Aggregation and Tax Shocks: Lessons from Firm-Level Data

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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 components of investment. Second, I show that the firm-level corporate investment response to a 1% 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, intangible-intensive firms, and multinational firms tend to exhibit little response to tax shocks, while large firms, dividend-payers, highly productive firms, and highly leveraged firms all react significantly. 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 firms depending on various time-invariant characteristics. For example, the investment response of a highly productive firm may differ strongly from one which is financially constrained. 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 two fundamental issues with the current literature. First, if the tax shock is excessively aggregated, then the response of even slightly disaggregated components of investment yield theoretically nonsensical or insignificant results.[1] Second, if the response variable is too aggregated, then the reported results will be irrelevant if the share of the components of the response variable change over time and respond differently to a tax shock. 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] This applies to much of the most

[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 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

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). I use the work of Romer and Romer (2010) to illustrate my 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. 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 more immediate, shallower, and shorter-lived than aggregate investment, 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. On its own, this result indicates that there is likely a great deal of heterogeneity between types of firms in investment response.

In order to examine heterogeneity, I use dummies that can be broadly lumped into three different categories: indicators of financial constraint, indicators of intangible-intensive firms, and indicators of firm type (multinationals and highly productive firms). 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

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.

constrained firms, which by definition face a steeply increasing marginal cost curve for investment. Moreover, I show that the tax treatment of intangibles leads to a relatively weak investment response from intangible-intensive firms and I show that firms with high organizational capital tend to respond significantly less than their counterparts. Moreover, multinationals respond insignificantly. Moving forward, these points will be critical for economists and policymakers to consider given the increasing prevalence of intangible-intensive firms and the dominance of multinationals; their rise dampens the aggregate response of tax changes compared to the past. Finally, I show that the most efficient firms respond significantly to negative shocks, reaching peak impact of 12% after five quarters, and insignificantly to positive shocks.

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

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

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 approach, their methodological approach to studying the question is uniform insofar as they all aggregate shocks and response variables over time.[4] 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 firm size and implied credit constraints. My purpose is different. I seek not merely to establish an empirical relationship between tax shocks and firm investment, but show why an excessive focus on aggregates may lead to analytical issues. Additionally, I seek to establish and investigate heterogeneity between different types of firms across many domains rather than just a proxy for financial constraint.

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)

[4] Blanchard and Perotti (2002) is a slight exception; they analyze particular tax events as well as the aggregate effect.

likewise emphasises 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

To illustrate the aggregation issue in the prevailing literature on tax shocks, I use Romer and Romer (2010) as an example because their approach is simplest and therefore easiest to replicate and illustrate. However, the problem is general; overaggregation is endemic in these studies.

2.1 Overaggregation in Shock Variable

Both Romer and Romer (2010) and Mertens and Ravn (2013) (henceforth R-R and M-R, respectively) use the narrative record to identify a vector of exogenous tax shocks for the postwar period leading up to just before the start of the 2008 financial crisis. My concern with both vectors (though to a lesser extent the M-R vector) is that the level of aggregation necessary to construct them masks substantial heterogeneity within actual changes in tax law.

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.

One way to illustrate the problem with aggregated shocks is to show that atheoretical results occur at even minimal levels of disaggregation in the components of output and investment. If the disaggregated response to a shock is nonsensical relative to the aggregate response, then it is suggestive of excessive aggregation. To start, consider the basic specification of R-R without controls:

$$\Delta Y_t = \alpha + \sum_{i=0}^{12} \beta_i \Delta T_{t-i} + \epsilon_t \tag{1}$$

Considering the initial purpose of the R-R study, the dependent variable was initially specified as the growth rate of real GDP. Hence the vector of tax shocks, measured in terms of the impact on current tax liabilities at the level of current GDP, is plausibly orthogonal to the other factors affecting output growth. If so, then it is plausible to assume that the shocks are orthogonal to factors affecting the subcomponents of GDP, including investment. If I instead replace log output as the dependent variable with a component of log investment, few coefficients have the right sign and few are significant. If the dependent variable is in terms of levels rather than growth rates, no coefficients are significant and few have the correct sign.[5]

A different test is to see how the shock variable behaves with disaggregated components of investment in a three-variable VAR with the shock ordered first followed by log real GDP and a log component of real investment. This setup mimics the R-R VAR specification, though I use the most recent update of NIPA rather than the 2008 vintage so that I can also look at intellectual property.[6] Running this specification with structures, equipment, and intellectual property gives another curious result: both structures and intellectual property, which together make up approximately 2/3 of nonresidential fixed investment for any given quarter, give results that are highly insignificant.[7]

[Figure 1 here]

These puzzles are of course not conclusive evidence that the shocks are not orthogonal to investment. To the contrary, given the presentation of Romer and Romer (2010), it is highly likely that the shocks *are* orthogonal. The problem may instead be overaggregation; the shock may be relevant for output as a whole but may affect the components of output in different ways.[8] For example, the bulk of a particular tax increase could conceivably come as a personal income tax increase which would have little effect on investment if pass-throughs constitute a small share of total investment. This is the problem that Mertens and Ravn (2013) address in their decomposition of the Romer and Romer (2010) tax shocks. Relative to other aggregate studies, Mertens and Ravn (2013) suffers the least from excessively aggregated shocks; it is the only paper which considers slightly more specified shocks.

[5] See Appendix A Tables 1 and 2 for regression summaries. I utilize the main tax shock measure in the R-R paper, which excludes retroactive components.

[6] The BEA's national income and product accounts underwent a major revision following the publication of Romer and Romer (2010) that recategorized intangible spending as investments rather than intermediate processes. The R-R results are not significantly different following this revision and are available upon request.

[7] See Appendix A for results, in particular Figures 1a, 1b, and 1c. Note that the sign on structures is significantly positive in the first few quarters.

[8] To their credit, Romer and Romer (2010, p. 799) recognize this problem: "[O]ur estimates are not highly precise. The overall estimates of the effects on output are overwhelmingly significant, but the confidence interval is nevertheless substantial. And when I ask narrower questions—such as how a volatile component of output responds to tax changes, or how output behaves following a deficit-driven change—the confidence interval is generally quite wide."

More precisely, the issue is that we cannot adequately measure the response of even minimally disaggregated components of output if the shock variable is not specific enough to the response variable. This overaggregation in the shock variable is not necessarily problematic if one only wishes to study the effect of a tax shock on output. Nevertheless, output is (trivially) the sum of many different variables which potentially respond differently to tax shocks and interest in output is necessarily interest in those components. In that sense, it is difficult to discern which mechanism is most important for movements in output when the components are ignored or given short shrift. Hence even if we want to take the broadest possible view of the effect of tax shocks, we need to have some knowledge of how the components are affected, which is apparently very difficult if the shock variable is too heavily aggregated. Morever, unbiased estimates of the output response to a tax shock are not possible to extract if heterogeneity in the components of investment exhibit time trends, something I examine in the following section.

2.2 Overaggregation in the Response Variable

Tax shocks are one of the main levers through which fiscal policy affects output generally and investment in particular. 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

$$\Psi = (r + \delta) \left(\frac{(1 - \tau_c)(1 - \lambda\tau)}{1 - \tau} \right), \tag{2}$$

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

$$\frac{\tau}{\Psi}\frac{\partial\Psi}{\partial\tau} = \frac{\tau}{1-\tau}\frac{1-\lambda}{1-\lambda\tau}. \tag{3}$$

Given current (and previous) tax law, equation (3) 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 should lead to greater investment and hence greater output investment in physical capital does tend to follow the general theory underlying the idea that a negative tax shock leads to more 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. The difference in response between tangibles and intangibles is non-trivial; intangibles make up a large share of investment (Peters and Taylor, 2017).

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 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-1980 period.

[Figure 2 here.]

Given the rise in intangibles in the post-1980 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 Romer-Romer sample in two and obtain split-sample impulse response functions. With the rise in intangibles as a rival production technology to physical capital and the absolute growing importance of intangibles, it should be the case that the post-1980 sample shows a relatively smaller response to a tax shock. We run this experiment with a three-variable VAR with the Romer-Romer tax shock ordered first, followed by output, and an investment component. This mirrors their specification.

Running this experiment yields several interesting 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-1980 period, only going significantly below zero after 5 quarters with a peak impact of

-2.5% after 10 quarters, whereas the pre-1980 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 similar differences between the pre-1980 and post-1980 periods. Residential investment is a notable exception (see Figure 3d). The pre-1980 IRF behaves contrary to theory, reaching a peak impact of positive 22% after 13 quarters, which again suggests a peculiarity about the nature of the shock variable. The object of greatest interest is the response of nonresidential fixed investment. 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-1980 period should exhibit little response whereas the pre-1980 IRF should have a more persistent, negative, and significant response. This is precisely what happens in Figure 3e, 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-1980 sample demonstrates a substantially more persistent and highly negative response to a tax shock.[9]

[Figure 3e 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. 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. This is particularly true when one considers that the rise of intangible capital has continued unabated since the end date of the Romer-Romer study in 2007.[10] The point is not that these estimates have no utility—it

[9] 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.

[10] It is worth pointing out that the consequences of the rise of intangible capital have not gone unnoticed

is quite useful to have an idea of the average historical effect of a macroeconomic shock—but that these estimates should not in general be used as inputs to theoretical or policy work because they fail to account for underlying trends in the data that bias the aggregate result.[11] Moreover, these results point to cross-sectional heterogeneity across investment type and legal form to a shock as more relevant factors to consider than the overall effect conditional on stationarity in the components of output. In principle, knowledge of these effects combined with knowledge of the relative share of each component can then be used to build up a multiplier.

2.3 Aggregates from Disaggregates

A key question is how to construct an unbiased estimate of a tax shock. There are essentially two statistical methods for doing so. One is 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. The effect of a proposed tax change can then be obtained by applying these estimates as in Kopp et al. (2019). The problems with this have already been discussed. A second method 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

in the macroeconomic literature. Prominent examples of this include Corrado et al. (2009), Crouzet and Eberly (2019), McGrattan and Prescott (2014), and McGrattan (2020), all of which resolve conflicts between theory and data with intangible capital.

[11] 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.

