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Fiscal Stimulus and Firms: A Tale of Two Recessions

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Abstract

In this paper, I examine the effects of a countercyclical fiscal policy that gave firms additional tax refunds—additional liquidity—at the end of the past two recessions. I take advantage of a discontinuity in the slope of the tax refund formula to estimate the policy’s impact. I find that after passage of the policy in 2002, firms allocated \$0.40 of every tax refund dollar to investment. After passage of the policy in 2009, in contrast, firms used the refunds to increase cash holdings (\$0.96 of every refund dollar) before paying down debt in the following year. I provide evidence that differences in macroeconomic conditions across the two periods drove these differences in firm responses, illustrating how the effects of stimulus vary across recessionary states of the world. I also show that while the policy had no discernable effect on investment in the most recent recessionary period, it did reduce firms’ bankruptcy risk and the probability of a future credit-rating downgrade.

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1. Introduction

In an attempt to stem declines in employment and output during the last two U.S. recessions and to spur a recovery, Congress enacted numerous fiscal stimulus measures—over \$100 billion in stimulus during the 2001 recession and an unprecedented \$1.9 trillion in stimulus during the 2007-09 recession.¹ A number of these measures were targeted at the corporate sector with the goals of increasing firm investment and reducing firm financial distress. While the policy goals were clear, from the perspective of economic theory it remains unclear whether fiscal stimulus targeted at firms was likely to meet those goals. First, the stimulus measures were implemented during weak economic conditions when firms' incentives to invest may have been poor. Second, the economic state of the world was quite different across the two recessions, and the effects of fiscal stimulus may have varied accordingly. The 2001 recession was the mildest in postwar history, while the 2007-09 recession followed a financial crisis and was the most severe in postwar history.² In this paper, I study the effects of a stimulus policy that gave firms additional tax refunds at the end of the past two recessions. I ask three questions: (1) How do firms allocate fiscal stimulus funds? (2) Do stimulus policies improve firm financial conditions? and (3) How do the effects of stimulus vary across different types of recessions?

The policy I study is the five-year carryback of net operating losses. Under the U.S. tax code, any firm experiencing a net operating loss (NOL) in a particular tax year can receive a refund for taxes paid in the previous two years—called an NOL “carryback” deduction. In 2002 and 2009, Congress expanded the carryback window from two years to five years to give additional tax refunds to firms. The policy was essentially an intertemporal transfer of tax benefits, not a cash windfall. A firm with an NOL also has the option to carry the NOL forward for 20 years to offset future taxable income—called an NOL “carryforward” deduction. Under the five-year carryback

¹ Major packages passed by the U.S. Congress included a \$120 billion package in March 2002, a \$150 billion package in February 2008, an \$830 billion package in February 2009, a \$45 billion package in November 2009, and an \$860 billion package in December 2010. For information on the cost estimates of provisions in these packages, see the Joint Economic Committee Publication “Estimated Budget Effects of the Conference Agreement for H.R. 4520, The ‘American Jobs Creation Act of 2004’” and the following Congressional Budget Office publications: “H.R. 5140, Economic Stimulus Act of 2008: Cost Estimate” (February 2008), “H.R. 1, American Recovery and Reinvestment Act: Cost Estimate for the Conference Agreement for H.R. 1” (February 2009), “H.R. 3548, Worker, Homeownership, and Business Assistance Act of 2009: Cost Estimate” (November 2009), “H.R. 4853, Tax Relief, Unemployment Insurance Reauthorization, and Job Creation Act of 2010: Cost Estimate” (December 2010), and “Estimated Impact of the American Recovery and Reinvestment Act on Employment and Economic Output in 2013” (February 2014).

² Real GDP declined 4.3 percent from the recession peak to the recession trough in the 2007-09 recession, while it increased 0.5 percent from peak to trough in the 2001 recession.

extension policy, most firms that received tax refunds would have expected to pay higher taxes in the future due to the reduction in NOL carryforwards available to offset future taxable income.³

From a theoretical standpoint, how one would expect firms to allocate the tax refund dollars is an open question. As discussed above, the policy acted as an intertemporal transfer of tax benefits—i.e., a temporary cash flow change. With unrestricted access to financial markets, short-term changes in cash flow should not affect investment or performance because firms should already be optimizing their investment and financing policies.⁴ The policy did provide firms with liquidity in the short term, however, and financially constrained firms may have used the tax refunds to increase investment. A large literature dating back to Fazzari, Hubbard, and Petersen (1988) shows that cash flow can be important for investment decisions in financially constrained firms.⁵ Alternatively, theory suggests firms may have had incentives to use the refunds to increase cash holdings or reduce debt. Firms facing high idiosyncratic risk may have increased cash due to precautionary savings motives (Opler, Pinkowitz, Stulz, and Williamson, 1999; Bates, Kahle, and Stulz, 2009). Or financially constrained firms may have increased cash (Almeida, Campello, and Weisbach, 2005; Han and Qui, 2007) or lowered debt outstanding (Acharya, Almeida, and Campello, 2007) to finance future investment opportunities. Finally, the choice of whether to allocate the tax refund to investment or another use may have depended on overall economic conditions, as in Bolton, Chen, and Wang (2011 and 2013) and Eisfeldt and Muir (2015), and may have varied across policy periods accordingly. Bolton, Chen, and Wang (2013) show that firms are more likely to hoard cash and reduce investment in a financial-crisis state of the world (as was the 2007-09 recessionary period) than a non-financial-crisis state (as was the 2001 recessionary period). Eisfeldt and Muir (2015) also highlight the importance of the aggregate state of the world in firm liquidity allocation. They show that when external financing costs are low, firms will invest the external capital they raise if productivity is high, but will hold the capital as cash if productivity is low.

³ The Joint Committee on Taxation (JCT) estimated that in the 2002 policy period, the IRS would issue about \$15 billion in additional refunds to firms, but that the 10-year revenue cost would only be about \$2 billion. In the 2009 period, the JCT estimated the IRS would issue about \$38 billion in additional refunds with a 10-year cost of about \$11 billion.

⁴ Seminal models include Modigliani and Miller (1958), Tobin (1969), Abel (1983), and Hayashi (1982).

⁵ Some of the notable papers regarding financial constraints include, for example, Blanchard, Lopez-de-Silanes and Shleifer (1994), Lamont (1997), Rauh (2006), and Hennessey and Whited (2007). Even when firms are unconstrained, Gomes (2001) shows that cash flow can be an important determinant of investment when external finance is costly.

Estimating the economic effects of fiscal stimulus is challenging in general, due to difficulties disentangling the effect of the policy from other economic factors influencing firm behavior at the time. The empirical challenge in analyzing the five-year carryback extension policy specifically is a potential endogeneity concern arising from omitted variable and simultaneity issues. Only firms with losses were eligible to receive refunds, and the firms receiving the largest refunds were those with the largest losses during the recession and the largest profits during the prior expansion (e.g., homebuilders in the 2009 policy period). Whether a firm received a tax refund and the size of a firm's tax refund could have been correlated, therefore, with other factors driving firm financial policies and performance, such as unobserved investment opportunities or managerial quality. In addition, firms had incentives to engage in tax planning to maximize their refunds, leading to the concern that managers may have chosen the tax-refund size based on a desire to allocate the funds to a specific purpose.

To overcome these challenges, I take advantage of a discontinuity in the formula that determines the size of the tax refund and use a regression kink design (RKD) empirical strategy. RKD has similar features to regression discontinuity design (RDD), but instead of exploiting variation around a discontinuity in the *level* of a policy variable of interest as in RDD, it exploits variation around a discontinuity in the *slope* of a policy variable (a “kink” in the variable). In my setting, a discontinuity in slope arises from the statutory formula that determines the size of a firm's tax refund. Under U.S. tax law, a firm's tax refund is based on its current losses and previous years' profits. Firms can only offset past tax liability up to the point where previous years' taxable income is equal to current losses. For example, if taxable income in the past five years is larger than current losses, a firm cannot receive any additional refund beyond the value based on current losses—the firm has already maximized its tax refund at this point. This statutory requirement introduces a kink in the slope of the tax refund formula at the point where previous years' taxable income equals current losses. The RKD identification strategy relies on assumptions that are similar to RDD: that the number of firms is distributed smoothly around the kink point and that predetermined firm characteristics evolve smoothly around the kink point (Card, Lee, Pei, and Weber, 2012). I present evidence that these assumptions appear to hold in my setting.

I first examine how firms allocated the tax refunds. Did firms increase investment or did they choose another use of funds, such as increasing cash holdings, reducing debt, or making payouts to shareholders? Next, I work to establish whether any difference in firm responses can be

attributed to differences in macroeconomic conditions. Finally, I analyze if the policy improved firm financial positions overall, testing whether receiving a tax refund affected firm bankruptcy risk, credit ratings, and the probability of bankruptcy or liquidation.

I document three main findings. First, I show that firms chose different uses for the tax refunds in the two policy periods. After passage of the 2002 policy, I find that firms allocated the funds to investment (\$0.40 of every refund dollar) in the year they received the refund; I find no effect in the following years. In contrast, I find that after passage of the 2009 policy, firms first held the tax refunds as cash and then used the refunds to pay down long-term debt. In 2010—the year of refund receipt—firms allocated \$0.96 of every refund dollar to higher cash holdings. Then, in 2011, firms reduced cash holdings and reduced long-term debt outstanding (\$1.26 out of every refund dollar). I find no effect on investment in any year of the 2009 policy period. I also find no effects of the policy on the change in employment—a key policy goal—in either the 2002 period or the 2009 period.

The use of tax refunds to increase investment in the 2002 period suggests that firms in my sample were financially constrained but had profitable investment opportunities at the time. In line with this hypothesis, I find that the increase in investment was concentrated in financially constrained firms and in firms with the highest investment opportunities. The finding that, in the 2009 policy period, firms used the tax refunds to increase cash holdings is consistent with the hypothesis that firms built cash either due to a precautionary savings motive in the face of high uncertainty or for financially constrained firms to fund future investment opportunities. My results suggest the uncertainty channel was the larger motivation for firms to increase cash. I find the cash increase was concentrated among firms with high historical cash flow volatility and realized stock-price volatility. I do not see the cash increase concentrated in financially constrained firms.

Second, I provide evidence that the difference in firm stimulus responses between policy periods was due to changes in macroeconomic conditions across the two periods—changes in the state of the world—and not changes in firm-level conditions across the two periods. Firms changing their allocations of marginal liquidity across policy periods is consistent with the theoretical prediction that firms may choose different uses of liquidity under different economic conditions, as in Bolton, Chen, and Wang (2011, 2013) and Eisfeldt and Muir (2015). Macroeconomic conditions across the two policy periods were quite different. The 2001 recession

was the mildest in postwar history—i.e., one of the shortest recessions with the smallest real GDP declines—though credit conditions for firms were tight during, and following, the recession. In contrast, the 2007-09 recession was the most severe since the Great Depression and was characterized by high volatility in markets, weak expectations for future economic growth, credit-market freezes, and tight credit conditions overall. (In Section 3, I provide additional background on economic conditions during the two recessions.) As macroeconomic conditions were changing, however, firm-level conditions were also changing, making it difficult to disentangle the two.

I attempt to isolate the effect of macroeconomic conditions by isolating a set of firms in the 2002 period that have similar characteristics to firms in the 2009 period using propensity score and Mahalanobis matching procedures. Holding firm characteristics constant between the 2002 and 2009 periods in this way, I still find that firms chose different responses in the two periods: investment in the 2002 period and an increase in cash in the 2009 period. This result suggests that differences in overall macroeconomic conditions—changes in the economic state of the world—as opposed to changing firm conditions drove the different stimulus responses.

The third main finding of this paper is that although the policy had no discernable effect on investment in the 2009 period, it did improve financial conditions overall in that period. I find that the tax refunds lowered the probability of a future credit rating downgrade and lowered firms' bankruptcy risk on average (as measured by distance to default, Altman's z-score, and Ohlson's o-score). These findings are consistent with my results that firms first held the tax refunds as cash in 2010 and then reduced cash in order to pay down long-term debt in 2011. These decisions would tend to reduce firm riskiness overall. The tax refund policy had a weaker effect on measures of financial conditions in the 2002 policy period. I only see a small effect on Altman's z-score. The policy had no effect staving off severe negative outcomes in either period, however. I find no effect on actual bankruptcy events.

I demonstrate that these results are generally robust to the empirical specification—to implementing a “sharp” RKD strategy instead of the preferred “fuzzy” RKD strategy, to the polynomial form of the regression specification, to narrowing the regression window around the kink point, and to excluding industries that were particularly hard hit in each recession (communications and airlines in 2002 and homebuilders in 2009). I find reasonable estimates of the average firm tax rate in both policy periods: 34 percent in the 2002 period and 31 percent in

the 2009 period. These estimates are not statistically different from each other and are close to the top marginal corporate tax rate of 35 percent. Finally, I find no evidence of confounding interactions with other firm-related fiscal policy measures enacted at the same time, such as “bonus” depreciation of investment expenses.

This work relates to three major strands of literature in finance and economics. First, the paper relates to the large literature on firm investment, cash, and debt management policies, as discussed above. I contribute to this literature by providing novel evidence on how firms’ liquidity allocations depend on overall macroeconomic conditions. Second, it relates to the literature that provides empirical evidence on the effects of fiscal policy targeted at firms.⁶ This paper is the first to study the economic effects of the tax carryback policy.⁷ In contrast to previous literature, I directly consider potential differences in responses across policy periods and show that firm responses can differ depending on economic conditions. I also study the effects of fiscal stimulus policies on firm financial conditions more broadly (i.e., bankruptcy risk and credit quality), in addition to studying how firms used fiscal stimulus funds.

Analyses of other stimulus policies targeted at firms have found mixed real effects to date. The most widely studied firm fiscal stimulus policy was the 2004 tax holiday on the repatriation of foreign earnings. Blouin and Krull (2009) and Dharmapala, Foley, and Forbes (2011) find that the main effect of the tax holiday was to increase shareholder payouts. This result suggests U.S. multinational firms were not financially constrained during the policy period. In contrast to the

⁶ A larger body of literature studies the effects of fiscal policy measures directed at consumers or direct government spending initiatives. For example, Johnson, Parker, and Souleles (2006) and Agarwal, Liu, and Souleles (2007) show that the 2001 income tax rebates caused a substantial increase in consumer spending after disbursement, particularly for liquidity constrained households. Johnson, Parker, Souleles, and McClelland (2013) find a similar result for the 2008 economic stimulus payments. Wilson (2010) estimates the effect of spending from the American Recovery and Reinvestment Act of 2009 (ARRA) broadly and finds that ARRA spending added 0.8 job-years per every \$100,000 of spending. Chodorow-Reich et al. (2012) show the ARRA provisions that increased federal Medicaid aid to states resulted in an additional 3.8 job-years for every \$100,000 in Medicaid spending. Mian and Sufi (2012) study the 2009 “Cash for Clunkers” policy and find that the policy had a large short-term effect in boosting automobile purchases but that most of the effect was reversed within the next 10 months.

⁷ Boynton and Cooper (2003) and Graham and Kim (2009) estimate the total value of the carrybacks and tax refunds to firms for varying carryback windows but do not study the effects of the policy as a stimulus measure. Graham and Kim (2009) also estimate how the carryback policy would affect firm marginal tax rates in the 2009 period and, hence, firm debt ratios, using estimates of the relationship between marginal tax rates and debt from Graham (1996). They estimate that the Obama Administration’s proposed policy would provide substantial additional liquidity to firms—\$19 billion and \$34 billion in 2008 and 2009, respectively, which would increase firm debt capacity by \$8 and \$10 billion in those years. Cohn (2011) studies the effect of net operating loss carryforwards on firm investment and shows that higher taxes are associated with firms reducing investment.

repatriation holiday, the tax carryback policy that I study was implemented right at the end of two recessions and explicitly targeted firms with losses during the recession—not just large, multinational firms. Firms with losses may have been more likely to be financially constrained during this time and more likely to use the funds for investment, which is the result that I find for the 2002 period. In this vein, Faulkender and Petersen’s (2012) study of the repatriation holiday shows that highly financially constrained firms used the repatriated earnings for investment. Studies of “bonus” depreciation—a policy that accelerated the schedule for deducting investment from taxable income—also find a substantial effect of the policy on firm investment (House and Shapiro, 2008; Mahon and Zwick, 2014a). Mahon and Zwick’s results suggest that bonus depreciation’s effect on investment stemmed from a cash flow channel—by lowering tax liabilities for financially constrained firms. They show that the policy’s effect on investment is concentrated among financially constrained firms and profitable firms, not in unconstrained firms or in firms that have losses and, hence, zero cash tax liability. My results for the 2002 period likewise highlight the importance of a cash flow channel in increasing firm investment through fiscal policy actions. I find no effect of fiscal stimulus on investment in the 2009 period, however, and instead find that firms allocate marginal liquidity to boosting cash and then to reducing debt. To the best of my knowledge, this paper is the first to empirically isolate the effect of different recessionary states of the world on corporate cash, financing, and investment policies.

Third, this paper relates to the literature that studies the role of financial constraints and uncertainty in propagating business cycles. A large body of work in macroeconomics shows that financing frictions can cause and amplify business cycles (Kiyotaki and Moore, 1997; Bernanke, Gertler, and Gilchrist, 1999).⁸ While I am not directly testing implications of these models, my paper provides empirical evidence on the role that external financial constraints played in the recoveries from the last two U.S. recessions. The tax carryback policy *directly relaxed firm financial constraints* at the end of the recessions by giving firms cash. My result from the 2002 policy period—that financially constrained firms used the tax refunds to increase investment—suggests financing constraints did indeed play a role restraining investment following the 2001 recession. In the 2009 period, in contrast, I find that when the NOL carryback extension lifted firm financial constraints by providing additional liquidity, firms did *not* invest the additional funds.

