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Essays in US Fiscal Policy

A dissertation presented

by

James F. Mahon III

to

The Department of Political Economy and Government

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

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Essays in US Fiscal Policy

Abstract

This dissertation presents three chapters about US tax and spending policy. The first chapter investigates the take-up of a tax refund for corporate losses. We find that few firms claim the refund despite that it dominates the alternative option. This finding indicates that many firms fail to optimize perfectly with respect to taxes. The second chapter estimates corporate responses to a tax incentive for investment. We find the largest responses among small firms and firms without an alternative tax shield, suggesting that the tax incentive operates through both the price and cash mechanisms. The third chapter tests for partisan effects on the distribution of federal spending within congressional districts. Even when conditioning on institutional contexts with greater partisan influence, I find little evidence that parties tilt the distribution of federal spending to favor co-partisan and swing voters.

Contents

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To my loved ones

Introduction

This dissertation presents three chapters about US tax and spending policy. The first chapter covers the take-up of a tax refund for corporate losses, the second chapter estimates corporate investment responses to a tax incentive, and the third chapter tests for partisan effects on the distribution of federal spending within congressional districts. Each chapter reports novel empirical facts about US fiscal policy.

The first chapter studies the role of paid preparers in the take-up of a tax refund for corporate losses, a provision of the US tax code that made $357 billion available to eligible firms between 1998 and 2011. Drawing a sample of 1.2 million observations from the population of corporate tax returns, we present three findings. First, only 37 percent of eligible firms claim their refund. Second, a cost-benefit analysis of the tax loss choice cannot explain the low take-up rate. Third, firms with sophisticated preparers, such as licensed accountants, are more likely to claim the refund. To show that firm selection cannot explain the preparer effect, we validate this result with a research design based on preparer deaths and relocations. Our results reject the standard view that firms optimize perfectly with respect to taxes.

The second chapter estimates the effect of temporary tax incentives on equipment investment using shifts in accelerated depreciation. Analyzing data for over 120,000 firms, we present three findings. First, bonus depreciation raised investment 17.3 percent on average between 2001 and 2004 and 29.5 percent between 2008 and 2010. Second, financially constrained firms respond more than unconstrained firms. Third, firms respond strongly when the policy generates immediate cash flows but not when benefits only come in the future. Implied discount rates are too high to match a frictionless model and cannot be explained entirely by costly finance, unless firms neglect future financial constraints.

The third chapter directly tests two-candidate election models of distributive politics in a novel setting: the distribution of federal spending and voters within US congressional districts. These models make competing predictions over whether co-partisan or swing voters receive disproportionately more spending. I test them by comparing the within-district distribution of spending and voters across counties. I find reasonably precise zero differences between counties in per-capita federal spending when including district-congress and county fixed effects. A regression discontinuity design on two-party elections and a first-differences design on redistricting replicates similar estimates. Even when considering the US Senate, partisan alignment with the House majority and the presidency, congressional committees, and bicameralism, I find limited evidence of favoritism toward broad groups of voters for electoral purposes in the within-district distribution of federal spending aggregates.

Chapter 1

The Role of Experts in Fiscal Policy Transmission[1]

1.1 Introduction

Recent research has emphasized that imperfect information mutes behavioral responses to tax policy (Chetty, Looney and Kroft 2009; Finkelstein 2009). This friction is a first order concern for policymakers because tax incentives cannot stimulate the economy if those affected do not know about them. Absent from theoretical treatments of this issue is the fact that most taxpayers hire third party preparers to help them with their tax returns. Just as managerial features may influence corporate decisions (Bertrand and Schoar 2003; Dyreng, Hanlon and Maydew 2010), how firms respond to tax policy could depend on the external experts they hire. This paper studies the role of paid preparers in their clients' decision to claim a tax refund for losses. We find that hired experts play a central role in the transmission of this fiscal policy.

We study the tax treatment of corporate losses, a permanent feature of the US tax code that affects most firms.[2] Under this provision, a firm reporting a loss can choose between

[1]Co-authored with Eric Zwick

[2]Between 1998 and 2011, 37 percent of firm-year observations reported a tax loss and 80 percent of firms

a carryback and a carryforward. A firm electing a carryback applies its loss against past taxable income and then receives a refund from the IRS. A firm electing a carryforward reserves its loss to deduct against taxable income in the future. In most cases, the carryback option is more valuable both because of discounting and because the firm risks losing its stock of carryforward deductions if it fails.[3] Prior research has estimated the impact of these rules on marginal tax rates (Auerbach and Poterba 1987*a*; Altshuler and Auerbach 1990), typically under the assumption that all firms elect the carryback when available.

Carryback refunds serve as an important automatic fiscal stabilizer: more firms report losses during recessions and as result aggregate eligible refunds increase (Altshuler et al. 2009). In addition, policymakers often expand carryback generosity in bad times with the goal of injecting cash into the economy to promote business activity. Between 1998 and 2011, the carryback provision made $357 billion in refunds available, of which $124 billion was available during the 2008-2009 recession.[4] Thus whether firms claim eligible refunds is a question of policy and macroeconomic relevance.

We explore the take-up of corporate tax refunds using a new dataset drawn from the population of US corporate tax returns filed between 1998 and 2011. Our data consists of more than 1.2 million firm-year observations that were eligible for tax refunds. In addition to coverage, the dataset improves on past samples by enabling us to measure eligible and actual refunds and to link firms to the experts they hire to help them file their returns. The median firm in our sample is small, with revenues of $1.5 million and payroll of less than $500 thousand. These firms are more likely both to face financial frictions that would make immediate refunds valuable[5] and to rely on external experts to make tax decisions that are

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reported a tax loss at least once.

[3]An exiting firm could still use its carryforward deduction if acquired by another firm. But the value of carryforwards from acquisition is limited by IRS rules that restrict the use of losses after a change in ownership.

[4]This figure includes eligible refunds for all C corporations. We restrict our analysis to this corporate form because the treatment of losses takes place at the entity level. Losses for pass-through business entities, such as S corporations and partnerships, are reported on the returns of their owners. As of 2008, C corporations accounted for 63 percent of all business receipts in the United States (Internal Revenue Service 2014).

[5]Zwick and Mahon (2014) find that firms only respond to investment tax incentives when they have an immediate impact on cash flows, suggesting that firms face financial frictions.

unrelated to their core business.

We present three empirical findings. First, take-up is surprisingly low. Only 37 percent of eligible firms claim their refund. This finding holds even when we restrict our attention to potential refunds that are large relative to a firm's operating cash flows. Although larger firms are more likely to claim a refund, the take-up rate only reaches fifty percent at the 90th percentile of firm size in our data. Just half of the potential aggregate refund amount was claimed and distributed to eligible firms. Thus the low take-up rate substantially limits the potential impact of this policy as fiscal stimulus.

Second, we find that a simple cost-benefit analysis of the carryback-carryforward trade-off cannot explain the low take-up rate. Because the loss provision presents firms with a simple binary choice and our dataset allows us to compute the ex post value of each option, our setting provides a unique opportunity to learn whether firms optimize with respect to the tax code. Most firms that fail to claim do not benefit from waiting and many non-claimers forgo more than thirty percent of the refund's value. This finding is based on firms for whom we can precisely compute the ex post net present value of the carryback and carryforward options using each firm's realized path of taxable income over time. In our calculations, we assume discount rates ranging from three to nine percent. If firms face financial frictions that generate higher discount rates, the net present value differences between the carryback and the carryforward options would be even greater.

These findings suggest that either informational frictions or transaction costs prevent firms from claiming their refunds. We consider these alternatives while exploring the connection between tax preparers and client claiming behavior. By reducing informational frictions and the cost of electing the carryback, preparers could play an important role in determining client take-up. We evaluate this hypothesis by testing whether preparer characteristics can account for the variation in corporate claiming behavior. The exercise is similar to the approaches used to explore whether managerial "style" affects corporate decisions (Bertrand and Schoar 2003; Kaplan, Klebanov and Sorensen 2012) and whether teachers affect student test scores (Jackson and Bruegmann 2009). In addition to providing insight into the take-up puzzle, this test also speaks to whether hired experts help firms optimize.[6]

Our third finding is that firms with sophisticated preparers, such as those licensed as certified public accountants (CPAs), are more likely to claim the refund. We begin with a specification that includes firm fixed effects, so that the coefficients on preparer characteristics are identified from firms that switch preparers while holding constant time-invariant client unobservables. Relative to preparers without a professional license, the clients of CPAs are 6.8 percentage points more likely to claim. This effect is large in comparison to a baseline take-up rate of 37 percent. In addition to professional licenses, other proxies for preparer sophistication—age, salary, client size, and client base size—also coincide with higher take-up.

The research design relies on the identifying assumption that changes in preparers are uncorrelated with unobservable changes in client determinants of take-up. Our estimates will be biased if hiring a more sophisticated manager leads to hiring a more sophisticated preparer and more sophisticated managers are more likely to claim refunds. We address this threat in three ways. First, we confirm that our results are robust to a variety of client control sets. Second, we confirm the absence of differential trends in claiming rates prior to a preparer switch. Third, we validate our estimates in a sample of switching events in which the prior preparer either dies or moves personal residence. Here it is more plausible that around the event client unobservables do not change. We find similar estimates as in our original design, indicating that selection does not confound our results.

Taken together, these facts reject the null that tax preparers do not influence the transmission of this policy. We attempt to quantify the relative impact of preparers using a simple variance decomposition. Our estimate comes from the within-firm covariance structure for observations that do and do not share the same preparer at different points in time. This decomposition relies on a strong assumption of independence between the unobserved

[6] A growing literature documents the impact of managers on firm performance. Key contributions include Bertrand and Schoar (2003), Bloom and Van Reenen (2007), Kaplan, Klebanov and Sorensen (2012), and Bloom et al. (2013).

preparer effect and the unobserved firm error term. Based on this approach, we find that the variance of the preparer effect equals 9 percent of the total variation in take-up. As a benchmark for this magnitude, the prediction from firm observables accounts for 9 percent of the variation in take-up. If selection into preparers does not affect take-up, preparers matter as much as firm observables for predicting claiming behavior.