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

$$\Delta I_t = \sum_{j}^{N} \gamma_j \alpha_{j,t-1}, \tag{4}$$

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 4, 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, and relatedly, 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. For my study, I begin with the vector of corporate tax shocks compiled by Romer and Romer (2010) from narrative evidence and further specified by Mertens and Ravn (2013). Each shock consists of an increase or decrease in the average corporate income tax rate. Because of the limitations of quarterly Compustat data, my sample period begins in 1975q1 and therefore contains fewer shocks than in the Romer and Romer (2010) sample.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, I 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 Jordà (2005) local projection of the form

$$\log I_{i,t+h} - \log I_{i,t} = \alpha_{i,h} + \beta_h \Delta T_{t-1} + \zeta_h Q_q \epsilon_{i,t,h} \tag{5}$$

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. As in Cloyne et al. (2018), firm-level investment has quarterly seasonality in my sample, so I include Q_q. 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:

$$\log I_{i,t+h} - \log I_{i,t} = \alpha_{i,h} + \beta_h \Delta T_{t-1} + \Gamma_h' Z_{i,t-1} + \epsilon_{i,t,h}, \tag{6}$$

where Equation 6 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. At the firm level, the determinants of investment may have little to do with the movements of aggregate variables. In particular, firm-specific variables such as leverage (Ahn et al., 2006) and liquidity (Boyle and Guthrie, 2003) all have a potentially large effect on the investment decisions of firms (which sum to aggregate investment), but are generally difficult to capture in aggregate analysis. Taking account of this is crucial for our analysis. Suppose that prior to a tax cut, the typical manufacturing firm is an overleveraged multinational. Then there would be little reason to expect the tax cut to be stimulative to those firms in particular and for aggregate investment in general because manufacturing makes up the bulk of the investment base. Failure to account for this will inevitably lead to the wrong conclusions about policy. Consequently, I also include controls for leverage, liquidity, Tobin's Q, and other balance sheet variables where appropriate.[12] 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:

$$\log I_{i,t} = \alpha_i + \beta \Delta T_{t-1} + \zeta Q_q + \epsilon_{i,t}, \tag{7}$$

and with firm controls:

$$I_{i,t} = \alpha_i + \beta \Delta T_{t-1} + \zeta Q_q + \Gamma' Z_{i,t-1} + \epsilon_{i,t}, \tag{8}$$

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. This particular specification is chosen because it better

[12] See Appendix C for details on construction and summary statistics.

shows how long it takes for investment to recover.[13] In all specifications, the magnitude of the regression coefficient is driven by variation within each firm over time because I control for firm fixed effects.

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, two distinctions immediately emerge between the firm-level response and the aggregate response. First, the firm-level response occurs more quickly and disappears more quickly. Regardless of controls, the firm-level effect goes away before the aggregate response even reaches peak impact. Second, the firm-level response is much shallower than the aggregate response, at least in the (Romer and Romer, 2010) data. On the other hand, the response is comparable to the nonresidential investment response to a Mertens and Ravn (2013) corporate income tax cut, which reacts immediately and at a similar magnitude but lasts several quarters longer. See Figure 6 and Figure 7 for full results.

[Figure 6a 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, but it is still worth considering. 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.

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

Table 3 shows results without firm controls and Table 4 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. This immediate impact, which tends also to be the peak impact, immediately distinguishes corporate investment in particular from aggregate investment in general, which tends to react more slowly. Additionally, this elasticity, while it is significant and has the correct sign, reflects a smaller reaction than shows up in the aggregate data.

I investigate heterogeneity with several different dummy variables. The results from interactions of tax shocks with dummy variables can be grouped into three broad categories: financial constraints, intangibles, and firm type. 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.

3.1.1 Financial Constraints

There are two ways to think of how financial constraints should translate into response to a shock. First, it is possible to think 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.[14] Unconstrained firms are thought to face a flatter marginal

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

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.

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 financial 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.[15] 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

[15] An issue with the analysis of Cloyne et al. (2018) is that it is difficult to test whether the response of younger firms to monetary policy shocks is an indicator of financial constraint or whether it is really measuring something else.

default risk; higher leveraged 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 C for details on variable construction. The results are contradictory and are shown in Table 3, Table 4, and Figure 6. 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). 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). 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) and sets up a puzzle.

[Figure 6 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 5, 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 predictions of the literature, namely that leverage decreases with age. I show that this the same holds for the full Compustat sample before filtering. One possible explanation for why the financial constraint indicators give contradictory results is that these firms are not actually financially constrained. I can 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):

$$\Delta D_{i,t} = \alpha_i + \beta \Delta T^{+}_{i,t-1} + \delta \Delta X_{i,t-1} + \epsilon_{i,t}, \tag{9}$$

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 5. 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 does cause the cost of capital to rise and therefore the equilibrium investment rate declines, 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 theirs 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 size previously failed the financial constraint test, age drives the Hadlock-Pierce result.

The initial 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. In this respect, we agree with Ottonello and Winberry (2018) and Eskandari and Zamanian (2020). 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 8a 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. Assuming these proxies are reasonable, then my results are largely in agreement with much of the monetary shocks literature in the sense that young firms and non-dividend paying firms tend to be adequate indicators of financial constraint (see, e.g., Cloyne et al. (2018)), but I agree with Ottonello and Winberry (2018) that the frictions effect dominates the accelerator effect.

As a means of comparison, splitting the shocks for the aggregate data by sign generates a somewhat different result. Running the three-variable Romer and Romer (2010) VAR specification over the same window as my sample results in Figure 4. Whereas my firm-level responses indicated that corporations respond more to positive shocks than negative shocks and the response was in either case short and immediate, the aggregate response does not become significant until after the corporate response has already recovered. It is difficult to determine whether the aggregate response is stronger to a positive or a negative shock because although the positive shock reaches a greater peak impact, it has wider standard errors.

[Figure 4 here]

3.1.2 Intangibles

I investigate the effect of a tax shock on intangible intensive-firms with three separate dummy variables, one for firms that have a high proportion of organizational capital as a share of total capital, one for firms that have a high proportion of knowledge capital as a share of total capital, and another for firms that have a high proportion of intangible capital as a share of total capital. All three are defined as in Peters and Taylor (2017). Organizational capital refers to the accumulated value of investments made in human capital, brand, customer relationships and distribution systems, while knowledge capital refers to investments made in R&D. Intangible capital is the sum of these components.

The results are not puzzling from an accounting perspective (see Figure 7a, Table 3, and Table 4). There is not a statistically significant difference in investment response between firms that invest heavily in intangible capital and those which do not. In the case of intangible-intensive firms, the response is not statistically distinguishable from zero. That is, a corporate income tax shock has no discernible effect on the physical investment patterns of intangible-intensive firms. Internally-created intangible assets are expensed on income statements and do not appear as assets, which means that shifts in the corporate income tax rate should not affect investment in intangibles, whereas investments in physical capital should in theory be affected by shocks to corporate taxation.

The firms in question here are at the right tail of the distribution for investments in intangible capital. If we consider intangible capital to be a substitute technology to tangible capital as in McGrattan (2020), then increases in marginal tax rates for corporations should shift investment at the margin to intangibles. However, it is likely that this relationship is nonlinear; a firm cannot be entirely intangibles nor can it be devoid of human capital. Even for the most intangible-intensive firms, continued investment in equipment is necessary.

At minimum, a modern service firm reliant on virtual work requires non-trivial continued investment in equipment like computers and the barest of structures for IT support staff and security. For these firms, the demand for certain types of physical investment will be so inelastic that it is nearly insensitive to changes in the user cost of capital. Hence it is likely to be the case that there is a negative relationship between tax sensitivity and intangible intensity as in Figure 10. The production function would in this case be nonlinear such that there would be no corner solutions in the choice between tangible and intangible capital.

[Figure 10 here]

If this holds and accounting for intangibles remains largely unchanged, then it follows that as firms—and the economy as a whole—become more intangible-intensive, the sensitivity of firms to tax shocks will diminish. I could test this by running a regression interacting tax shocks with intangible intensity. However, intangible intensity is at an annual frequency and the typical dynamic response by firms is sufficiently short that any effect effect would be lost using data at annual frequency. Even so, some evidence for this is presented in Section 2.2, where it was shown in Figure 3e that investment has indeed become less sensitive to tax shocks over time.

Another way to approach the issue is that intangible-intensive firms tend to lack the collateral that their tangible-intensive counterparts have, so they tend to hold more cash (Doidge et al., 2018; Haskel and Westlake, 2018). This is because intangibles tend to have no market at all, as with many forms of organizational capital, because intangible capital is often non-rival, or because intangibles are relatively more difficult to collateralize than, for example, physical structures (Loumioti, 2012). Consequently, intangible-intensive firms react less to a tax shock than the baseline specification because they have the requisite funding on hand to smooth out their physical investment spending.

Regardless, there is not currently sufficient data on tax shocks in the period where intangibles have risen the most and even by the time such data become available, it will be a difficult question to study without access to confidential data. This is primarily because private equity markets are better-suited as funding sources for intangible intensive firms, which Doidge et al. (2018) argues may be a contributor to the decline in publicly traded firms, something which in turn indicates that a study of intangibles using Compustat data will be necessarily incomplete in the cross-section and intertemporally.

3.1.3 Firm Type

I also examine the effect of tax shocks on different types of firms, in particular multinationals and highly productive firms (see 7e and 7d). The investment response of multinationals to a tax shock is not statistically significant, a result which is not particularly surprising. Multinationals, in comparison to domestic firms, can optimize their tax-burden across different localities worldwide through profit shifting. Because my study is of firms incorporated in the United States, some investment will of course be attributable to U.S. parents but the share of total investment undertaken by the company may be small. Moreover, particularly in the period examined, it was possible to conduct tax planning such that there were few if any profits to tax in the United States, so that there would be little if any effect of a tax cut on investment for these firms.[16] There exists substantial evidence for this phenomenon, so my result is not particularly surprising (Bilicka, 2019; Dharmapala and Riedel, 2013).

In contrast, highly productive companies do tend to respond significantly more to particular types of tax shocks than their counterparts. See Figure 9. This is an understudied question in the sense that existing studies tend to investigate causality in the opposite direction, i.e., how an exogenous tax shock affects productivity (see, e.g., Hussain (2015); Liu and Mao (2019)). In the case of highly productive firms, a tax shock that raises the user cost of capital tends to have little effect on investment while a tax shock that lowers the user cost of capital has a highly persistent, significantly positive effect, reaching peak impact of

[16] A particularly aggressive tax planning strategy adopted by some firms during the Romer-Romer sample, the Double Dutch with an Irish Sandwich, involved firms sending U.S. profits and revenues through one Irish company, then to a Dutch company, and finally to a second Irish company headquartered in a tax haven. This company could then loan back the profits (with tax deductible interest payments) to the parent company in the U.S.

13% after 5 quarters in response to a cut of 1% in the average corporate income tax rate.

There are two ways to view the reaction of productive firms. First, highly productive firms also tend to be more profitable firms, which means they will tend to have more valuable collateral, better access to external financing, and more cash on hand, which in turn will allow them greater access to external finance (or internal finance) to react optimally. This follows immediately from using ROA as a proxy for productivity, which is an increasing function of profits. Relatedly, productive firms tend to be profitable firms, which also means they are more affected by tax changes than unprofitable firms that may be carrying net operating losses from previous years. Second, more productive firms may also be more efficient firms in the sense that they are better at incorporating new information (shocks) into their decision-making more quickly. To the extent that a high ROA is persistent and therefore evidence of legitimate skill rather than noise, a high ROA signals that a firm is more efficient at resource allocation and investment decisions. Since firms are constantly hit by different kinds of shocks over time that would also affect the user cost of capital (for example, high-frequency monetary policy shocks), it is reasonable to assume that a firm which efficiently incorporates those shocks would also take account of tax shocks.

The differential response for different types of firms may also signal that the optimal response to a tax shock differs by type of firm. For a highly productive, highly profitable firm, it may be optimal to respond to a negative tax shock by substantially increasing investment while using internal funds and access to external capital to mute the effect of a positive shock. In either case, the firm is taking advantage of its superior position relative to less competitive, less productive competitors which may not have the means to respond.