⁸ Brunnermeier, Eisenbach, and Sannikov (2013) survey the literature on financing frictions in macroeconomics.

Instead, firms held the funds as cash, which suggests that financing constraints may not have been the key friction restraining investment after the 2007-09 recession. My results suggest that instead, high uncertainty may have been playing a larger role in restraining investment. In particular, I find that the cash holdings result was concentrated in firms facing higher uncertainty about future prospects. A rapidly growing literature stemming from work by Bloom (2009) highlights the role of aggregate uncertainty shocks as another important channel that causes and propagates business cycles. This work is grounded in the real options theory literature, which shows that firms delay investing until economic uncertainty is resolved over time or until the benefits of investment become sufficiently large (Cukierman, 1980; Bernanke, 1983; Pindyck, 1991).

The remainder of the paper proceeds as follows. Section 2 provides background on the applicable U.S. tax code statutes and the NOL carryback policy implementation. Section 3 gives background on the economic conditions in each policy period. Section 4 describes the empirical strategy and the data sample. Section 5 presents my results on the effects of the policy. Section 6 concludes.

2. Policy Background

2.1 U.S. Tax Code

In any given year, a firm sustains a net operating loss (NOL) for tax purposes when its allowable tax deductions exceed gross income. Under section 172 of the Internal Revenue Code, these losses can be used in two ways. First, they can be used to offset taxable income in either of the prior two years, for which the firm receives a tax refund. This policy is known as an “NOL carryback.” Alternatively, if the firm does not have positive taxable income in the prior two years or elects not to use its carryback, it can carry the loss forward for up to 20 years and use it to offset future taxable income, thereby lowering its tax bill at some point in the future. This policy is known as an “NOL carryforward.”⁹ According to the Joint Committee on Taxation (JCT), “the intent of

⁹ Using an NOL carryforward is fairly straightforward: Firms enter the amount of deduction they would like to take on line 29 of form 1120 when they file their tax returns. In the case of a carryback, they have the option to file form 1139 in the 12 months following the end of the taxable year in which the loss is incurred. After that one-year period, they can still use the carryback and get a refund by filing an amended tax return using Form 1120X. Firms keep track of their NOL carryovers and report the total on Schedule K of IRS Form 1120. Also, firms incurring a loss can make an election to permanently forego the carryback of that loss on Schedule K. In mergers and acquisitions, there are special rules that limit an acquiring company’s use of NOLs on the books of a target firm. There are also special rules

the NOL carryback/carryforward provision is to give taxpayers the ability to smooth out changes in business income, and therefore taxes, over the business cycle.”¹⁰ In 2002 and then again in 2009, Congress enacted legislation extending the NOL carryback window from two years to five years. I describe each policy action in Section 2.2.

I present an example of how the carryback deduction is applied under the two-year policy and the five-year extension policy in Table 1. The example firm had a \$100 million NOL in 2001 (hence, a maximum potential NOL deduction of \$100 million) and profits ranging from \$50 million in 1996 to \$10 million in 2000 before taking the NOL deduction into account. I assume a tax rate of 35 percent in each year.

Under the two-year carryback policy, the firm could take a \$20 million NOL deduction in 1999 and a \$10 million deduction in 2000—receiving a tax refund of \$10.5 million ($0.35 \times \30 million). In this case, the firm keeps a \$70 million NOL to carry forward in the future (\$100 million minus \$20 million minus \$10 million). Under the five-year carryback extension policy, the firm’s tax refund was substantially larger. This firm could take a \$50 million carryback deduction in 1996, a \$40 million deduction in 1997, and a \$10 million deduction in 1998. Since the firm can now deduct the full \$100 million NOL, it receives the maximum tax refund of \$35 million and has no losses remaining to carry forward. The firm receives an additional \$24.5 million as a result of the carryback extension. (This calculation assumes the firm would take the deductions starting in the earliest year of the window.)

2.2 The Job Creation and Worker Assistance Act of 2002¹¹

The Job Creation and Worker Assistance Act of 2002 (JCWA) was introduced in October 2001 and signed into law in early March 2002, allowing firms to carryback losses incurred in tax

governing the use of NOLs to offset Alternative Minimum Taxable Income, governing the use of NOLs for life insurance companies, and governing farming losses, disaster losses, casualty loss, etc.

¹⁰ U.S. Congress, Joint Committee on Taxation, Estimated Budget Effects of The Chairman’s Amendment in The Nature of a Substitute to H.R. 598, The “American Recovery and Reinvestment Tax Act of 2009,” 111th Cong., 1st sess., January 22, 2009, JCX-9-09.

¹¹ For cost estimates of each of the three pieces of legislation, see the following Joint Committee on Taxation publications: Estimated Revenue Effects of the “Job Creation and Worker Assistance Act of 2002,” March 6, 2002, JCX-13-02; Estimated Budget Effects of the Revenue Provisions Contained in the Conference Agreement for H.R. 1, the “American Recovery and Reinvestment Act of 2009,” February 12, 2009, JCX-19-09; and Estimated Revenue Effects of Certain Revenue Provisions Contained in the “Worker, Homeownership, and Business Assistance Act of 2009,” November 3, 2009, JCX-45-09.

years 2001 and 2002 for five years instead of the usual two. Losses in 2001 could be carried back to offset income in 1996, 1997, and 1998, in addition to income in 1999 and 2000. At the time of passage, the JCT estimated that the NOL provision would return an additional \$7.9 billion in tax refunds to firms in 2002 (for losses incurred in tax year 2001) and \$6.6 billion in tax refunds to firms in 2003 (for tax losses incurred in 2002). Over a 10-year horizon, the JCT estimated that the NOL provision would have a revenue cost of about \$2 billion, reflecting the future reduction in carryforwards available to offset taxable income. The JCWA also included measures to extend a number of expiring tax code provisions, to provide an extra 30 percent first-year expensing for qualified capital investments (known as bonus depreciation), and to extend unemployment insurance benefits for workers.

2.3 The American Recovery and Reinvestment Act of 2009 (ARRA)

The 2009 policy was enacted in two separate pieces of legislation. As part of the ARRA—the \$830 billion stimulus package that was signed into law in February 2009—Congress extended the carryback window for losses incurred in tax year 2008. This policy was limited to small businesses, i.e., those with less than an average of \$15 million in gross receipts per year over the previous three years. The JCT estimated that the policy would return \$4.7 billion in refunds to firms in 2009 with a 10-year cost of about \$1 billion. The five-year carryback was small relative to the overall package, which also included an extension of the bonus depreciation allowance (an extra 50 percent of first-year expensing), a payroll tax credit, an additional child tax credit, and additional government funding for health care, education, and infrastructure.

2.4 The Worker, Homeowner, and Business Assistance Act of 2009

The budget released in May 2009 by the Obama Administration included a proposal to allow the carryback to apply to all firms and to apply to losses in both 2008 and 2009. Congress introduced legislation to this effect in September 2009 and passed the Worker, Homeowner, and Business Assistance Act of 2009 (WHBA) to extend the five-year carryback window in November 2009. The carryback extension could only be applied to either 2008 losses or 2009 losses, not both. The exception was for firms that qualified for the policy under the ARRA. These firms were allowed to apply the extension to both years. Firms were only allowed to apply 50 percent of

taxable profits in the earliest year of the extension window to the policy. Also, firms that received assistance under the Troubled Asset Relief Program (TARP) were excluded from participating. The JCT estimated that the policy would return an additional \$33 billion to taxpayers in 2010 and that the expected 10-year cost of the program would be \$10.4 billion.

2.5 Policy Goals

Policymakers specified two main goals when implementing the five-year carryback policy: to boost economic growth by increasing investment and employment and to help firms smooth income and mitigate financial distress. A statement from Treasury Secretary Paul O’Neill after passage of the 2002 policy read: “This legislation will add momentum so that we have a more robust economic recovery and return to full prosperity.... [T]his legislation....will speed America back to work” (U.S. Treasury, 2002). A statement from the House Ways and Means Committee describing the 2009 policy prior to passage stated that the legislation would give firms “cash infusions that would help them weather the current economic storm” (Committee on Ways and Means, 2009). And a post-passage White House fact sheet stated that the legislation “creates jobs by cutting taxes for struggling businesses....putting \$33 billion of tax cuts in the hands of businesses this year when they need it most” (White House, 2009).

3. Empirical Strategy and Data Description

3.1 Regression Kink Design Overview

To estimate the effects of providing tax refunds under the five-year carryback policy, I use a regression kink design (RKD) strategy. This strategy takes advantage of a discontinuity in the slope of the formula that determines the size of a firm’s tax refund. In general terms, RKD identifies the causal effect of a particular policy variable on an outcome variable by using “kinks”—discontinuities in slope—in the assignment rule for the policy variable (Card, Lee, Pei, and Weber, 2012). For example, one can test the effect of unemployment insurance benefits (the policy variable) on the duration of joblessness (the outcome variable) based on the phase out of unemployment insurance benefits at higher income levels (the assignment variable) (Card, Lee, Pei, and Weber, 2012). Another example would be to test the effect of the earned income tax credit

(the policy variable) on labor force participation (the outcome variable) based on the phase out of the EITC at higher income levels (the assignment variable) (Jones, 2011).

The intuition behind the strategy is that the causal effect of a policy can be estimated by testing for a kink in the outcome variable that occurs at the kink in the assignment variable. RKD is a similar concept to regression discontinuity design (RDD). RDD identifies an effect using a discontinuity in the *level* of the function that relates an assignment variable to the outcome variable. RKD identifies an effect using a discontinuity in *slope* of the function.

As with RDD, RKD has a “sharp” and “fuzzy” variant. In sharp RKD, the change in slope that occurs at the kink point is precisely known and is equal for all affected entities. Fuzzy RKD, on the other hand, uses an estimate of the average change in slope across agents based on the observed data. In my setting, the estimated slope in the tax refund corresponds to the average tax rate of firms, as discussed below. I implement a fuzzy RKD strategy, therefore, to account for differences in average tax rates between firms and because I estimate firms’ tax refunds based on taxes paid as reported in financial statements, not on tax returns. In robustness tests, I present estimates from a sharp RKD strategy as well.

3.2 Regression Kink Design Applied to the NOL Carryback Policy

In my empirical setting, the outcome variables of interest are firm uses of cash flow (e.g., investment, change in cash holdings, and change in debt) and measures of financial health (e.g., bankruptcy risk and credit ratings). The policy variable is the size of a firm’s tax refund. The assignment variable that determines the value of a firm’s tax refund is a function of positive taxable income (which I will call profits) in previous years and the size of losses in a given policy year. Under the five-year carryback policy, a firm that incurs a loss in tax year 2001, for example, can receive a refund for taxes paid from 1996 to 2000 until the point at which total profits from those years equals the 2001 loss. This statutory condition introduces a kink in cash available to a firm from the tax refund at the point where losses in 2001 equal previous years’ profits.

As an example, take three firms that each sustained a \$100 million loss in 2001. Firm A earned \$80 million in profits from 1996 to 2000, firm B earned \$100 million in profits, and firm C earned \$120 million in profits. Under the five-year carryback policy, firm A would receive a \$28 million tax refund ($\80×0.35, assuming a 35 percent tax rate), while both firms B and C would

receive a \$35 million tax refund. Although firm C had a higher level of profits from 1996 to 2000, firm C can only receive a refund for taxes paid in previous years until the point at which previous profits equal current losses.

Figure 1 shows an example of the kink in the tax refund formula for a set of firms with \$100 million in 2001 losses and varying amounts of profits from 1996 to 2000 (assuming a 35 percent tax rate). The firm's tax refund is a function of two variables: 1) profits over the five-year carryback window and 2) the firm's policy-year losses. The kink point occurs where previous years' profits equal policy-year losses—at \$100 million, in this example. To the left of the kink, in the region where past profits were less than current losses, the slope of the tax refund function is the firm's tax rate. For every extra dollar of past profits, a firm receives an extra \$0.35 in tax refund. To the right of the kink point—the region where past profits exceed current losses—the slope of the function is zero. A firm receives no additional refund for an additional dollar of past profits in this region.

This example illustrates the tax refund function for firms with \$100 million in losses. In my sample, however, I have firms with a wide range of losses in the policy years. Each firm would receive its maximum refund at the point at which their policy-year losses equal their previous years' profits. To standardize the tax refund function across firms, therefore, I generate one assignment variable to describe the function: previous years' profits *minus* policy-year losses. I denote this variable as “V” for the remainder of the paper. The kink point in this variable occurs at zero for all firms. As in Figure 1, when previous years' profits are less than policy-year losses, the slope of the function is the firm's tax rate. When previous years' profits are greater than policy-year losses, the slope is zero.

In Figure 2, I provide examples of kinks in the tax refund and outcome variables around the assignment variable V. The left panel of the figure shows a discontinuity in slope in firm tax refunds for the 2002 policy period around the kink point $V=0$, and the right panel shows a kink in the firm investment for the 2002 policy period around the kink point $V=0$.

3.3 A General RKD Model

In this section, I describe the RKD methodology in detail. As in Nielsen, Sorensen, and Taber (2010), let the following model represent the general, causal relationship between an outcome variable of interest (Y) and a policy variable of interest (X):

$$Y = \beta_1 X(V) + g(V) + \epsilon.$$

In this model, X is a deterministic and continuous function of the assignment variable V , and the function relating X and V has a kink at $V = V^*$. The outcome variable Y may be a direct function of V as well— $g(V)$ —and the error term ϵ is a vector of unobservable random variables. In my setting, Y is the firm outcome variable, X is the tax refund, and V is total profits over the five-year carryback window minus policy-year losses. As discussed above, the kink occurs where previous years' profits equal policy-year losses ($V^*=0$).

The typical problem in evaluating a model like the one above is that the error term ϵ is correlated with X , leading to bias in estimates of β_1 . In RKD, however, if $g(V)$ and $E(\epsilon|X)$ have no kink in V at V^* —i.e., they have derivatives that are continuous in V at $V = V^*$ —then the parameter of interest β_1 identifies the causal effect of X on Y and is equal to the following term:

$$\beta_1 = \frac{\lim_{v \uparrow v^*} \frac{dE[Y|V=v]}{dv} - \lim_{v \downarrow v^*} \frac{dE[Y|V=v]}{dv}}{\lim_{v \uparrow v^*} \frac{dX(v)}{dv} - \lim_{v \downarrow v^*} \frac{dX(v)}{dv}}. \quad (A)$$

The expression on the right-hand side of the equation is the change in the slope of the conditional expectation of Y given the assignment variable V at the kink point, divided by the change in the slope of the deterministic function that relates X and V at the kink point. The policy's effect is identified by estimating the kink in the outcome variable with respect to the assignment variable and then making an adjustment for the magnitude of the kink in the policy variable with respect to the assignment variable.

Card, Lee, Pei, and Weber (2012) show that under two major identifying assumptions, expression (A) can recover the treatment on the treated parameter in a generalized non-separable model as well:

$$Y = f(X, V, W),$$

where Y is the outcome variable, X is the policy variable of interest, V is the assignment variable that enters the model with a “kink” at V^* , and W is an unobservable, non-additive error term.

The first identification assumption in Card et al. (2012) is that the probability density function of firms is continuously differentiable in V —i.e., the density is smooth around the kink point. In short, all firms cannot perfectly choose the level of current losses relative to past profits that they can apply to the tax refund policy.

The second assumption is that predetermined firm characteristics are continuously differentiable with respect to V around the kink point. In other words, firms must be similar in other respects around the kink point so as to be comparable. If firms have a kink in characteristics on either side of the kink point, these other characteristics may be driving the result, rather than the policy variable of interest driving the result. I provide evidence that suggests both assumptions are satisfied in my setting in Section 3.6.

3.4 RKD Empirical Specification

To estimate a fuzzy RKD, I use a two-stage least-squares instrumental variable (IV) strategy (Card, Lee, Pei, and Weber, 2012). In the first stage, I estimate the change in slope of the policy variable—the tax refund—at the kink point at which previous years’ profits equal policy-year losses. The variable that estimates this change in slope is an excluded instrument in the IV strategy. In the second stage, I use the fitted values of the tax refund to estimate the effect of the tax refund on firm outcomes.¹² I describe each stage below.

First-Stage Regression

The empirical specification for the first-stage regression is as follows:

¹² As I do not directly observe take-up of the policy and instead assume that all firms in my sample take it up, I estimate the intent-to-treat (ITT) effect of the policy as opposed to the average treatment effect (ATE). Estimating the ITT effect indicates that my results are a lower bound for the average treatment effect. Mahon and Zwick (2014b) show that the value of claimed carryback refunds increases substantially during the two policy periods and that take-up rates are higher for larger firms (as Compustat firms are), suggesting the tax carryback policy was relevant for at least a large fraction of firms in my sample.

$$(1) TaxRefund_{it} = \delta_0 + \sum_{p=1}^P [\delta_p (V_{it-1} - V^*)^p + \gamma_p D \cdot (V_{it-1} - V^*)^p] + \theta Controls_{it} + \omega_n + \epsilon_{it},$$

where i is firm, t is year, and n is industry. V is the assignment variable (previous years' profits minus policy-year losses) and V^* is zero. I use the level of V (in millions of dollars) in the regression as opposed to scaling V by assets or another measure because the policy kink occurs in the level of the tax refund and V . In the context of Figure 1, the function relating V and the tax refund is linear in the level of V to the left of the kink point. This function is not linear in V as a share of assets or in a re-centered logarithm of V .