Our paper sits at the intersection of several strands in the economics, finance, and accounting literatures. The literature on optimization frictions and behavioral responses to tax policy mostly focuses on individual taxes and settings where imperfect information or search costs affect responses to tax incentives.[7] The literature on public program take-up surveyed by Moffit (2003) and Currie (2006) has traditionally focused on social welfare programs targeted at low-income and vulnerable populations. Many studies in this area argue that these programs have low participation rates because of filing requirements and poor information.[8] We show that similar considerations apply to firms and demonstrate that their claiming decisions depend on the third party experts they hire to help them. Our results reject the standard view that firms optimize perfectly with respect to taxes.

The paper extends past research about the tax treatment of corporate losses.[9] These studies focus on how loss rules affect marginal incentives to invest and borrow. While they emphasize that marginal tax rates do not equal statutory tax rates once firms take into account the ability to offset gains in one year with losses in another, these papers typically assume that firms always claim the carryback when available. Our results raise questions about whether many firms take dynamic corporate tax incentives into account.

The paper also relates to a growing literature on the role of human capital in firm decision

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[7]Key empirical studies include Chetty, Looney and Kroft (2009), Finkelstein (2009), Chetty et al. (2011), Chetty (2012), Chetty and Saez (2013), Chetty, Friedman and Saez (2013), and Goldin and Homonoff (2013). These papers have found evidence that individuals under-respond to taxes in the context of sales taxes, highway tolls, and the individual income tax.

[8]Daponte and Taylor (1999), Currie and Grogger (2001), Bitler, Currie and Scholz (2003), Heckman and Smith (2004), and Aizer (2007) make this point in the context of food stamps, job training programs, and public health insurance.

[9]Key papers include Auerbach and Poterba (1987*a*), Altshuler and Auerbach (1990), Graham (1996), and Graham and Mills (2008).

making. These studies have documented that firm investment, leverage, and effective tax rates depends on managerial style.[10] In recent work, Klassen, Lisowsky and Mescall (2012) find a cross-sectional relationship between the aggressiveness of corporate tax positions and whether a firm's financial auditor prepares the tax return. We introduce a novel research design using quasi-experimental preparer switches based on deaths and relocations to show that, in addition to internal managers, external consultants can significantly affect how firms make decisions.

The paper proceeds as follows. Section 1.2 introduces the tax code's loss rules, describes the corporate tax data and sample selection process, and documents refund take-up among eligible firms. Section 1.3 performs a cost-benefit assessment of the tax loss choice and shows that the low take-up puzzle survives this analysis. Motivated by these findings, Section 1.4 describes the corporate market for tax preparation services and explores the relationship between paid preparers and their clients' claiming patterns. Section 2.6 discusses policy implications and future research directions.

1.2 Corporate Losses and Tax Refunds

1.2.1 The Tax Code's Loss Rules

Consider a firm that reports a tax loss. The corporate tax code allows the firm to apply losses in one year to offset profits in other years and thus reduce its average tax burden. In general, the firm can choose either to carry the loss back against past taxable income or to carry the loss forward into the future. In tax code terminology, the option is between a *carryback* and a *carryforward*.

The tax loss choice has economic consequences for the firm because the two options differ in the timing of the tax benefit. Under the carryback, firms immediately receive a

[10] Bertrand and Schoar (2003) study the role of managers in corporate decision making. Bloom and Van Reenen (2007) and Kaplan, Klebanov and Sorensen (2012) document strong correlations between management practices and firm performance measures. Dyreng, Hanlon and Maydew (2010) and Armstrong, Blouin and Larcker (2012) show that managers influence corporate effective tax rates.

Table 1.1: *Legislative background on tax loss carrybacks and carryforwards, 1998-2011*

Ending fiscal period (year-month)[a]	Carryback period	Carryforward period	Enacting legislation
1998-12 to 2000-12	2 years	20 years	TRA 1997 (permanent)[c]
2001-01 to 2002-12	5 years	20 years	JCWAA 2002 (temporary)[d]
2003-01 to 2007-12	2 years	20 years	TRA 1997 (permanent)
2008-01 to 2010-11	5 years	20 years	ARRA 2009 (temporary)[b,e]
			WHBAA 2009 (temporary)[b,f]
2010-12 to 2012-11	2 years	20 years	TRA 1997 (permanent)

Notes: This table summarizes the statutory window for eligible carrybacks and carryforwards between 1998 and 2011. The policy rules apply to corporate tax returns with ending fiscal periods that fall within the range detailed in the first column of the table. The last column lists the legislation that enacted the policy changes. In this period, the carryback window was twice expanded temporarily as part of fiscal stimulus legislation. The information for this table was pulled from bulletins and revenue procedures released by the Internal Revenue Service.

a. Corporations file income taxes for the fiscal year instead of the calendar year
b. ARRA 2009 and WHBAA 2009 limited deductions against the fifth fiscal year preceding a firm's current tax loss to 50 percent of taxable income
c. TRA: Taxpayer Relief Act of 1997
d. JCWAA: Job Creation and Worker Assistance Act of 2002
e. ARRA: American Recovery and Reinvestment Act of 2009
f. WHBAA: Worker, Homeowner, and Business Assistance Act of 2009

refund for the taxes they paid in the past. Under the carryforward, firms defer the tax benefit to future periods when they deduct their loss against future taxable income. The carryback is typically more valuable because the firm gets cash now, but the carryforward can be better if the firm expects to pay a higher marginal tax rate in the future.

A statutory window limits the application of loss deductions to past and future tax years. Table 1.1 summarizes the statutory window for carrybacks and carryforwards in the US tax code over the 1998-2011 period. The carryback window was typically two years during this time, except when Congress twice lengthened it to five years in response to recessions. These policy changes enhanced the automatic stabilizer feature of the carryback provision, which generates more refunds in bad times when corporate losses are common. The carryforward window was twenty years throughout this same period.

The size of the refund generated by the carryback election depends on how much the firm has paid in past taxes. When a firm claims the carryback, it must fully apply the loss to all eligible past income. Loss firms are not eligible for a carryback refund when they do not have any past income within the statutory window. In cases where the current loss exceeds eligible past taxable income, a carryback election generates both a tax refund for past taxes paid and a carryforward deduction equal to the losses in excess of past income.

To claim a carryback, the firm must file a special form to document how it computed its carryback refund. The form details how the loss deduction is applied to past tax returns to generate a tax refund.[11] Upon approving the firm's claim, the tax authority sends a refund check equal to the amount of overpaid taxes in past years after taking into account the loss deduction. To claim a carryforward, the firm must keep a record of its carryforward stock from past losses and then take a net operating loss deduction on its future tax return. All loss deductions against past and future taxable income are computed in nominal terms.

1.2.2 Business Tax Data

We use administrative IRS databases to document the impact of preparers on the claiming of the carryback refund. This database includes all corporations that file a tax return in the United States, approximately 6.5 million per year. We rely on two main files: a tax return file that records line items from corporate income tax returns[12] and a transactions file that records debits and credits to individual tax liability accounts. We measure corporate characteristics[13] from the tax return file and claimed refunds from the transactions file.

We limit our study to C corporations because they are taxed at the firm level and retain the decision over whether to claim the tax refund for losses. We exclude firms with mean revenue and mean payroll measures less than $100,000 because they may not represent

[11] A firm claims the carryback by filing either Form 1139 or Form 1120X. To remain eligible for the carryback, the firm must file within three years of the due date (plus extensions) of the tax return where it reports the loss. Alternatively, the firm can elect to irrevocably forgo the carryback and fully carry forward the loss when it files its income tax return. This election is made by checking a box on its income tax return.

[12] Form 1120 and Form 1120S.

[13] Corporate characteristics include revenue, assets, payroll, industry codes, and tax losses.

operating firms (Knittel et al. 2011). And to focus on firms with a meaningful carryback option, our sample only includes firm-year observations that are eligible for a carryback refund of at least $1,000.

Table 1.2 reports summary statistics for our sample. It consists of 1.24 million firm-year observations and 612,070 individual firms. The median firm is small, with $1.5 million in revenue, $489 thousand in assets, and $469 thousand in payroll. The eligible carryback refunds are also moderate in size, with a median that equals almost $5.7 thousand. To benchmark this amount, we compute the ratio of refund to revenue and the ratio of EBITDA to revenue. The median for each quantity equals 0.4 percent and 4.6 percent respectively. Compared to each other, they imply that the eligible refunds are a moderate share of earnings.[14]

Table 1.2 also includes variables for the preparer and the tax firm matched to each corporate tax return. Most corporations hire small tax firms. The median corporation hires a tax firm with $0.8 million in revenue and 98 corporate clients. In cases where a preparer or a tax firm cannot be matched to the corporate tax return, most typically the corporation does not report using a third-party preparer.

Table 1.3 reports summary statistics for a subset of firms that switch preparers between 1998 and 2011. All observations in this subsample match to a preparer. This subsample only includes two observations per firm: the last observation before switching preparers and the first observation after switching preparers. It consists of 124,862 firm-year observations and 62,431 individual firms. Similar to the overall sample, the median firm is small with $1.9 million in revenue. The table also includes the preparer characteristics used to test whether client claiming behavior depends on which preparer is employed.

We simulate each firm's eligible carryback refund because the administrative IRS data does not explicitly record this amount. Our algorithm first imputes each firm's past taxable income from its historical tax liability. We next use the policy rules to determine the eligible

[14]We do not report the ratio of refunds to EBITDA directly because EBITDA is often negative.