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).[17] 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 my 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 significantly 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

[17] 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.

heterogeneity between types of firms in investment response. Second, I show that there is considerable heterogeneity in the cross-sectional response. Financially constrained firms 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. Additionally, I show that intangible-intensive firms tend to exhibit minimal response. This follows largely from the tax treatment of intangibles and may indicate something important about the future of tax policy. With the clearly documented rise in intangibles and without a change in tax treatment, it is highly likely that the aggregate and firm-level investment response will dampen in the future. Finally, I show that the most efficient firms respond significantly to negative shocks and insignificantly to positive shocks, which is precisely the response policy should aim for.

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A Results

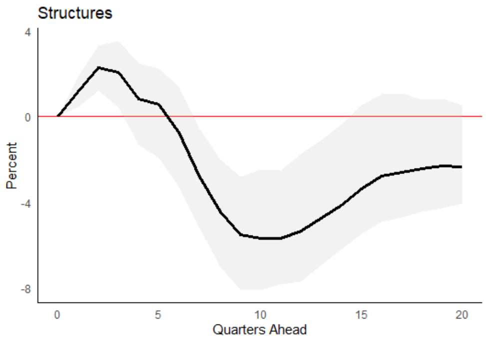
A.1 Replication of Romer-Romer

Coefficient	Dependent Variable $\Delta\log$(GPDI)	$\Delta\log$(FI)	$\Delta\log$(Res. Invt.)	$\Delta\log$(Nonres. Invt.)
Intercept	0.0955 0.0036**	0.0098 0.0018***	0.0077 (0.0033)*	0.0106 (0.0018)***
ΔT_t	-0.118 (0.0135)	-0.0016 (0.0066)	-0.0139 (0.0123)	0.0035 (0.0071)
ΔT_{t-1}	-0.010 (0.0135)	0.0054 (0.0066)	0.0070 (0.0123)	0.0039 (0.0071)
ΔT_{t-2}	0.0084 (0.0135)	0.0041 (0.0066)	0.0108 (0.0123)	-0.0005 (0.0071)
ΔT_{t-3}	0.0229 (0.0135)$^{\cdot}$	0.0014 0.087	0.0017 (.0123)	-0.0018 (0.0071)
ΔT_{t-4}	0.0099 (0.0135)	-0.0022 (0.0066)	-0.0119 (0.0123)	-0.0005 (0.0071)
ΔT_{t-5}	-0.0173 (0.0135)	-0.0064 (0.0066)	-0.0208 (0.0123)$^{\cdot}$	0.0009 (0.0071)
ΔT_{t-6}	0.0024 (0.0137)	-0.0106 (0.0067)	-0.0263 (0.0125)*	-0.0015 (0.0072)
ΔT_{t-7}	-0.0561 (0.0137)***	-0.0173 (0.0067)*	-0.0174 (0.0124)	-0.0148 (0.0072)*
ΔT_{t-8}	-0.0131 (0.0137)	-0.0096 0.0897	0.0058 (.0124)	-0.0148 (0.0072)*
ΔT_{t-9}	-0.0077 (0.0137)	-0.0073 0.0897	0.0005 (.0124)	-0.0112 (0.0072)
ΔT_{t-10}	-0.010 (0.0137)	0.0111 (0.0067)$^{\cdot}$	0.0313 (0.0124)*	0.0018 (0.0072)
ΔT_{t-11}	0.0361 (0.0137)**	0.0231 (0.0067)***	0.0384 (0.0124)**	0.0167 (0.0072)*
ΔT_{t-12}	0.0333 (0.0137)*	0.0162 (0.0070)***	0.0208 (0.0131)	0.0107 (0.0075)

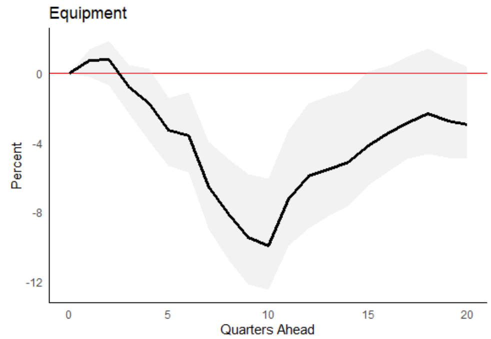
Table 1: OLS regression results for log-differenced investment variables under Romer-Romer specification.

Coefficient	Dependent Variable log(GPDI)	log(FI)	log(Res. Invt.)	log(Nonres. Invt.)
Intercept	3.537 (0.050)***	3.560 (0.050)***	4.019 (0.033)***	3.371 (0.057)***
ΔT_t	0.152 (0.189)	0.153 (0.186)	0.106 (0.122)	0.178 0.0153
ΔT_{t-1}	0.139 (0.188)	0.155 (0.186)	0.110 (0.122)	0.179 (0.215)
ΔT_{t-2}	0.138 (0.188)	0.150 (0.186)	0.113 (0.122)	0.168 (0.215)
ΔT_{t-3}	0.150 (0.189)	0.140 (0.186)	0.106 (0.122)	0.155 (0.215)
ΔT_{t-4}	0.153 (0.189)	0.132 (0.186)	0.090 (0.122)	0.147 (0.215)
ΔT_{t-5}	0.129 (0.189)	0.119 (0.186)	0.062 (0.122)	0.141 (0.215)
ΔT_{t-6}	0.0623 (0.191)	0.038 (0.188)	-0.019 (0.124)	0.062 (0.218)
ΔT_{t-7}	0.023 (0.191)	0.038 (0.188)	-0.021 (0.124)	0.066 (0.217)
ΔT_{t-8}	0.003 (0.191)	0.021 (0.188)	-0.021 (0.124)	0.044 (0.218)
ΔT_{t-9}	-0.002 (0.191)	0.016 (0.188)	-0.017 (0.124)	0.035 (0.217)
ΔT_{t-10}	-0.006 (0.191)	0.034 (0.188)	0.020 (0.124)	0.043 (0.217)
ΔT_{t-11}	0.032 (0.191)	0.059 (0.188)	0.061 (0.124)	0.063 (0.218)
ΔT_{t-12}	0.173 (0.201)	0.184 (0.198)	0.164 (0.130)	0.193 (0.229)

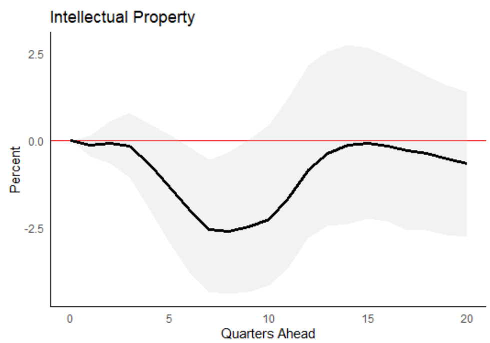
Table 2: OLS regression results for log investment variables under Romer-Romer specification.



(a) Dynamic response of structures



(b) Dynamic response of equipment



(c) Dynamic response of intellectual property

Figure 1: Impulse responses of NIPA Table 1.1.3 Lines 10-12 to a Romer-Romer shock.

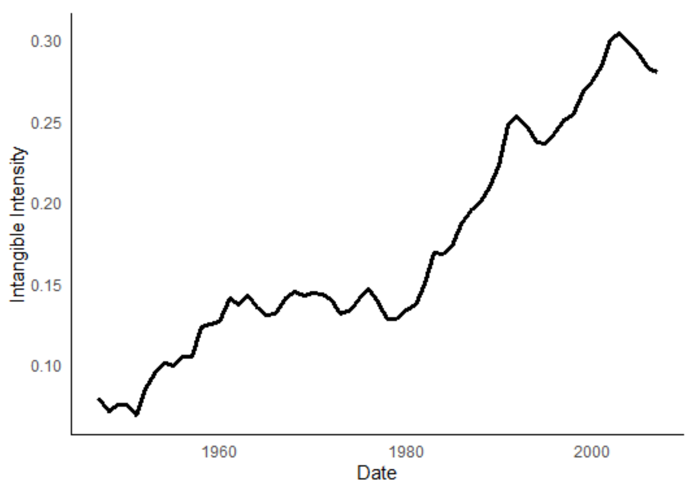
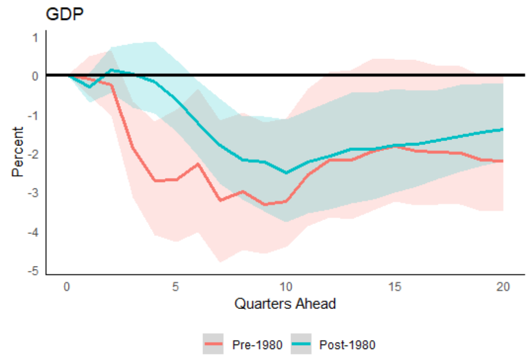
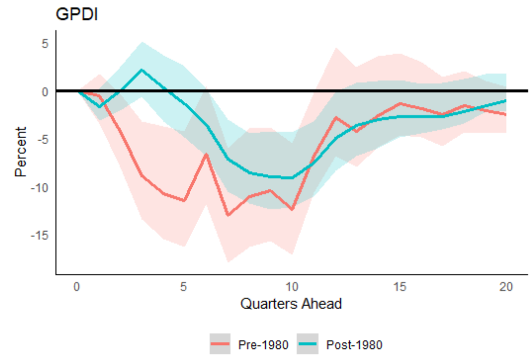


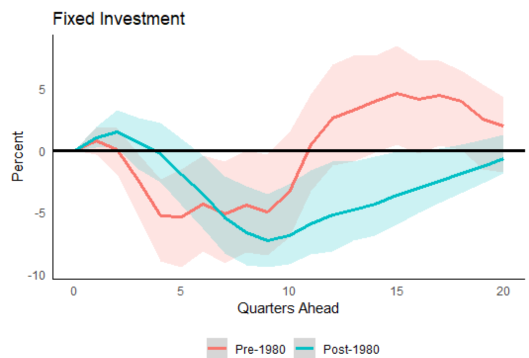
Figure 2: Ratio of annual intellectual property investments (NIPA Table 1.1.5 Line 12) to annual nonresidential fixed investment (NIPA Table 1.1.5 Line 9).



