. (2018) and Ottonello and Winberry (2018) argue that age and leverage are key determinants of the response of firms to shocks. In the former case, younger firms tend to respond more to monetary policy shocks than older firms, and this response is largely driven by dividend-paying firms. For Ottonello and Winberry (2018), leverage is a proxy for default risk; higher leverage firms have poorer access to external finance because they are riskier and therefore respond less to monetary policy shocks than their counterparts.

There is therefore good reason to investigate heterogeneous firm responses using proxies for financial constraints. I look at five: dividend-paying status, the Hadlock-Pierce Index, leverage status, size, and age. See Appendix B for details on variable construction. The results are contradictory and are shown in Table 1, Table 2, and Figure 9. Big firms and dividend payers—proxies for firms which are not financially constrained—react as expected, decreasing investment significantly for several quarters in response to a positive tax shock. Dividend-payers react relatively more than their counterparts, reducing investment by 1.59% in response to a 1% increase in the corporate income tax rate. Big firms decrease investment by 2.54% relative to the previous quarter, whereas small firms do not have a reaction significantly different from zero. This contradicts the results of Crouzet and Mehrotra (2020), who find that small firms are more sensitive to shocks than large firms. Firms categorized as Hadlock-Pierce Constrained, highly leveraged firms, and young firms—proxies for constrained firms—do not agree. The dynamic and immediate responses of Hadlock-Pierce and young firms are insignificant. This contradicts Cloyne et al. (2018), who found that younger firms tend to react *more* than older firms to monetary policy shocks. However, the dynamic and static responses of highly leveraged firms are significantly negative, decreasing investment immediately by 2.72% and not recovering until five quarters later. This result directly contradicts Ottonello and Winberry (2018), who find that firms with higher leverage react less to monetary policy shocks than their counterparts. Given the disagreement between these proxies, there is a clear puzzle.

[Figure 9 here]

In principle, it could be the case that my leverage result—which is an outlier compared to the other indicators—is a function of sample selection, but this does not appear to be the case. In Figure 8, I show the age-leverage relation for the full Compustat sample and my filtered sample. While my sample has lower leverage at every age, the trend is basically the same and contradicts a prediction of the literature, namely that leverage decreases with age. One possible explanation for why the financial constraint indicators give contradictory results is that these firms are not actually financially constrained. I test the extent to which firms in my sample have access to debt markets using the following specification adapted from Farre-Mensa and Ljungqvist (2016):

$$\Delta D_{i,t} = \alpha_i + \beta \Delta T^{+}_{i,t-1} + \delta \Delta X_{i,t-1} + \epsilon_{i,t}, \tag{8}$$

where $D_{i,t}$ is long-term leverage, $\Delta T^{+}_{i,t-1}$ is a positive tax shock in the previous quarter, and $X_{i,t-1}$ is a vector of lagged controls. We use a panel regression with firm fixed effects. Results are given in Table 3. The general idea is that, given a positive tax shock, the relative value of debt to equity as a tax shield rises so that leverage should increase in the long run if firms are not facing financial frictions (ceteris paribus). Even though a positive tax shock would cause the cost of capital to rise and therefore the equilibrium investment rate to decline, leverage should still rise unless gross investment falls to zero. Of the indicators of financial constraints, only highly leveraged and small firms had a significantly positive response to a positive tax shock, which indicates that these are not good indicators of financial constraint in the sample. The results therefore do not disagree with Ottonello and Winberry (2018) because the dummy for high leverage is not a good proxy for financial constraint, whereas their proxy presumably is. This leaves the Hadlock-Pierce Index, young firms, and non-dividend paying firms as indicators of financial constraint. Because the Hadlock-Pierce Index is a decreasing function of size and age and the former previously failed the financial constraint test, age drives the Hadlock-Pierce result.

It could be the case that whichever effect is dominant depends on the sign of the shock. Negative tax shocks, i.e., those which decrease the cost of capital, could have a smaller or negligible effect compared to positive shocks because firms may need to rely on quick access to external financing to take advantage of a lower cost of capital, whereas the decision to simply invest *less* does not require access to additional capital and should in principle have little to do with financing constraints. In other words, the upward-sloping marginal cost curve for constrained firms matters more if the tax shock decreases the cost of capital, but the traditional accelerator mechanism is relatively more important for positive shocks. If

I run the above specifications again with shocks segregated by sign, the results are stark. Negative shocks do not have a significant effect on investment, but positive shocks do have a strongly negative effect. See Figure 10a for results. The interpretation after segregating by shock for financially constrained firms is largely the same. Hadlock-Pierce constrained firms, young firms, and non-dividend payers are largely unresponsive to tax shocks, though they seem to react slightly more to positive tax shocks than negative shocks, which is indicative of financial constraint.

The results indicate that the frictions effect tends to be dominant for tax shocks. In general, more attention in the literature has been paid to the financial accelerator effect, something which has generally been borne out by empirical studies (Crouzet and Mehrotra, 2020; Cloyne et al., 2018). But the work of Ottonello and Winberry (2018) has highlighted that this is not necessarily the dominant effect; the upward sloping marginal cost curve for constrained firms may matter more. There are two distinct areas where this paper can weigh in on the financial frictions debate: first, which proxies may be most relevant for study and second, whether the frictions effect dominates the accelerator effect. With respect to the first issue, my results agree with much of the monetary shocks literature that young firms and non-dividend paying firms tend to be adequate indicators of financial constraint (see, e.g., Cloyne et al. (2018)). However, I disagree with much of this literature (with the exception of Ottonello and Winberry (2018)) insofar as my results indicate that the frictions effect dominates the accelerator effect.

4 Conclusion

Aggregate studies of tax shocks on output and their respective components have been an important input into policy-making and discussions over tax policy in recent years. For example, the 2017 Tax Cuts and Jobs Act was partially motivated by aggregate studies on the impact of tax cuts on output (CEA, 2018) and further analyses of its impact took aggregate multipliers from a collection of these papers (Mertens, 2018).[12] Such studies are not isolated nor inconsequential if they are inputs into policy. Without meaning to pick on any one paper, it is apparent that there exists a serious problem for economists and policymakers if these inputs are potentially biased.

I have argued and shown that this is possibly the case. Statistical estimates of tax shocks over a large time frame are naturally affected by random fluctuations and trends, particularly if the sample of shocks is small, and this is not a problem if it is merely noise. When considering current policy, it makes little sense to apply estimated multipliers which are relics of a time when intangible capital—which faces a different tax treatment than tangible capital—was largely irrelevant in comparison to our era. Given the rise in intangible capital, it is reasonable to consider existing estimates of output and investment multipliers, which are effectively unweighted averages of tax shocks from the postwar period, to be biased upward. Without such excessive aggregation, this problem would have been avoided.

Taking that bias as given, the importance of historical studies of tax shocks on investment and output is unclear and it remains for economists to determine how aggregate studies should be conducted moving forward. An alternative solution lies with disaggregated data. In principle, if we have some idea of the cross-sectional effects of a tax cut and also the composition of the cross-section, then we can build up an aggregate response for those building blocks. Further work remains to be done on that front; it may prove more fruitful than the aggregate method in the future.