The instruments for *TaxRefund* are a dummy variable that equals one if previous years' profits were less than policy losses (D) interacted with a polynomial in V . In the context of Figure 1 above, D is a dummy variable that equals one if a firm is to the left of the kink point. The coefficient γ_1 in this specification recovers the change in the slope of the tax refund value with respect to V around the kink point (the denominator from the estimand in expression A). In my setting, this value is equal to the average estimated tax rate.

Under the identification assumptions of regression kink design, the instruments satisfy the assumptions required for a valid IV strategy. In my setting, the instruments satisfy the relevance condition because location relative to the kink point strongly affects the size of a tax refund, as illustrated above. The exclusion condition—that being on one side of the kink point or the other does not affect firm outcomes through another channel besides the size of the tax refund—also appears reasonable. The kink point is a statutory requirement, and one of the model identification assumptions is that predetermined firm characteristics have a smooth distribution around the kink point. (This assumption appears to be satisfied in my setting as I show below.) Because firm characteristics are similar around the kink and evolve smoothly, but the kink point is a sharp discontinuity set in law, it is reasonable that being above or below the kink has no effect on firm outcomes except through the formula that determines the tax refund.

My preferred polynomial order is $P = 2$, in line with other RKD studies, and I present robustness to other polynomial orders in Table A2. I include the following controls in the regression: pretreatment values of Tobin's q , return on assets, cash flow/assets, sales/assets, leverage, the firm's marginal tax rate, the log of assets, and a quadratic in the value of losses that can be applied to the policy. I include Fama-French 48 industry fixed effects to account for

macroeconomic shocks that affect industries differently and cluster the standard errors at the Fama-French 48 industry level to account for unobserved correlation in errors within industries.

Second-Stage Regression

The empirical specification for the second-stage regression is as follows:

$$(2) \text{ FirmOutcome}_{it} = \alpha_0 + \beta_1 \widehat{\text{TaxRefund}}_{it} + \sum_{p=1}^P [\alpha_p (V_{it-1} - V^*)^p] + \theta \text{Controls}_{it} + \omega_n + \epsilon_{it},$$

where i is firm, t is year, and n is industry. $\widehat{\text{TaxRefund}}$ is the fitted values from the first-stage regression and V is the assignment variable as described above. I include the same controls as in (1) and I cluster the standard errors at the Fama-French 48 industry level.

I study two types of outcome variables of interest: potential uses of the tax refunds (i.e., investment, payout, and the change in cash holdings) and measures of firm financial conditions including bankruptcy risk and credit conditions (Altman's z-score, Ohlson's o-score, distance-to-default, S&P credit rating upgrades and downgrades, and actual bankruptcy or liquidation). For analyzing the potential uses of funds variables, I use the level of spending, in millions of dollars. I use levels because the kink in the tax refund formula occurs in the level of V , and I would expect the corresponding kink to occur in the level of the potential uses of funds. Measuring these variables in levels also results in a convenient interpretation of β_1 as a firm's marginal propensity to invest or otherwise allocate the funds out of every additional dollar of the tax refund.

Finally, I study the effects of the 2002 policy and the 2009 policy separately. The time dimension of the above regressions varies depending on the policy period. For the 2002 policy, firms received refunds in 2002 and in 2003 for losses incurred in 2001 and 2002, respectively. I regress firm outcomes in 2002 and 2003 as a function of the tax refunds received in those years and the assignment variable V in the previous year. This is a two-year panel regression. For the 2009 policy, most firms received only one refund—in 2010—for losses in 2008 or 2009. For the empirical specification of the 2009 policy period, I regress firm outcomes in 2010 (or 2011) on the value of the tax refund received in 2010 and the assignment variable V at the end of 2009. This is a one-year, cross-sectional regression.

3.5 Data Description

In this analysis, I use financial variables from Compustat and CRSP, as well as data on S&P credit ratings from Capital IQ and analyst forecast dispersion from I/B/E/S. I use data on marginal tax rates provided by John Graham (Graham, 1996) and data on bankruptcy filings provided by Sudheer Chava (Chava and Jarrow, 2004; Chava, Stefanescu, and Turnbull, 2011; Alanis, Chava, and Kumar, 2013).

The crux of the analysis relies on calculating the firm tax refund and generating the assignment variable V —profits (positive taxable income) available to apply to the policy from the five-year carryback window minus total losses (negative taxable income) to apply to the policy. As taxable income is not available on firm financial statements, I calculate an estimate of taxable income based on Compustat data in a manner similar to Graham and Kim (2009). The difference between my measure of taxable income and the Graham and Kim measure is that I calculate a U.S.-specific measure because a firm's tax refund under the policy is based only on U.S. taxable income and taxes paid. The Graham and Kim (2009) measure is based on worldwide pre-tax income. I then calculate total profits and total losses that can be applied to a carryback for a given policy year, as well as the firm's tax refund following Graham and Kim (2009) and Boynton and Cooper (2003). These calculations are detailed in the appendix.

Following Graham and Kim (2009), I require that firms in my data sample 1) experience a loss that can be applied to the policy, 2) are present in Compustat for the five-year window required to calculate the carryback value, 3) and have total assets of greater than \$1 million. To remove the influence of extreme outliers, I exclude firms in the 1 percent tails of V , the 1 percent tails of investment (for variables measuring potential uses of the tax refund), and a few extreme outlier points.¹³

Table 2 shows summary statistics for the outcome variables for all firm-years in the sample. I presents details on the construction of each variable in the appendix.

¹³ For example, in the 2002 policy period sample, I observe one firm (Lucent Technologies) with an estimated tax refund of \$2.5 billion, whereas the second-largest estimated tax refund is \$1.1 billion. I calculate an average federal tax rate of 50.5 percent for Lucent Technologies from 1996 to 2000. In 2000, the estimated rate is particularly unrealistic (92 percent), suggesting my methodology has overestimated this refund. Therefore, I exclude this firm from the sample.

3.6 Empirical Strategy Validity

I present evidence that the RKD identification assumptions hold in Figure 3, Figure 4, and Table 3. Regarding the first assumption—that probability density function of firms is continuously differentiable in V —Figure 3 shows a histogram of firms around the $V = 0$ kink point in \$0.25 million bins of the assignment variable V for both the 2002 policy period and the 2009 policy period. Though the distributions are somewhat noisy—particularly in the 2009 period—there is no obvious discontinuity around the kink point.¹⁴

Regarding the second assumption—that predetermined firm characteristics have a smooth distribution around the kink point—Figure 4 shows the average value of a wide range of pre-treatment firm characteristics for firms in \$1 million bins above and below the zero kink point for the 2009 policy data set: leverage, Tobin’s q , return on assets, and the book value of total assets. While again the distributions of these variables are noisy, they appear to evolve fairly smoothly around the kink point. I test for a kink in characteristics more formally by collapsing the data into \$0.5 million bins and estimating the following specification in a narrow window around the kink point (-\$25 million to \$25 million) for both the 2002 and 2009 policy periods, as in Turner (2014):

$$(3) Y_{it-1} = \alpha_0 + \alpha_1(V_{it-1} - V^*) + \beta_1 D \cdot (V_{it-1} - V^*) + \epsilon_{it}.$$

In this specification, Y is the re-centered residual from a regression of a firm characteristic on industry fixed effects (to control for industry-specific effects). The coefficient β_1 recovers the change in slope around the kink point. A statistically significant coefficient would suggest a kink in the characteristic, but I find no evidence of kinks in firm characteristics in either policy period (Table 3).

A potential concern for the research design may be that firms are using tax-management strategies to increase the size of their tax refunds; this would only be a concern if tax management resulted in a kink in the distribution of firms or in firm characteristics around the kink point. First, it is worth noting that the assumption of a smooth distribution of firms does not require that firms could in no way manipulate their tax refund position. Indeed, firms have incentives to do extensive tax planning (Armstrong, Blouin, and Larcker, 2012). Instead, the identification assumption

¹⁴ A McCrary (2008) test of the continuity of the firm distribution around the kink point shows no manipulation. Note the McCrary test does not test that the firm distribution is continuously differentiable as the RKD assumptions require, but continuity is a necessary condition for continuous differentiability.

requires that there is sufficient randomness such that firms cannot perfectly sort themselves on either side of the kink point.¹⁵

Second, I investigate whether there is a kink in the distribution of firms that may use aggressive tax-planning strategies and do not find evidence of such a kink. To proxy for aggressive tax planning, I use two measures of aggressive earnings management: the absolute value of accruals and the level of accruals (Bergstresser and Philippon, 2006). The distribution of firms with an above-median absolute value of accruals (aggressive earnings management) does not appear appreciably different than the distribution of firms with a below-median absolute value of accruals (less aggressive earnings management) for either the 2002 or the 2009 policy period (Figure A1). In addition, all of the distributions have a larger mass to the left of the kink point where V is negative, which indicates generally that many firms were not maximizing their tax refunds. Firms receive their maximum tax refund at the point $V = 0$. In addition, I find that the pretreatment level of accruals and absolute value of accruals have a smooth distribution around the kink point (Table 3).

4. Background on Economic Conditions: 2002 vs 2009 Policy Period

While the tax refund policies enacted in 2002 and 2009 were similar, economic conditions across the two recessions and recovery periods were quite different. To give context to the policy analysis, I offer some brief background on the two periods in Figure A3. There are three main takeaways. First, economic conditions and perceptions about future economic conditions were much weaker in the 2007-09 recession than in the 2001 recession. Real GDP declines during the 2007-09 recession were the largest since the Great Depression. In contrast, there were only two mild quarterly declines in the 2001 recession (Figure A3.1). Furthermore, CFO optimism about future economic performance was lower during the 2009 post-recession period than the 2001 post-recession period, as were professional forecasters' expectations about future GDP growth (Figure A3.2).

¹⁵ During recessionary periods, firms face unanticipated negative shocks; it may be expected that they would have less room to maneuver their tax position in these periods. In addition, the policy assignment variable V also depends on five years of taxable income history during which the policy implementation was likely unanticipated, introducing another element of difficulty in perfectly manipulating the tax refund size.

The second takeaway is that although credit conditions were much worse during the 2007-09 recession than the 2001 recession, conditions were tight in both recovery periods. Figure A3.3 shows that the Baa-Aaa corporate bond spread and the TED spread (the three-month LIBOR minus the three-month Treasury bill rate—a measure of interbank lending conditions) spiked in 2008 and 2009. The Baa-Aaa spread remained elevated in both recoveries, however, and the net percentage of domestic banks reporting tighter lending standards for commercial and industrial (C&I) loans was elevated longer in the 2001 recovery period than the 2007-09 recovery period (Figure A3.4).

The third takeaway is that measures of uncertainty about future economic conditions were higher in the 2007-09 recession than the 2001 recession and economic policy uncertainty was higher in the 2007-09 recovery period. Stock volatility as measured by the VIX hit record levels in the 2007-09 recession (Figure A3.5), though after the recession, the VIX fell to levels that followed the 2001 recession. Dispersion in professional forecasters' future expectations of growth was also particularly high in the 2007-09 recession, though it fell following the recession. However, economic policy uncertainty, as measured by the Baker, Bloom, and Davis (2013) index, remained substantially elevated in the 2007-09 recovery period as compared with the 2001 recovery period (Figure A3.6).

5. Firm Responses to the NOL Carryback Extension Policy

5.1 First-Stage Estimates of the Tax Refund Rate

I begin by presenting results from the first-stage regression (specification 1) that estimates the change in slope of the tax refund function around the kink point. As discussed above, this change should recover the average firm tax rate in the two carryback window periods. Observing a reasonable tax rate is an important test for the validity of my empirical strategy and I do observe reasonable rates in both policy periods: 34 percent in the 2002 policy period and 31 percent in the 2009 policy period (Table 4). These coefficient estimates are not statistically different from each other and are in a reasonable range for average statutory corporate income tax rate estimates.¹⁶ The

¹⁶ The tax rate I estimate is the average rate paid on positive taxable income, not the average effective tax rate across firms. The average effective U.S. corporate tax rate is lower because it averages firms that pay few taxes due to sustaining losses or due to having NOL carryforwards available to offset taxable profits. From 1996 to 2000, for example, total U.S. corporate tax receipts as a percentage of domestic economics profits averaged 25.9 percent. From

highest marginal corporate tax rate in the United States is 35 percent, which phases in at taxable income of \$18.3 million. Any taxable income above \$75,000 is subject to a tax rate of at least 34 percent; most Compustat firms with taxable income would fall into this category.

5.2 Tax Refund Allocation in the 2002 Policy Period

I next study how firms allocate their refund dollars. I look at each policy period separately so as not to assume that firms would have taken the same actions across the two periods. Table 5 shows the effect of the tax refund on potential uses of funds for the 2002 policy period (empirical specification 2). Panel A shows the effect on the major uses of funds on a firm's cash flow statement: investment, change in cash, change in total debt, payout, and other potential uses (acquisitions, short-term financial investment, and long-term financial investment). Column 6 of Table 5 shows the tax refund's estimated effect on the total of these potential uses. The estimated coefficient in each of these regressions is interpreted as the marginal propensity of a firm to invest or use the funds otherwise out of each additional dollar of tax refund. Column 7 shows the effect on the change in employment. The coefficient in this regression is interpreted as the change in the number of employees (in thousands) for each million dollars of tax refund received.

I find that in the 2002 policy period, firms allocated \$0.40 of each refund dollar to investment in the year they received the tax refund (2002 and 2003). I cannot identify a statistically significant average use of the rest of the funds and I find no effect of the refund on firm hiring. I find that the regression estimates across the major uses of funds sum to a point estimate of 1.09, indicating that \$1.09 of each \$1 of tax refund was allocated to one of these uses. This estimate is relatively close to \$1 of total uses and shows that the empirical specification is capturing the uses of the tax refund reasonably well. In the years following receipt, I do not find any effects for the 2002 policy period (Table A5).

Next, I study whether financial constraints and investment opportunities affected firms' responses to the tax refund policy. If firms were financially unconstrained, the tax refunds should have little effect on investment. As canonical theories in finance show, in a frictionless environment, a firm with attractive investment projects could receive necessary funding from

2003 to 2007, total U.S. corporate tax receipts as a percentage of domestic economics profits averaged 22.4 percent (CBO, 2014).

capital markets. The result from the 2002 policy period is consistent with the hypothesis that, at the time, firms had profitable investment opportunities but were financially constrained. When the five-year carryback policy lifted constraints, firms invested the funds. To test this hypothesis, I examine whether the investment increase was concentrated in financially constrained firms and whether it was concentrated in firms with better investment opportunities.

I separate firms into subsamples of financially constrained firms and unconstrained firms (and, likewise, into firms with high and low investment opportunities) and I compare the effects of tax refund receipt on investment between subsamples. I use three measures of financial constraints. First, I sort firms in the sample by the level of the Kaplan-Zingales (1997) index of financial constraints, and I classify a firm as financially constrained if the firm falls in the top quartile of the distribution.¹⁷ Second, I classify firms as constrained if they do not pay dividends or repurchase stock. Third, I use a Dharmapala, Foley, and Forbes (2011) measure and define a firm as financially constrained if total payout to operating income is less than or equal to zero. All measures are calculated using lagged values of the firm characteristic.

To test whether the increase in capital expenditures was concentrated in firms with high investment opportunities, I classify a firm as having high investment opportunities in three ways: 1) if the firm falls in the top quartile of the distribution of lagged Tobin's q , 2) if the tax refund was received in 2003 (rather than 2002), and 3) if the firm is a "multinational" firm, defined as having a substantial share of foreign activity (foreign pre-tax income of more than 5 percent of total pre-tax income in absolute value as in Graham and Mills, 2008). By 2003, the economic recovery was gaining speed; it is therefore likely that firm investment opportunities were better in 2003 than in 2002. Multinational firms are also likely to have a larger investment opportunity set than domestic firms. I classify low investment opportunity firms, therefore, as: 1) those in the bottom quartile of Tobin's q , 2) as having received the refund in 2002, and 3) as having primarily domestic activity (foreign pre-tax income less than or equal to 5 percent of total pre-tax income in absolute value).

I present results for the effect of tax refund receipt on firm investment for financially constrained and unconstrained firms in Table 6, Panel A. As hypothesized, the investment result

¹⁷ Using the Compustat variable names, this index is defined as $-1.002*(IB + DP)/PPENT_{t-1} + 0.283*(AT + PRCC_C*CSHO - CEQ - TXDB)/AT + 3.139*(DLTT + DLC)/(DLTT + SEQ) - 39.368*((DVC + DVP)/PPENT_{t-1}) - 1.315*(CHE / PPENT_{t-1})$.

from the 2002 policy period is concentrated in financially constrained firms. I estimate that these firms spent between \$1.00 and \$1.09 of each \$1 of tax refund on investment. I see no statistically significant effect of the policy on the investment of unconstrained firms.