Table 1.2: *Summary statistics of C corporations eligible for carryback refunds, 1998-2011*

Covariates	Mean	P10	P50	P90
Firm variables				
revenue ($1M)	42.189	0.307	1.485	12.442
assets ($1M)	91.631	0.048	0.489	6.394
payroll ($1M)	5.336	0.103	0.469	3.356
EBITDA ($1M)	2.020	-0.118	0.079	0.603
EBITDA / revenue	-0.101	-0.092	0.046	0.296
Refund variables				
take-up of carryback refund	0.3742			
eligible refund ($1K)	286.490	1.463	5.696	70.670
eligible refund / revenue	0.0415	0.0008	0.0042	0.0281
Preparer variables				
indicator for matching tax return	0.7107			
labor income ($1K)	127.824	5.080	99.450	269.583
mean client revenue ($1M)	9.676	0.463	1.339	7.323
number of corporate clients	51.55	8.00	37.99	103.26
Tax firm variables				
indicator for matching tax return	0.7673			
revenue ($1M)	132.119	0.136	0.785	10.741
indicator for sole proprietor	0.1637			
mean client revenue ($1M)	7.738	0.538	1.577	7.031
number of corporate clients	498.35	21.51	98.49	539.88

Notes: Number of observations: 1,244,729. Number of firms: 612,070. This table reports summary statistics for all C corporations with tax losses between 1998-2011 that were eligible for a carryback refund of at least $1,000. The sample is derived from the US population of corporate tax returns. All dollar values are normalized to 2013 price levels. We do not report standard deviations because the variables are highly skewed due to the firm size distribution. The firm variables are based on the corporate tax return. EBITDA refers to earnings before interest, taxes, depreciation, and amortization. See Appendix Section A.3 for details about how we construct these measures from the individual line items on the corporate income tax return. We directly observe take-up of the carryback refund, but we impute the eligible refund based on the policy rules and each firm's historical tax liability. The preparer and tax firm variables are based on their matching tax returns. Their statistics exclude observations that do not have a matching tax return. Labor income equals the sum of W-2 wages and self-employment income. Mean client revenue refers to the corporate clients of each preparer and each tax firm. Percentiles are computed as percentile means.

Table 1.3: *Summary statistics of C corporations that change preparers, 1998-2011*

Covariates	Mean	Standard deviation	P10	P50	P90
Firm variables					
revenue ($1M)	24.877		0.338	1.873	23.370
assets ($1M)	38.817		0.063	0.650	15.285
payroll ($1M)	5.027		0.103	0.572	5.801
EBITDA ($1M)	0.525		-0.228	0.075	0.796
EBITDA / revenue	-0.149		-0.109	0.037	0.264
Refund variables					
take-up of carryback refund	0.3572				
eligible refund ($1K)	233.866		1.566	7.045	125.411
eligible refund / revenue	0.0506		0.0007	0.0042	0.0298
Preparer variables					
I(certified public accountant)	0.8314				
I(attorney)	0.0214				
I(other professional license)	0.0556				
log(labor income)	11.36	1.17	9.98	11.57	12.51
I(self-employment)	0.1794				
age	49.89	11.17	35.52	50.00	63.48
log(mean client revenue)	14.59	1.50	13.06	14.27	16.62
log(total client revenue)	17.86	1.82	15.50	17.94	20.11

Notes: Number of observations: 124,862. Number of firms: 62,431. This table reports summary statistics for the sample of C-corporations that were eligible for carryback refunds of at least $1,000, that switched preparers between 1998 and 2011, and that reported preparer identifiers which match to a tax return. All dollar values are normalized to 2013 price levels. We do not report standard deviations for the firm and refund variables because they are highly skewed due to the firm size distribution. The firm variables are based on the corporate tax return. EBITDA refers to earnings before interest, taxes, depreciation, and amortization. See Appendix Section A.3 for details about how we construct these measures from the individual line items on the corporate income tax return. We directly observe take-up of the carryback refund, but we impute the eligible refund based on the policy rules and each firm's historical tax liability. The preparer variables are based on their matching tax returns. Labor income equals the sum of W-2 wages and self-employment income. The self-employment indicator reflects preparers that derive at least half of their labor income from self-employment. Mean and total client revenue refer to the corporate clients of the individual preparers. Percentiles are computed as percentile means.

carryback window. Starting with the earliest eligible year, we deduct the current tax loss[15] against imputed past taxable income. We continue with these deductions until either the current loss or past taxable income is exhausted. We then re-computes the historical tax liability based on the post-deduction taxable income. The difference between the pre-deduction and post-deduction tax liability equals our simulation for the eligible carryback refund.

We verify our algorithm for the eligible refunds using firms that claim the carryback. Although the IRS database does not track the eligible refunds, it does record the claimed amounts. For the subset of firms that make the carryback election, we can directly compare the claimed amount to the simulated amount. Our results indicate that we impute the eligible refunds with a high degree of accuracy. When we regress log(claimed amount) on log(eligible amount), we find a coefficient of 0.9636 and an R^2 of 0.9336. Appendix Section A.1 describes in more detail how we construct and validate our measure for the eligible refund.

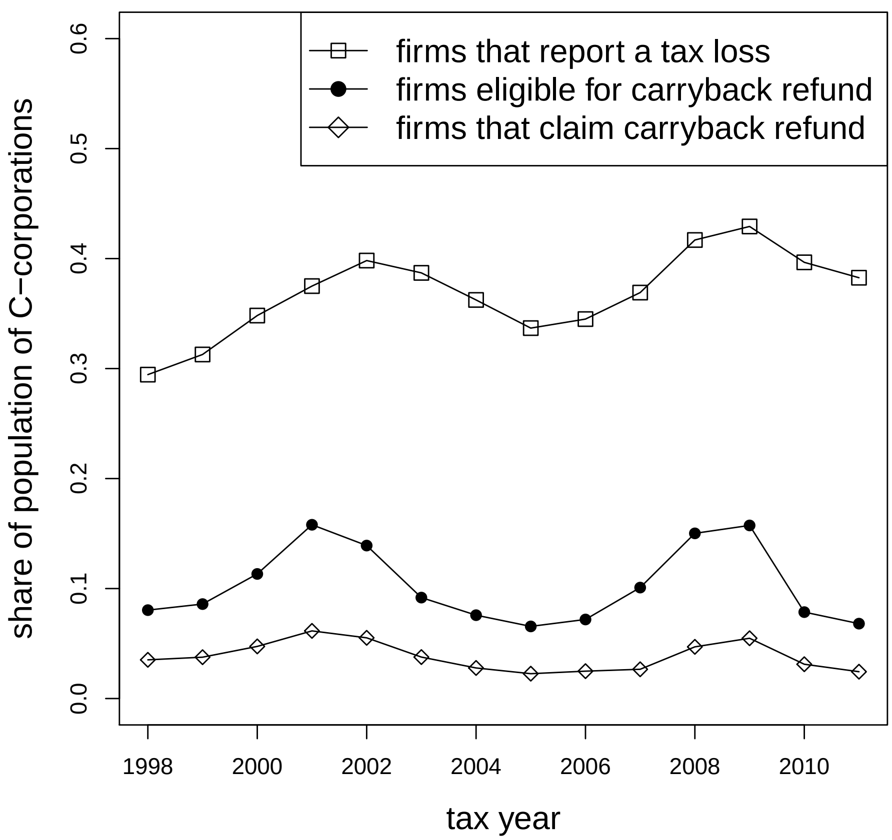
1.2.3 Low Take-up of Tax Refunds for Losses

Eligibility for the carryback refund is common. Figure 1.1 reports the annual share of loss firms from the population of C corporations. It also reports the share of firms eligible for a carryback refund of at least $1,000. Over the 1998-2011 period, 37 percent of firm-year observations report tax losses and 80 percent of firms report a loss at least once. Among tax loss firms, 28 percent are eligible for a carryback refund. Firms frequently face the choice between whether to apply their tax loss deduction as a carryback or a carryforward.

In addition, the aggregate magnitude of the carryback refunds is macroeconomically relevant. Figure 1.2 reports the annual amount of eligible and claimed refunds for the population of C corporations. Over the 1998-2011 period, C corporations claimed $187

[15]Tax losses are defined from the front page of the income tax return for C corporations. We use the statutory definition of tax losses for ordinary income. It equals net income (Line 28) + special deductions (Line 29b). This definition excludes capital income losses. It also excludes losses obtained from mergers and acquisitions, which are reported with the stock of losses from prior periods (Schedule K, Line 12).

Figure 1.1: *Frequency of tax losses and carryback refunds*



Notes: This figure plots the share of C corporations that report a tax loss, that are eligible for a carryback refund, and that claim the carryback refund. It is based on the population of corporate tax returns for C corporations. We limit eligibility to firms that have the option to claim a carryback refund of at least $1,000. We exclude firms with mean revenue and mean payroll less than $100,000 because they may not represent real operating entities.

billion. Carryback refunds play an even larger role as countercyclical policy, totaling $68 billion in 2008 and 2009. As a benchmark, payments for unemployment insurance equaled $209 billion during these years (US Department of Labor 2014).

Claimed refund amounts, however, significantly understate the potential size of the policy. Only 37 percent of eligible firms claimed their refund. In aggregate, eligible refunds are nearly twice as large as claimed refunds. During the 1998-2011 period, C corporations were eligible for $357 billion in carryback refunds. In 2008 and 2009 alone, they were eligible for $124 billion. Thus low take-up substantially undermines the potential effect of the carryback refund as fiscal stimulus.