One avenue for exploring the cross-section is firm-level data. Going down this path, I show two important sets of results. First, I show that, on average, the corporate firm-level investment response to a tax shock is more immediate, shallower, and shorter-lived than aggregate investment. On its own, this indicates that there is likely a great deal of heterogeneity between types of firms in investment response. Second, I show that there is considerable heterogeneity in the cross-sectional response. Financially constrained firms

[12] It is not altogether clear whether TCJA even qualifies as an exogenous shock. Kumar (2020) acknowledges this, though he tentatively treats it as such regardless.

tend to respond significantly less than their counterparts. This is in contradiction to some of the monetary shocks literature, which tends to find that the financial accelerator mechanism dominates and *constrained* firms drive the result. Even if my proxies for financial constraint are not acceptable, I show at a minimum that the same proxies which are used in the monetary literature respond differently to tax shocks.

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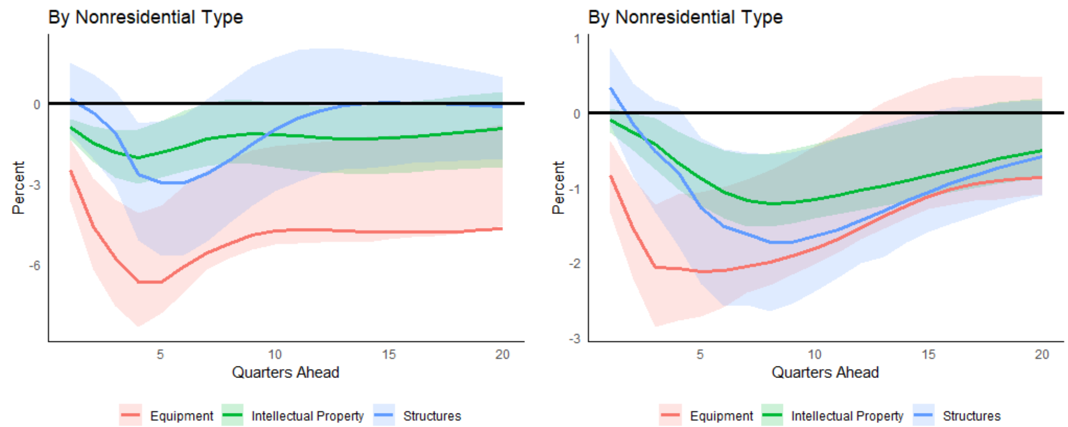
A Results



(a) PI Shock

(b) CI Shock

Figure 1: Impulse responses of components of nonresidential fixed investment to a Mertens-Ravn personal income and corporate income shock. Responses are to a one percentage point in the average personal income tax rate and to a one percentage point increase in the average corporate income tax rate. Impulses responses are estimated using a seven variable proxy SVAR. PI shocks are ordered first for PI shocks, while CI shocks are ordered first for CI impulses. The variable used are the average corporate income tax rate, average personal income tax rate, government spending, real GDP, federal debt, the investment variable, and the corporate income tax base. Except for the rate variables, all others are log-transformed. 68% confidence interval reported.

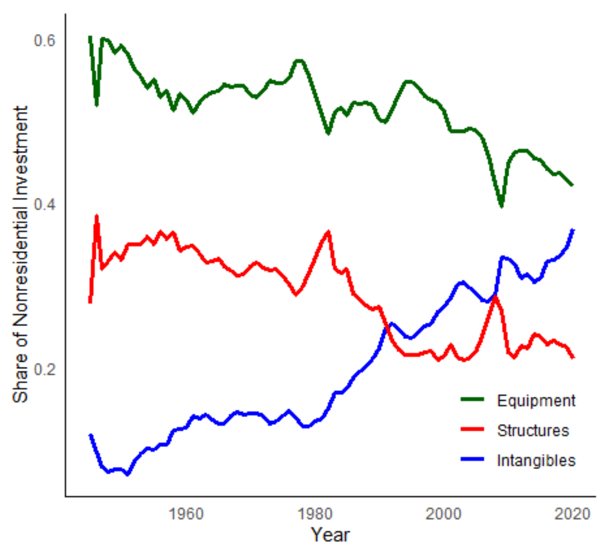
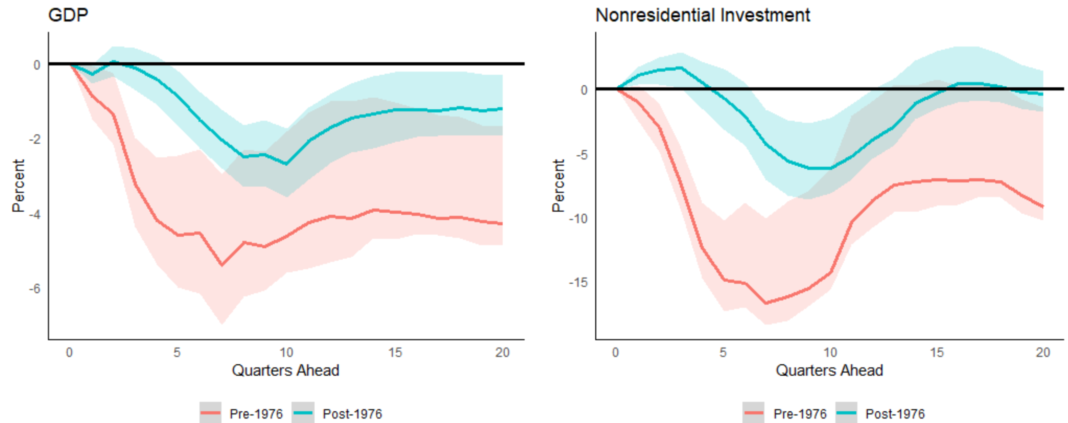
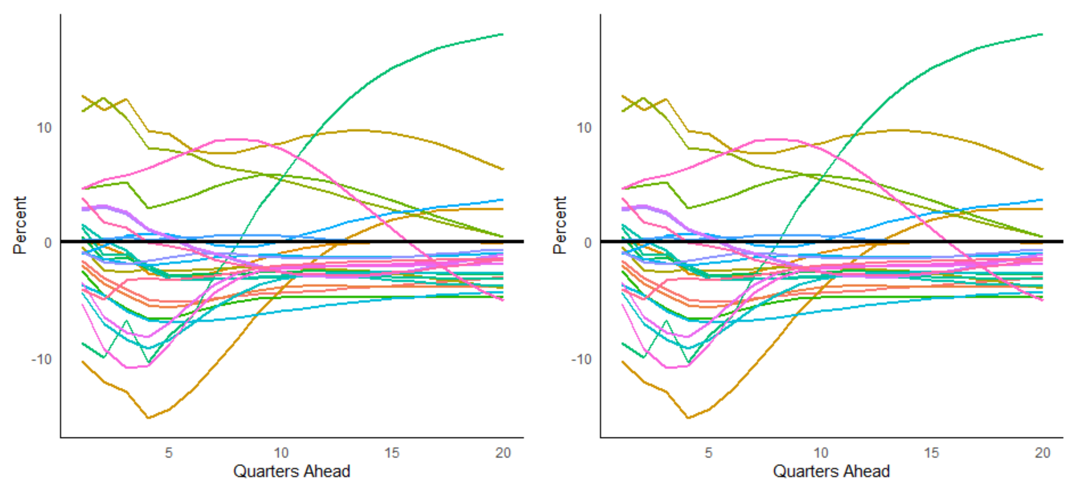


Figure 2: Annual ratio of each major component of nonresidential investment to total nonresidential investment. For each of structures, equipment, and intellectual property respectively, computed as NIPA Table 1.1.5 Line 10, Line 11, and Line 12 divided by Line 9.



(a) GDP **(b)** Nonresidential Investment

Figure 3: Impulse responses of GDP and nonresidential investment to a Romer-Romer shock split into pre-1980 and post-1980 samples. Data taken directly from appendix of Romer and Romer (2010). The split-sample output response is estimated using separate two-variable VARs split in 1976Q1 with the tax shock ordered first, while the split sample investment response is estimated using a three-variable VAR with the shock ordered first followed by logs of output and investment. 68% confidence interval reported.