I also find evidence that investment spending out of the tax refund was concentrated in firms with higher investment opportunities (Table 6, Panel B): high Tobin's q firms (\$1.02 of each tax refund dollar, column 1), in 2003 (\$0.75 of each tax refund dollar, column 4), and in multinational firms (\$0.68 of each tax refund dollar, column 5). I find no statistically significant effect of tax refund receipt on investment in low Tobin's q firms, in 2002, or domestic firms (Columns 2, 3, and 6, respectively). Note that investment reported in Compustat is a worldwide measure of investment. Given that the investment response was concentrated in multinational firms, I cannot say for certain whether investment resulting from the refund policy was carried out in the United States as policymakers intended or whether firms transferred the tax refunds to invest overseas.¹⁸ My results represent an upper bound of the effect on domestic investment.

5.3 Tax Refund Allocation in the 2009 Policy Period

In the 2009 policy period, I find that firms chose different uses of funds. In the year firms received the refund (2010), they allocated \$0.96 to higher cash holdings for every dollar of tax refund on average (Table 7, Panel A, column 2). The regression estimates across all uses of the funds sum to a point estimate of 1.3 in this year, again suggesting that the specification is doing a reasonable job allocating \$1 of tax refund. Notably, I find no effect on firm investment in this period—even for financially constrained firms or firms with higher investment opportunities (Table A6)—and again, no effect on firm hiring. The cash effect is only marginally significant, however, with a p-value of 0.096. Note that because most firms receive just one tax refund in the 2009 period, the sample size in this period is much smaller than in the first policy period. The regression kink design is a relatively low-power methodology (Card, Lee, Pei, and Weber, 2012) and a number of my regressions for the 2009 period appear to suffer from low power.

¹⁸ Firms report investment by geographic segment (domestic and non-domestic) in the Compustat Segments database, but the coverage appears poor in my sample. Of the 891 firm-years in the sample that report substantial foreign activity in this period, only 100 firms report foreign capital expenditures in the Segments database.

I find that in the year after receiving the refund (2011), firms reduced cash holdings and used the funds to pay down long-term debt. Firms reduced cash holdings by \$1.54 per dollar of tax refund in 2011 and reduced long-term debt in 2011 by \$1.26 of every tax refund dollar (Table 7, Panel B). I find no effects of the tax refunds in any following year. While these point estimates appear slightly high (they imply that for each dollar of tax refund, firms pay down more than a dollar in debt), the estimates are not statistically different from the cash increase estimate in Table 7, Panel A or from \$1. Panel C of Table 7 shows results for the uses of the tax refunds over 2010 and 2011 in total. I find no average change in cash over the two years, consistent with firms first increasing and then paying down cash. The average effect over the two years was that firms paid down long-term debt in those years—\$1.36 of an additional tax refund dollar was allocated to debt reduction (significant at the 5 percent level).

Why did firms increase cash in the 2009 period? Theory points to two main hypotheses for the savings response: 1) holding cash due to higher idiosyncratic risk or uncertainty about future prospects in order to insulate the firm from future negative shocks (Opler, Pinkowitz, Stulz, and Williamson, 1999), and 2) holding cash when facing financing constraints in order to fund future investment opportunities (Almeida, Campello, and Weisbach, 2005; Han and Qui, 2007). To test these hypotheses, I divide firms into subsamples based on proxies for high and low uncertainty about future prospects and I divide firms into samples based on measures of high and low financial constraints.

For the uncertainty test, I generate three proxies for uncertainty about future cash flows or performance by industry, following measures used in the literature: historical cash flow volatility, stock volatility, and analyst forecast dispersion. The first proxy is historical cash flow volatility, constructed in the vein of Bates, Kahle, and Stulz (2009). I calculate the Fama-French 48 industry average of the 10-year standard deviation in firm cash flow/assets. For the stock return volatility measure, I calculate the industry average of the standard deviation of firm weekly stock returns over the past calendar year. For the analyst forecast dispersion proxy, I calculate the industry average of the standard deviation of analysts' earnings forecasts for the year forward (scaled by the prior year-end stock price). For each of these three measures, I classify firms in the top quartile of industry volatility or dispersion as the “high uncertainty” sample and firms in the bottom quartile as the “low uncertainty” sample. The stock volatility proxy and analyst forecast proxy are in the vein of Zhang (2006), but I use industry-level measures for consistency with the Bates, Kahle, and

Stulz (2009) measure of cash flow volatility. For the financial constraints test, I use the same measures of financial constraints as in Section 5.2.

I find that the cash holdings result is indeed concentrated in firms facing higher uncertainty as measured by past cash flow volatility and stock market volatility (Table 8, Panel A). The estimate for the cash change effect in the high cash flow volatility sample (column 1) is close to the point estimate from the main specification (1.09 versus 0.96) and is significant at the 1 percent level. The point estimate of the cash effect in the high stock volatility sample is somewhat high at 1.97 (column 3), though this estimate is not statistically different from a value of \$1. (Given the small size of these subsamples, additional noise may be expected.) For the subsample split by analyst forecast dispersion, there is no statistically significant effect on the change in cash for firms in either subsample, though the point estimate of the effect suggests that the cash effect is larger in the sample with higher analyst forecast dispersion (column 5).

Turning to the financial constraints hypothesis, I find no evidence that suggests the change in cash is concentrated in the most financially constrained firms. I see no statistically significant increase in cash in either the “high” or “low” financial constraint subsamples (Table 8, Panel B), suggesting that high financial constraints are not the primary motivation for firms holding the tax refunds as cash in 2010. I cannot rule out the fact that these may be poor measures of financial constraints in this period, however, given the tightness of credit conditions overall in the 2007-09 recession, or that these may be poor measures of whether a firm expects to be financially constrained in the future.

5.4 Macroeconomic Conditions vs. Firm-Level Characteristics

In this section, I attempt to disentangle whether firm uses of the tax refunds differed in the two policy periods due to changes in macroeconomics conditions (Bolton, Chen, and Wang, 2011 and 2013; Eisfeldt and Muir, 2015) or changes in firm-level characteristics (Bates, Kahle, and Stulz, 2009; Almeida, Campello, and Weisbach, 2005; Acharya, Almeida, and Campello, 2007; Fazzari, Hubbard, and Petersen, 1988). Both were changing simultaneously, making it challenging to assess the root cause of the varying responses. As discussed above, macroeconomic conditions were quite different across the two periods. The 2007-09 recession was much more severe than the 2001 recession and was characterized by acute credit market stress and much higher uncertainty

about future economic conditions. Firm characteristics were quite different in the two periods as well. Comparing ex-ante firm characteristics across the two periods for the full regression samples, I find notable differences, including lower ROA, lower leverage, and a higher cash-to-assets ratio in the 2009 sample (Table 9, Panel B).

To test whether different macroeconomic conditions or different firm-level conditions were responsible for the firms' liquidity choices, I use propensity score and Mahalanobis matching to isolate regression samples with similar observable firm-level characteristics in the 2002 and 2009 periods. In this way, I hold observable firm characteristics roughly constant across the two periods for a subset of firms. I match firms in the smaller 2009 sample to firms in the larger 2002 sample on a wide range of characteristics, including firm size, leverage, ROA, Tobin's q , the marginal tax rate, and the cash-to-assets ratio. For robustness, I use a first and second nearest neighbor match for both the propensity score and Mahalanobis matching procedures. As Table 9, Panel B shows, firm-level characteristics are not statistically different between the matched-characteristics samples in 2002 and 2009.

I find that firms with *similar* characteristics in the 2002 and 2009 policy periods allocated the tax refunds to *different* uses in the two periods: investment in the 2002 period and cash increases/debt reduction in the 2009 period. In the 2002 policy period, firms in the matched sample allocated between \$0.52 and \$0.86 of each refund dollar to investment in the year they received the tax refund, depending on the matching procedure (Table 9, Panel A). The investment results remain highly statistically significant in most cases as well.

The fact that firms with similar characteristics across the two periods chose different allocations of tax refunds suggests that changes in individual firm characteristics did not drive firms to make different allocations. Instead, the results suggest that changes in the overall macroeconomic state of the world drove the different stimulus responses. This result is in line with Bolton, Chen, and Wang's (2013) theoretical work showing that firms' decisions on allocating liquidity are state dependent. In their model, firms are more likely to hoard cash and invest less in a crisis state as compared with a non-crisis state—a theoretical result that is similar to the empirical result presented here, as the 2009 period was a crisis-driven recession.

5.5 Did the Tax Refunds Improve Firm Financial Conditions?

Next, I examine how the tax refunds affected firm financial conditions. Another stated policy goal of the five-year tax carryback extension was helping firms “weather the storm.” I study six measures of firm financial conditions: three bankruptcy risk measures (Altman’s z-score, Ohlson’s o-score, and distance to default), two credit risk measures (the probabilities of a future S&P credit rating upgrade or downgrade), and the probability of a future bankruptcy or liquidation.

Table 10 shows results for the bankruptcy risk and credit rating measures for the 2002 policy period (Panel A) and the 2009 policy period (Panel B). To provide economic magnitudes for the results, I present standardized coefficients that are interpreted as the standard deviation change in the outcome variable resulting from a one standard deviation change in the tax refund.

For the 2002 policy period, I see a small effect on bankruptcy risk: a statistically significant increase in Altman’s z-score, which indicates a reduction in bankruptcy risk for a firm. A one standard deviation increase in a firm’s tax refund results in about a tenth of a standard deviation increase in z-score on average in this sample. I see no statistically significant effect of the tax refunds on any of the other measures of bankruptcy risk, however, or on firm credit ratings in this period.

I see larger effects on firm financial conditions for the 2009 policy period. A one standard deviation increase in the tax refund results in a 0.18 standard deviation increase in Altman’s z-score, a smaller change in Ohlson’s o-score (-0.05 standard deviation—note, a decrease in o-score indicates a reduction in bankruptcy risk), and about a fifth of a standard deviation decrease in distance to default one year and two years forward. Looking at the effect of the policy on a firm’s credit risk, I see that it resulted in a statistically significant reduction in the probability of a credit rating downgrade. A one standard deviation increase in a firm’s tax refund resulted in about a fifth of a standard deviation decrease in the probability of a credit rating downgrade over the next 24 months or 36 months. These findings are consistent with the results on the allocation of tax funds in the 2009 policy period that firms held the tax refunds as cash first and then reduced cash in order to pay down long-term debt. These financial decisions would tend to reduce firm riskiness overall and the riskiness of firm debt positions, in particular.

Finally, I look at whether receiving a tax refund lowered the probability of a firm experiencing bankruptcy or liquidation (columns 9 and 10). I see no statistically significant effect,

suggesting that while the tax refunds helped improve broad financial conditions in 2009, they did not stave off severe negative outcomes for firms. The incidence of bankruptcy in the sample is quite low overall, however, with only a few firms leaving the sample for this reason in the years after the policies were enacted.

5.6 Potential Confounding Interactions and Robustness

An issue of concern for the validity of my results is any confounding interaction of other firm-related fiscal policy measures that were enacted around the NOL carryback extension. In this section, I present evidence these policies are not confounding my results. In addition, I carry out several robustness tests and show that my results are generally robust to the empirical specification and sample used, though the investment result for the 2002 policy period is more robust than the cash and debt results for the 2009 policy period. First, I conduct a “sharp RKD” test of the kink in the outcome variables. Second, I vary the order of the polynomial in V . Third, I narrow the bandwidth of the regression window. Fourth, I exclude industries that were particularly hard hit in each recession (telecommunications and airlines in 2002 and homebuilders in 2009). I present the results in appendix tables.

One major policy enacted in both 2002 and 2009 was “bonus” depreciation, which accelerated the schedule for deducting investment expenses from taxable income. Because investment is deductible from taxable income, this policy would be a concern for my identification strategy if it caused certain types of firms to accelerate investment and increase losses such that firm characteristics differed from one side of the kink point to the other. Two factors suggest that bonus depreciation is not confounding my results. First, the tax carryback policy is restricted to firms with losses, and Mahon and Zwick (2014a) find that the bonus policy had no effect on investment for firms with losses. Second, I do not find evidence that firms which would have been more likely to take advantage of bonus depreciation—those that invest in long-lived assets—sort deterministically to one side of the kink point. Figure A2 shows histograms of V for firms in long-duration asset industries (with the highest value of the depreciation allowance) compared with

firms in the short-duration asset industries (with the lowest value of the depreciation allowance).¹⁹ The distribution of firms appears relatively smooth around the kink point in both cases.

Other tax-related policies passed around the time were unlikely to have caused discontinuity in firm characteristics around the discontinuity in the NOL tax refund function. The dividend tax cut passed in 2003 reduced the top tax rate on U.S. dividend income from 38.6 percent to 15 percent. Yagan (2015) finds no differential effects of the policy by firm size or other firm characteristics. The repatriation holiday affected repatriations in 2005—outside of my analysis window. Finally, the 2002 tax carryback policy was passed concurrently with extensions of expiring tax provisions including the research and experimentation (R&E) tax credit, a credit for the production of electricity from wind, and the work opportunity tax credit. These provisions are widely expected to be extended each year. For example, the R&E tax credit was originally passed in 1981 and has been extended 15 times.

In the first robustness test, I show results from a sharp RKD test of the main results in Table A1. From the 2002 policy period, I show the increase in investment (column 1), and in the 2009 policy period, I show the following results: the increase in cash in 2010 (column 2), the reductions in cash and long-term debt in 2011 (columns 3 and 4), and the two-year cumulative reduction in long-term debt in 2010 and 2011 (column 5). The sharp RKD strategy is estimated under empirical specification 1. As in the first-stage regression of the tax refund, the coefficient γ_1 recovers the change in slope of the outcome variable around the kink point. In the sharp RKD strategy, one estimates the change in slope of the outcome variable of interest (e.g., investment) with respect to the assignment variable (V) and then divides by the change in slope of the policy variable (the average firm tax rate).²⁰

¹⁹ Following Mahon and Zwick (2014a), I separate firms into the ten most common three-digit NAICS industries in the top three deciles of the present discounted value of a dollar of deductions for investment—firms that benefit the most from bonus depreciation—and the ten most common industries in the bottom three deciles. In the top three deciles are the following: professional, scientific, and technical services (541); specialty trade contractors (238); computer and electronic product manufacturing (334); durable goods wholesalers (423); construction of buildings (236); heavy and civil engineering construction and land subdivision (237); truck transportation (484); rental and leasing services (532); nondepository credit intermediation (522); and administrative and support service (561). In the bottom three deciles are the following: motor vehicle and parts dealers (441), food manufacturing (311), real estate (531), telecommunications (517), fabricated metal product manufacturing (332), food services and drinking places (722), transportation equipment manufacturing (336), oil and gas extraction (211), nondurable goods wholesalers (424), and primary metal manufacturing (331).

²⁰ This corresponds to the RKD estimand from expression A. The intuition for this calculation is that empirical specification (1) estimates the additional funds spent for each additional dollar of previous years' profits on the left

Assuming a tax rate of 35 percent for all firms, the “sharp RKD” results are quite similar to the “fuzzy RKD” results for each of the variables. For example, the estimated change in the slope of investment in the 2002 period is 0.145, which corresponds to an estimate of \$0.41 per dollar of the tax refund spent on investment ($0.145/0.35$); this result is quite close to the fuzzy RKD estimate of \$0.40 in the baseline specification. In the first year of the 2009 period, the estimated slope change of the change in cash in the 2009 period is 0.31, which corresponds to an estimate of \$0.88 allocated to higher cash; this result is again quite close to the fuzzy RKD estimate of \$0.96. While the change in cash in 2010 is only marginally significant (p-value of 0.11), the other estimates are significant at the 5 percent level.

Table A2 shows results from tests that vary the polynomial order for the first-stage regression and the second-stage regression. I test 1) including only a linear interaction term with D, and 2) including the full polynomial interaction with D in the first-stage regression. I then test both first-stage options using a second-order and third-order polynomial in V in the second-stage regression. I report results for the same results as in the sharp RKD test. Each panel in the table reflects a separate regression. Each row shows results for a separate dependent variable and each column shows results for a different polynomial order.

My preferred regression specification uses a second-order polynomial in V with the second-order polynomial interacted with D also included in the first-stage excluded instrument. For the investment result, the estimated coefficient is fairly stable over a second- and third-order polynomial in V in the second stage. It is also stable using a linear polynomial term in the first-stage regression for both second-stage polynomial choices. Estimates range from \$0.39 cents per tax refund dollar spent on investment to \$0.47 per tax refund dollar. For the 2009 policy period, the regression coefficient on the cash reduction in the second year (2011) is also fairly stable over all specifications. For the other outcome variables, most regression coefficients are also in the general range of the preferred estimate. The majority of results remain statistically significant, though the 2010 cash change result in column 3 loses significance in the non-preferred

side of the kink point. The coefficient estimate in Table A1 suggests that firms spent an additional \$0.145 on investment for each \$1 of previous years’ profits in the 2002 period. But for each additional dollar of previous years’ profits, the firm did not receive an extra dollar of tax refund—the firm only received an extra \$0.35 of tax refund (if the tax rate was \$0.35). So the effect of the policy is that the firm spent \$0.145 out of the \$0.35 it received— $\$0.145/\0.35 or \$0.41 in total.

specifications. In addition, specifications in column 2 (a linear term in the first stage and a second-order polynomial in the second stage) also lose significance. Including a second- or third-order polynomial interaction term in the first-stage regression appears reasonable as there may be unmeasured effects from only using a linear term. A linear term may not capture the tax refund function well for larger firms that are subject to the corporate Alternative Minimum Tax or have different uses of tax credits, for example.