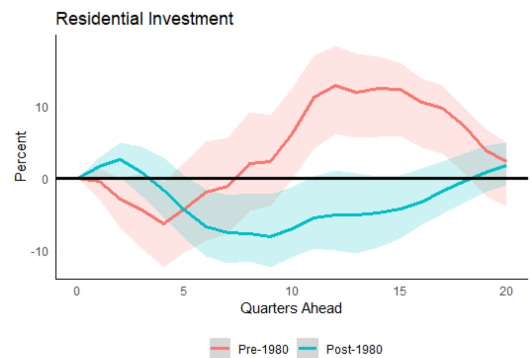
(a) GDP



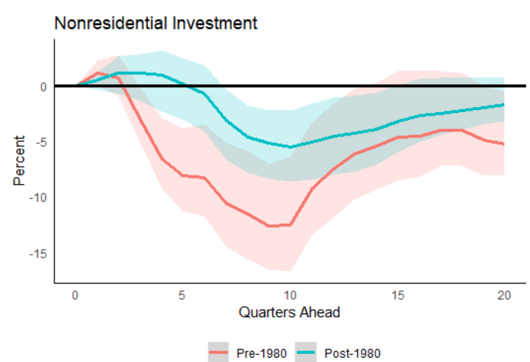
(b) GPDI



(c) Fixed Investment

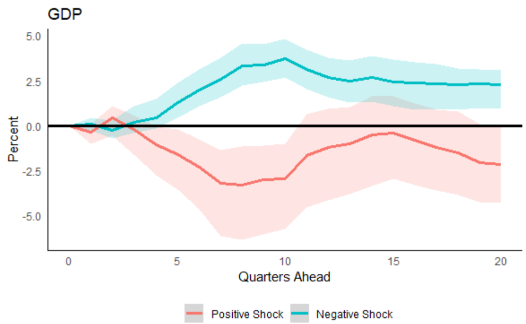


(d) Residential Investment

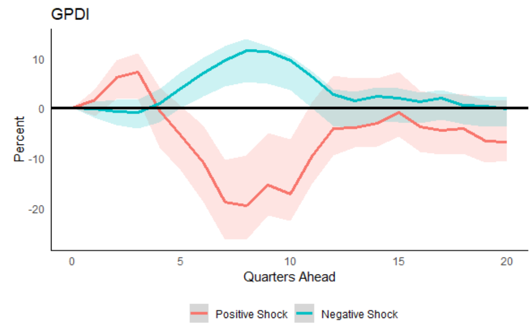


(e) Nonresidential Investment

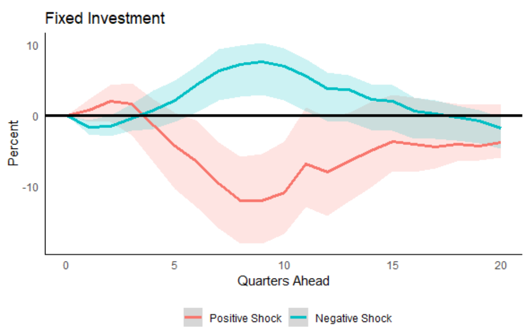
Figure 3: Impulse responses of GDP and components of investment to a Romer-Romer shock split into pre-1980 and post-1980 samples. Data taken directly from appendix of Romer and Romer (2010).



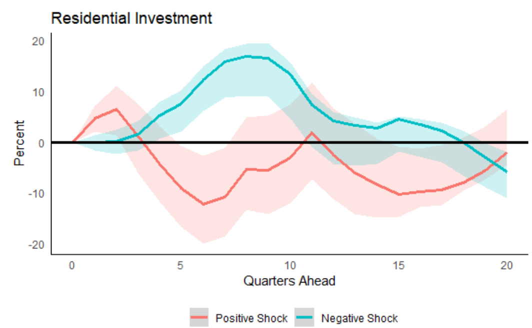
(a) GDP



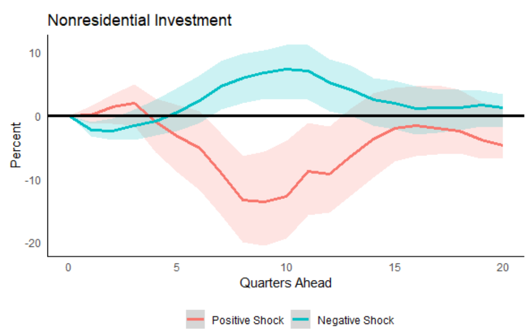
(b) GPDI



(c) Fixed Investment



(d) Residential Investment



(e) Nonresidential Investment

Figure 4: Impulse responses of GDP and components of investment to a Romer-Romer shock split by shock sign. Data taken directly from appendix of Romer and Romer (2010) and covers 1975q1-2007q1.

A.2 Panel Regressions

	log($I_{i,t}$)										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
ΔT_{t-1}	−1.25***	−0.84*	−0.97**	−1.24**	−1.79***	−0.96	−1.36***	−1.27***	−1.67***	−1.17***	−0.68
	(0.43)	(0.45)	(0.46)	(0.54)	(0.51)	(0.59)	(0.45)	(0.43)	(0.46)	(0.45)	(0.63)
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)					
ΔT_{t-1}:High Intan.							0.56				
							(1.43)				
ΔT_{t-1}:High Know. K								2.6			
								(5.4)			
ΔT_{t-1}:High Org. K									2.88**		
									(1.2)		
ΔT_{t-1}:High ROA										−0.77	
										(1.6)	
ΔT_{t-1}:MNC											−1.14
											(0.85)
Sector Dummies?	No	No	No	No	No	No	No	No	No	No	No
Individual Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarterly Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls?	No	No	No	No	No	No	No	No	No	No	No
Observations	82,827	82,827	82,827	82,827	82,826	82,827	82,827	82,827	82,827	82,755	82,827
No. Firms	2726	2726	2726	2726	2726	2726	2726	2726	2726	2726	2726
R^2	0.011	0.023	0.011	0.034	0.030	0.024	0.018	0.013	0.025	0.012	0.011
Adjusted R^2	−0.022	−0.010	−0.022	0.001	−0.003	−0.009	−0.016	−0.021	−0.009	−0.022	−0.022

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

Table 3: 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. 7.

	log($I_{i,t}$)										
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
ΔT_{t-1}	-1.0^{**}	-0.71^{*}	-0.6	-1.5^{***}	-1.67^{***}	-0.6	-1.0**	-1.0^{**}	-1.48^{***}	-1.0^{**}	-0.63
	(0.41)	(0.43)	(0.43)	(0.51)	(0.48)	(0.57)	(0.43)	(0.41)	(0.44)	(0.43)	(0.61)
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)					
ΔT_{t-1}:High Intan.							0.41				
							(1.34)				
ΔT_{t-1}:High Know. K								3.3			
								(5.3)			
ΔT_{t-1}:High Org. K									3.51^{***}		
									(1.1)		
ΔT_{t-1}:High ROA										0.003	
										(1.4)	
ΔT_{t-1}:MNC											-0.7
											(0.8)
Sector Dummies?	No	No	No	No	No	No	No	No	No	No	No
Individual Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarterly Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	82,827	82,827	82,827	82,827	82,826	82,827	82,827	82,827	82,827	82,755	82,827
No. Firms	2726	2726	2726	2726	2726	2726	2726	2726	2726	2726	2726
R^2	0.15	0.15	0.15	0.16	0.15	0.15	0.15	0.15	0.15	0.15	0.15
Adjusted R^2	0.12	0.12	0.12	0.13	0.12	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 4: 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. 8.

	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 5: Results of test for financial constraint. The exact specification is given in eqn. 9. Firm fixed effects are used, so coefficients are driven by within-firm variation over time.

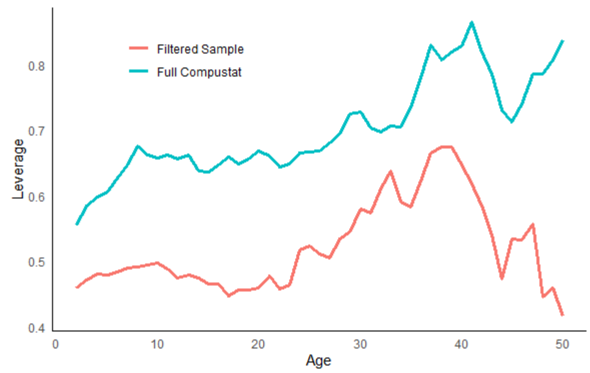


Figure 5: Mean leverage by age from Compustat. The filtered sample is the sample used for our analysis, while full Compustat is all of quarterly Compustat after filtering out financial services, firms not incorporated in the U.S., firms without tickers, firms without sales, and firms with positive assets and sales.

A.3 Jorda Local Projections

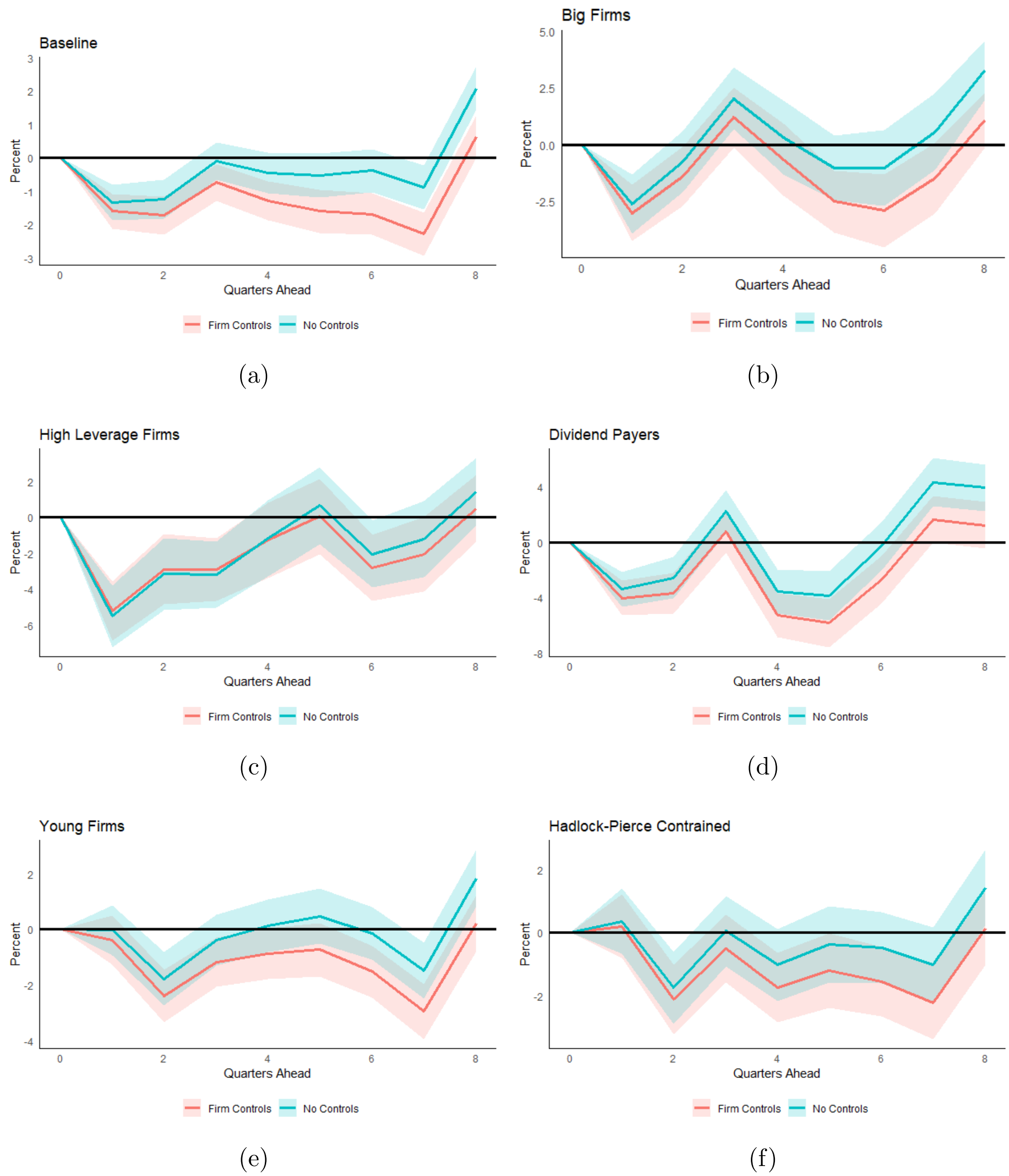
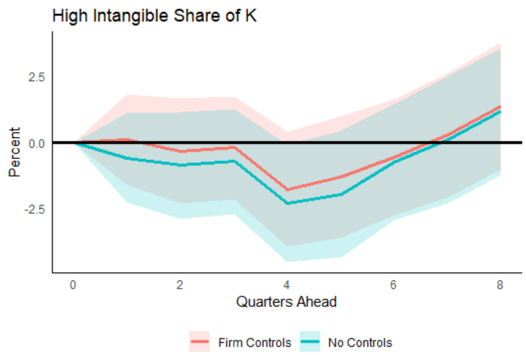
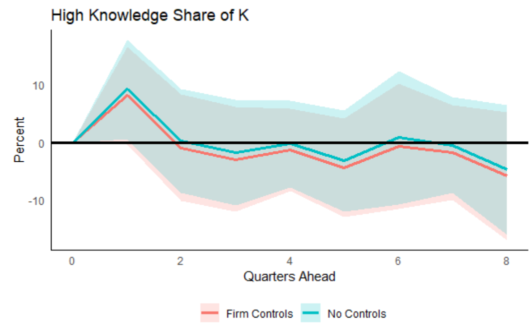