(a) PI Shock **(b)** CI Shock

Figure 4: Impulse responses of components of fixed investment to a Mertens-Ravn personal income and corporate income shock (NIPA Table 5.3.3. Lines 1-26). Responses are to a one percentage point in the average personal income tax rate and to a one percentage point increase in the average corporate income tax rate. Impulses responses are estimated using a seven variable proxy SVAR. PI shocks are ordered first for PI shocks, while CI shocks are ordered first for CI impulses. The variable used are the average corporate income tax rate, average personal income tax rate, government spending, real GDP, federal debt, the investment variable, and the corporate income tax base. Except for the rate variables, all others are log-transformed. 68% confidence interval reported.

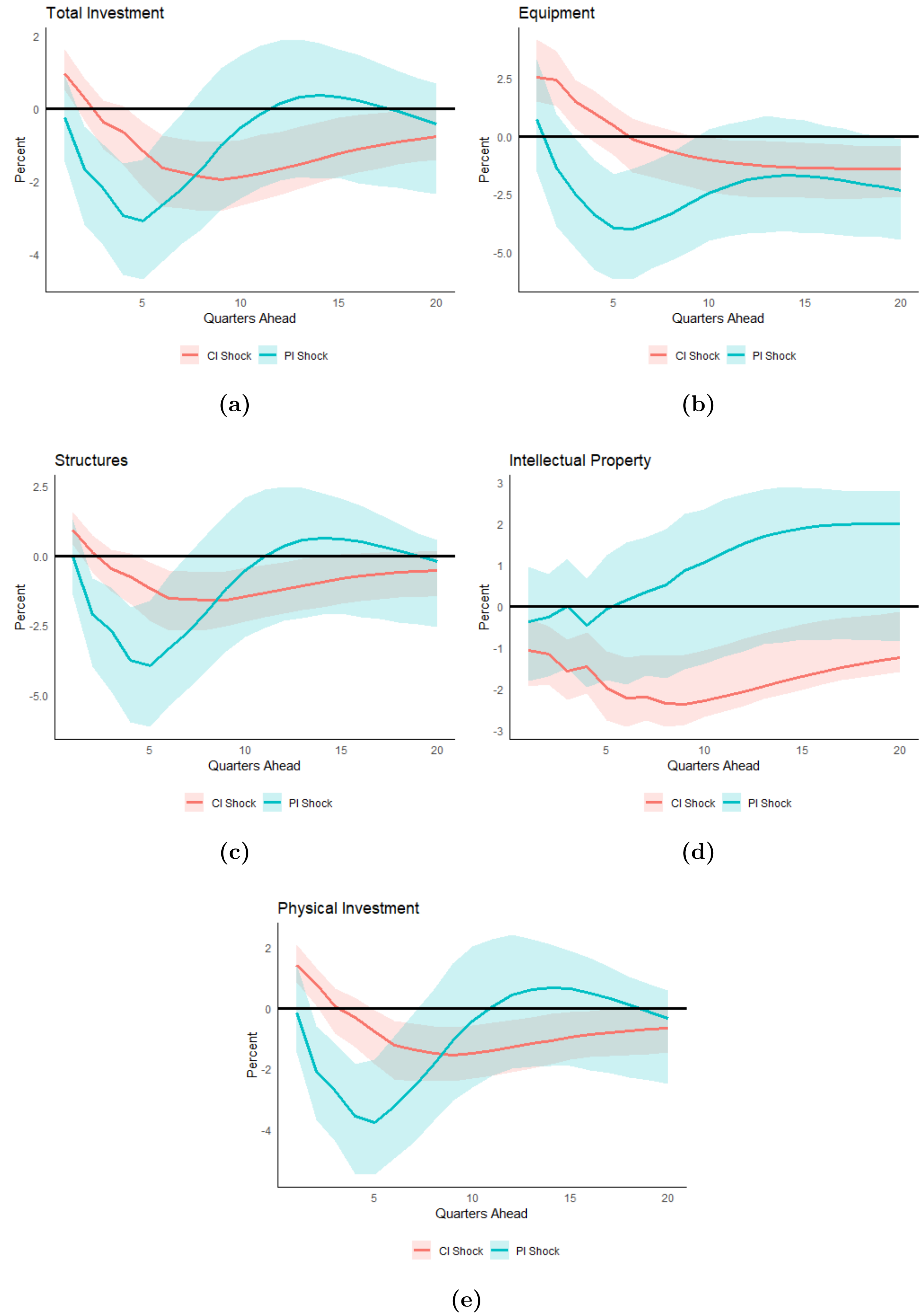


Figure 5: Impulse responses to Mertens-Ravn tax shocks by corporate legal form (including s- and c-corporations) from 1953Q1-2007Q1. Construction of quarterly data is detailed in Appendix C. 68% confidence interval reported.