1.3 Evidence on Tax Loss Choices

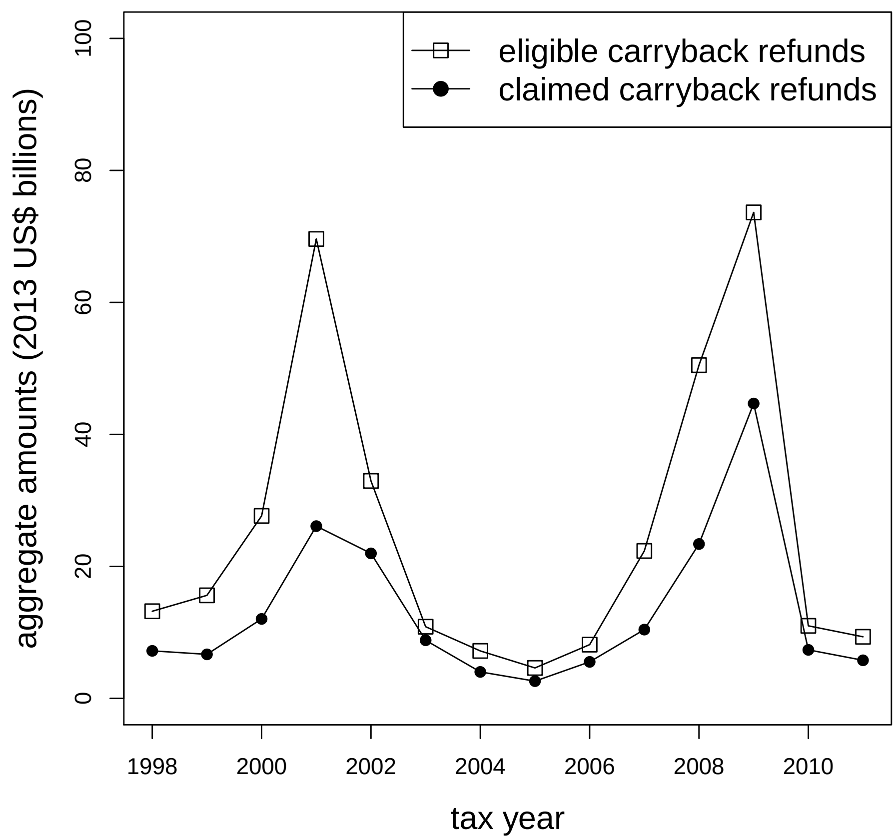
In this section, we implement a cost-benefit analysis on the set of eligible firms to compare the net present value of the carryback and carryforward options. This setting provides a rare opportunity to evaluate whether firms make the value-maximizing choice. Despite the low take-up rate, 79 percent of firms value the carryback more than the carryforward. We discuss alternative reasons for the low take-up rates.

1.3.1 A Cost-Benefit Analysis of Tax Loss Choices

Loss firms deciding between the carryback and the carryforward elections need to consider whether it would be more valuable to use the loss as a deduction against past taxable income or against future taxable income. The value of the carryback depends on the tax rates that the firm paid in the past. In contrast, the carryforward value depends on the tax rates that it will pay in the future, the length of time that it will take the firm to return to a profitable state, and the firm's discount rate. These considerations also arise when the corporate loss exceeds eligible past taxable income because the carryback election generates a carryforward deduction equal to the loss in excess of eligible past income.

Computing the value of the carryback and carryforward elections involves a net present value calculation because either option can generate carryforward deductions to be applied

Figure 1.2: *Aggregate magnitudes of carryback refunds*



Notes: This figure plots the aggregate dollar amounts of eligible and claimed carryback refunds for C corporations. All dollar amounts are indexed to 2013 price levels.

against future taxable income. The key difference between their formulas is that the carryback election deducts the loss against past taxable income and the carryforward election does not. Carryback deductions against past taxable income are not discounted because they generate an immediate tax refund.

We formalize the net present value formulas for the carryback and carryforward elections under the assumption that the firm has perfect foresight over the timing of future taxable income,

$$\begin{aligned} NPV^b &= \sum_{t=T_{\min}}^{-1} \tau_t \, D_t^b + \sum_{t=1}^{T_{\max}} \frac{\tau_t \, D_t^b}{(1+r)^t} \\ NPV^f &= \sum_{t=1}^{T_{\max}} \frac{\tau_t \, D_t^f}{(1+r)^t} \end{aligned} \tag{1.1}$$

where τ_t is the tax rate in time t, D_t^b is the deduction taken in time t under the carryback election, D_t^f is the deduction taken in time t under the carryforward election, and r is the firm's discount rate for future tax savings. Time is indexed relative to the loss at time $t = 0$. Deductions applied to past taxable income are not discounted because the refund is immediate. In either case, the nominal sum of the deductions cannot exceed the loss reported at time $t = 0$. The nominal sum of the deductions can be less than the current loss in cases where the firm does not have sufficient past and future taxable income to offset the loss.

Table 1.4 uses a numerical example to clarify the differences between the carryback and carryforward elections. For a firm with a loss of \$100, we compute how deductions under the carryback and carryforward elections would be applied to the firm's taxable income. Under the carryback election, the firm first deducts its loss against taxable income in period $t = -2$. It deducts its remaining loss against taxable income in period $t = -1$. Assuming a tax rate of 35 percent, the net present value of the carryback election equals $\$100 \times \tau = \$35$. Under the carryforward election, the firm deducts all of its loss against taxable income in period $t = 2$. Assuming a tax rate of 35 percent and a discount rate of 7 percent, the net present value of the carryforward election equals $\frac{100 \times \tau}{(1+r)^2} = \$30.57$. In this example, the carryback election has a higher net present value because the tax rate is constant over time and the firm discounts future tax savings.

Table 1.4: *Example of loss deduction under carryback and carryforward elections*

	Event time relative to loss year					
	-2	-1	0	1	2	3
Taxable income before loss deduction	50	100	-100	0	100	100
Panel A: carryback election						
Loss deduction	-50	-50	+100	0	0	0
Taxable income after loss deduction	0	50	0	0	100	100
NPV of carryback election	$100\,\tau = \$35$					
Panel B: carryforward election						
Loss deduction	0	0	+100	0	-100	0
Taxable income after loss deduction	50	100	0	0	0	100
NPV of carryback election	$\frac{100\,\tau}{(1+r)^2} = \$30.57$					

Notes: This table illustrates the application of carryback and carryforward deductions for a firm that reports a tax loss of \$100 at time $t = 0$*. Panel A assumes that the firm makes the carryback election and Panel B assumes that the firm makes the carryforward election. The illustration also assumes that the firm pays a tax rate of* $\tau = 0.35$ *and has a discount rate of* $r = 0.07$*. Under the carryback election in Panel A, the hypothetical firm applies its loss deduction against its past taxable income. It starts with the earliest eligible tax year (*$t = -2$*) and then proceeds to the next tax year (*$t = -1$*). Under the carryforward election in Panel B, the hypothetical firm applies its loss deduction against its future taxable income. In this example, we assume that the firm claims the loss deduction as early as possible (*$t = 2$*). Even though this hypothetical firm always pays the same tax rate, the net present value of these two elections differ because they realize the tax benefits at different times. The carryback election realizes the tax benefits immediately at time* $t = 0$ *as a tax refund. In contrast, the carryforward election defers the tax benefits until time* $t = 2$ *when it claims its loss deduction. In this example, the carryback election has a higher net present value than the carryforward election because it realizes its tax benefit earlier.*

1.3.2 Empirical Evaluation of Cost-Benefit Formulas

We empirically evaluate the net present value formulas in Equation 1.1 for firms with losses between 1998 and 2002. We restrict our sample to this period because we want to use a future 10-year period of realized taxable income to value each firm's carryforwards. We assume that all firms in this period do not have any carryforwards from prior tax years. We make this assumption because the administrative tax data does not begin to collect this information until 2003. We find similar results when we replicate our analysis on firms with losses in 2003 where we do not need make assumptions about their pre-existing stock of carryforwards. We also limit our sample to firms with eligible refunds of at least $1,000 to exclude firms that do not have a meaningful carryback option.

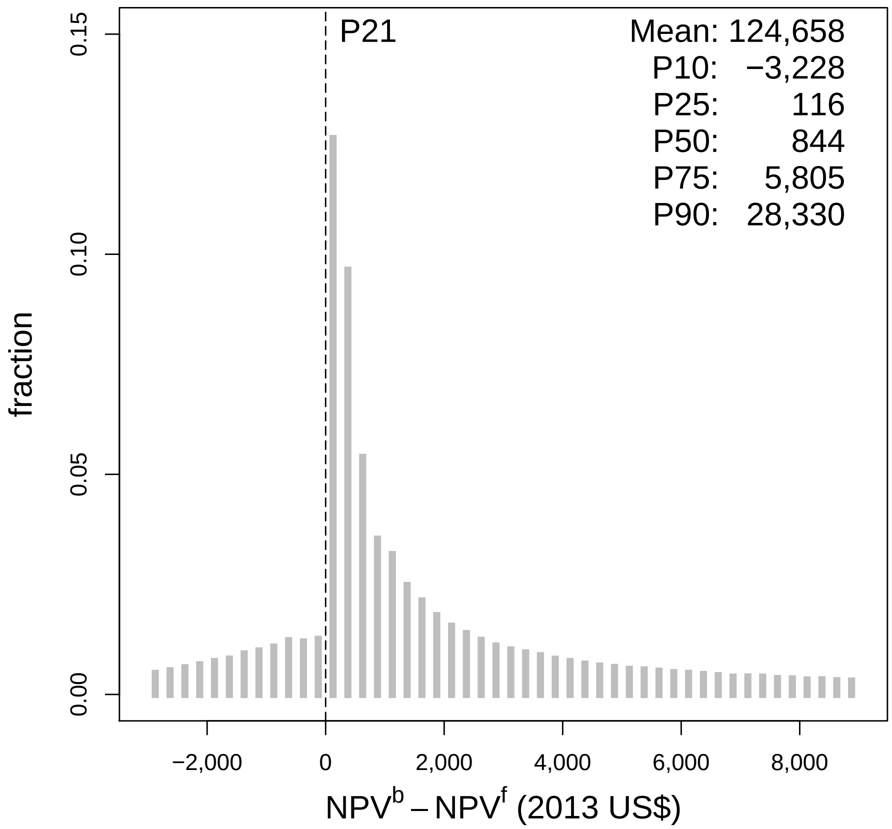
We simulate the claiming of future carryforward deductions over a 10-year period based on their realized taxable income. We perform this simulation under both the carryback and carryforward elections. We assume that firms will claim their future carryforward deductions as soon as possible and, in cases of surviving firms that have unused losses after 10 years, that all unused losses are claimed in the 11th year. We then compute the net present values of the carryback and carryforward elections assuming a discount rate of 7 percent.