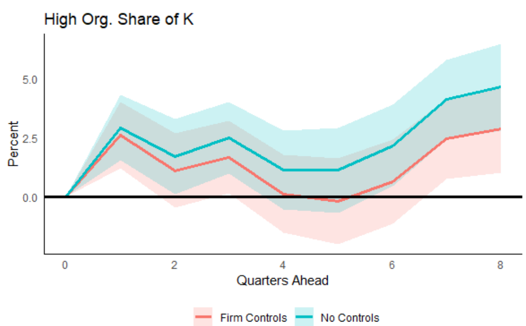
Figure 6: Impulse responses of firms to a Mertens-Ravn corporate income tax shock. Grouped by financial constraint indicator.



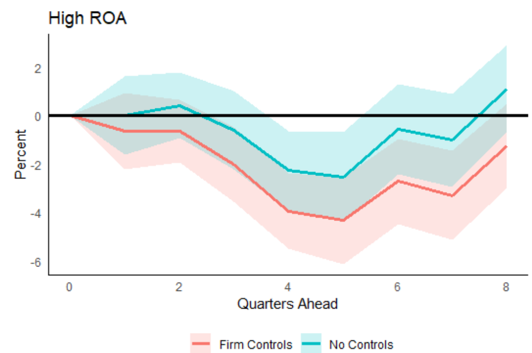
(a)



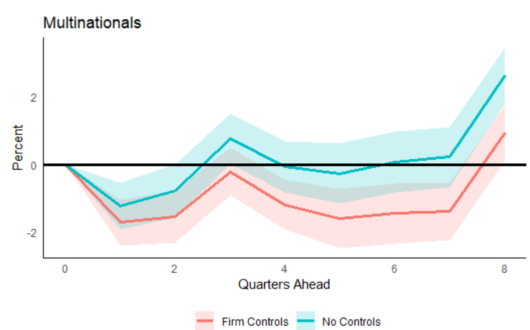
(b)



(c)



(d)



(e)

Figure 7: Impulse responses of firms to a Mertens-Ravn corporate income tax shock.

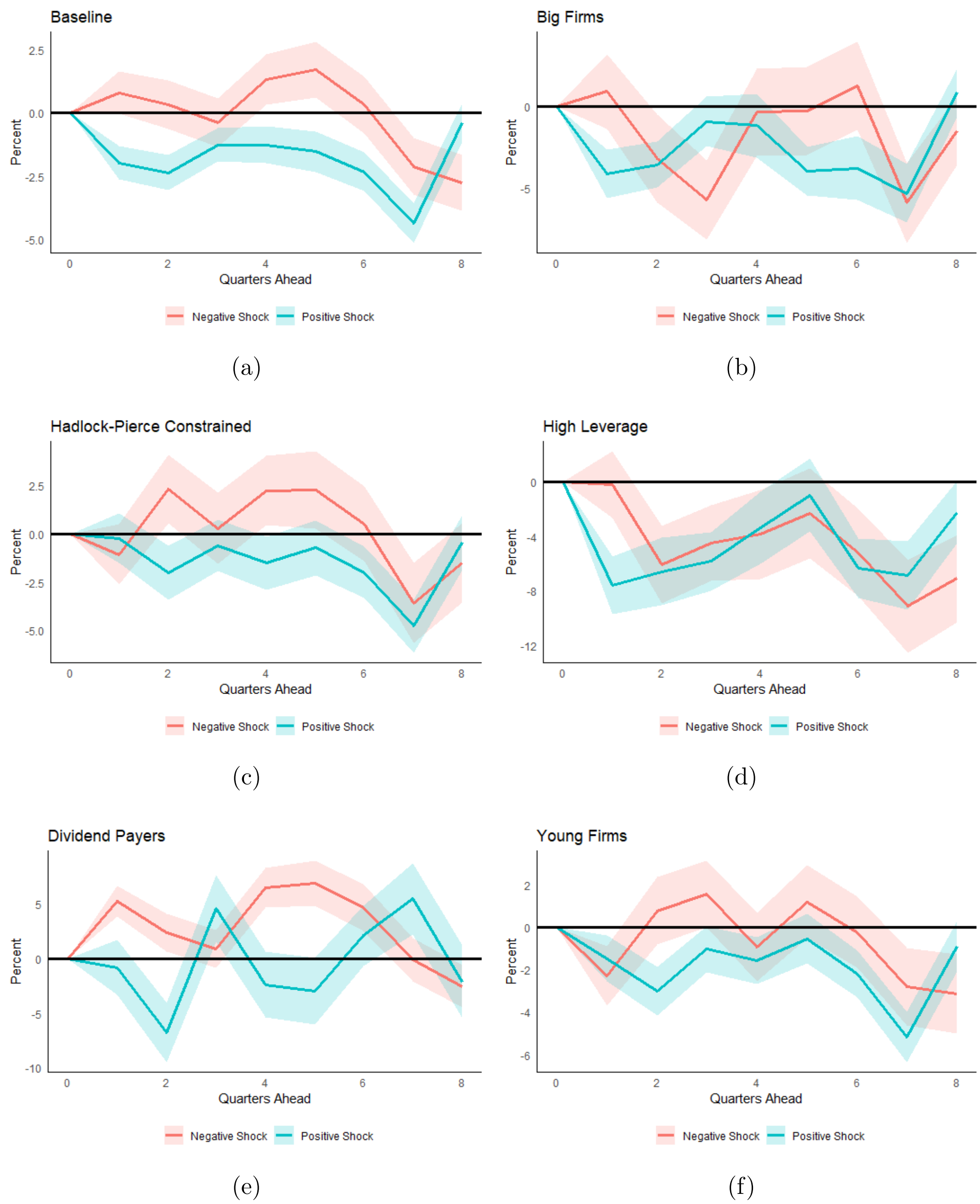


Figure 8: Impulse responses of firms to a positive Mertens-Ravn corporate income tax shock. Interpretation for a positive shock is in terms of a 1% increase in the average corporate income tax rate isolating for positive shocks, while it is the mirror image for negative shocks.

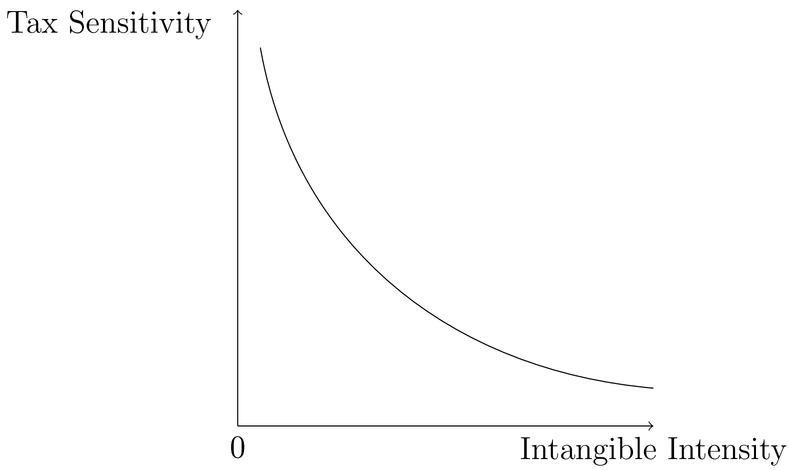


Figure 10: Proposed relationship between intangible intensity and tax shock sensitivity.

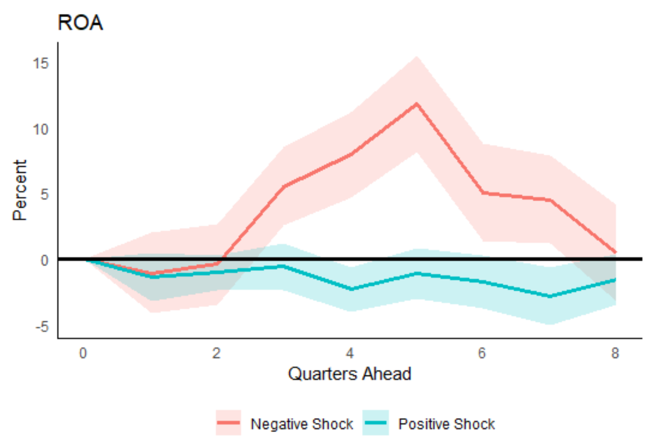


Figure 9: Impulse responses for high ROA firms split by positive and negative shocks

B Heterogeneity by Type and Legal Form in Aggregate Data

B.1 Heterogeneity Across Investment Type

We established in 2.2 that if the results of aggregate studies are taken seriously, the implicit assumption must be that the share of the components of output must be constant over time. Given the rise in intangible capital, this is likely incorrect. Consequently, it is difficult to avoid the conclusion that while the estimates compiled by Romer and Romer (2010), Mertens and Ravn (2013), and other highly aggregated studies may be useful as indicators of what has happened on average historically following a tax shock, their utility at present for discussions of policy is minimal. In this section, we briefly explore other avenues of heterogeneity in investment response to a tax shock.

Figure 11a gives impulse responses for all subcomponents of fixed investment.[18] There are major differences in response, with some components reaching peak impact much sooner, others with a deeper and more persistent response, and others exhibiting no response or a positive response. A further breakdown of fixed investment shows that the response of residential fixed investment tends to be shorter and less negative than nonresidential investment. Looking more closely at the components of nonresidential investment, it is clear that intellectual property products do not respond significantly, while structures have a shallower trough than equipment. Theoretically, this makes sense for the reasons enumerated above; due to current (and past) tax law, tax shocks should not have a significant impact on intellectual property products. See Figures 11e, 11d, and 11f for breakdowns by types of equipment, structures, and intellectual property products.

The point is not to provide precise point estimates of peak impact multipliers or a narrative for why the subcomponents of investment behave as in Figure 11. We do not think the vector of Romer-Romer shocks is capable of giving an unbiased estimate, particularly for disaggregated categories like the subcomponents of investment. Rather, it is to highlight that there is considerable heterogeneity in the subcomponents of investment and it would be unreasonable to assume constancy over time in either their response or relative share of total investment. Consequently, the results of aggregate studies done in the vein of Romer and Romer (2010) will give biased estimates.