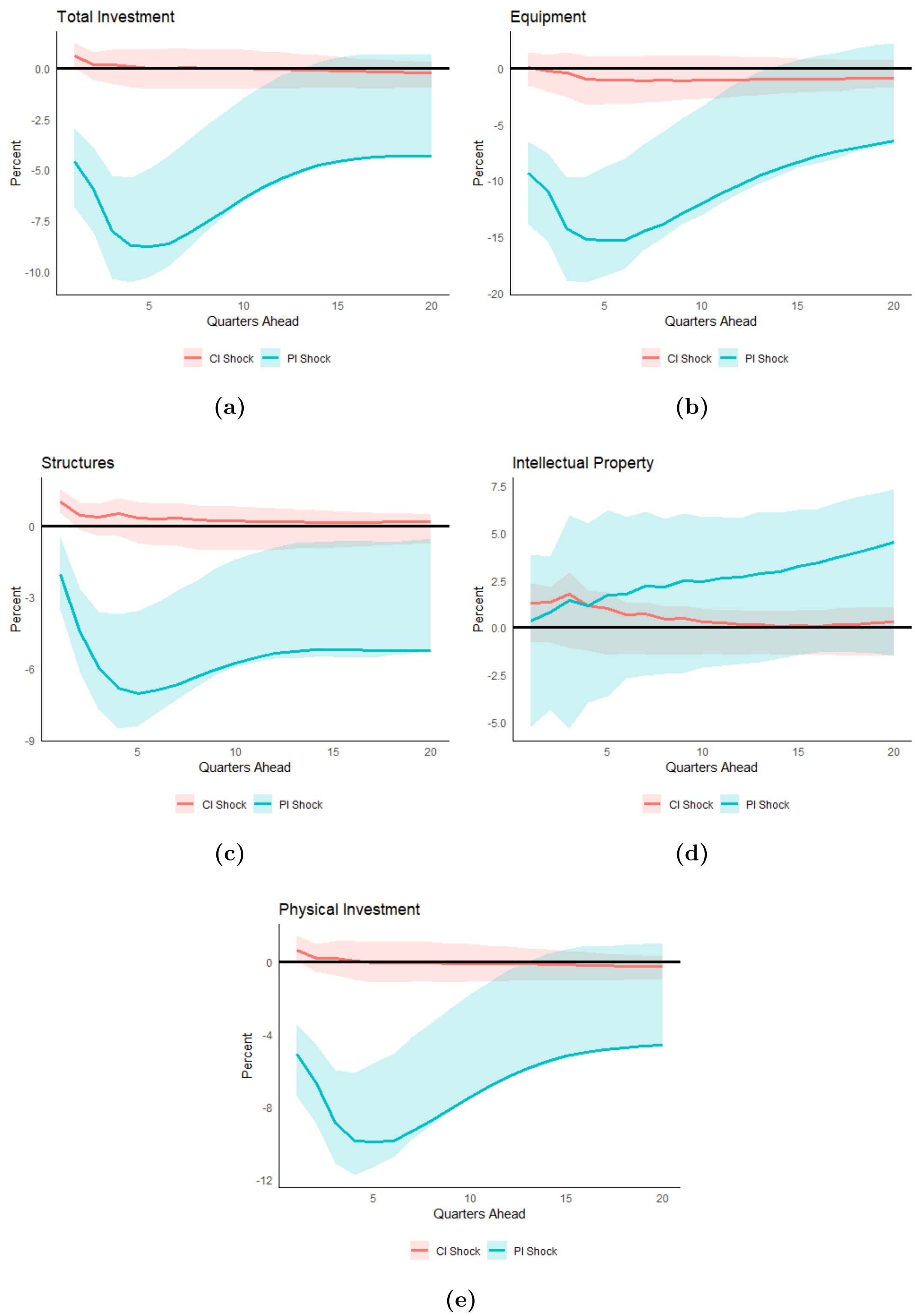
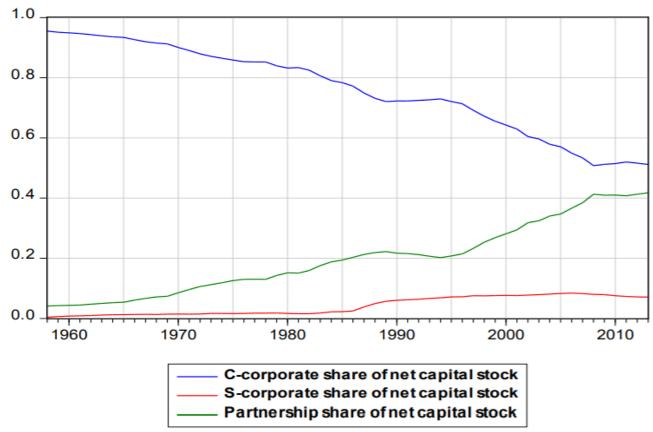
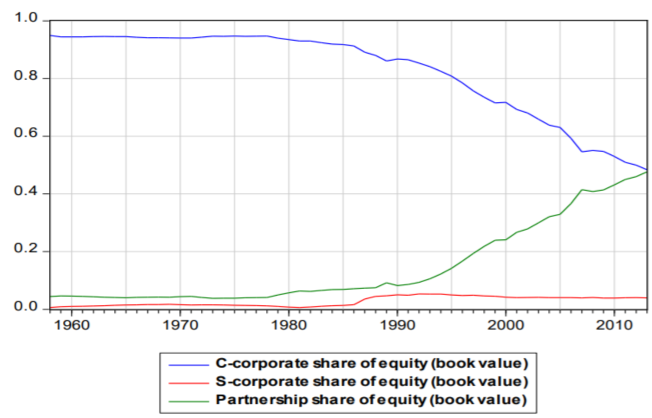


Figure 6: Impulse responses to Mertens-Ravn tax shocks by non-corporate legal form from 1960Q1-2007Q1. Construction of quarterly data is detailed in Appendix C. 68% confidence interval reported.



(a) Barro and Wheaton (2019) Figure 7: Share of Net Capital Stock



(b) Barro and Wheaton (2019) Figure 8: Share of Book Equity

Figure 7: Figures taken from Barro and Wheaton (2019).

	$\log(I_{i,t})$					
	(1)	(2)	(3)	(4)	(5)	(6)
ΔT_{t-1}	-1.25^{***}	-0.84^{*}	-0.97^{**}	-1.24^{**}	-1.79^{***}	-0.96
	(0.43)	(0.45)	(0.46)	(0.54)	(0.51)	(0.59)
ΔT_{t-1}:Big Firm		-2.54^{*}				
		(1.42)				
ΔT_{t-1}:High Leverage			-1.98^{*}			
			(1.33)			
ΔT_{t-1}:Young Firm				0.218		
				(0.87)		
ΔT_{t-1}:HP Con.					1.68^{*}	
					(0.91)	
ΔT_{t-1}:Div. Payer						-1.61^{*}
						(0.82)
Sector Dummies?	No	No	No	No	No	No
Individual Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Quarterly Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls?	No	No	No	No	No	No
Observations	82,827	82,827	82,827	82,827	82,826	82,827
No. Firms	2726	2726	2726	2726	2726	2726
R^2	0.011	0.023	0.011	0.034	0.030	0.024
Adjusted R^2	-0.022	-0.010	-0.022	0.001	-0.003	-0.009

Note: Reported coefficients are for dummy interactions with the tax shock.
$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 1: Panel regression results without controls. For ease of interpretation, coefficients are multiplied by 100 so that the interpretation is that for a one percentage point increase in the average corporate tax rate, investment changes by X%. The specification is as in eqn. 6.

	log($I_{i,t}$)					
	(1)	(2)	(3)	(4)	(5)	(6)
ΔT_{t-1}	-1.0^{**}	-0.71^{*}	-0.6	-1.5^{***}	-1.67^{***}	-0.6
	(0.41)	(0.43)	(0.43)	(0.51)	(0.48)	(0.57)
ΔT_{t-1}:Big Firm		-2.35^{*}				
		(1.33)				
ΔT_{t-1}:High Leverage			-2.69^{**}			
			(1.27)			
ΔT_{t-1}:Young Firm				1.15		
				(0.83)		
ΔT_{t-1}:HP Con.					1.82^{**}	
					(0.87)	
ΔT_{t-1}:Div. Payer						-1.57^{**}
						(0.78)
Sector Dummies?	No	No	No	No	No	No
Individual Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Quarterly Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	82,827	82,827	82,827	82,827	82,826	82,827
No. Firms	2726	2726	2726	2726	2726	2726
R^2	0.15	0.15	0.15	0.16	0.15	0.15
Adjusted R^2	0.12	0.12	0.12	0.13	0.12	0.12

Note: Reported coefficients are for dummy interactions with the tax shock.
$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 2: Panel regression results with firm controls. For ease of interpretation, coefficients are multiplied by 100 so that the interpretation is that for a one percentage point increase in the average corporate tax rate, investment changes by X%. The specification is as in eqn. 7.

	Dependent Variable: Long-term Leverage										
		Leverage		Size		Dividend Status		HP Con.		Age	
	Baseline	High	Low	Big	Small	Payer	Non-Payer	Con.	Uncon.	Young	Old
ΔT^{+}_{t-1}	0.0042	0.0509	-0.0030	-0.0129	0.0055	0.0125	0.0035	0.0004	0.0040	-0.0013	0.0047
	(0.0022)*	(0.050)***	(0.0126)***	(0.0071)*	(0.0024)**	(0.0097)	(0.0023)	(0.003)	(0.0031)	(0.0034)	(0.0044)
ROA_{t-1}	-0.0103	-0.0103		-0.0103		-0.0103		-0.0103		-0.0103	
	(0.0006)***	(0.0006)***		(0.0006)***		(0.0006)***		(0.0006)***		(0.0006)***	
Int. Intensity_{t-1}	-0.0882	-0.0880		-0.0882		-0.0882		-0.0883		-0.0882	
	(0.0208)***	(0.0208)***		(0.0208)***		(0.0208)***		(0.0208)***		(0.0208)***	
Size_{t-1}	0.0274	0.0274		0.0274		0.0274		0.0274		0.0274	
	(0.0028)***	(0.0028)***		(0.0028)***		(0.0028)***		(0.0028)***		(0.0028)***	
Tobin's Q_{t-1}	-0.0137	-0.0136		-0.0136		-0.0136		-0.0136		-0.0136	
	(0.0013)***	(0.0013)***		(0.0013)***		(0.0013)***		(0.0013)***		(0.0013)***	
No. Observations	82756	82756	82756	82756	82756	82756	82756	82755	82755	82756	82756
No. Firms	2726	2726	2726	2726	2726	2726	2726	2726	2726	2726	2726

Note: Reported coefficients are for dummy interactions with the tax shock.