We calculate the net present value difference between the carryback and carryforward elections, $NPV^b - NPV^f$, and plot its histogram in Figure 1.3. For 79 percent of the sample, the carryback election has a larger net present value than the carryforward election. This difference is greater than $844 for half the sample.[16] Based on this simple net present value comparison, the majority of firms value the carryback more than the carryforward election.

This finding is robust to our assumption of a 7 percent discount rate. In Table 1.5, we show the sensitivity of our results to the assumed discount rate. For a given threshold and discount rate, the table reports the share of firms where the ratio of NPV^f to NPV^b is less than the threshold. Each column assumes a different threshold and each row assumes a

[16]These results are robust to constructing an ex ante measure of net present value based on forecasting the ex post net present value in a linear regression with firm covariates.

Figure 1.3: *Histogram of NPV difference between carryback and carryforward*



Notes: This figure plots a histogram of the net present value difference between the carryback and carryforward elections. The sample includes firms with tax losses between 1998 and 2002 that were eligible for a carryback refund of at least $1,000. We calculate the net present value based on each firm's realized taxable income over a 10-year period. We use their realized taxable income to simulate the claiming of future carryforward deductions and to compute the net present value of future tax benefits. Please see Section 1.3.2 for further details.

Table 1.5: *Share of firms below alternative thresholds for NPV_f / NPV_b*

Firm discount rate	Maximum value for NPV_f / NPV_b			
	1	0.9	0.8	0.7
3%	0.7499	0.4163	0.3246	0.2828
5%	0.7719	0.4955	0.3634	0.3001
7%	0.7911	0.5727	0.4109	0.3261
9%	0.8087	0.6127	0.4644	0.3574

Notes: This table compares the net present value of the carryforward and carryback elections for firms with tax losses between 1998 and 2002. It shows the sensitivity of our results to the assumed firm discount rate. The table reports the share of firms for whom the ratio of the carryforward net present value to the carryback net present value is below a maximum threshold. Each column assumes a different maximum threshold and each row assumes a different firm discount rate for the net present value calculation. The sample only includes firms that were eligible for a carryback refund of at least $1,000. NPV_f indicates the net present value for the carryforward election. NPV_b indicates the net present value of the carryback election. Please see Section 1.3.2 for further details.

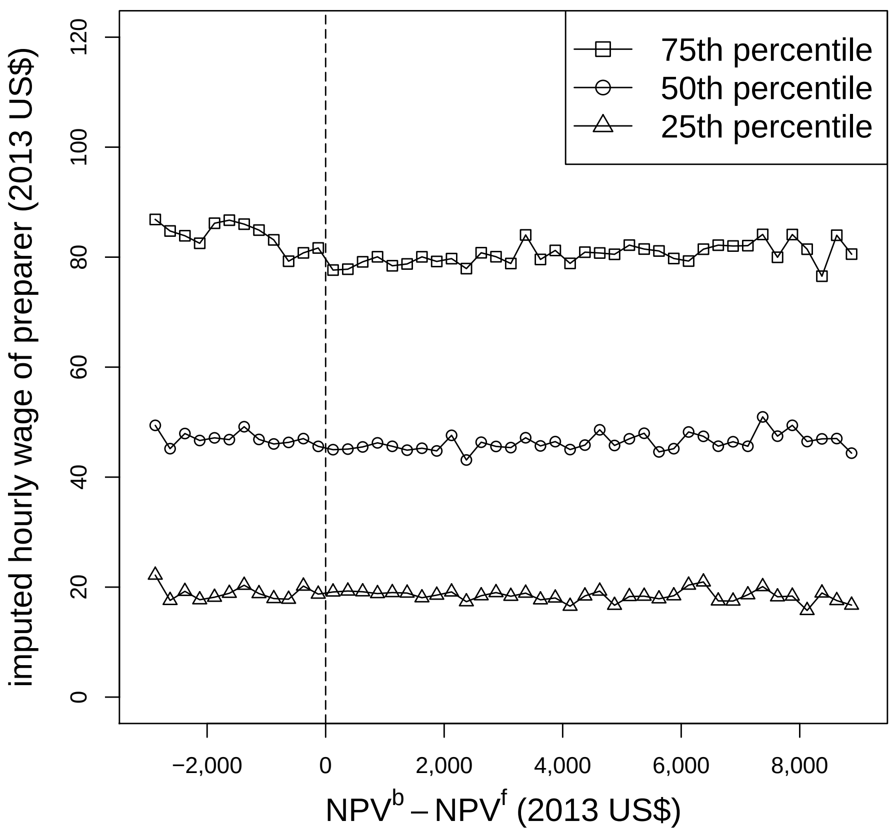
different discount rate. Varying the discount rate between 3 and 9 percent, the share of firms where the net present value of the carryback election is greater than the carryforward election ranges between 75 and 81 percent.

Figure 1.4 compares the net present value difference between the carryback and the carryforward options to an estimate of the labor cost for submitting a carryback application. It provides a benchmark for evaluating the magnitude of the net present value difference. Anecdotal conversations with preparers that serve firms in the size range of our sample suggest that filing for the carryback involves one to two hours of additional work. Figure 1.4 plots the imputed hourly wage of preparers at the 25th, 50th, and 75th percentiles by the net present value difference between the carryback and the carryforward options. We impute the hourly wage by dividing each individual preparer's annual labor income[17] by $40 \times 50 = 2000$. This denominator assumes that preparers work 40 hours each week for 50 weeks over the course of a year.

We find that the imputed hourly wage remains relatively constant regardless of the net present value difference. The imputed wage equals approximately $20, $45, and $80 at the

[17] We define labor income as the sum of W-2 earnings and self-employment income.

Figure 1.4: *Imputed preparer wage by NPV difference between carryback and carryforward*



Notes: This figure plots the 25th, 50th, and 75th percentiles in imputed preparer wages by the net present value difference between the carryback and the carryforward options. The sample includes firms with tax losses between 1998 and 2002 that were eligible for a carryback refund of at least $1,000. Wages are imputed by dividing preparer labor income by 2000. This computation assumes that preparers bill 40 hours per week over 50 weeks in one calendar year. Labor income equals the sum of W-2 earnings and self-employment income. Dollar amounts are normalized to 2013 price levels.

25th, 50th, and 75th percentiles. Even allowing for a mark-up for overhead expenses and profit, the net present value differences between the carryback and the carryforward options are large relative to these estimates of the labor costs.[18]

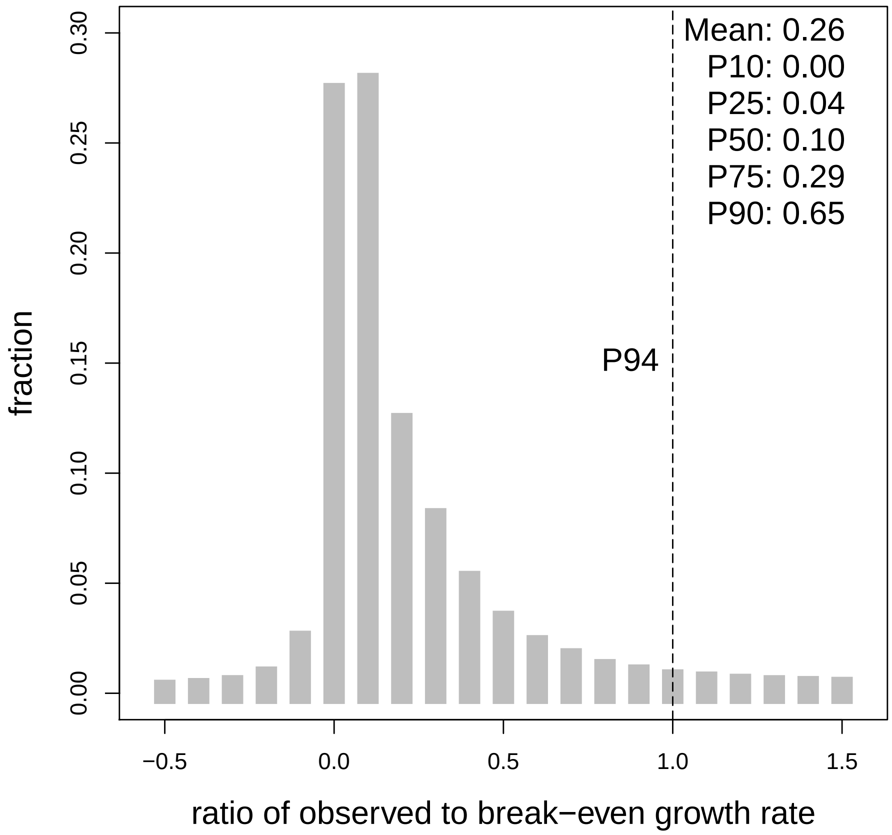
Figure 1.5 provides an alternative benchmark for whether firms should make the carryback or the carryforward election. It compares the observed growth rates in corporate taxable income to hypothetical growth rates at which the net present value of the carryback and carryforward options equal each other. The observed growth rates are computed using each firm's observed taxable income between the year of the net operating loss and the tenth year after the loss. We find the growth rate with a linear fit that intersects the y-axis at the value of the initial loss. The break-even growth rates are computed from a linear forecast over a ten-year period following the loss year. The initial value for the linear forecast also equals the firm's net operating loss.

We present this comparison as a histogram of the ratio between the observed growth rate and the break-even growth rate. The observed and break-even growth rates equal each other when the ratio equals one. The ratio is less than one in cases where the observed growth rate is less than the break-even growth rate. The ratio can be negative because some firms experience negative growth rates in taxable income following their loss.