B.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 passthrough. 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 as a share of total investment changes over time.

There is some evidence in favor of this notion. First, it is reasonable to suggest that the type and quality of investment undertaken by c-corporations is different from that undertaken by passthroughs. For example, Barro and Wheaton (2019) suggest that the very fact that c-corporations exist despite the large tax penalty associated with organizing that way is de facto evidence of a productivity benefit 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 that privately organized firms, which also tend to be much smaller. Size becomes relevant

[18] We adopt the same specification as Romer and Romer (2010), which is a three-variable VAR with the tax shock ordered first, the log of GDP next, and then a log-transformed subcomponent of investment.

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). Yet another possibility exists that agency problems related to managerial handling of investment may mean that passthroughs are, in some sense, more efficient investing bodies even if they have reduced access to external funds (Stein, 2001). Given the possibility that legal type non-trivially affects a firm's approach to investment, then just as above, the dynamics of the aggregate investment response will be affected by the composition of firms by legal type. Additionally, it is clear that the share of firms organized as c-corporations and the absolute number of c-corporations has declined since the 1990s (Auerbach, 2018; Doidge et al., 2018), which implies a further disaggregation by legal type is necessary. Finally, there is substantial evidence that tax changes induce switching between forms (Clarke and Kopczuk, 2017).[19] This is somewhat 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.

We investigate heterogeneity in legal type using the same three-variable VAR setup utilized throughout the paper as a means of tying our work to (Romer and Romer, 2010). Our objective is not to report precise point estimates for peak impact multipliers, but rather to establish whether there is plausibly a difference in response by legal form. The difficulty is that fixed asset data by legal form is available only at annual frequency or when available at quarterly frequency, is often not directly comparable between legal types. To mitigate this issue, we use three different sets of data and as a means of validating results. First, we take differences in the current cost stock of structures, equipment, and intellectual property for nonfinancial corporate and nonfinancial noncorporate businesses from the Flow of Funds. This gives us a rough measure of net investment. Second, we construct quarterly estimates of fixed asset data from NIPA. 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 Flow of Funds and apply a Kalman smoother. We construct quarterly estimates for Fixed Asset Table 4.7 Lines 37-44 and obtain impulse responses from these estimates.[20] 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). Finally, we use gross fixed capital formation from the Flow of Funds for each legal type.[21]

[19] See, for example, Plesko and Toder (2013) on the effects of the 1986 Tax Reform Act on organizational choice.

[20] See Appendix D for details on the procedure.

[21] Optimally, we would only use this measure, but the Flow of Funds does not provide equivalent measures of gross fixed capital formation for each legal type.

Results for net investment from the Flow of Funds are presented in Figures 14 and 15, while impulse responses for the estimated NIPA data and gross fixed capital formation are plotted in Figures 12, 13, and 16. Compared to the Flow of Funds net investment responses, the NIPA estimated series and the GFCF exhibit a far more muted response, largely because net investment is more volatile than gross investment.[22] Despite that difference, several commonalities are present in the impulse responses. First, it is difficult to conclude that any legal type exhibits a strongly negative or persistently negative response to a tax shock of any kind. However, it is clear that for both types of tax shocks, the response by corporate entities tends to be more negative, which may indicate that the hypothesis of Arin et al. (2021) that dividend-paying corporations respond on the margin to personal income tax shocks has some merit. Third, structures is the only investment type that shows a significantly negative and persistently negative response to any kind of income tax shock. The response of corporate investment in structures to a corporate income tax shock is significantly lower than the response of non-corporate entities to a personal income tax shock. Finally, noncorporate intellectual property investment behaves atheoretically across estimates, increasing sharply and significantly following a corporate income tax shock.[23]

The impulse responses should be taken as indications of heterogeneity by legal form, but should not be interpreted as literal estimates of peak impact multipliers. A confounding element in all available data is that s-corporations, which are taxed as pass-throughs, are grouped with c-corporations by both the Flow of Funds and NIPA Fixed Asset tables.[24] Consequently, there is a small but significant share of firms whose investment decisions are being hit by a shock at a margin that may not be directly relevant when considering corporations as a whole.[25] This likely explains the ineffective response of corporate investment to a corporate income tax shock, but it does not explain the weak investment response of noncorporate entities, which should theoretically still react significantly even if a substantial number of firms which would be useful to include under the noncorporate umbrella are instead classified as corporations. There are three possible explanations for this weak response. First, the estimated investment series is not sufficiently robust. This explanation can likely be rejected because all three types of data show the same response. Second, the vector of tax shocks are misidentified or not orthogonal to other factors affecting noncorporate investment. This is possible but not likely given the work of Romer and Romer (2010) and Mertens and Ravn (2013). A more likely explanation would be that the income tax shocks are operating at an irrelevant margin

[22] Another source of dissension between the estimates is that NIPA and the Federal Reserve's Flow of Funds use different accounting standards for depreciation.

[23] Conditional on relatively smaller personal income tax shock and given the scalability of intangibles, noncorporate firms could be seizing the opportunity to invest in products that would normally be left to much larger corporate firms. On the other hand, the response may simply reflect the secular increase in the intangible intensity of noncorporate entities regardless of a corporate income tax shock that is potentially irrelevant to the activities of a noncorporate firm. In any case, further research is necessary.

[24] This is somewhat useful for our purposes because to the extent that switching occurs, it is typically between s-corporations and c-corporations. Grouping them together avoids the problem described above.

[25] Despite that, the investment rates in Compustat the annual and estimated quarterly corporate series from the Fixed Asset tables are highly correlated. See Appendix C.1 for further details.

for some average tax rate. Indeed, it is possible that particular tax events could on net increase personal income taxes but the provisions relevant for investment reduce taxes taken by themselves. Finally, both series are sufficiently constructed, but the theoretical prediction that investment will fall in response to a tax shock is without merit. It is also possible in this case that noncorporate firms, which tend to be much smaller than corporate firms, are slower to react and make investment decisions that are relatively costlier to reverse than corporate investment decisions. It is difficult to determine the weight which should be given to each explanation, but it has been sufficiently demonstrated that there is heterogeneity in response across legal form, something which would be overlooked by aggregate studies and may lead to biases of the kind associated with the rise in intangible capital if there are time trends associated with the share of investment undertaken by particular legal forms.

One indicator that significant heterogeneity by legal type likely exists is the substantial difference in response at the aggregate level and by legal type. Whereas structures tend to respond less at the aggregate level than equipment, the response by structures is *more* persistent and negative than equipment at the corporate level. However, heterogeneity across legal form and heterogeneity across investment type point to further puzzles that can be better answered by firm-level data. In particular, it is unclear how reliable the estimates of peak impact multipliers will be unless we have further information about cross-sectional heterogeneity regarding the investment responses of firms which are classified as overleveraged, multinationals, and other characteristics which exhibit definite time trends. For example, the corporate investment response to a corporate tax cut must be conditioned on the fact that the absolute number of corporations has declined substantially over the relevant time period, which would be lost in an aggregate analysis.

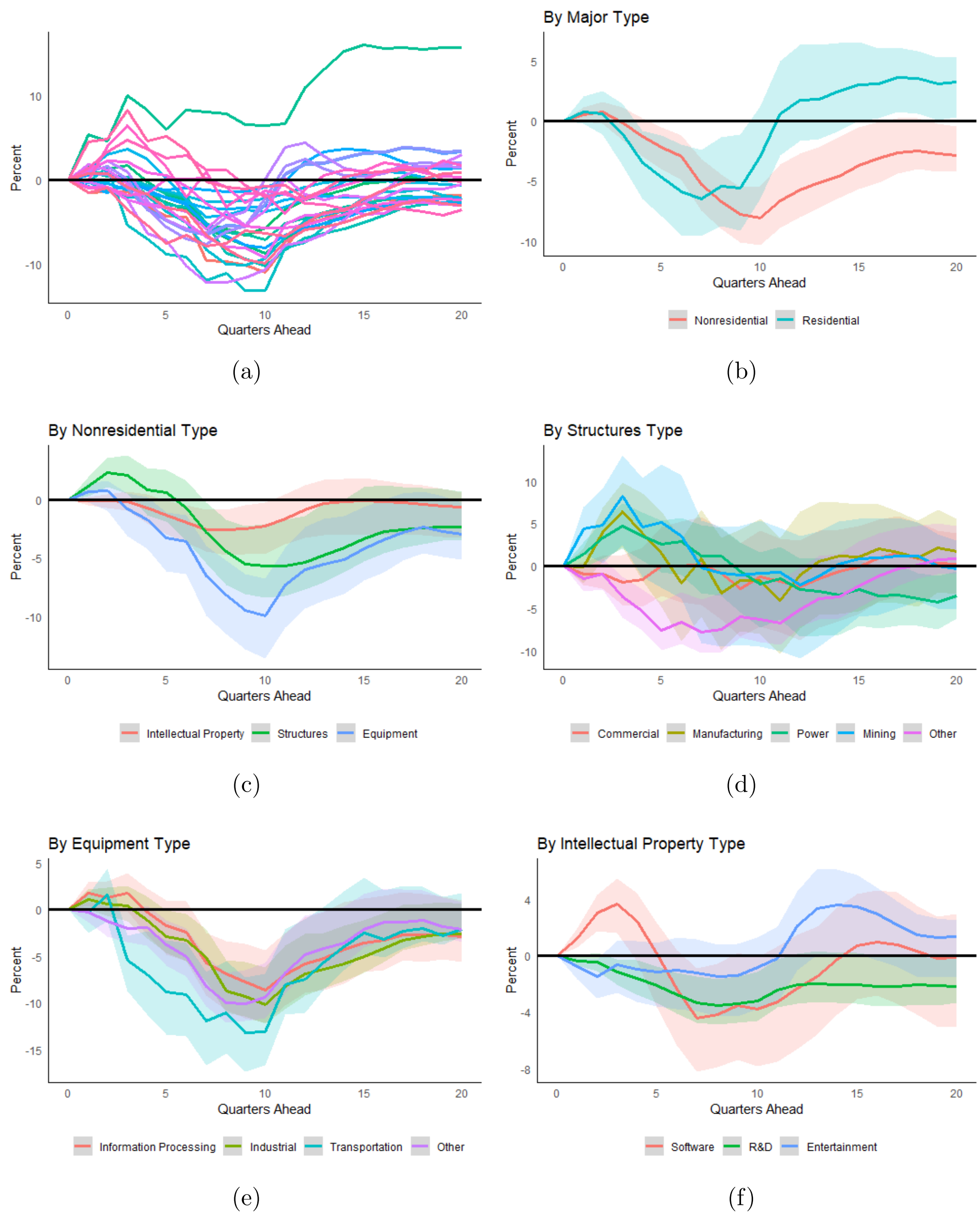
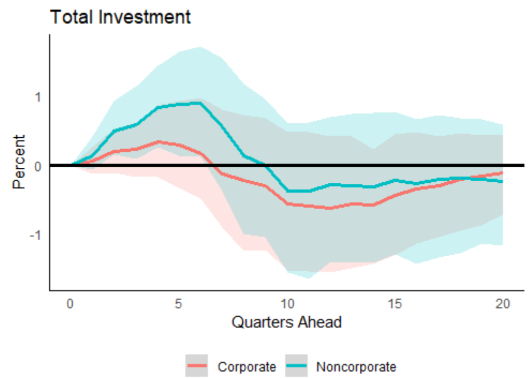
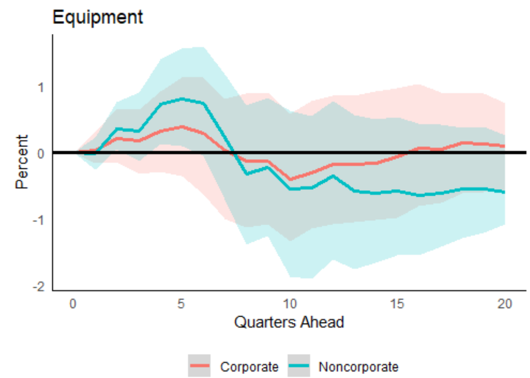