$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table 3: Results of test for financial constraint. The exact specification is given in eqn. 8. Firm fixed effects are used, so coefficients are driven by within-firm variation over time.

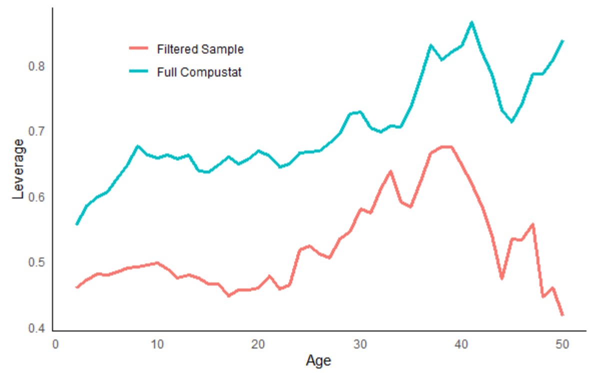


Figure 8: Mean leverage by age from Compustat. The filtered sample is the sample used for our analysis, while full Compustat is all of quarterly Compustat after filtering out financial services, firms not incorporated in the U.S., firms without tickers, firms without sales, and firms with positive assets and sales.

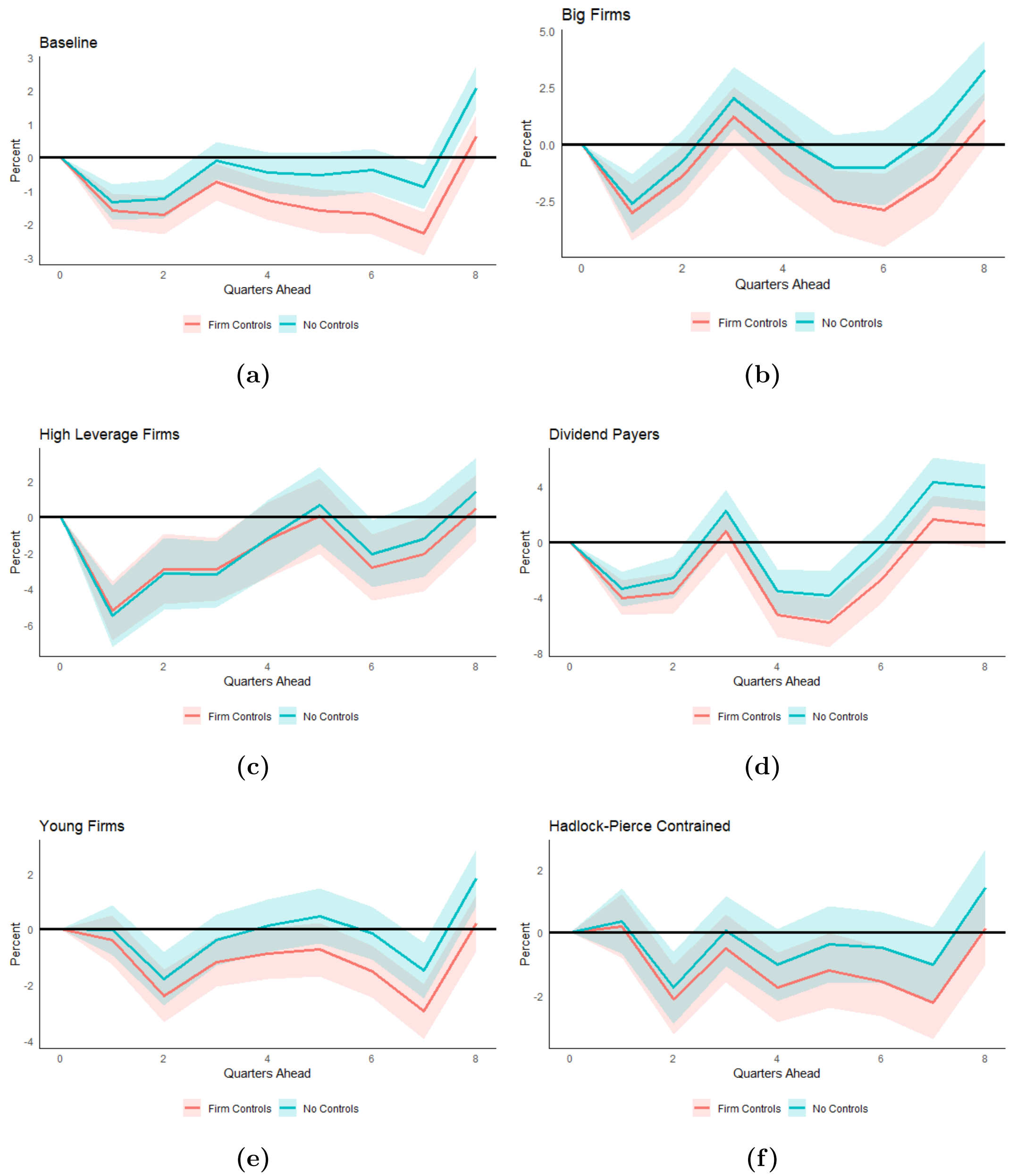


Figure 9: Impulse responses of firms to a Mertens-Ravn corporate income tax shock. Grouped by financial constraint indicator. 68% confidence interval reported.