Next, I test narrowing the regression bandwidth (Table A3). In the preferred specification, I use the full sample because the regression kink design is a relatively low power methodology (Card, Lee, Pei, and Weber, 2012). For the 2002 period, the estimated coefficient in the investment regression increases when narrowing the window (although not monotonically) and retains statistical significance for most specifications. Narrowing the bandwidth of the regression for the 2009 policy period, which reduces the sample size significantly, I see that the regression coefficients are not nearly as stable and I lose statistical significance in many windows. This result is consistent with Card, Lee, Pei, and Weber's result that RKD estimates tend to become noisy and lose power at lower bandwidths.

Finally, I exclude from the regression sample industries that experienced particularly large losses during the two recessions: telecom and airlines in 2002 and homebuilding in 2009 (Table A4). For the 2002 sample, I find that excluding these industries, firms increased investment by \$0.51 for every dollar of tax refund. This estimate is similar to the full-sample estimate (\$0.40) but slightly bigger, which is sensible because the excluded industries likely had poorer investment opportunities at the time and would have been less likely to use the funds for investment. For the 2009 period, I find that the coefficient estimates on the changes in cash (columns 2 and 3) remain largely the same (\$1.04 for every refund dollar versus a \$0.96 baseline estimate in the first year and -\$1.48 versus -\$1.54 in the second year), though the statistical significance becomes marginal as the sample size declines. The estimates on the debt reductions in columns 4 and 5 are also of similar magnitudes as the baseline estimates and are statistically significant at the 5 percent level.

6. Conclusion

In this paper, I show that firm responses to a fiscal stimulus policy enacted in 2002 and in 2009 differed across the two periods. The policy I study granted additional tax refunds to firms by

extending the carryback window for net operating losses. In the 2002 period, I find that firms used the tax refunds to increase investment in the year they received the refund. In the 2009 period, in contrast, I find that firms used the refunds to increase cash holdings in the year they received the refund. In the following year, firms decreased their cash holdings and used the funds to pay down long-term debt. I find that the tax refunds had an effect on improving firm financial conditions broadly in the 2009 period as well, lowering bankruptcy risk and lowering the probability of a credit rating downgrade, but I find fewer effects on financial health in 2002.

I find that firms chose these different uses for the tax refunds in the two periods even in samples of firms with similar characteristics across the periods. This result suggests changes in macroeconomic conditions drove the different policy responses, not changes in firm-level conditions. The contrasting results over the two periods are also consistent with the theoretical result that firms may choose different uses of liquidity under different economic states of the world, as shown in dynamic models of firm investment and financing policies such as Bolton, Chen, and Wang (2011, 2013) and Eisfeldt and Muir (2015). Comparing economic conditions across the policy periods, in the 2002 period, the economy was recovering from a much milder recession. Growth prospects were higher and policy uncertainty was lower, though credit conditions remained tight. My finding that investment was concentrated in financially constrained firms and those with higher investment opportunities is consistent with the hypothesis that a number of firms had profitable investment opportunities at the time but were financially constrained. When the five-year carryback policy eased financial constraints in 2002 and 2003, therefore, these firms took advantage of the tax refunds to boost investment. In contrast, the 2009 cash holdings result was concentrated in firms facing higher uncertainty. This fact is consistent with a hypothesis that due to high economic uncertainty, an increase in cash due to a precautionary savings motive was the highest value use of funds at the time.

This work should be informative to policymakers considering implementing the five-year NOL carryback policy in the future. Is there evidence that the policy achieved the two goals of boosting investment and improving firm financial conditions? Yes, but my results suggest that the policy only achieved one of these goals in each period and I find no effect of the policy on employment—another stated policy goal. In addition, I cannot say for certain whether the policy boosted domestic investment in the 2002 period as policymakers desired. I measure an effect on

worldwide investment and it was multinational firms in my sample that increased investment, not domestic firms. These firms may have transferred the funds overseas to invest.

This work also highlights the importance of policymakers carefully considering policy goals and broad economic conditions in evaluating the potential effectiveness of firm fiscal stimulus actions. If the main policy goal is to increase investment, for example, my results suggest that the carryback policy is more likely to be effective during a period when firms appear financially constrained but are not facing especially weak economic prospects or high levels of uncertainty. My results suggest that the policy is less likely to be effective increasing investment during a period of high uncertainty and when firms have weak investment opportunities. If the policy goal is to improve firm financial conditions broadly, however, my results suggest the carryback policy may indeed be effective during such times.

Appendix: Variable Construction

The analysis relies on constructing estimates of the firm losses and previous years' profits that were available to apply to the carryback policy, as well as the size of the firm tax refund received as a result of the policy.

1) Taxable Income and Tax Rates

A. I define taxable income as follows, with Compustat variable names in parenthesis:

$$\text{Taxable Income} = \text{Domestic Pretax Income (PIDOM)} - \text{Federal Deferred Taxes (TXDFED)}/\tau + \text{Extraordinary Items and Discontinued Operations (XIDO)}/(1 - \tau)$$

Here, $\tau = 0.35$ is assumed to be the marginal tax rate. Pretax Income equals Operating Income After Depreciation (OIADP) – Interest and Related Expenses (XINT) + Special Items (SPI) + Non-Operating Income (NOPI). When domestic pretax income and federal deferred taxes are missing, I use total Pretax Income (PI) and Deferred Income Taxes (TXDI). I replace any missing values for extraordinary items with zero.

B. I define the tax rate as follows:

$$\text{Tax Rate} = \text{Federal Income Taxes (TXFED)}/\text{Taxable Income}$$

If data on federal income taxes paid are missing, I replace missing values with Total Income Taxes (TXT) – Foreign Income Taxes (TXFO) – State Income Taxes (TXS) – Deferred Taxes (TXDI) – Other Income Taxes (TXO). I replace missing values for foreign income taxes, state income taxes, deferred taxes, and other income taxes with zeroes.

2) Losses to Apply to the Five-Year Carryback Policy

A. For the 2002 policy period, I assume a firm would have applied any 2001 losses (negative taxable income) to the 2002 refund and any 2002 losses to the 2003 refund.

B. For the 2009 policy period, firms were only allowed to apply either 2008 *or* 2009 losses to the five-year carryback.

- I estimate the potential tax refunds if the firm applied 2008 losses or applied 2009 losses to the five-year carryback. I assume that firms would have chosen the higher refund.
- If I estimate that the firm would have applied the 2008 losses to the five-year carryback policy, losses that apply to the policy for the 2010 refund equal 2008 plus 2009 losses. I assume the firm would also have applied any 2009 losses to the standard two-year carryback policy in that year.
- If I estimate that the firm applied the 2009 losses to the five-year carryback policy, losses that apply to the 2010 refund just equal the 2009 losses. Firms cannot receive any additional refund based on 2008 losses.

3) Previous Years' Profits to Apply to the Policy

- A. For each year in the five-year carryback window, if any year in the window had a loss— $\text{year}_{(t)}$ —I calculate if there were any adjustments for a two-year carryback during that time.
- a. If the firm had profits in year $(t-2)$:
 - If $\text{profits}_{(t-2)}$ were larger than the $\text{loss}_{(t)}$, I assume the firm received a refund for the $\text{loss}_{(t)}$. The $\text{profits}_{(t-2)}$ remaining for the five-year carryback policy equal $\text{profits}_{(t-2)}$ minus the $\text{losses}_{(t)}$. The firm has no more losses to apply to the two-year carryback.
 - If $\text{profits}_{(t-2)}$ were smaller than the loss, I assume the firm received a refund for the $\text{profits}_{(t-2)}$. $\text{Profits}_{(t-2)}$ remaining for the five-year carryback policy are zero. $\text{Losses}_{(t)}$ that remain to apply to a refund equal $\text{losses}_{(t)}$ minus $\text{profits}_{(t-2)}$.
 - b. If the firm had $\text{profits}_{(t-1)}$ and had $\text{losses}_{(t)}$ remaining to apply for a carryback:
 - If $\text{profits}_{(t-1)}$ were larger than the remaining $\text{loss}_{(t)}$, I assume the firm received a refund for the whole $\text{loss}_{(t)}$. The $\text{profits}_{(t-1)}$ remaining for the five-year carryback policy equal $\text{profits}_{(t-1)}$ minus the $\text{loss}_{(t)}$.
 - If $\text{profits}_{(t-1)}$ were smaller than the $\text{loss}_{(t)}$, I assume the firm received a refund for the $\text{profit}_{(t-1)}$. $\text{Profits}_{(t-1)}$ remaining for the five-year carryback policy are zero.
- B. For profits that applied to the 2002 tax refund, previous years' profits that were potentially available for the tax refund in 2002 equal the sum of profits from 1996 to 2001 that remained on the firm's books.
- C. For profits that applied to the 2003 tax refund, I assume that any profits applied to the 2002 tax refund were not available for the 2003 tax refund.
- a. If the 2001 loss was larger than the sum of profits from 1996 to 2000 that could apply to the policy, previous years' profits that apply to the 2003 refund equal zero.
 - b. If the 2001 loss was smaller than the 1996 profit that could be applied to the refund, previous years' profits that applied to the 2003 refund equal the sum of profits from 1997 to 2000.
 - c. If the 2001 loss was larger than the 1996 profit that could be applied to the refund but smaller than the total sum of profits from 1996 to 2000, previous years' profits that applied to the 2003 refund equaled the sum of profits from 1996 to 2000 minus the 2001 loss.
 - d. If 2001 was a profit year, the previous years' profits that could apply to the 2003 refund equal the sum of profits from 1997 to 2001.
- D. For profits that applied to the 2010 tax refund, if I calculate that firms apply 2008 losses to the five-year carryback policy, profits that apply equal the sum of profits from 2003 to 2007. If I calculate that firms apply 2009 losses to the five-year carryback policy, profits that apply to the policy equal the sum of profits from 2004 to 2008.

4) Tax Refunds

To calculate the tax refund each firm would have received, I assume that firms receive a refund based on the 5th year of the window first, then the 4th year of the window, then the 3rd year etc.... Note that for the 2009 policy period, only 50 percent of profits in the 5th year of the window could apply to the policy.

Starting with the last year of the window, year_(t-5):

- A. If the firm's policy losses_(t) are larger than the profits in year_(t-5), the tax refund equals that year's tax rate times profits_(t-5) available. The losses_(t) that now can apply to a refund equals losses_(t) minus profits_(t-5). Profits_(t-5) remaining to apply for another carryback equal zero.
- B. If the firm's policy losses are smaller than the profits in that year, the tax refund equals the year's tax rate times the losses_(t) available. The firm has now exhausted the losses available to apply to the policy. Profits_(t-5) remaining to apply for another carryback equal profits_(t-5) minus losses_(t).
- C. I repeat the algorithm for the other four years in the window.

5) Other Variables

I use financial and accounting data from the Compustat fundamental annual database and the CRSP daily and monthly annual update databases. I source data on firm credit ratings from the Capital IQ S&P credit ratings database. I source data on analyst forecast dispersion from I/B/E/S, and data on firm marginal tax rates are from John Graham. Variables definitions used in the analysis are as follows:

$$\text{Investment} = \text{CAPXV} - \text{SPPE}$$

$$\text{Change in cash} = \text{CH} - \text{CH}_{t-1}$$

$$\text{Change in total debt} = (\text{DLC} + \text{DLTT}) - (\text{DLC}_{t-1} + \text{DLTT}_{t-1})$$

$$\text{Change in long-term debt} = \text{DLTT} - \text{DLTT}_{t-1}$$

$$\text{Change in short-term debt} = \text{DLC} - \text{DLC}_{t-1}$$

$$\text{Payout} = \text{DVC} + \text{PRSTKC}$$

$$\text{Change in short-term investments} = \text{IVST} - \text{IVST}_{t-1}$$

$$\text{Change in investments} = \text{IVCH}$$

$$\text{Acquisitions} = \text{AQC}$$

$$\text{Change in Employment} = \text{EMP} - \text{EMP}_{t-1}$$

Altman's z-score = $3.3 \cdot \text{EBIT}/\text{AT} + 1.0 \cdot \text{SALE}/\text{AT} + 1.4 \cdot \text{RE}/\text{AT} + 1.2 \cdot \text{WCAP}/\text{AT}$

Ohlson's o-score = $-1.32 - 0.407 \cdot \ln(\text{AT}) + 6.03 \cdot (\text{LT}/\text{AT}) - 1.43 \cdot (\text{ACT} - \text{LCT})/\text{AT} + 0.0757 \cdot (\text{LCT}/\text{ACT}) - 2.37 \cdot \text{NI}/\text{AT} - 1.83 \cdot (\text{PI} + \text{DP})/\text{LT} + 0.285 \cdot 1 \cdot [(\text{NI}_{t-1} < 0 \ \& \ \text{NI}_{t-2} < 0)] - 1.72 \cdot (\text{LT} > \text{AT}) - 0.521 \cdot (\text{NI} - \text{NI}_{t-1})/(|\text{NI}| + |\text{NI}_{t-1}|)$

Distance to default is calculated as the naive measure in Bharath and Shumway (2008):

$N(-\text{DD})$, where

$$\text{DD} = \frac{\ln\left(\frac{E+F}{F}\right) + \left(r_{it-1} - \frac{\sigma_V^2}{2}\right)T}{\sigma_V \sqrt{T}}$$

$$\sigma_V = \left(\frac{E}{E+F}\right) \sigma_E + \left(\frac{F}{E+F}\right) (0.05 + 0.25 \sigma_E)$$

E is the market value of equity (PRC*SHROUT in CRSP); F is the face value of debt, defined as $\text{DLC} + 0.5 \cdot \text{DLTT}$; r_{it-1} is the firm's buy-and-hold stock return over the previous year; T is the time to maturity (one or two years); σ_E is the standard deviation of the firm's stock price over the previous year; and σ_V is the approximation to the total volatility of each firm.

S&P credit rating upgrade is a dummy variable equal to one if a firm experiences a ratings upgrade on long-term debt within a given period and zero otherwise. The S&P credit rating variable used is SPLTICRM.

S&P credit rating downgrade is a dummy variable equal to one if a firm experiences a ratings downgrade on long-term debt within a given period and zero otherwise. The S&P credit rating variable used is SPLTICRM.

Bankruptcy or liquidation is a dummy variable equal to one if a firm files for bankruptcy in a given period.

ROA = OIBDP/AT

Tobin's q = $(\text{AT} + \text{PRCC_F} \cdot \text{CSHO} - (\text{SEQ} + \text{TXDITC} - \text{PSTK}))/\text{AT}$

Cash Flow/Assets = $(\text{IB} + \text{DP})/\text{AT}$

Ln(Assets) = $\ln(\text{AT})$

Leverage = $(\text{DLC} + \text{DLTT})/(\text{DLC} + \text{DLTT} + \text{PRCC_F} \cdot \text{CSHO})$

Sales/Assets = SALE/AT

Marginal Tax Rate is defined as MTR_BEFINT from John Graham's marginal tax rate file. When a data point is missing, I use the algorithm to estimate the book stimulated marginal tax rate from Graham and Mills (2008): $0.331 - 0.075 \cdot 1 \cdot [\text{TXFED}/\text{PIDOM} < 0.1] - 0.012 \cdot 1 \cdot [\text{TLCF} > 0] - 0.106 \cdot 1 \cdot [\text{PI} < 0] + 0.037 \cdot 1 \cdot [|\text{PIFO}/\text{PI}| > 0.05]$. If TXFED/PIDOM is missing, I substitute TXT/PI.

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Table 1: NOL Carryback Deduction Example

	Tax Year					
	1996	1997	1998	1999	2000	2001
Panel A: Before Carryback						
Taxable income before carryback (\$Mil)	50	40	30	20	10	-100
Taxes Paid, $\tau=0.35$ (\$Mil)	17.5	14.0	10.5	7.0	3.5	0
Panel B: 2-Year Carryback						
NOL carryback deduction (\$Mil)	0	0	0	-20	-10	30
Taxable income after carryback (\$Mil)	50	40	30	0	0	-70
Taxes Paid, $\tau=0.35$ (\$Mil)	17.5	14.0	10.5	0	0	0
Tax Refund (\$Mil)						10.5
Panel C: 5-Year Carryback						
NOL carryback deduction (\$Mil)	-50	-40	-10	0	0	100
Taxable income after carryback (\$Mil)	0	0	20	20	10	0
Taxes Paid, $\tau=0.35$ (\$Mil)	0	0	7.0	7.0	3.5	0
Tax Refund (\$Mil)						35

This table presents an example, for a hypothetical firm, of how the NOL tax carryback deduction is applied under the two-year carryback policy and the five-year carryback extension policy. Panel A shows the taxes paid before any carryback election. Assuming a tax rate of 0.35 percent, a firm with taxable income ranging from \$50 million to \$10 million from 1996 to 2000 would have paid between \$17.5 and \$3.5 million in taxes in those years. After experiencing a \$100 million loss in 2001, the firm would not pay taxes. Panel B shows that under a two-year tax carryback policy, the firm could carry back \$30 million of its \$100 million loss to offset profits in 1999 and 2001, and would receive a tax refund of \$10.5 million. Under the five-year carryback extension policy (Panel C), the firm could carry back the full \$100 million loss to offset profits in 1996, 1997 and 1998, and would receive a tax refund of \$35 million.