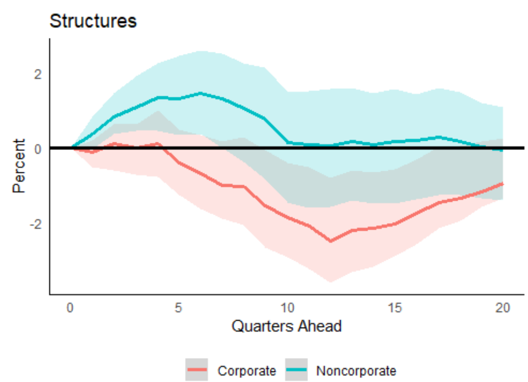
Figure 11: Impulse responses to components of fixed investment. Data taken directly from NIPA Table 5.3.3 responding to a Romer-Romer shock.



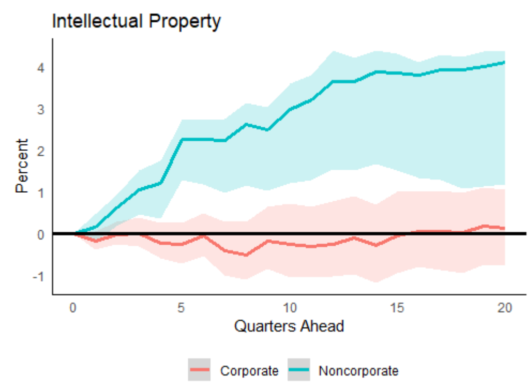
(a) Total Investment



(b) Equipment

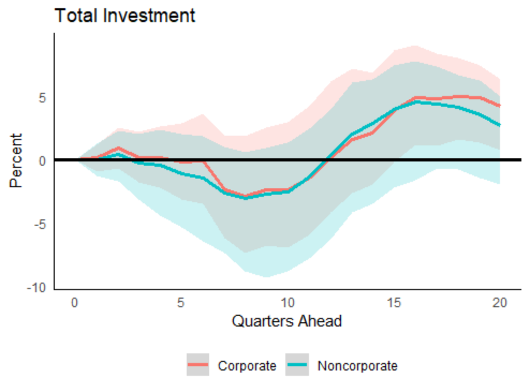


(c) Structures

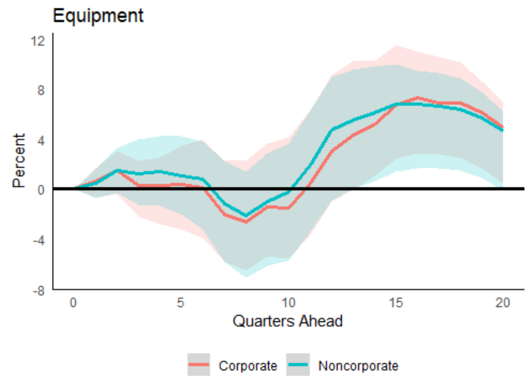


(d) Intellectual Property

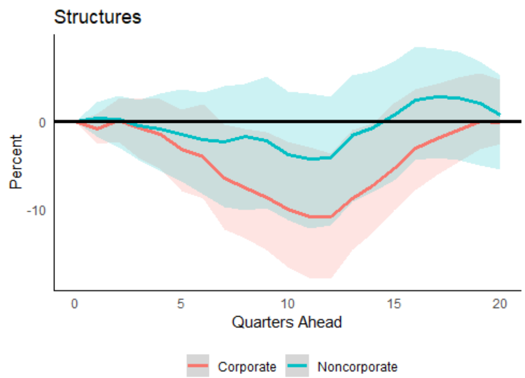
Figure 12: Impulse responses to a corporate income tax shock by legal form.



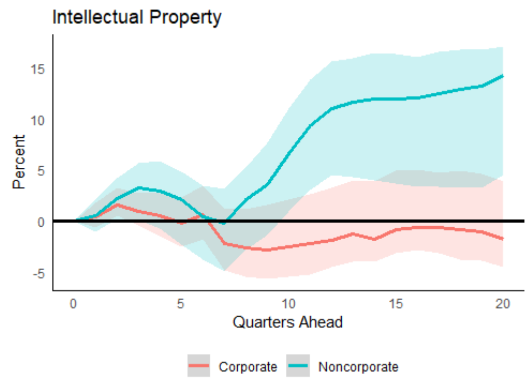
(a) Total Investment



(b) Equipment

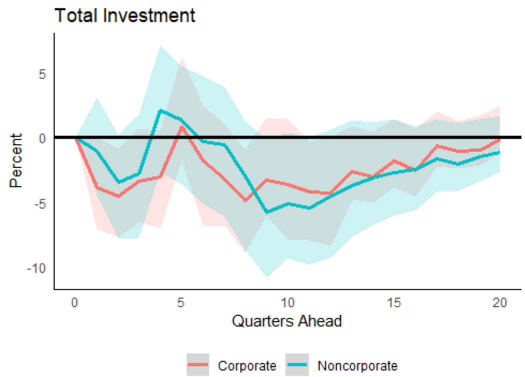


(c) Structures

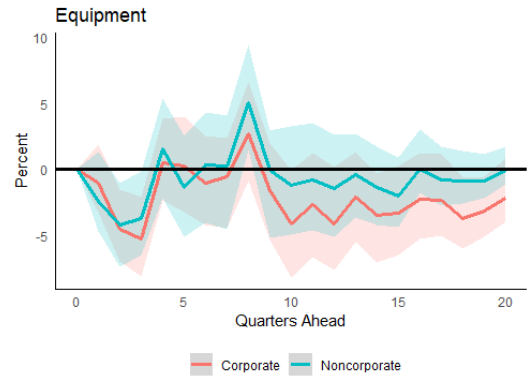


(d) Intellectual Property

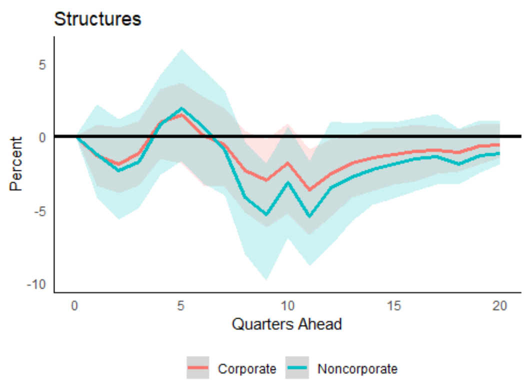
Figure 13: Impulse responses to a personal income tax shock by legal form.



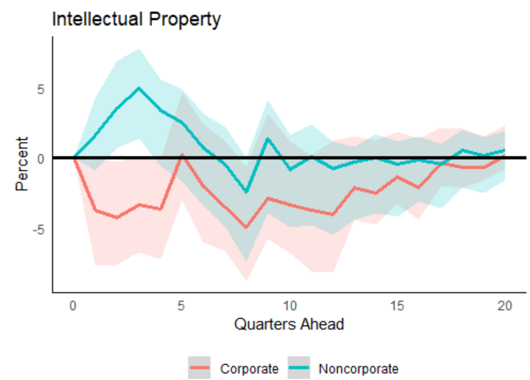
(a) Total Investment



(b) Equipment

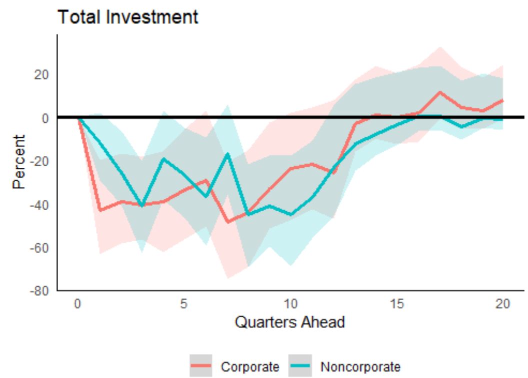


(c) Structures

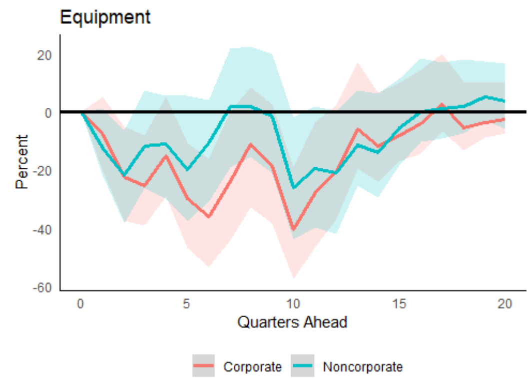


(d) Intellectual Property

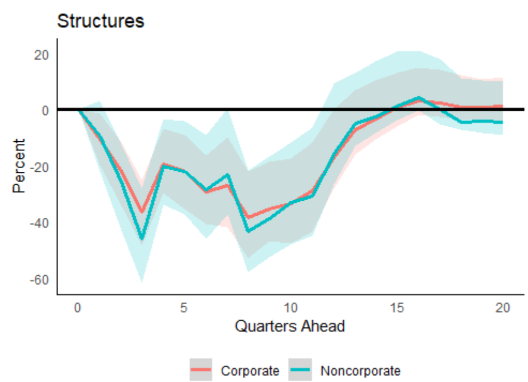
Figure 14: Impulse responses to a corporate income tax shock by legal form using Flow of Funds balance sheet data for nonfinancial corporate and nonfinancial noncorporate entities.