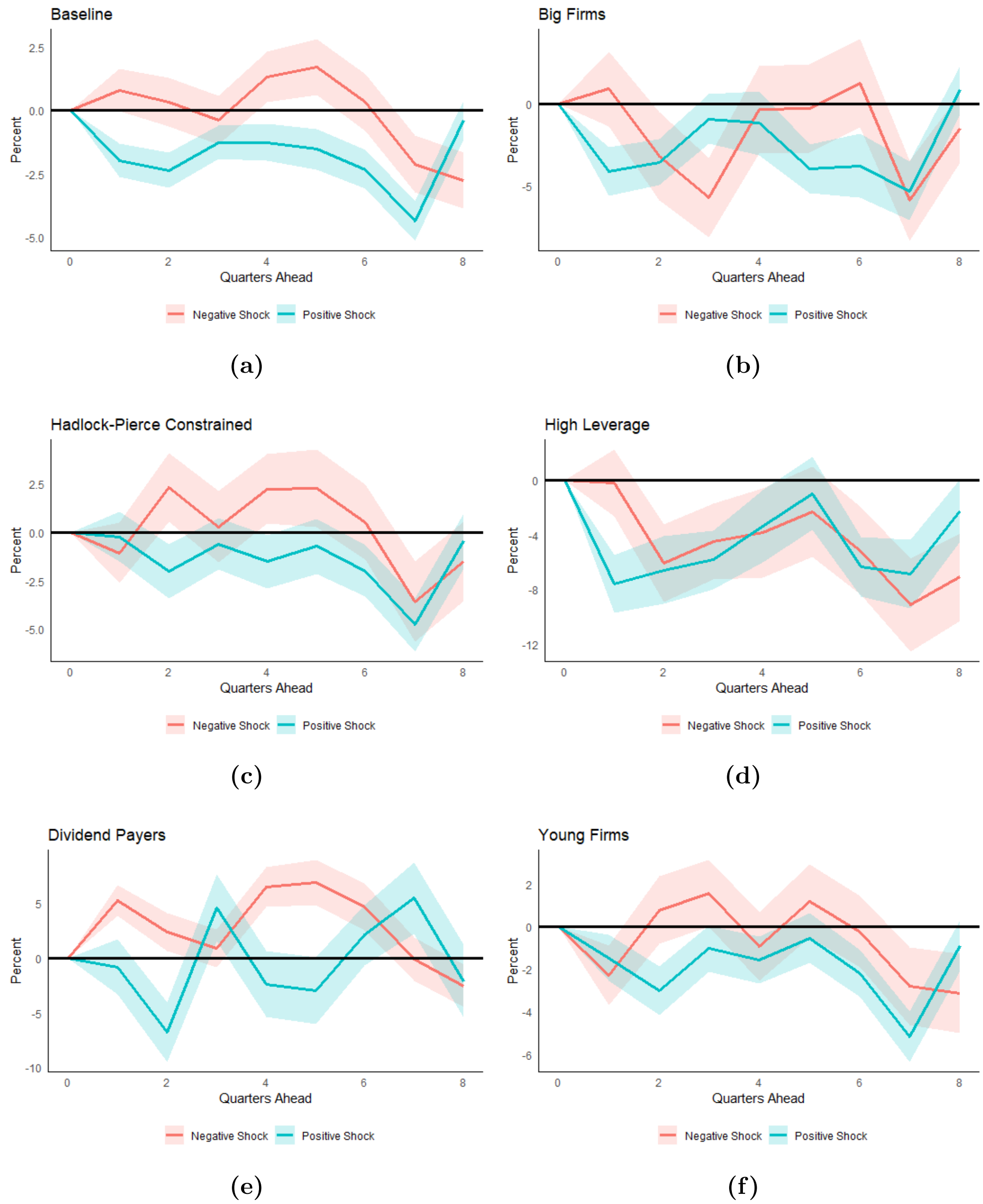


Figure 10: Impulse responses of firms to a positive Mertens-Ravn corporate income tax shock. Interpretation for a positive shock is in terms of a 1% increase in the average corporate income tax rate isolating for positive shocks, while it is the mirror image for negative shocks. 68% confidence interval reported.

B Data Construction

Firm-level data is sourced from Compustat, which in principle includes all public firms in the United States. There are five reasons for using this dataset rather than another (e.g., the U.S. Census Bureau's Quarterly

Financial Report). First, quarterly data is necessary to make a comparison to aggregate measures and to account for the fact that shocks are quarterly rather than annual. Second, it is a long panel, something which allows us to exploit individual fixed effects. Third, it measures critical balance sheet variables for firms that are important determinants of investment. Fourth, Compustat has a broad array of firms from every sector rather than exclusive or heavy concentration in one particular industry. Finally, Compustat's broad array of variables allows us to more easily investigate firm-level heterogeneity than other, more limited datasets. No other dataset has these key characteristics.[13]

Here, we describe the firm-level variables used in the analysis. Note that, following Ottonello and Winberry (2018), the definitions of the variables used are typical in the literature and are based on Compustat data. Each item taken from Compustat has been deflated by the GVA deflator for non-financial corporate business using 2012 as the base year. The GVA quantity deflator is calculated as NIPA Table 1.14 Line 17 divided by Table 1.14 Line 41.

Variables

Continuous Variables

1. *Investment*: Our main measure of investment is the natural logarithm of gross investment after adjusting for inflation. This is defined as

$$I_{i,t} = K_{i,t} - K_{i,t-1} + \delta_{i,t} K_{i,t-1},$$

   where the net capital stock $K_{i,t}$ is net property, plant, and equipment (PPENTQ, Compustat item 42) and depreciation is $\delta_{i,t} K_{i,t-1}$ is depreciation (DPQ, Compustat item 5) less annual amortization (AM, Compustat item 65). Amortization is estimated in each quarter by dividing the annual measure by four it it exists and setting it to zero otherwise.

2. *Tax Shocks*: Our measure of a tax shock is taken from the vector of exogenous shocks to corporate taxation developed by Mertens and Ravn (2013) and Romer and Romer (2010). The shock measure used to replicate Romer and Romer (2010) and extend their analysis is their EXOGENRRATIO. The Mertens-Ravn shocks for personal income tax and corporate income tax are T_PI and T_CI. of For each quarter, the value is set to zero if there is no shock. In quarters with a shock, we take the expected value of the tax change divided by corporate profits. Each measure is taken from the appendix of their respective papers.

3. *Leverage*: defined as the ratio of total debt to stockholder's equity (seqq, item 60). Total debt is defined as the sum of long-term financial debt (dlttq, item 51) and financial debt in current liabilities (dlcq, item 45). Each item is adjusted for inflation using the GVA quantity deflator.

4. *Tobin's Q*: Taken as the sum of total assets and market capitalization less book value divided by total assets. Market capitalization is defined as the product of quarterly price per share (prccq) and common shares outstanding (cshoq, item 61). Book value is measured as common/ordinary equity (ceqq, item 59). Each item is adjusted for inflation using the GVA quantity deflator.

[13] The related study of Eskandari and Zamanian (2020) utilizes the QFR but the limitations of the QFR prevent them from examining many sources of heterogeneity.

5. *Net leverage*: Total debt less net current assets divided by total assets. Net current assets is defined as current assets (item 40) minus current liabilities (item 49). Each item is adjusted for inflation using the GVA quantity deflator (NIPA Table 1.1.3 Line 1).
6. *Real sales growth*: Measured as log differences in sales (saleq, Compustat item 2). Each item is adjusted for inflation using the GVA quantity deflator.
7. *Size*: We use the natural logarithm of total assets (atq, Compustat item 44) as a proxy for firm size.
8. *Age*: A firm's age is calculated in the following order of priorities:
   (a) If a founding date from Loughran and Ritter (2004) exists, use that as the year of origination.
   (b) Otherwise, use the first year a firm appears in Compustat with a final stock price.
9. *Liquidity*: Defined as the ratio of cash and short-term investments (cheq, item 36) to total assets (atq). Each item is adjusted for inflation using the GVA quantity deflator.
10. *Debt*: Sum of current debt (dlcq, item 45) and long-term debt (dlttq, item 51). Log-transformed.
11. *Intangible Intensity*: Intangible capital from Peters and Taylor (2017) scaled by the sum of tangible capital (ppentq) and intangible capital.
12. *Long-term Leverage*: Long-term debt (dlttq) scaled by shareholder equity (seqq).
13. *ROA*: Net income (piq, item 23) divided by total assets (atq) multiplied by 100.