Table 2: Data Sample Summary Statistics

	2002 Policy		2009 Policy	
	Mean	Std. Dev	Mean	Std. Dev
V (Profits Minus Losses) (\$M)	11.27	290.13	28.95	480.43
Tax Refund (\$M)	8.84	44.70	12.51	50.70
Investment (\$M)	28.72	97.35	50.20	146.77
Change in Cash (\$M)	5.00	86.03	-2.10	171.99
Payout (\$M)	7.37	45.90	18.66	94.95
Change in Debt (\$M)	-11.04	198.70	-5.04	262.75
Change in Short-term Investments (\$M)	-0.30	64.76	5.14	85.35
Change in Long-term Investments (\$M)	23.70	225.99	38.40	244.39
Change in Employment (Thousands)	-0.17	1.98	0.07	3.21
Altman's z-score	-1.99	10.10	-2.60	11.89
Ohlson's o-score	1.63	8.55	0.47	6.73
Bankruptcy or Liquidation, 1yrF	0.00	0.03	0.00	0.03
Bankruptcy or Liquidation, 2yrF	0.01	0.07	0.00	0.06
S&P Credit Downgrade, 1yrF	0.26	0.44	0.14	0.35
S&P Credit Downgrade, 2yrF	0.35	0.48	0.20	0.40
S&P Credit Downgrade, 3yrF	0.43	0.50	0.26	0.44
S&P Credit Upgrade, 1yrF	0.13	0.34	0.27	0.44
S&P Credit Upgrade, 2yrF	0.24	0.43	0.36	0.48
S&P Credit Upgrade, 3yrF	0.32	0.47	0.47	0.50
Distance-to-default, 1yr	0.11	0.23	0.03	0.12
Distance-to-default, 2yr	0.11	0.24	0.03	0.12

The table reports summary statistics for outcome variables used in the analysis of the 2002 policy period (columns 1 and 2) and 2009 policy period (columns 3 and 4). Definitions of each variable are given in Appendix 2.

Table 3: Estimated Kink in Pre-Treatment Covariates

	2002 Policy (1)	2009 Policy (2)
ROA	0.00499 [0.00461]	0.0023 [0.00363]
Cash Flow/Assets	0.00697 [0.00564]	0.000666 [0.00522]
Cash/Assets	-0.0032 [0.00215]	-0.00267 [0.00314]
PPE/Assets	-0.000405 [0.00128]	-0.0011 [0.00234]
Leverage	0.00217 [0.00200]	-0.000185 [0.00204]
Tobin's Q	0.00447 [0.0141]	-0.0207 [0.0352]
Marginal Tax Rate	0.0000115 [0.00206]	-0.000738 [0.00270]
Total Assets	-9.996 [10.15]	-21.67 [19.30]
Accruals	0.00474 [0.00377]	-0.000988 [0.00211]
Accruals	0.0046 [0.00399]	0.00277 [0.00223]

This table presents the estimated coefficient β_1 from empirical specification 3, a regression that estimates the change in slope of pre-treatment firm characteristics (re-centered residuals from a regression on industry fixed effects) in \$0.5 million bins in a -\$25 million to -\$25 million window around $V=0$. Each panel represents results from a separate regression. ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

**Table 4: First-stage Regression Estimates:
Average Firm Tax Rate**

Dependent variable = Tax Refund		
	2002 Policy	2009 Policy
	(1)	(2)
Change in Slope	0.337*** [0.0356]	0.309*** [0.0514]
Controls	+	+
Industry F.E.	+	+
F-test (p-value)	0.00	0.00
Chi-squared test for coefficient differences (p-value)		0.71
Observations	3,337	1,496
R-squared	0.830	0.813

This table presents results from the first-stage regression, which estimates the change in slope of the tax refund as a function of the assignment variable around the kink point (the estimated coefficient γ_1 from empirical specification 1). The change in slope equals the estimated average tax rate across firms, as described in Section III. Columns 1 and 2 report the estimated tax rate for the 2002 period and the 2009 period, respectively. Regressions include a second-order polynomial in the assignment variable V (previous-years' profits minus policy-year losses), industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q , ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the Fama-French 48 industry level and are reported in brackets. ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

**Table 5: Tax Refund Allocation in the 2002 Policy Period:
Year of Tax Refund Receipt (2002 and 2003)**

	Dependent variable =						
	Investment (1)	Change in Cash (2)	Change in Total Debt (3)	Payout (4)	Other Uses (5)	Total (6)	Change in Employment (7)
Tax Refund							
	0.403*** [0.155]	-0.189 [0.252]	-0.606 [0.871]	-0.0855 [0.130]	0.358 [0.579]	1.093 [0.786]	0.00271 [0.00711]
Controls	+	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+	+
Observations	3,337	3,337	3,337	3,337	3,337	3,337	3,232
R-squared	0.404	0.073	0.050	0.145	0.056	0.235	0.066

This table presents results from the second-stage regression (the coefficient β_1 from empirical specification 2), which estimates the effect of the tax refund on potential uses of the funds in the 2002 policy period in the year that the firms received the tax refund (2002 and 2003). Each column presents results from one regression; the dependent variable is listed at the top of each column. Other uses are acquisitions, change in short-term investments and change in investments. Total is the sum of all uses listed in columns 1 to 5 (taking the negative of the change in debt). Coefficients in columns 1 to 6 are interpreted as the dollars spent on each use of funds for every additional dollar of tax refund received. The coefficient in column 7 is interpreted as the change in employment (in thousands) for every \$1 million in tax refund received. Regressions include a second-order polynomial in the assignment variable V (previous-years' profits minus policy-year losses), industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q , ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

**Table 6: Investment, Financial Constraints, and Investment Opportunities:
Effects of the Tax Refund in the 2002 Policy Period**

Panel A: Financial Constraints

Dependent Variable = Investment

Financially Constrained?	KZ Index		Payout		DFF	
	Yes (1)	No (2)	Yes (3)	No (4)	Yes (5)	No (6)
Tax Refund	1.091* [0.659]	0.0534 [0.228]	1.004* [0.535]	0.418 [0.307]	1.002** [0.509]	0.501 [0.389]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	785	787	2,196	1,141	2,501	816
R-squared	0.571	0.544	0.413	0.453	0.402	0.465

Panel B: Investment Opportunities

Dependent Variable = Investment

High Investment Opportunities?	Tobin's Q		Year of Tax Refund		Multi-national	
	High Yes (1)	Low No (2)	2002 No (3)	2003 Yes (4)	Yes (5)	No (6)
Tax Refund	1.017* [0.593]	-1.454 [1.481]	0.277 [0.384]	0.753*** [0.243]	0.676* [0.372]	0.171 [0.177]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	833	835	1,727	1,610	891	2,434
R-squared	0.500	0.371	0.395	0.428	0.441	0.419

This table presents results from the second-stage regression (the coefficient β_1 from empirical specification 2), which estimates the effect of the tax refund on investment in the 2002 policy period in subsamples of financially constrained and unconstrained firms and subsamples of firms with high and low investment opportunities. Panel A restricts the sample by three measures of firm financial constraints. Columns 1 and 2 show results for subsamples of firms in the top and bottom quartiles of the Kaplan-Zingales (KZ, 1997) index, respectively. Columns 3 and 4 show results for subsamples of firms with zero and non-zero dividend issuance and stock repurchases, respectively. Columns 5 and 6 show results for subsamples of firms for which total payouts to operating income is and is not less than or equal to zero, respectively, as in Dharmapala, Foley, and Forbes (DFF, 2011). Panel B restricts the sample by three measures of firm investment opportunities. Columns 1 and 2 show results for subsamples of firms in the top and bottom quartiles of Tobin's q, respectively. Columns 3 and 4 show results for tax refund receipt in 2002 and 2003, respectively. Columns 5 and 6 show results for firms with substantial foreign activity (foreign pre-tax income greater than 5 percent of total pre-tax income in absolute value) and domestic firms, respectively. All financial constraint and investment opportunity measures are calculated for the year prior to tax refund receipt. All coefficients are interpreted as the dollars spent on investment for every additional dollar of tax refund received. Regressions include a second-order polynomial in the assignment variable V (previous-years' profits minus policy-year losses), industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table 7: Tax Refund Allocation in the 2009 Policy Period**Panel A: Year of Tax Refund Receipt (2010)**

Dependent variable =	Investment	Change in Cash	Change in Long-Term Debt	Change in Short-Term Debt	Payout	Other Uses	Total
Tax Refund	-0.406	0.958*	-0.0406	-0.431	0.313	-0.0257	1.312
	[0.380]	[0.576]	[0.588]	[0.482]	[0.496]	[1.000]	[1.916]
Controls	+	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+	+
Observations	1,496	1,496	1,496	1,496	1,496	1,496	1,496
R-squared	0.40	0.27	0.18	0.45	0.22	0.14	0.33

Panel B: Year After Tax Refund Receipt (2011)

Dependent variable =	Investment	Change in Cash	Change in Long-Term Debt	Change in Short-Term Debt	Payout	Other Uses	Total
Tax Refund	-0.632	-1.542*	-1.260**	0.0568	0.124	-0.0972	-0.943
	[0.542]	[0.828]	[0.626]	[0.534]	[0.680]	[1.545]	[2.610]
Controls	+	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+	+
Observations	1,355	1,355	1,355	1,355	1,355	1,355	1,355
R-squared	0.363	0.026	0.076	0.079	0.245	0.129	0.254

Panel C: Two-Year Total Effect After Tax Refund Receipt (2010 and 2011)

Dependent variable =	Investment	Change in Cash	Change in Long-Term Debt	Change in Short-Term Debt	Payout	Other Uses	Total
Tax Refund	-1.008	-0.524	-1.361**	-0.363	0.419	-0.317	0.295
	[0.887]	[0.968]	[0.622]	[0.477]	[1.174]	[2.532]	[4.593]
Controls	+	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+	+
Observations	1,355	1,355	1,355	1,355	1,355	1,355	1,355
R-squared	0.384	0.150	0.114	0.378	0.254	0.154	0.307

This table presents the coefficient β_1 from empirical specification (2), the second-stage regression, and estimates the effect of the tax refund on potential uses of the funds in the 2009 policy period. Panel A reports estimates for the year of tax refund receipt (2010). Panel B reports estimates for the year following tax refund receipt (2011) and Panel C reports estimates for the cumulative use of funds over 2010 and 2011. Other uses are acquisitions, change in short-term investments and change in investments. Total is the sum of all uses listed in columns 1 to 6. Regressions include a second-order polynomial in the assignment variable "V", industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively

**Table 8: Change in Cash, Uncertainty, and Financial Constraints:
Effects of the Tax Refund in the 2009 Policy Period**

Panel A: Uncertainty Proxies						
Dependent Variable = Change in Cash						
Volatility?	Cash Flow Volatility		Stock Volatility		Analyst Forecast	
	High	Low	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Refund	1.061*** [0.266]	-0.113 [0.505]	1.972* [1.015]	0.382 [0.843]	0.909 [1.193]	-0.748 [1.048]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	295	467	342	525	352	461
R-squared	0.678	0.548	0.251	0.078	0.368	0.205

Panel B: Financial Constraints						
Dependent Variable = Change in Cash						
Financially Constrained?	KZ Index		Payout		DFF	
	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Refund	-0.484 [0.520]	0.535 [1.254]	0.0661 [0.701]	0.749 [0.790]	0.359 [0.705]	0.951 [0.766]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	355	356	939	557	1,087	406
R-squared	0.162	0.492	0.570	0.213	0.541	0.236

This table presents the coefficient β_1 from empirical specification (2), the second-stage regression, and estimates the effect of the tax refund on the change in cash. Panel A restricts the sample by three proxies for uncertainty: firms in the top and bottom quartiles of Fama-French 48 industries based on the standard deviation of 10-year cash flow volatility (columns 1 and 2, respectively), top and bottom industry quartiles of the standard deviation of 1-year stock returns (columns 3 and 4) and top and bottom industry quartiles of the dispersion of analyst year-ahead earnings per share forecasts (columns 5 and 6). Panel B restricts the sample by three measures of firm financial constraints. Columns 1 and 2 show results for subsamples of firms in the top and bottom quartiles of the Kaplan-Zingales (1997) index, respectively. Columns 3 and 4 show results for subsamples of firms with zero and non-zero dividend issuance and repurchases, respectively. Columns 5 and 6 show results for subsamples of firms for which total payouts to operating income is and is not less than or equal to zero, respectively, as in Dharmapala, Foley, and Forbes (2011). All uncertainty and financial constraint measures are calculated for the year prior to tax refund receipt. Regressions include a second-order polynomial in the assignment variable "V", industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

**Table 9: Tax Refund Allocation in the 2002 Policy Period:
Observations with Similar Firm Characteristics in the 2002 and 2009 Policy Periods**

Panel A: Investment Effects in the 2002 Policy Period for the Matched Firm Sample

Dependent Variable = Investment				
	Propensity Score Match		Mahalanobis Match	
	1-NN	2-NN	1-NN	2-NN
	(1)	(2)	(3)	(4)
Tax Refund	0.521*** [0.196]	0.531*** [0.185]	0.864* [0.511]	0.825*** [0.263]
Controls	+	+	+	+
Industry F.E.	+	+	+	+
Observations	1,496	1,748	1,064	1,725
R-squared	0.445	0.462	0.461	0.457

**Panel B: Ex-ante Firm Characteristics Comparisons:
Full Samples and Samples with Characteristics Matched between the 2002 and 2009 Samples**

	Full Regression Sample			Matched Sample (Propensity Score: 1-NN)		
	2002 Sample	2009 Sample	p-value of difference	2002 Sample	2009 Sample	p-value of difference
	(1)	(2)	(3)	(4)	(5)	(6)
ROA	-0.047 (0.315)	-0.071 (0.404)	0.046	-0.059 (0.323)	-0.071 (0.404)	0.377
Cash Flow/Assets	-0.153 (0.519)	-0.145 (0.697)	0.686	-0.145 (0.422)	-0.145 (0.697)	0.984
ln(Assets)	4.904 (1.972)	5.282 (2.028)	0.000	5.308 (1.984)	5.282 (2.028)	0.728
Tobin's q	4.729 (10.025)	3.162 (6.685)	0.546	5.974 (12.280)	3.162 (6.685)	0.376
Leverage	0.315 (0.306)	0.255 (0.270)	0.000	0.257 (0.281)	0.255 (0.270)	0.856
Cash/Assets	0.200 (0.241)	0.258 (0.259)	0.000	0.260 (0.276)	0.258 (0.259)	0.890
PPE/Assets	0.267 (0.228)	0.243 (0.241)	0.002	0.234 (0.210)	0.243 (0.241)	0.289
Marginal Tax Rate	0.230 (0.103)	0.239 (0.102)	0.010	0.239 (0.100)	0.239 (0.102)	0.947
No. Obs	3,337	1,496		1,496	1,496	

In this table, Panel A presents estimates of the tax refund's effect on investment in the 2002 policy period for regression samples of firms in the 2002 period that have similar characteristics to firms in the 2009 policy period. Columns (1) and (2) report regression estimates for observations that were matched on ex-ante firm characteristics using a 1st nearest neighbor (1-NN) and 2nd nearest neighbor (2-NN) propensity-score methodology, respectively. Columns (3) and (4) report regression estimates for observations that were matched on ex-ante firm characteristics using a 1-NN and 2-NN Mahalanobis-matching methodology, respectively. Regressions are as described in Tables 5 and 7. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively. Panel B presents summary statistics of ex-ante firm characteristics for the full sample of firms in the 2002 and 2009 policy periods and the sample of firms matched using the 1-NN propensity-score methodology. Columns (1) and (2) report estimates for the full 2002 and 2009 policy period regression samples, respectively. Column (3) reports the p-value from a t-test of the differences between the two full regression samples. Column (4) reports estimates for observations in the 2002 policy period that have similar characteristics to the 2009 policy period sample, based on a 1-NN propensity-score match of the observations. Column (5) reports estimates for the full 2009 sample and column (6) reports the p-value from a t-test of the differences between the samples.

Table 10: Effect of the Tax Refund on Firm Financial Conditions

		Bankruptcy Risk				S&P Credit Rating		S&P Credit Rating		Bankruptcy Filing	
		Altman's Z-score	Ohlson's O-score	Distance-to-Default (1yr)	Distance-to-Default (2yr)	(24mF)	(36mF)	(24mF)	(36mF)	(12mF)	(24mF)
Dependent variable =		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Tax Refund	0.151*	-0.0106	-0.0225	-0.0285	0.0927	0.092	-0.0593	-0.0647	0.000239	0.00161	
	[0.0911]	[0.0471]	[0.0559]	[0.0520]	[0.0659]	[0.0802]	[0.0862]	[0.0818]	[0.00134]	[0.00134]	
Controls	+	+	+	+	+	+	+	+	+	+	
Industry F.E.	+	+	+	+	+	+	+	+	+	+	
Observations	3,276	3,290	2,057	2,057	754	768	751	756	3,337	3,337	
R-squared	0.296	0.139	0.248	0.235	0.115	0.121	0.101	0.114	0.036	0.053	

		S&P Credit Rating		S&P Credit Rating		Bankruptcy Filing				
Dependent variable =		(24mF)	(36mF)	(24mF)	(36mF)	(12mF)	(24mF)			
		(5)	(6)	(7)	(8)	(9)	(10)			
Tax Refund	0.178***	-0.0540*	-0.179**	-0.201***	-0.172*	-0.205***	-0.0398	0.0108	0.00127	0.000189
	[0.0649]	[0.0287]	[0.0752]	[0.0768]	[0.0882]	[0.0789]	[0.0823]	[0.0883]	[0.00163]	[0.00106]
Controls	+	+	+	+	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+	+	+	+	+
Observations	1,466	1,478	966	966	361	364	358	362	1,496	1,496
R-squared	0.45	0.37	0.237	0.211	0.189	0.191	0.196	0.221	0.025	0.055

This table presents the coefficient β_1 from empirical specification (2), the second-stage regression, and estimates the effect of the tax refund on measures of bankruptcy risk (Altman's z-score, Ohlson's o-score and distance-to-default in columns 1 to 4) and the probability of an S&P credit rating upgrade or downgrade (in columns 5 to 10). Panel A presents results from the 2002 policy period and Panel B presents results from the 2009 policy period. The coefficients are standardized to be interpreted as the standard deviation change in the dependent variable resulting from a one standard deviation change in the firm tax refund. Regressions include a second-order polynomial in the assignment variable "V", industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, ln(assets), and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Figure 1: Example of Kink in the Tax Refund Formula

This figure shows an example of the discontinuity in slope in the tax refund formula for a set of firms with \$100 million in losses in 2001, but varying level of taxable profits from 1996 to 2000. The example is discussed in detail in Section 3.2.