We find that the observed growth rate is less than the break-even rate in most cases. The mean ratio of observed to break-even growth rates equals 0.26. The observed growth rate is less than the break-even growth rate for 94 percent of observations. This result differs from a comparison of the net present value of the carryback and the carryforward options because, in this exercise, we assume a linear growth rate in taxable income (which smooths the volatility). This comparison implies that few firms experience growth rates in taxable income that would make electing the carryforward more valuable than the carryback option.

[18] In cases where a team of preparers file a tax return for a client, the head of the team will typically sign the client's return. Because our sample consists predominantly of small corporations that hire small tax firms, we suspect that most client returns are prepared by individuals.

Figure 1.5: *Histogram of the ratio between observed and break-even income growth rates*



Notes: This figure plots a histogram of the ratio between the observed and break-even growth rates in corporate taxable income. The sample is based on firms with tax losses between 1998 and 2002 that were eligible for a carryback refund of at least $1,000. It also excludes firms for whom the value of the carryforward is dominated by the carryback (i.e., the value of the carryback is greater than the carryforward regardless of the firm's future growth rate). The break-even growth rate refers to the rate at which taxable income must grow for the carryforward and carryback options to equal in value. This computation assumes a linear growth rate that starts with each firm's reported loss. The observed growth rate is based on each firm's observed taxable income in the ten year window following its reported loss. The dotted line at 1.0 indicates the point at which the observed growth rate equals the break-even growth rate. The ratio between these rates can take negative values because some firms experience negative growth rates following their loss.

1.3.3 Alternative Explanations for Low Take-Up

The results from our cost-benefit exercise make the the low take-up rate of the carryback refund puzzling. Based on a net present value comparison alone, most firms should claim the carryback. We next consider alternative rationales for why a minority of eligible firms would claim the carryback.

First, small firms may not know how to file for the carryback refund, or even that this option is available to them. Claiming it involves submitting an additional form and recomputing the firm's income tax for each prior tax year affected by the carryback. Small firms without professional expertise regarding the tax code may find the filing requirements to claim the carryback refund too complicated.

Second, a firm's preparer may charge additional fees for claiming the carryback refund. While the preparer may know how to claim it, filing for the carryback still involves additional effort on their part. In this industry, it is common for preparers to bill their clients by the hour or by the tax form. The additional fees for claiming the refund may be sufficient to deter clients.

Third, firms may be concerned that filing for a carryback refund will put them at risk for an IRS audit. When a firm applies for the carryback, an IRS employee must review their recomputed tax liability for prior years. This carries the risk that the IRS will spot something that will prompt an audit. Even if the actual risk is small, the perceived risk may be sufficient to deter filing for the carryback claim.

Each of these alternative explanations creates opportunities for preparers to determine whether their client claims the carryback refund. Firms hire preparers to inform them about the tax code, file tax returns on their behalf, and warn them about the audit risk of different tax reporting choices. Preparers may differ in whether they encourage their clients to claim the tax refund based on their own beliefs about its merits for their clients, its filings costs, and its audit risks.

1.4 Tax Preparers and the Take-up of Tax Refunds

In this section, we provide evidence that client take-up of the carryback refund depends on preparers. We start with background information about the corporate market for tax preparation services to provide context for our results. We then show that preparer characteristics predict whether firms claim the carryback refund using a research design based on firms that switch preparers. We also causally validate our results by focusing on a subset of switching events where the prior preparer either dies or moves their personal residence. In these cases, we find it more plausible that changes in client unobservables do not confound our estimates. We conclude with an analysis of variance exercise that finds that, if selection into preparers does not affect take-up, an unobserved preparer effect accounts for as much of the variation in claiming behavior as firm observables.

1.4.1 Corporate Market for Tax Preparation Services

A large private market provides tax preparation services to firms. In 2012, 96 percent of corporations hired an external preparer to file their income taxes. The market employs 188,735 individual preparers who file tax returns for corporations. Although federal regulations do not mandate any licensing requirements for preparers, 89 percent of firms hired a preparer with a professional license[19] (predominantly certified public accountants). The remaining 10 percent of firms hired preparers without any professional credentials.[20]

The tax preparation market includes a wide variety of tax firms. They range from sole proprietorships with a single employee to national brands with thousands of locations. These firms also vary in their degree of specialization. Some focus on tax preparation (e.g., H&R Block, Inc.) whereas others offer a broad portfolio of professional services for businesses (e.g., BDO USA, LLP). Despite these differences, employees at most tax firms use

[19] Either a certified public accountant, attorney, enrolled agent, or state licensed preparer. Enrolled agents are licensed by the Internal Revenue Service. They must pass an examination and fulfill 72 hours of continuing education every three years.

[20] Based on corporate preparers for the 2012 tax year.

a tax preparation software to service their clients (Internal Revenue Service 2009).

1.4.2 Claiming Decisions and Preparer Characteristics

Baseline Specification. We use firms that switch preparers to show that preparer characteristics predict claiming behavior. Our analysis uses a sample of firms that were eligible for carryback refunds of at least $1,000 between 1998 and 2011. We restrict the sample to firms that were eligible in multiple years and that switched preparers. Because we want to identify our result from variation due to changing preparers, we only include the last observation before switching preparers and the first observation after switching preparers for each firm in the sample. These observations are often not consecutive because firms are not eligible for the carryback refund in each tax year. If a firm changes preparers multiple times, we only include observations associated with the last switching event.

We estimate Equation 1.2 in a panel regression given by

$$\text{I(carryback take-up)}_{ijt} = Z_{\mathbf{J}(i,t)}\gamma + X_{it}\beta + \alpha_i + \delta_t + \epsilon_{it} \tag{1.2}$$

where the subscripts represent client i with preparer j in tax year t, $Z_{\mathbf{J}(i,t)}$ are preparer characteristics, X_{it} are client characteristics, α_i is the client fixed effect, and δ_t is the tax year fixed effect. Preparer observables include indicators for professional credentials,[21] log(labor income), I(self-employment),[22] age, log(mean client revenue), and log(total client revenue). Client observables include log(revenue), log(assets), and log(EBITDA).

Our estimates of Equation 1.2 rely on the following identifying assumption:

> **Assumption 1 [Switchers Design]**: The error term ϵ_{it} must satisfy the strict exogeneity condition $\mathrm{E}[\epsilon_{it}|Z_{\mathbf{J}(i,t)}, X_{it}, \alpha_i, \delta_t] = 0$.

This condition implies that client unobservables in the error term must be uncorrelated

[21] We include separate indicators for certified public accountants, attorneys, and preparers with another professional license. The last category includes enrolled agents and state licensed preparers. The omitted category are preparers without any professional credential.

[22] The self-employment indicator equals one if the preparer derives at least half of their labor income from self-employment.

with preparer characteristics, client observables, a client fixed effect, and a tax year fixed effect. Because the switchers design uses within-firm variation, this assumption will hold if unobservable determinants of carryback take-up remain unchanged before and after switching preparers.

We report estimates from the switchers design in Table 1.6. The regressions are univariate with respect to preparer characteristics. All regressions include a firm fixed effect, a tax year fixed effect, and firm controls. They also include dummies for missing values of the preparer characteristics and the client controls. We block bootstrap the standard errors by firm[23] and report them in parentheses. With the exception of the category for "other professional license", all preparer covariates are statistically significant at the one percent level.

We find that proxies for preparer sophistication predict claiming of the carryback refund. Preparers that are certified public accountants, that are attorneys, that are better paid, that do not work for themselves, that are older, and that have bigger client bases are more likely to claim the carryback refund for their clients. Our results indicate that preparers matter for client claiming behavior.

The professional certification categories and the client base measures have the coefficients with the largest magnitudes. Relative to preparers without a professional license, certified public accountants are 6.8 percentage points more likely to claim the carryback refund for their clients. Similarly, attorneys are 4.7 percentage points more likely to claim. The results also imply that a one standard deviation increase in log(mean client revenue) would increase take-up by 2.7 percentage points. Likewise, a one standard deviation increase in log(total client revenue) would increase take-up by 2.3 percentage points. These effects are substantial relative to a baseline take-up rate of 37 percent in the population.

We test the sensitivity of our results to varying the set of controls and to including all preparer characteristics in a multivariate regression. Table 1.7 reports these estimates. All regressions include a firm fixed effect. Columns (2) and (5) add a tax year fixed effect.

[23]We bootstrap with 250 replications.

Table 1.6: *Panel regression of carryback take-up on individual preparer characteristics*

Sample: All events where a client changed its tax preparer

Covariates	(1)	(2)	(3)	(4)	(5)	(6)
I(certified public accountant)	0.0681***					
	(0.0055)					
I(attorney)	0.0474***					
	(0.0124)					
I(other professional license)	0.0099					
	(0.0080)					
log(labor income)		0.0074***				
		(0.0014)				
I(self-employment)			-0.0205***			
			(0.0045)			
age				0.0003		
				(0.0002)		
log(mean client revenue)					0.0179***	
					(0.0017)	
log(total client revenue)						0.0125***
						(0.0011)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	124,862	124,862	124,862	124,862	124,862	124,862

Notes: This table reports coefficients from a regression of carryback take-up on preparer characteristics. The standard errors are block bootstrapped with 250 replications and are reported in parentheses. All regressions include a firm fixed effect, a tax year fixed effect, and firm controls. Firm controls include log(eligible refund), log(revenue), log(assets), and log(EBITDA). The sample only includes the last observation before a client changes its preparer and the first observation after a client changes its preparer. The "other professional license" category represents enrolled agents and state licensed preparers. Preparers that do not have a professional license are the omitted certification category. Please see Section 1.4.2 for further details.