Variable	N	Mean	S.D.	Pct. 10	Median	Pct. 90
Gross Investment (log)	82828	1.42	2.28	-1.53	1.46	4.37
Gross Investment Rate	82828	0.0968	0.156	0.0178	0.0606	0.195
Net Investment Rate	82828	0.0343	0.140	-0.0302	0.0114	0.107
Leverage	82828	0.505	0.671	0	0.312	1.20
Tobin's Q	82828	1.91	1.51	0.923	1.48	3.30
Net Leverage	82828	-0.110	0.319	-0.539	-0.103	0.315
Total Assets (log)	82828	5.73	1.95	3.25	5.64	8.31
Sales (log)	82828	4.57	1.97	2.02	4.54	7.17
Sales Growth	82828	0.0255	0.170	-0.151	0.0258	0.20
Age	82828	19.6	14.3	6	17	34
Liquidity	82828	0.134	0.163	0.007	0.0652	0.371
Debt (log)	82828	3.52	2.51	0	3.57	6.90
Intangible Intensity	82828	0.594	0.273	0.142	0.659	0.905
Long-term Leverage	82828	0.405	0.597	0	0.214	1.01
ROA	82757	2.39	4.07	-0.818	2.59	6.09

Table 4: Summary Statistics for Continuous Variables

Categorical Variables

1. *Sectoral Dummies*: We consider 2-digit NAICS codes excluding NAICS 22 (utilities). NAICS 52 (finance and insurance), and NAICS 53 (real estate and rental and leasing). Using the Census Bureau's 1987-1992 SIC-NAICS crosswalk, we convert 4-digit SIC codes to their 2-digit NAICS counterparts for years preceding 1985.
2. *Big Firm Dummy*: We assign a firm-quarter dummy value of one if a firm has total assets in the top decile of firms in a particular quarter.
3. *High Leverage*: Defined as taking value 1 if leverage is greater than one and zero otherwise.
4. *Quarterly Dummies*: Due to observed intrayear investment seasonality and a non-random distribution of tax shocks, quarterly dummies were added (Q1, Q2, Q3, Q4). Q1 served as the reference value in regressions.
5. *Dividend Payer*: A firm is categorized as a dividend payer if total dividends (backed out from year-to-date dividends, dvy) less preferred dividends (backed out from year-to-date preferred dividends, dvpy) is greater than zero.
6. *Hadlock-Pierce Constrained*: A firm is defined as Hadlock-Pierce constrained if, following the literature standard, a firm-quarter falls in the top tercile of the Hadlock-Pierce index for that quarter (Hadlock and Pierce, 2010). The Hadlock-Pierce index is calculated using the coefficients of Hadlock and Pierce (2010): $HPIndex_{i,t} = -0.737\log(atq) + 0.043\left(\log(atq)\right)^2 - 0.04(age)$. Following convention, total assets are capped at \$4.5B and age is capped at 37.
7. *Young Firm*: Following Cloyne et al. (2018), a firm is categorized as young if it is younger than 15 and old otherwise.

Variable	N	Mean	S.D.	Min	Max
Big Firm	82828	0.101	0.301	0	1
High Leverage	82828	0.14	0.35	0	1
Dividend Payer	82828	0.24	0.43	0	1
H-P Constrained	82828	0.34	0.47	0	1
Young Firm	82828	0.42	0.49	0	1

Table 5: Summary Statistics for Categorical Variables

Sample Selection

Largely following the procedure of Ottonello and Winberry (2018), we selected our sample as follows:

1. We excluded firms in utilities, finance, insurance, and real estate (NAICS codes 22, 52, and 53).
2. Excluded firms not incorporated in the United States
3. Interpolate ppentq, leverage, dpq, atq, net current assets, liquidity and Tobin's Q if there is only one missing observation

4. Firm-quarter observations that fail to satisfy the following conditions:
   (a) Positive capital
   (b) Positive assets
   (c) Positive sales
   (d) Positive liquidity
   (e) Leverage between zero and ten (inclusive)
   (f) Net current assets as a share of total assets less than 10 and greater than -10.
   (g) Quarterly sales growth between -1 and 1
5. Because we were working with investment rates and investment levels, followed a winsorization procedure for obtaining investment figures:
   - Winsorize ppentq, dpq, and am at the .005 and .995 percentiles.
   - Compute net investment and depreciation, then winsorize at the .005 and .995 percentiles, then compute gross investment by summing net investment and depreciation.
   - Obtain investment rates (gross and net) by dividing by lagged ppentq, then winsorize once more at the .005 and .995 percentiles.
6. Exclude firms that do not have at least twenty consecutive quarters after carrying out filtering procedures. It is possible for firms to have separate 20+ quarter spells. For example, Apple may be present from 1984q1-1995q2 and from 1997q2-2005q1. In such cases, we treat these as separate firms for the purposes of our panel regressions and Jorda local projections.

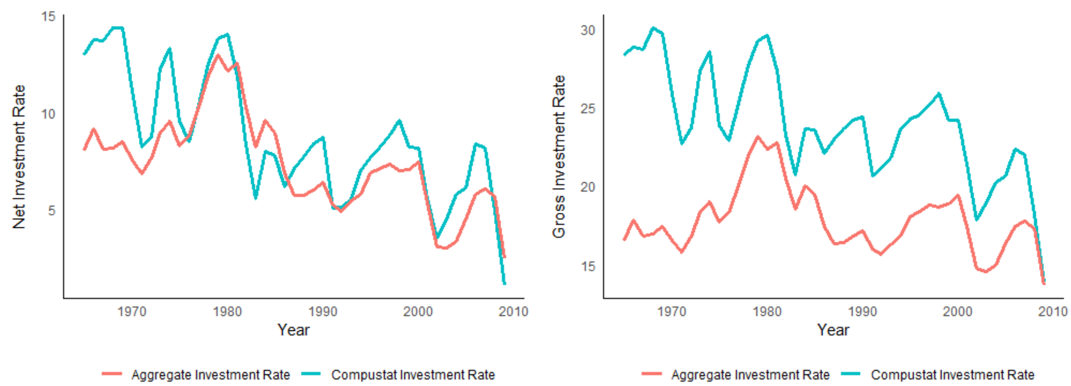
B.1 Matching Aggregate and Firm-Level Data

Because we are interested in drawing inferences about aggregate corporate responses to tax shocks from firm-level analysis, it is crucial that our Compustat series is at least somewhat representative of aggregate data. This is useful not only because we will then be able to estimate the causal effects of a tax shock, but because it will enable us to make broad comparisons with the existing aggregate literature. We compute net investment rates for the aggregate and firm-level data. An investment rate is used rather than a level because Compustat is an incomplete sample of publicly traded firms and changes in aggregate Compustat investment tend to reflect changes in entry and exit of firms in addition to changes in investment by the same firms. We focus in this case on the net investment rate rather than the gross investment rate for two reasons. First, depreciation is calculated differently by the BEA and firms. Second, fixating on the gross investment rate would ignore the changing composition of depreciation over time (Feldstein, 1983; Gutiérrez and Philippon, 2017).

We use Fixed Asset Table 4.3, 4.6, and 4.7 Lines 37-40 to construct our net investment rates. Figure 11a shows the median annual net investment rate from Compustat and the net investment rate computed from Fixed Asset Table 4.7 Line 37.[14] The correlation between these series is 71%. We also show the annual

[14]The net investment rate is calculated as $I_t/K_{t-1} \equiv \frac{K_t - K_{t-1}}{K_{t-1}}$. Since Table 4.3 is historical cost, we use the nominal version of the net investment rate in Compustated, calculated as $\frac{K_t - K_{t-1}}{K_{t-1}}$, where $K_t \equiv K_{int} + PPENT_t$. K_{int} is taken from Peters and Taylor (2017).

gross investment rate for clarity; it has a correlation of 60%. The quarterly series is not quite as correlated but still reasonably follows the same trend; they have a correlation of 38%. Note that the quarterly series is only *physical* investment, which means that we sum Lines 38 and 39 of Fixed Asset Table 4.3 to get a measure of aggregate physical capital.



(a) Net Investment Rates **(b)** Gross Investment Rates

Figure 11: Matched aggregate and firm-level investment rates.