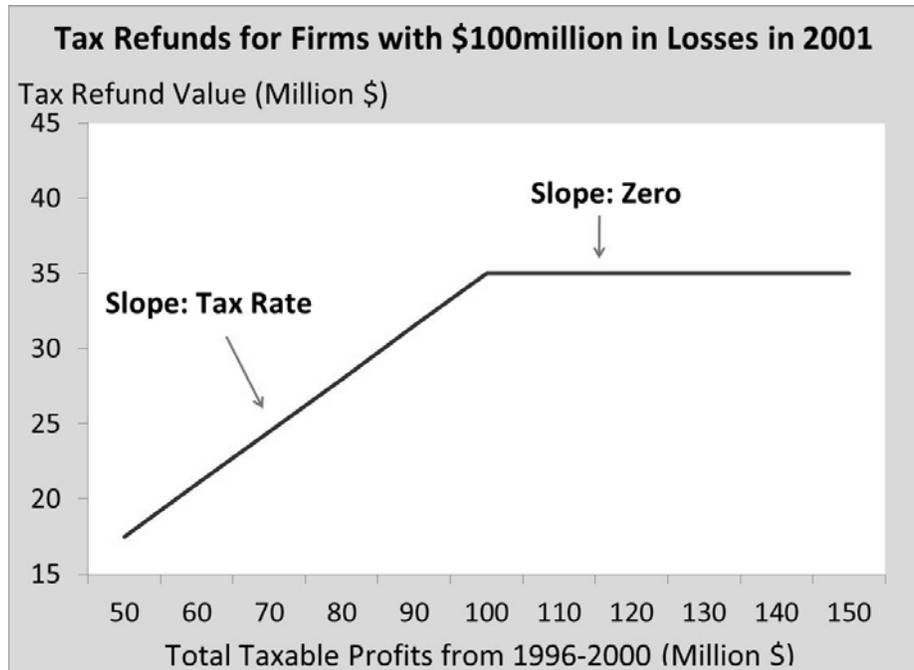


Figure 2: Examples of Kinks in Variables

This figure shows examples of the discontinuity in slope in the tax refund and firm investment. In the left panel, the blue solid lines represent the linear fit of the tax refund in the 2002 period (the recentered residuals from a regression on industry fixed effects and controls as in specification (1)) on the assignment variable V , to the left of the kink point ($V=0$) and to the right of the kink point. The dots represent the average tax refund in \$1million bins of V . The shaded area represents a 99 percent confidence interval. The right panel presents analogous information for investment in the 2002 period.

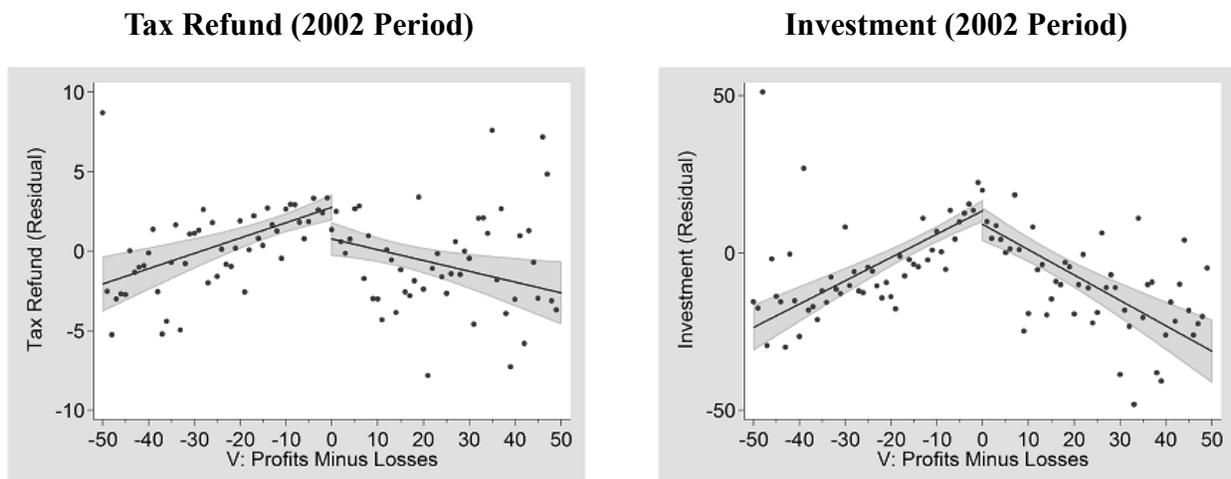


Figure 3: Distribution of Sample Firms around the Kink Point (V=0)

Figure 3 presents histograms of the number of firms in \$0.25 million bins of the assignment variable V for the 2002 period (left panel) and the 2009 period (right panel).

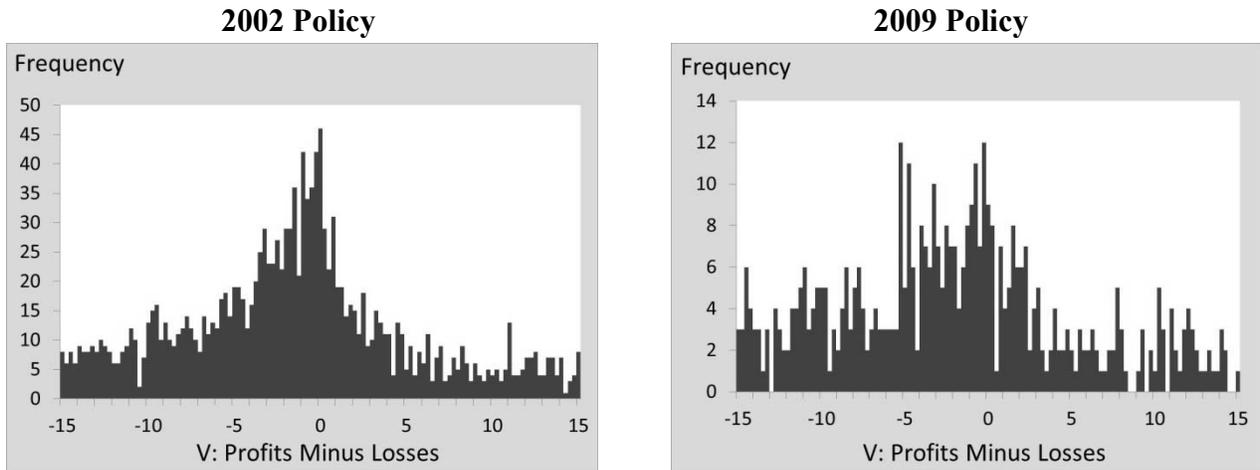
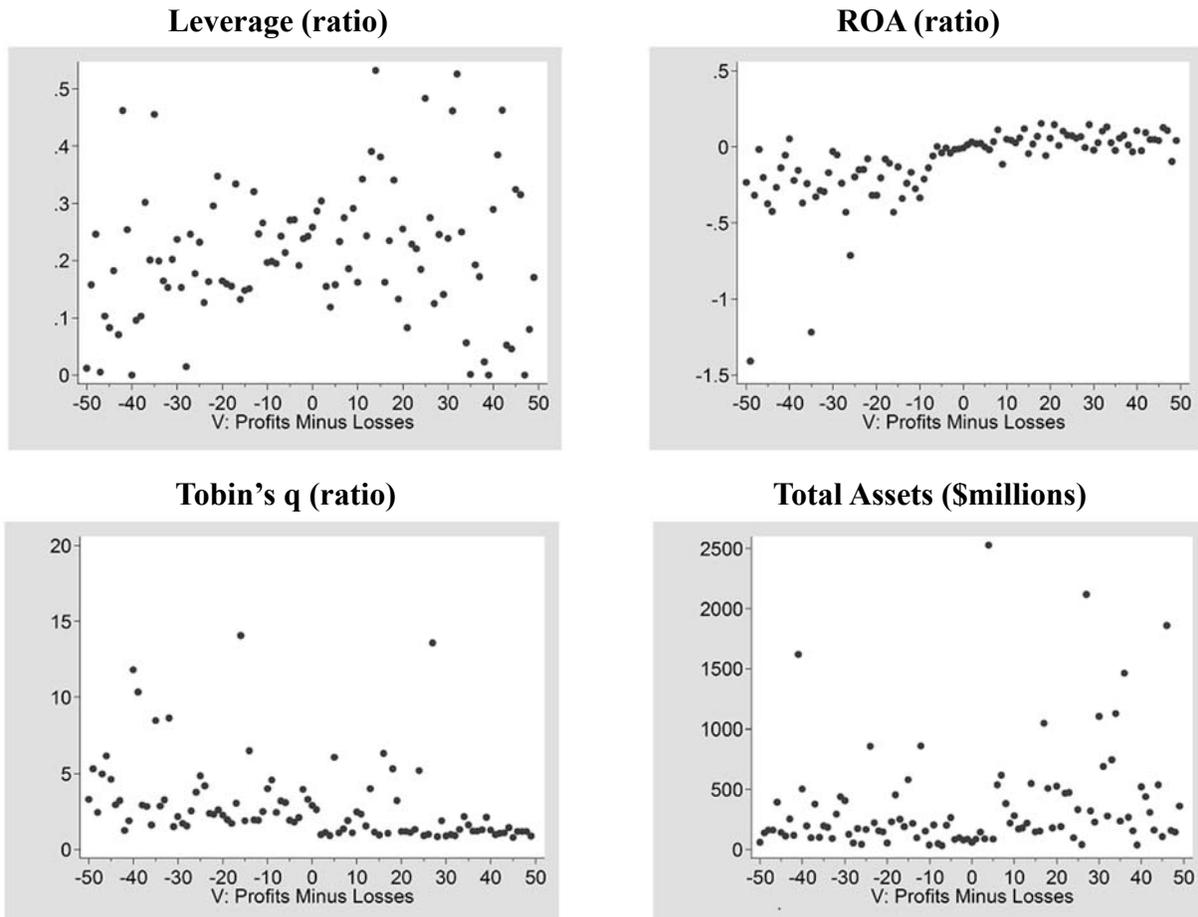


Figure 4: Distribution of Pre-Treatment Firm Characteristics for the 2009 Policy Period

Figure 4 presents the average value of four pretreatment firm characteristics in \$1 million bins of the assignment variable V.

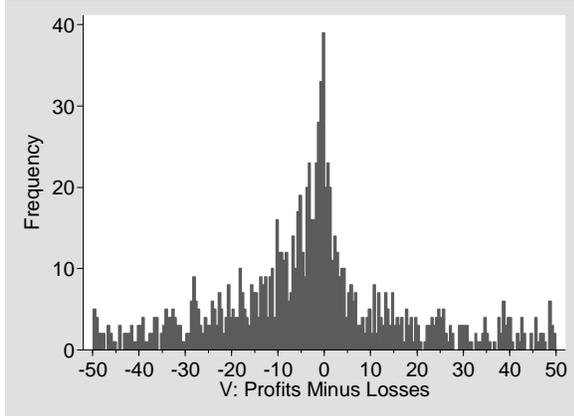


**Appendix Tables and Figures for
“Fiscal Stimulus and Firms: A Tale of Two Recessions”**

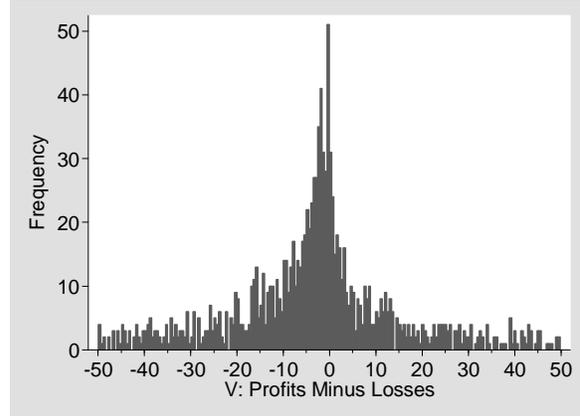
Figure A1: Firm Distribution by the Absolute Value of Accruals

Figure A1 presents histograms of the number of firms in \$0.25 million bins of the assignment variable V for firms below median values of accruals (panel 1 shows the 2002 period and panel 3 shows the 2009 period) and firms in short-duration-asset industries (panel 2 shows the 2002 period and panel 4 shows the 2009 period). Details are provided in Section 3.6.

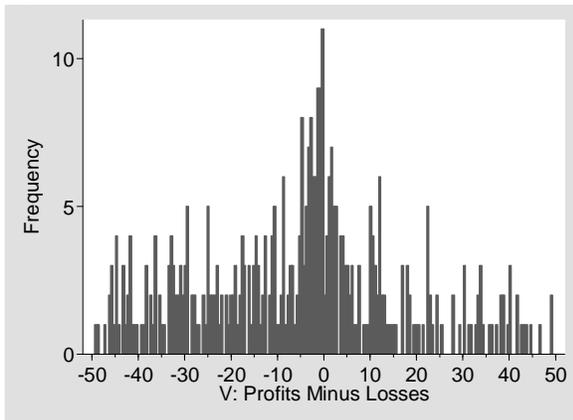
(1) 2002 Period: Below-Median Accruals



(2) 2002 Period: Above-Median Accrual



(3) 2009 Period: Below-Median Accruals



(4) 2009 Period: Above-Median Accruals

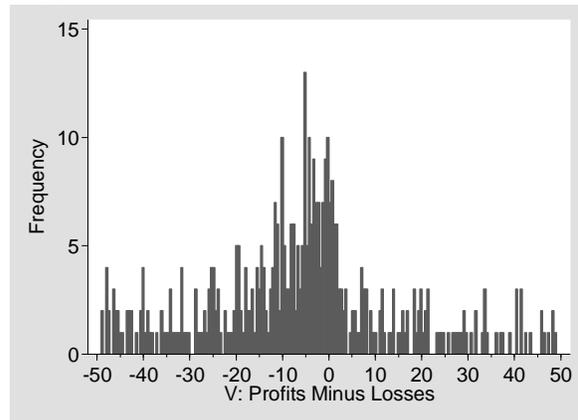
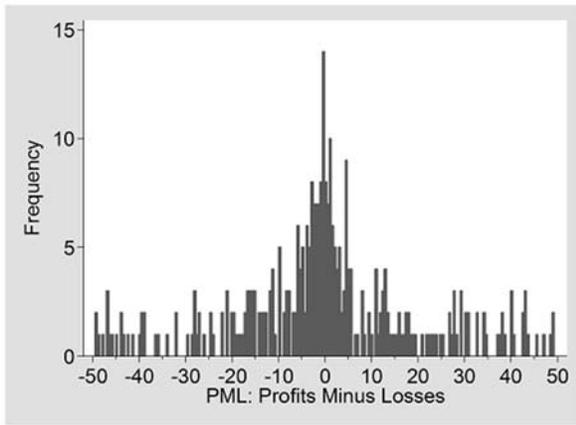


Figure A2: Firm Distribution by Duration of Investment Goods, 2002 Period

Figure A2 presents histograms of the number of firms in the 2002 policy period in \$0.25 million bins of the assignment variable V for firms in long-duration-asset industries (left panel) and firms in short-duration-asset industries (right panel). Details are provided in Section 5.6.