Table 1.7: *Panel regression of carryback take-up on multiple preparer characteristics*

Sample: All events where a client changed its tax preparer

Covariates	(1)	(2)	(3)	(4)	(5)	(6)
I(certified public accountant)	0.0718***	0.0725***	0.0681***	0.0605***	0.0611***	0.0590***
	(0.0059)	(0.0065)	(0.0058)	(0.0061)	(0.0063)	(0.0062)
I(attorney)	0.0510***	0.0496***	0.0474***	0.0404***	0.0385***	0.0387***
	(0.0141)	(0.0130)	(0.0129)	(0.0140)	(0.0139)	(0.0130)
I(other professional license)	0.0049	0.0044	0.0099	0.0043	0.0043	0.0092
	(0.0089)	(0.0086)	(0.0081)	(0.0089)	(0.0087)	(0.0087)
log(labor income)				0.0049***	0.0047***	0.0042***
				(0.0016)	(0.0016)	(0.0015)
I(self-employment)				-0.0142***	-0.0140***	-0.0158***
				(0.0049)	(0.0047)	(0.0046)
age				0.0006***	0.0007***	0.0006***
				(0.0002)	(0.0001)	(0.0001)
log(mean client revenue)				0.0122***	0.0136***	0.0091***
				(0.0024)	(0.0026)	(0.0024)
log(total client revenue)				0.0066***	0.0063***	0.0057***
				(0.0015)	(0.0017)	(0.0016)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	No	Yes	Yes	No	Yes	Yes
Firm controls	No	No	Yes	No	No	Yes
Number of observations	124,862	124,862	124,862	124,862	124,862	124,862

Notes: This table reports coefficients from a regression of carryback take-up on preparer characteristics. The standard errors are block bootstrapped with 250 replications and are reported in parentheses. All regressions include a firm fixed effect. Columns (2) and (5) add a tax year fixed effect. Columns (3) and (6) add firm controls, which include log(eligible refund), log(revenue), log(assets), and log(EBITDA). The sample only includes the last observation before a client changes its preparer and the first observation after a client changes its preparer. The "other professional license" category represents enrolled agents and state licensed preparers. Preparers that do not have a professional license are the omitted certification category. Please see Section 1.4.2 for further details.

Columns (3) and (6) add firm controls. All regressions also include dummies for missing values of the preparer characteristics and the client controls. The first three columns limit the preparer characteristics to the professional license categories. The last three columns include all preparer characteristics. We block bootstrap the standard errors by firm[24] and report them in parentheses.

The point estimates are not sensitive to the specification tests in Table 1.7. The magnitudes slightly decrease with the expansion of the set of controls. The coefficients have a similar

[24]We bootstrap with 250 replications.

response to including all preparer characteristics in a multivariate regression. But in only a few cases do the specification tests generate statistically distinguishable point estimates from the previous results. All coefficients retain the same sign as before.

Balanced Event Study Specification. A common validation for an event study design plots trends before and after the event in a balanced panel. This placebo test evaluates whether there appears to be an effect in periods when there is no treatment. If present, it would suggest a failure of the strict exogeneity assumption that requires unobservables to be uncorrelated with the treatment. To implement this test, we focus on a subsample of events where we have four observations per firm: two observations before changing preparers and two observations after changing preparers.

Within each firm, we order the observations by tax year and define them relative to the first observation after the firm changes preparers. We call this order event time e, where $e \in \{-2, -1, 0, 1\}$ and each firm has four observations. We restrict ourselves to a balanced panel because changes in the sample over time can introduce the appearance of trends.

We construct a measure of the treatment effect associated with each event from our estimates of Equation 1.2.

$$\Delta\widehat{\mu}_{\mathbf{J}(i,0)} = Z_{\mathbf{J}(i,0)}\widehat{\gamma} - Z_{\mathbf{J}(i,-1)}\widehat{\gamma} \tag{1.3}$$

We obtain the estimated coefficients $\widehat{\gamma}$ from Column (6) of Table 1.7. We then estimate a variant of our original panel regression where we allow the coefficient θ_e on the treatment effect $\Delta\widehat{\mu}_{\mathbf{J}(i,0)}$ to vary with event time.

$$\text{I(carryback take-up)}_{ijt} = \Delta\widehat{\mu}_{\mathbf{J}(i,0)}\theta_e + X_{it}\beta + \alpha_i + \delta_t + \zeta_e + \nu_{it} \tag{1.4}$$

The regression equation above also includes client characteristics X_{it}, a client fixed effect α_i, a tax year fixed effect δ_t, and an event time fixed effect ζ_e.

Estimating Equation 1.4 tests for pre-trends and post-trends that are correlated with the treatment effect $\Delta\widehat{\mu}_{\mathbf{J}(i,0)}$. Because we omit a dummy for the event time $e = -2$ to avoid collinearity, the coefficients θ_e are estimated relative to the coefficient at event time $e = -2$.

By construction, $\theta_{-2} = 0$. We expect to find that $\theta_{-1} = 0$ because the clients have not yet changed preparers. We expect to find that $\theta_0 = 1$ because the client has changed preparers and take-up should reflect the change in the predicted preparer effect. This relationship should be one-for-one because the predicted preparer effect reflects the relationship between client take-up and preparer characteristics. And, we also expect to find $\theta_1 = 1$ because most clients are still with the same preparer at event time $e = 1$.

There is also a mechanical component to some of our results for Equation 1.4 because we estimate the treatment effect $\Delta\widehat{\mu}_{\mathbf{J}(i,0)}$ from the switchers design. The difference $\theta_0 - \theta_{-1}$ should equal one by construction because the switchers design uses observations from event time $e = -1$ and $e = 0$. The estimated difference may not equal one exactly because the balanced event study panel uses a subset of the firms in the switchers design. However, the estimates for the coefficients θ_{-1} and θ_1 are still informative about pre-trends and post-trends because the switchers design excludes observations from event time $e = -2$ and event time $e = 1$.

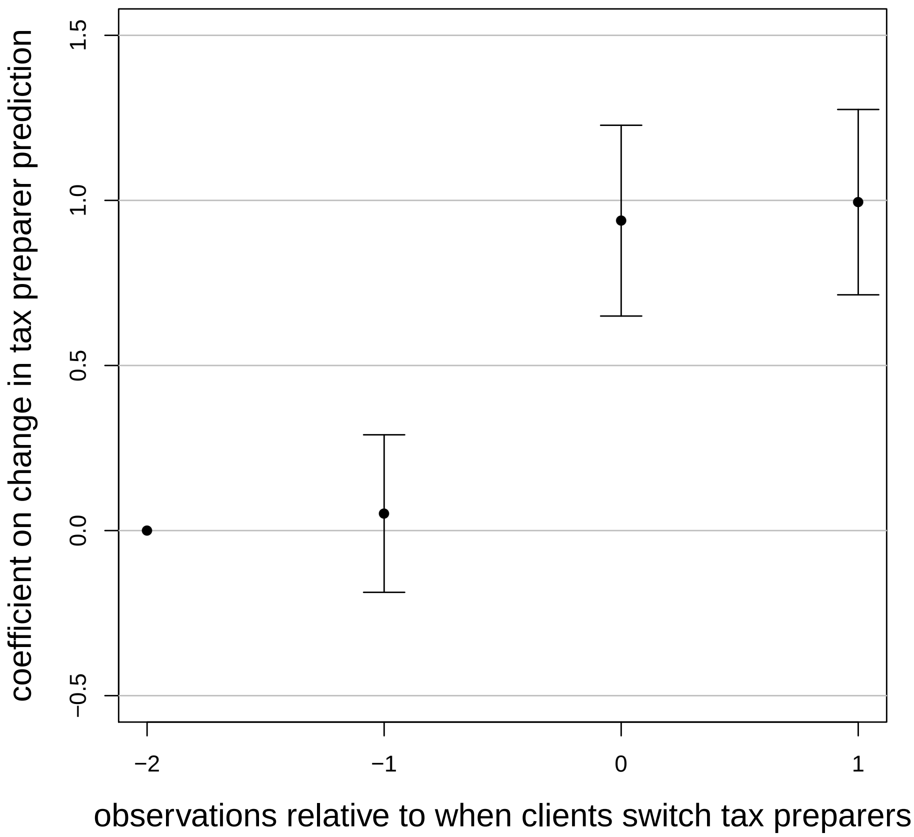
We plot our estimates of the coefficients θ_e in Figure 1.6. The regression includes dummies for missing values of the preparer characteristics and the client controls. We block bootstrap the standard errors by firm.[25] As stated earlier, the coefficient θ_{-2} equals zero by construction because we omit a dummy for event time $e = -2$ from the regression.

We cannot reject the null of zero for the coefficient θ_{-1}, but we can reject it for the coefficients θ_0 and θ_1. We find a point estimate close to zero for θ_{-1} and point estimates close to one for θ_0 and θ_1. Our results confirm the absence of both pre-trends and post-trends that are correlated with the treatment effect. We do not find a placebo effect of the treatment.

Preparer Deaths and Relocations. Our estimates of Equation 1.2 rely on the identifying assumption that unobservable determinants of client take-up remain unchanged before and after switching preparers. But, clients may change preparers in response to a change in their firm. For example, a client may hire a new preparer when it hires a new manager. The

[25] We bootstrap with 1,000 replications.

Figure 1.6: *Panel regression estimates of carryback take-up on predicted preparer effect*



Notes: This figure plots the coefficients from a regression of carryback take-up on interactions between event time and the change in the predicted preparer effect at event time $e = 0$. Standard errors are block bootstrapped by firm with 1,000 replications. We construct the predicted preparer effects using the estimated coefficients from Column (6) of Table 1.7. The change in the predicted preparer effect at event time $e = 0$ equals $\Delta\widehat{\mu}_{\mathbf{J}(i,0)} = Z_{\mathbf{J}(i,0)}\widehat{\gamma} - Z_{\mathbf{J}(i,-1)}\widehat{\gamma}$. The regression includes a firm fixed effect, a tax year fixed effect, firm controls, and an event time fixed effect. Firm controls include log(eligible refund), log(revenue), log(assets), and log(EBITDA). The regression also includes dummies for missing values in firm controls. The regression omits a dummy for event time $e = -2$ to avoid collinearity. The plotted coefficients are estimated relative to event time $e = -2$. The coefficient at event time $e = -2$ equals zero by construction. Please see Section 1.4.2 for further details.

change in client unobservables that cause the firm to switch preparers could also affect its claiming behavior. Here, we focus on a subsample of events where the prior preparer either dies or relocates to a new zip code at least 75 miles away. In these cases, we find it more plausible that client unobservables remain unchanged around the switching event.