C Estimates of Quarterly Fixed Asset Data

Fixed Asset data from the Bureau of Economic Analysis is published on an annual basis, which presents a clear issue for matching aggregate fixed asset data with firm-level data at a quarterly frequency. The key variables in the firm-level analysis are the net capital stock, gross investment, and depreciation. To obtain estimates of these at a quarterly frequency for the aggregate fixed asset data, I follow the methodology of McGrattan (2020). Because the law of motion of capital necessarily holds, i.e., $K_{t+1} = I_t + (1 - \delta_t)K_t$, it is required only to obtain estimates of two variables and the third follows. There is a categorical difference in the estimation procedure for a stock variable (K_t) versus a flow variable (I_t and $\delta_t K_t$). Whereas stock variables published at an annual frequency contain sufficient information about the timing of the value of the variable, flow variables do not. The value of the capital stock published at the end of year t gives the value of the capital stock at the end of Q4, but the value of a flow variable published at the end of period t contains no information about *when* the flows were initiated. In the extreme, it could be that all investment published by the BEA occurred in Q1 or Q4 and there is little reason to assume lack of seasonality.

To estimate either a stock or a flow, the first step is the same. Let Z_t be an annual variable, whether it is the capital stock, investment, or depreciation. Select X_t variables published from other sources available at quarterly frequency and used to make inferences about the quarterly value of Z_t, which I call $\hat{Z}_t$. The first step is to detrend all time series Z_t and X_t using the Hodrick-Prescott filter with smoothing parameter $\lambda = 1600$ for the quarterly series and $\lambda = 100$ for annual series. Then, to obtain quarterly estimates of $\hat{Z}_t$, we estimate A and B in the following state space system via maximum likelihood:

$$\begin{aligned} x_{t+1} &= Ax_t + B\epsilon_{t+1} \\ y_t &= Cx_t, \end{aligned} \tag{9}$$

where $x_t = \left[X_t, \hat{Z}_t, X_{t-1}, \hat{Z}_{t-2}, X_{t-2}, \hat{Z}_{t-3}, X_{t-3}, \hat{Z}_{t-1}, X_{t-4}, \hat{Z}_{t-4}\right]^T$, $y_t = \left[X_t, Z_t\right]^T$, and ϵ_t are normally distributed shocks. Coefficients are given by

$$A = \begin{bmatrix} a_1 & a_2 & \dots & a_j \\ I & 0 & \dots & 0 \\ 0 & I & \dots & 0 \\ \vdots & \vdots & \vdots & \vdots \\ 0 & 0 & \dots & 0 \end{bmatrix}, \; B = \begin{bmatrix} b \\ 0 \\ 0 \\ \vdots \\ 0 \end{bmatrix}, \; C = I \tag{10}$$

With estimates for $\left(\hat{A}, \hat{B}\right)$, it becomes possible to estimate forecasts $\hat{Z}_t = \mathbb{E}[Z_t|y_1, \dots, y_T]$ of annual data at a quarterly frequency by applying a Kalman smoother and then adding back the low-frequency Hodrick-Prescott trend to the estimated time series. At this point, the estimation procedure for the net capital stock is complete. To finish estimating flow variables, I add another step. Because I have estimated the flows as if they are stocks and therefore obtained estimates of depreciation and investment many magnitudes greater than would make sense given the value of the annual variable, I group estimated gross investment by year, divide each quarterly estimate by the sum of quarterly estimates, and multiply the result by the actual annual gross investment value. For example, suppose the published value of gross investment in 2010 is \$800B and I estimate that gross investment in 2010Q1 is \$150B, 2010Q2 is \$200B, 2010Q3 is \$250B, and 2010Q4 is \$200B. Then to estimate the flow value of gross investment undertaken in 2010Q1, I would take $\left(\frac{\$150B}{\$150B+\$220B+\$250B+\$200B}\right) \times \$800B = \$146.3B$, and so on for each year and set of four quarters. The estimate for depreciation then becomes mechanical:

$$\hat{D}_t = \hat{I}_t - \left(\hat{K}_t - \hat{K}_{t-1}\right), \tag{11}$$

where D is total depreciation in period t. Following this procedure produces estimates that are reasonable given the available data, particularly stocks. In Figure 12, I show the percent deviation between my procedure and what would happen if I employed a naive linear interpolation between annual figures for the total net capital stock of non-financial corporate institutions (Fixed Asset Table 4.7, Line 37). Compared to linear interpolation, my method captures much of the variability that would otherwise be missed.

I utilize the procedure outlined above to obtain quarterly estimates of Fixed Asset Tables 4.3, 4,6, and 4.7 Lines 37-44. The variables used to estimate series directly using the Kalman smoother method are given in Table 6. Table 4.7 Line 37 is the sum of 4.7 Lines 38-40 and Table 4.7 Line 41 is the sum of 4.7 Lines 42-44. Table 4.6 Lines 37-40 are estimated indirectly as in equation 11. Estimates for non-financial corporate entities begin in 1953 and estimates for non-financial non-corporate entities begin in 1960. Before getting impulse responses with the Mertens-Ravn proxy SVAR, I deflated each gross investment series using the GVA deflator as defined above.

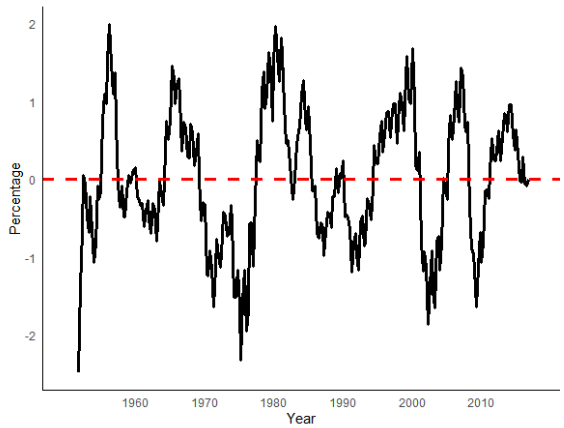


Figure 12: Percent deviation between my method and linear interpolation for estimation of Fixed Asset Table 4.7, Line 37.

Table	Line	Variables
4.3	37	TTAATASHCBSHNNCB, NCBWSPQ027S, NCBCFCQ027S (all FOF)
	38	ESATASHCBSNNCB, NCBWSPQ027S, NCBCFCQ027S (all FOF)
	39	HCVSNNWHCBSNNCB, NCBWSPQ027S, NCBCFCQ027S (all FOF)
	40	NCBNIPPHCB, NCBWSPQ027S, NCBCFCQ027S (all FOF)
4.7	38	NCBWSPQ027S (FOF), ESATASHCBSNNCB (FD, FOF), 1.14.19 (NIPA), 1.5.5.30 (NIPA)
	39	NCBWSPQ027S, HCVSNNWHCBSNNCB (FOF, FD), 1.14.19 (NIPA), 1.5.5.30 (NIPA)
	40	NCBWSPQ027S, NCBNIPPHCB (FOF, FD), 1.14.19 (NIPA), 1.5.5.37 (NIPA)
	42	NESABSNNB (FOF, FD), 1.5.5.30 (NIPA), NNBNVAQ027S (FOF)
	43	RCVSNWBSNNB (FOF, FD), 1.5.5.29 (NIPA), NNBNVAQ027S (FOF)
	44	NNBNIPPCCB (FOF, FD), 1.5.5.37 (NIPA), NNBNVAQ027S (FOF)

Table 6: Variables used to construct quarterly estimates of annual fixed asset series. Source is in parentheses. FOF refers to Flow of Funds from the Federal Reserve and NIPA refers to the National Income and Product Accounts published by the Bureau of Economic Analysis. FD means the variable is first-differenced.