(1) Long-Duration-Asset industries



(2) Short-Duration-Asset Industries

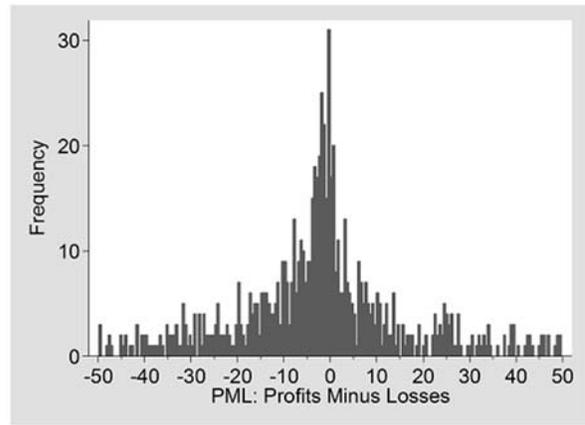
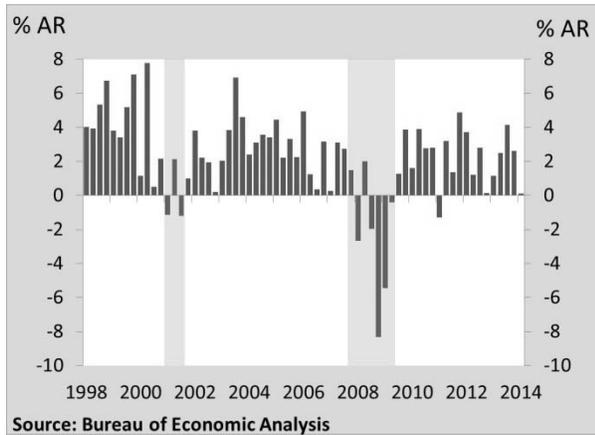
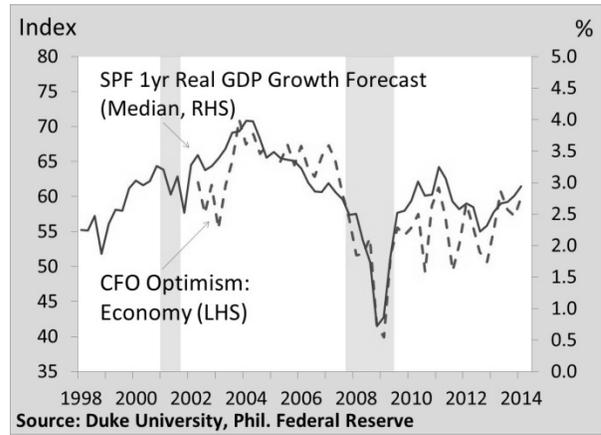


Figure A3: Economic Conditions: 2002 and 2009 Policy Period

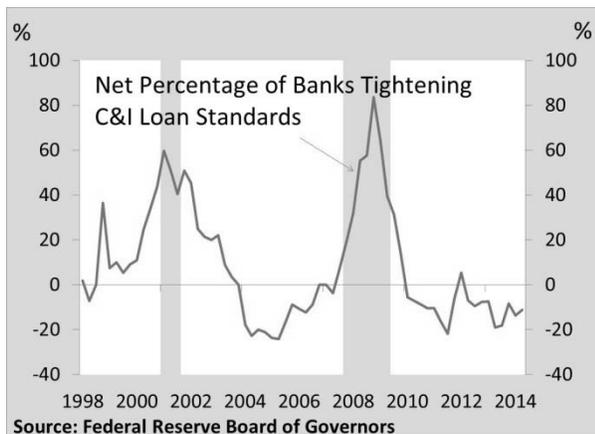
(1) Real GDP Growth



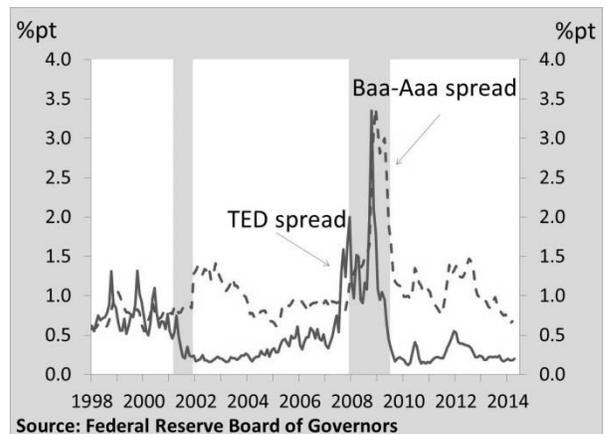
(2) Economic Outlook



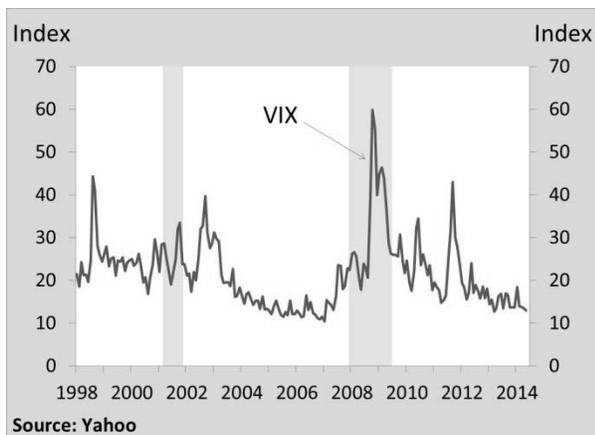
(3) Bank Lending Conditions



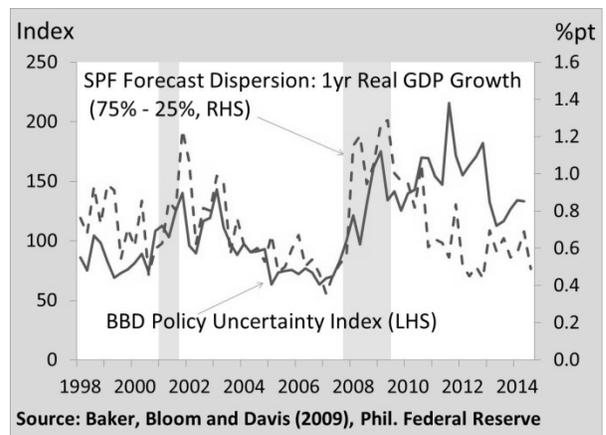
(4) Loan Spreads



(5) Stock Volatility



(6) Policy and Economic Uncertainty



Note: Gray shading represents recession periods

Appendix Tables

Table A1: Sharp RKD

Dependent variable =	2002 Policy	2009 Policy			
	Investment	Change in Cash (2010)	Change in Cash (2011)	Change in L.T. Debt (2011)	Change in L.T. Debt (2010 & 2011)
	(1)	(2)	(3)	(4)	(5)
Change in slope (β_1)	0.145** [0.0642] (0.0295)	0.309 [0.186] (0.105)	-0.494** [0.209] (0.0231)	-0.390** [0.187] (0.0435)	-0.444** [0.191] (0.0254)
Controls	+	+	+	+	+
Industry F.E.	+	+	+	+	+
Observations	3,337	1,496	1,355	1,355	1,355
R-squared	0.403	0.254	0.104	0.094	0.120

This table estimates the change in slope of the outcome variable around the kink point (the coefficient β_1 from empirical specification 1). The estimated coefficient is interpreted as the dollars spent on each additional use of funds for every additional dollar of the assignment variable V (previous-years' profits minus policy-year losses). The coefficient divided by 0.35 (the highest marginal tax rate) is a "sharp" RKD estimate of the effect of the tax refund on firm outcomes, as explained in Section V. Column 1 reports results for investment as the dependent variable in the 2002 policy period and columns 2 through 5 report results for the following dependent variables in the 2009 policy period: change in cash in 2010, change in cash in 2011, change in long-term debt in 2011 and the cumulative 2-year change in long-term debt over 2010 and 2011. Regressions include a second-order polynomial in the assignment variable V, industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. P-values are reported in parentheses. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table A2: Robustness to Polynomial Choice

	Polynomial Order:				
	Second Stage:	Two		Three	
	First Stage:	Two	One	Three	One
		(1)	(2)	(3)	(4)
2002 Policy					
Investment		0.403*** [0.155] (0.00953)	0.467*** [0.181] (0.00980)	0.386** [0.186] (0.0379)	0.431** [0.176] (0.0145)
2009 Policy					
Change in Cash (2010)		0.958* [0.576] (0.0963)	0.461 [0.730] (0.528)	0.895 [0.550] (0.104)	0.668 [0.532] (0.210)
Change in Cash (2011)		-1.260** [0.626] (0.0440)	-1.165 [0.741] (0.116)	-1.183* [0.647] (0.0674)	-1.186* [0.680] (0.0810)
Change in L.T. Debt (2011)		-1.542* [0.828] (0.0626)	-0.841 [0.736] (0.253)	-1.677** [0.842] (0.0463)	-1.522* [0.818] (0.0627)
Change in L.T. Debt (2010 & 2011)		-1.361** [0.622] (0.0285)	-0.473 [0.906] (0.602)	-1.515** [0.717] (0.0345)	-1.540** [0.725] (0.0337)
Controls		+	+	+	+
Industry F.E.		+	+	+	+

This table presents results from the second-stage regression (the coefficient β_1 from empirical specification 2), which estimates the effect of the tax refund on potential uses of the funds with varying orders of polynomials in the assignment variable V (previous-years' profits minus policy-year losses) in the second-stage and first-stage regressions. Each panel reflects a separate regression. Dependent variables are reported in rows. Column 1 reports results for the preferred specification: a second-order polynomial in V in the second stage [$\sum_{p=1}^2 (V_{it-1} - V^*)^p$] and a second-order polynomial in the excluded instrument [$\sum_{p=1}^2 D \cdot (V_{it-1} - V^*)^p$] in the first stage. Column 2 reports results for a second-order polynomial in V in the second-stage and a first-order polynomial in the excluded instrument in the first-stage. Columns 3 and 4 report results for a third-order polynomial in V in the second stage and a third- and first-order polynomial in the excluded instrument in the first stage, respectively. All coefficients are interpreted as the dollars spent on each use of funds for every additional dollar of tax refund received. Regressions include industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q , ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the Fama-French 48 industry level and are reported in brackets. P-values are reported in parenthesis. ***, **, and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table A3: Robustness to Varying Bandwidths

Dependent variable =	2002 Policy		2009 Policy		
	Investment	Change in Cash (2010)	Change in Cash (2011)	Change in L.T. Debt (2011)	Change in L.T. Debt (2010 & 2011)
	(1)	(2)	(3)	(4)	(5)
Bandwidth Range:					
Full Sample	0.403*** [0.155]	0.958* [0.576]	-1.542* [0.828]	-1.260** [0.626]	-1.361** [0.622]
[-2750, 2750]	—	0.856* [0.472]	-1.706** [0.797]	-1.707** [0.710]	-2.072** [0.846]
[-2500, 2500]	—	0.512 [0.496]	-1.204 [0.747]	-1.413** [0.683]	-1.338* [0.799]
[-2250, 2250]	0.474*** [0.173]	0.134 [0.569]	-0.852 [0.758]	-1.844** [0.797]	-2.201** [0.977]
[-2000, 2000]	0.425* [0.257]	0.11 [0.619]	-0.958 [0.890]	-2.072*** [0.766]	-2.823*** [1.089]
[-1750, 1750]	0.371 [0.267]	0.494 [0.764]	-1.543 [0.952]	-1.362 [0.946]	-1.53 [0.994]
[-1500, 1500]	0.485** [0.217]	-0.0987 [0.882]	-1.331 [1.021]	-1.652* [0.854]	-2.127** [1.080]
[-1250, 1250]	0.915*** [0.349]	0.852 [0.816]	-1.273 [1.166]	-1.611 [1.078]	-0.355 [1.514]
[-1000, 1000]	0.662** [0.331]	1.346 [0.863]	0.588 [1.709]	-1.668 [1.433]	0.162 [1.887]
[-750, 750]	0.915** [0.377]	0.96 [0.702]	0.114 [1.139]	-0.199 [1.270]	0.218 [1.866]
[-500, 500]	1.772*** [0.653]	1.344 [1.062]	-0.946 [1.811]	-1.521* [0.876]	-1.104 [2.092]
Controls	+	+	+	+	+
Industry F.E.	+	+	+	+	+

This table presents results from the second-stage regression (the coefficient β_1 from empirical specification 2), which estimates the effect of the tax refund on potential uses of the funds with varying bandwidths of the regression window. Each panel reflects a separate regression. Column 1 reports results for investment as the dependent variable in the 2002 policy period and columns 2 through 5 report results for the following dependent variables in the 2009 policy period: change in cash in 2010, change in cash in 2011, change in long-term debt in 2011 and the cumulative 2-year change in long-term debt over 2010 and 2011. All coefficients are interpreted as the dollars spent on each use of funds for every additional dollar of tax refund received. Regressions include a second-order polynomial in the assignment variable V (previous-years' profits minus policy-year losses), industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q , ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table A4: Excluding Hard-Hit Industries

Dependent variable =	2002 Policy	2009 Policy			
	(Ex. Air & Telecom)	(Ex. Homebuilders)			
	Investment	Change in Cash (2010)	Change in Cash (2011)	Change in L.T. Debt (2011)	Change in L.T. Debt (2010 & 2011)
	(1)	(2)	(3)	(4)	(5)
Tax Refund	0.505* [0.268] (0.0592)	1.036 [0.664] (0.119)	-1.48 [0.954] (0.121)	-1.562** [0.713] (0.0286)	-1.685** [0.687] (0.0141)
Controls	+	+	+	+	+
Industry F.E.	+	+	+	+	+
Observations	3,213	1,449	1,311	1,311	1,311
R-squared	0.400	0.259	0.028	0.076	0.111

This table presents results from the second-stage regression (the coefficient β_1 from empirical specification 2), which estimates the effect of the tax refund on potential uses of the funds. Column 1 reports results for investment as the dependent variable in the 2002 policy period and columns 2 through 5 report results for the following dependent variables in the 2009 policy period: change in cash in 2010, change in cash in 2011, change in long-term debt in 2011 and the cumulative 2-year change in long-term debt over 2010 and 2011. Column 1 excludes firms in Fama-French industries 23 and 32: aircraft and communications. Columns 2 to 5 exclude firms in Fama-French industries 17 and 18: construction materials and construction. All coefficients are interpreted as the dollars spent on each use of funds for every additional dollar of tax refund received. Regressions include a second-order polynomial in the assignment variable V (previous-years' profits minus policy-year losses), industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q , ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. P-values are reported in parenthesis. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

Table A5: Tax Refund Allocation in the 2002 Policy Period: Following Years**Panel A: Year After Tax Refund Receipt (2003 and 2004)**

Dependent variable =	Investment	Change in Cash	Change in Debt	Payout	Other Uses	Total
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Refund	0.182 [0.449]	0.613 [0.484]	1.128 [1.064]	-0.0898 [0.186]	0.333 [0.668]	-0.0905 [1.085]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	2,952	2,952	2,952	2,952	2,952	2,952
R-squared	0.33	0.00	0.01	0.11	0.13	0.25

Panel B: Two-Year Total Effect After Tax Refund Receipt

Dependent variable =	Investment	Change in Cash	Change in Debt	Payout	Other Uses	Total
Tax Refund	0.425 [0.642]	0.415 [0.523]	0.467 [0.892]	-0.146 [0.336]	0.771 [1.065]	0.997 [1.211]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	2,952	2,952	2,952	2,952	2,952	2,952
R-squared	0.39	0.05	0.08	0.15	0.11	0.31

This table presents results from the second-stage regression (the coefficient β_1 from empirical specification 2), which estimates the effect of the tax refund on potential uses of the funds in the 2002 policy period. Panel A reports estimates for the year following tax refund receipt (2003 and 2004) and Panel B reports estimates for the cumulative use of funds over the two years after tax refund receipt. Other uses are acquisitions, change in short-term investments and change in investments. Total is the sum of all uses listed in columns 1 to 5 (taking the negative of the change in debt). All coefficients are interpreted as the dollars spent on each use of funds for every additional dollar of tax refund received. Regressions include a second-order polynomial in the assignment variable V (previous-years' profits minus policy-year losses), industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, $\ln(\text{assets})$, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.

**Table A6: Investment, Financial Constraints, and Investment Opportunities:
Effects of the Tax Refund on Investment in the 2009 Policy Period**

Panel A: Financial Constraints

Dependent Variable = Investment

Financially Constrained?	KZ Index		Payout		DFF	
	Yes	No	Yes	No	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Tax Refund	-0.978 [0.880]	0.298 [0.360]	-1.157 [0.794]	-0.324 [0.409]	-1.065 [0.767]	-0.229 [0.333]
Controls	+	+	+	+	+	+
Industry F.E.	+	+	+	+	+	+
Observations	355	356	939	557	1,087	406
R-squared	0.44	0.50	0.44	0.46	0.41	0.52

Panel B: Investment Opportunities

Dependent Variable = Investment

High Investment Opportunities?	Tobin's Q		Multi-national	
	High	Low	Yes	Domestic
	Yes	No	Yes	No
	(1)	(2)	(3)	(4)
Tax Refund	-0.702 [0.799]	-3.944 [3.277]	-0.581 [0.580]	-0.341 [0.274]
Controls	+	+	+	+
Industry F.E.	+	+	+	+
Observations	373	374	551	942
R-squared	0.39	0.00	0.47	0.42

This table presents results from the second-stage regression (the coefficient β_1 from empirical specification 2), which estimates the effect of the tax refund on investment in the 2009 policy period. Panel A restricts the sample by three measures of firm financial constraints. Columns 1 and 2 show results for subsamples of firms in the top and bottom quartiles of the Kaplan-Zingales (1997) index, respectively. Columns 3 and 4 show results for subsamples of firms with zero and non-zero dividend issuance and stock repurchases, respectively. Columns 5 and 6 show results for subsamples of firms for which total payouts to operating income is and is not less than or equal to zero, respectively, as in Dharmapala, Foley, and Forbes (2011). Panel B restricts the sample by two measures of firm investment opportunities. Columns 1 and 2 show results for subsamples of firms in the top and bottom quartiles of Tobin's q, respectively. Columns 3 and 4 show results for firms with substantial foreign activity (foreign pre-tax income greater than 5 percent of total pre-tax income in absolute value) and domestic firms, respectively. All financial constraint and investment opportunity measures are calculated for the year prior to tax refund receipt. All coefficients are interpreted as the dollars spent on investment for every additional dollar of tax refund received. Regressions include a second-order polynomial in the assignment variable V (previous-years' profits minus policy-year losses), industry fixed effects at the Fama-French 48 level, and the pre-treatment controls Tobin's q, ROA, cash flow/assets, sales/assets, leverage, the marginal tax rate, ln/assets, and a quadratic in stimulus losses. Standard errors are clustered at the industry level and are reported in brackets. ***, ** and * indicate levels of 1 percent, 5 percent, and 10 percent significance, respectively.