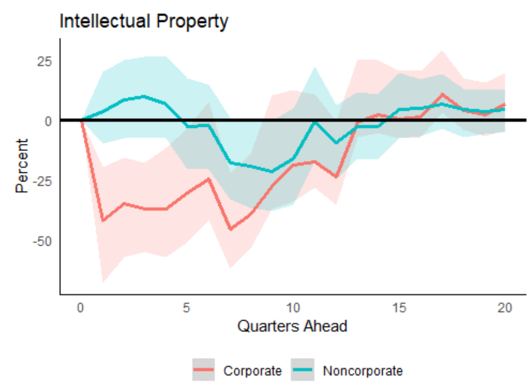
(a) Total Investment



(b) Equipment

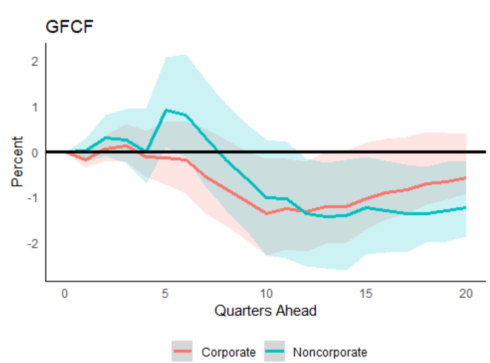


(c) Structures

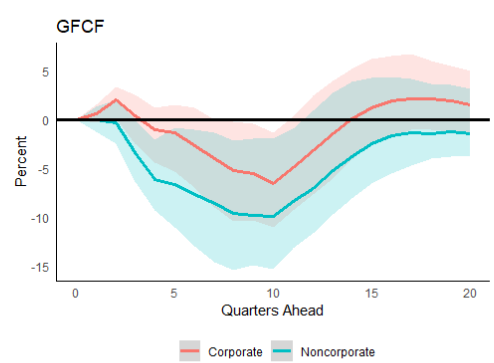


(d) Intellectual Property

Figure 15: Impulse responses to a personal income tax shock by legal form using Flow of Funds balance sheet data for nonfinancial corporate and nonfinancial noncorporate entities.



(a) Corporate Income Tax Shock



(b) Personal Income Tax Shock

Figure 16: Impulse responses to a tax shock from gross fixed capital formation by legal type.

C 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.[26]

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 nonfinancial 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.

[26] 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.

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.

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. *EBITA/AT*: Sales (saleq, item 2) minus cost of goods sold (cogsq, item 30) minus SG&A (xsgaq, item 1).

11. *Debt*: Sum of current debt (dlcq, item 45) and long-term debt (dlttq, item 51). Log-transformed.

12. *Collateral/AT*: Sum of net property, plant, and equipment (ppentq, item 42), inventories (invtq, item 38), and receivables (rectq, item 37). Scaled by total assets.

13. *Cash Flow/AT*: Sum of operating activities - net cash flow (oancfy, item 108) and interest expense (xintq, item 22). Scaled by total assets.

14. *Intangible Intensity*: Intangible capital from Peters and Taylor (2017) scaled by the sum of tangible capital (ppentq) and intangible capital.

15. *Long-term Leverage*: Long-term debt (dlttq) scaled by shareholder equity (seqq).

16. *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
EBITDA/AT	75677	0.0383	0.0451	0.0081	0.0405	0.0744
Cash Flow/AT	54713	0.0293	0.0475	-0.0204	0.0303	0.0752
Collateral/AT	80999	0.685	0.217	0.354	0.737	0.927
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 6: 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. *Multinational Dummy*: Following Kim and Milner (2019), we assign a firm-quarter observation a dummy value of one if the ratio of pretax foreign income (pifo, Compustat item

273) to total pretax income (pi, Compustat item 122) exceeds 0.02139. The foreign income variable is only available annually so if this condition is true for a particular year, then a firm is categorized as a MNC for all quarters within that year.

4. *High Intangible Capital Intensity*: I define intangible capital intensity following Peters and Taylor (2017). Using their measure of capital intensity K_{int} and taking physical capital as $K_{phy} \equiv ppentq$, intangible intensity is $\frac{K_{int}}{K_{int}+K_{phy}}$. If this figure is in the top decile for a particular year, then it is categorized as high intangible intensity for all four quarters.

5. *High Knowledge Capital Intensity Intensity*: This is also defined using the work of Peters and Taylor (2017). If their measure of knowledge capital as a share of total capital is greater than 0.66 for a particular year, then it is categorized as high knowledge intensity for all four quarters.

6. *High Organizational Capital Intensity*: This is also defined using the work of Peters and Taylor (2017). If their measure of organizational capital as a share of total capital is greater than 0.66 for a particular year, then it is categorized as high organizational intensity for all four quarters.

7. *High Return on Assets*: Used as a proxy for productivity, a firm was categorized as having a high return on assets (ROA), if a firm-quarter fell in the top decile of ROA for the previous quarter.

8. *High Leverage*: Defined as taking value 1 if leverage is greater than one and zero otherwise.

9. *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.

10. *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.

11. *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.

12. *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
Multinational	82828	0.50	0.50	0	1
High Intangible K	82828	0.107	0.309	0	1
High Knowledge K	82828	0.013	0.114	0	1
High Organizational K	82828	0.136	0.343	0	1
High ROA	82756	0.103	0.304	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
K-Z Constrained	58526	0.34	0.47	0	1
Young Firm	82828	0.42	0.49	0	1

Table 7: 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

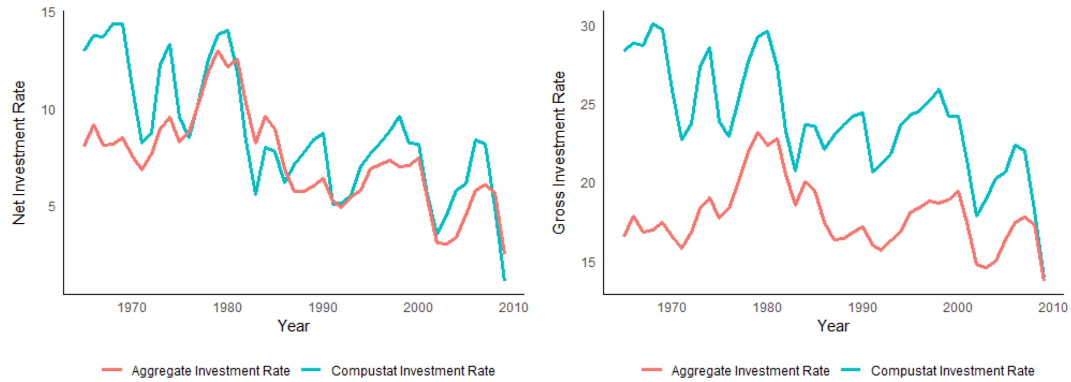
5. Because we were working with investment rates and investment levels, followed a winsorization procedure for obtaining investment figures:
   - Winsorize ppentq, dpq, and am at the .005 and .995 percentiles.
   - Compute net investment and depreciation, then winsorize at the .005 and .995 percentiles, 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.

C.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 are 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 17a shows the median annual net investment rate from Compustat and the net investment rate computed from Fixed Asset Table 4.7 Line 37.[27] The correlation between these series is 71%. We also show the annual 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.

[27]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 Computstated, 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).



(a) Net Investment Rates

(b) Gross Investment Rates

Figure 17: Matched aggregate and firm-level investment rates.

D Estimates of Quarterly Fixed Asset Data

Fixed Asset data published by 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. In the firm-level analysis, the key variables are the net interest rate and the gross interest rate. 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 only two variables and the third will follow. 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 also 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 we have no reason to assume that there is no seasonality, so this presents a particular difficulty.

In an attempt to alleviate this, I followed the following procedure. 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 following McGrattan (2020) I will 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:

$$\begin{aligned} x_{t+1} &= Ax_t + B\epsilon_{t+1} \\ y_t &= C_t x_t, \end{aligned} \tag{10}$$

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

$$A = \begin{bmatrix} a_1 & a_2 & \dots & a_j \\ I & 0 & \dots & 0 \\ 0 & I & \dots & 0 \\ \vdots & \vdots & \vdots & \vdots \\ 0 & 0 & \dots & 0 \end{bmatrix}, \; B = \begin{bmatrix} b \\ 0 \\ 0 \\ \vdots \\ 0 \end{bmatrix}, \; C_t = I \tag{11}$$

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 grpss 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:

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

where D is total depreciation in period t. Following this procedure produces estimates that are reasonable given the available data, particularly stocks. In Figure 18, 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. Note that because the net capital stock was much smaller at the beginning of the estimation period, perceived quarterly volatility is much smaller than it actually is.

I utilize the procedure outlined above to obtain quarterly estimates of Fixed Asset Tables 4.3

Lines 37-40, 4.6 Lines 37-40, and 4.7 Lines 37-40. To estimate the net capital stocks, I mainly use series from the Flow of Funds published by the Federal Reserve. The total net capital stock is estimated using the stock of non-financial assets at historical cost for non-financial corporate businesses (TTAATASHCBSHNNCB) and non-financial corporate consumption of fixed capital (NCBCFCQ027S). All estimates of net capital stocks utilize the latter. The net capital stock for structures is estimated using the historical cost value of non-financial corporate structures (HCVSNNWHCBSNNCB). The net capital stock for equipment is estimated using the historical cost value of non-financial corporate equipment (ESATASHCBSNNCB), and the net stock of intangible capital is estimated using the historical cost value of non-financial corporate intangibles (NCBNIPPHCB). I decided to construct the quarterly series for gross investment rather than depreciation using this procedure because the available aggregate series are better suited for estimating investment. For each of structures, equipment, and intangible investment, I utilized non-financial corporate net value added (Table 1.14 Line 19) as well as nonresidential fixed investment in structures (NIPA Table 1.1.5 Line 29), nonresidential fixed investment in equipment (NIPA Table 1.1.5 Line 30), and nonresidential fixed investment in intellectual property (NIPA Table 1.5.5 Line 37), successively. To compare properly with the Compustat investment rate, I also summed Table 4.3 Lines 38-39, Table 4.6 Lines 38-39, and Table 4.7 Lines 38-39 to obtain a physical investment rate. I then summed my quarterly estimates of 4.7 Lines 38-40 to get an estimate for 4.7 Line 37.

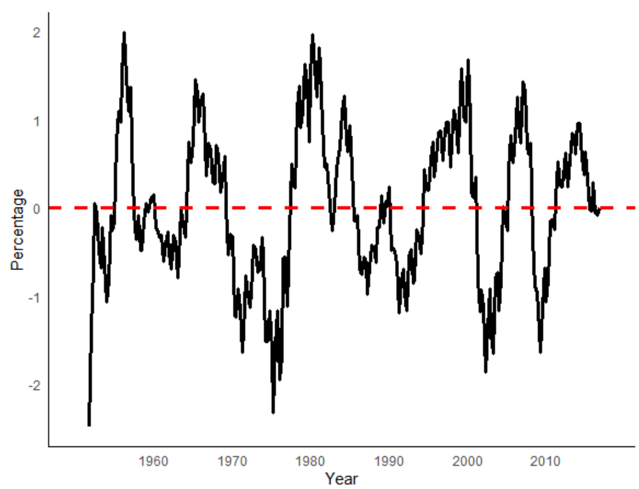


Figure 18: Percent deviation between my method and linear interpolation for estimation of Fixed Asset Table 4.7, Line 37.

A similar procedure was carried out to estimate Fixed Asset Table 4.7 Line 41-44. In this case, I used first difference of the current-cost net stock of equipment (NESABSNNB), structures (RCVSNWBSNNB), and intellectual property (NNBNIPPCCB) for lines 42, 43, and 44, respectively, and nonfinancial noncorporate net value added (NNBNVAQ027S) for each line. Additionally, I utilized NIPA Table 1.5.5 Lines 30, 29, and 37 for each of equipment, structures, and intellectual property, respectively. I then summed each of these estimates to get an estimate of total quarterly investment by nonfinancial noncorporate entities.