We identify deaths and relocations by linking preparers to a social security file and to their individual income tax returns. We compute the distance between personal residence addresses based on the centroids of their reported zip codes. We then identify firms that change preparers contemporaneously with either the death or relocation of the prior preparer.

We re-estimate Equation 1.2 on this subset of events. We report the results in Table 1.8. We estimate regressions separately for each preparer characteristic, and we also include the predicted preparer effect based on Column (6) of Table 1.7 as an additional covariate. All regressions include a client fixed effect, a tax year fixed effect, and firm controls. They also include dummies for missing values of the preparer characteristics and the client controls. The standard errors are block bootstrapped by firm[26] and reported in parentheses.

The estimates are broadly similar to our earlier results. With the exception of the covariates I(other professional license) and I(self-employment), we find coefficients close to our earlier point estimates. We have less statistical power to detect effects, but we still find strongly significant results for I(certified public accountant), log(mean client revenue), and log(total client revenue). And, we estimate a strongly significant coefficient of 0.9372 on the predicted preparer effect. This last result implies that the switchers design estimates an unbiased preparer effect. Together, our estimates indicate that changes in client unobservables do not confound the original results from the switchers design.

We focus on deaths and relocations because we believe it is more likely that client unobservables remain unchanged before and after the switching event. But, selection could still arise in this subsample from the hiring of new preparers. Our results could be confounded if the same client unobservables that determine preparer hiring also determine

[26] We bootstrap with 1,000 replications.

Table 1.8: *Panel regression of carryback take-up on individual preparer characteristics*

Sample: Events contemporaneous with preparer deaths and relocations

Covariates	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I(certified public accountant)	0.0857***						
	(0.0219)						
I(attorney)	0.0728						
	(0.0526)						
I(other professional license)	0.0551*						
	(0.0319)						
log(labor income)		0.0073					
		(0.0050)					
I(self-employment)			0.0095				
			(0.0151)				
age				0.0007			
				(0.0005)			
log(mean client revenue)					0.0162**		
					(0.0068)		
log(total client revenue)						0.0144***	
						(0.0041)	
predicted preparer effect							0.9372***
							(0.1880)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	9,824	9,824	9,824	9,824	9,824	9,824	9,824

Notes: This table reports coefficients from a regression of carryback take-up on preparer characteristics. The standard errors are block bootstrapped with 1,000 replications and are reported in parentheses. The sample is limited to switching events contemporaneous with either the death or relocation of the prior preparer. It also only includes the last observation before a client changes its preparer and the first observation after a client changes its preparer. Relocations are defined based on moving personal residences to a new zip code at least 75 miles away. The "other professional license" category represents enrolled agents and state licensed preparers. Preparers that do not have a professional license are the omitted certification category. The predicted preparer effect is constructed using the estimated coefficients from Column (6) of Table 1.7. Please see Section 1.4.2 for further details.

take-up of the carryback refund.

We address this additional concern with a two-stage least squares estimate with the deaths and relocations subsample. Intuitively, we instrument for the change in the preparer effect with the prior preparer characteristic because we think that the change in client unobservables is unrelated to the prior preparer. To clarify the interpretation of our identifying assumption, we express our estimates for this design in a first-differences version of Equation 1.2.[27] We also index our notation by event time e.[28]

$$\Delta \mathrm{I}(\text{carryback take-up})_{ije} = \Delta Z_{\mathbf{J}(i,e)}\gamma + \Delta X_{ie}\beta + \Delta\delta_{\mathbf{T}(i,e)} + \Delta\epsilon_{ie} \tag{1.5}$$

The difference in the equation above is between the first observation after the switching event and the last observation before the event. These observations are not always consecutive because firms are not eligible for the carryback refund in every year. We use the following instrument for the change in the preparer characteristic $\Delta Z_{\mathbf{J}(i,e)}$.

$$\Delta\widetilde{Z}_{\mathbf{J}(i,e)} = \bar{Z} - Z_{\mathbf{J}(i,e-1)} \tag{1.6}$$

Our instrument $\Delta\widetilde{Z}_{\mathbf{J}(i,e)}$ equals the difference between the sample mean $\bar{Z}$ and the preparer characteristic $Z_{\mathbf{J}(i,e-1)}$ from the pre-event period.

The two-stage least squares estimates identify the causal treatment effect of preparer covariates under the following assumption.

> **Assumption 2 [Deaths and Relocations Instrument]**: The instrument $\Delta\widetilde{Z}_{\mathbf{J}(i,e)}$ must satisfy the exclusion restriction $\mathrm{E}[\Delta\epsilon_{ie}|\Delta\widetilde{Z}_{\mathbf{J}(i,e)}, \Delta X_{ie}, \Delta\delta_{\mathbf{T}(i,e)}] = 0$.

The condition above implies that the change in client unobservables is uncorrelated with the characteristics of the prior preparer (as represented by the instrument $\Delta\widetilde{Z}_{\mathbf{J}(i,e)}$), the change in client observables, and the tax year fixed effects. We find it plausible that our setting satisfies this assumption because we do not believe that changes in client unobservables

[27]The estimates from a fixed effects specification and a first-differences specification are numerically equivalent when the panel has two observations per firm.

[28]The function $\mathbf{T}(i,e)$ maps firm i at event time e to tax year t.

determine preparer deaths and relocations.

We report results from the instrument design for the predicted preparer effect.[29] We do not include results for the individual preparer covariates because we lack the statistical power to estimate informative coefficients for them. Our subsample only includes 9,824 observations, 8 percent of the sample with all switching events. We have more statistical power with the predicted preparer effect because it combines the individual covariates into one summary statistic.

Table 1.9 reports our two-stage least squares estimates. Each column represents a different regression. All regressions include a client fixed effect. Column (2) adds a tax year fixed effect. Column (3) adds firm controls. We include dummies for missing values of the client controls. The standard errors are block bootstrapped by firm[30] and reported in parentheses.

We expect to find a coefficient of one on the predicted preparer effect. As stated earlier, the predicted preparer effects captures the estimated relationship between the preparer covariates and carryback take-up. Our estimates will not mechanically equal one, however, because they are based on a different research design.

We find point estimates of 1.1780, 0.9950, and 0.7532 in columns (1), (2), and (3). The confidence intervals are relatively large, but in all specifications we can reject the null of a zero coefficient at a 5 percent level. Our estimates are not statistically distinguishable from a coefficient of one. These results validate the predicted preparer effect estimated under the switchers design.

1.4.3 Variance of Unobserved Preparer Effect

We have shown that claiming behavior depends on observable preparer characteristics, but these results most likely understate the impact of preparers on take-up. Preparer unobservables could have large impacts on client claiming decisions. For example, preparers

[29] We obtain the estimated coefficients $\widehat{\gamma}$ from Column (6) of Table 1.7.

[30] We bootstrap with 1,000 replications.

Table 1.9: *Two-stage least squares estimates of carryback take-up on predicted preparer effect*

Sample: Events contemporaneous with preparer deaths and relocations

Covariates	(1)	(2)	(3)
predicted preparer effect	1.1780***	0.9950***	0.7532**
	(0.3839)	(0.3774)	(0.3728)
Firm FE	Yes	Yes	Yes
Year FE	No	Yes	Yes
Firm controls	No	No	Yes
Number of observations	9,824	9,824	9,824

Notes: This table reports coefficients from a two-stage least squares regression of carryback take-up on preparer characteristics. The standard errors are block bootstrapped with 1,000 replications and are reported in parentheses. The predicted preparer effect is constructed using the estimated coefficients from Column (6) of Table 1.7. The instrument equals the preparer covariate in the pre-event period and the sample mean for the preparer covariate in the post-event period. All regressions include a firm fixed effect. Column (2) adds a tax year fixed effect. Column (3) adds firm controls, which include log(eligible refund), log(revenue), log(assets), and log(EBITDA). The sample is limited to switching events contemporaneous with either the death or relocation of the prior preparer. It also only includes the last observation before a client changes its preparer and the first observation after a client changes its preparer. Relocations are defined based on moving personal residences to a new zip code at least 75 miles away. Please see Section 1.4.2 for further details.

could differ in their familiarity with the tax loss rules or in their beliefs about the audit risk associated with carryback refunds. For either example, we do not have direct measures of the unobserved heterogeneity. In this section, we estimate the variance of an unobserved preparer effect and benchmark our results against the variance of predicted take-up from client observables.

We use an analysis of variance approach to find the standard deviation of the unobserved preparer effect. We isolate the preparer variance from the within-firm covariances for pairs of observations that do and do not share the same preparer. In the former case, we think that the covariance partly reflects the preparer variance. In the latter, we think that it does not. The difference between these two covariances estimates the preparer variance.

We use the sample of C corporations eligible for a carryback refund of at least $1,000 between 1998 and 2011. We define take-up as a function of firm characteristics and a preparer fixed effect in Equation 1.7.

$$\text{I(carryback take-up)}_{ijt} = W_{it}\pi + \mu_{\mathbf{J}(i,t)} + \eta_{it} \tag{1.7}$$

In the equation above, firm characteristics W_{it} include deciles in the eligible carryback refund, deciles in revenue, deciles in assets, deciles in payroll, state-year fixed effects, and industry-year fixed effects. We then compute residual take-up with respect to firm observables.

$$T_{ijt} = \text{I(carryback take-up)}_{ijt} - W_{it}\pi = \mu_{\mathbf{J}(i,t)} + \eta_{it} \tag{1.8}$$

We construct the residuals by estimating Equation 1.7.

We next estimate the within-firm covariance structure of the residual T_{ijt}. We make the following assumptions of independence and stationarity to arrive at our estimate for the preparer variance.

> **Assumption 3 [Independence]**: The unobserved preparer effect $\mu_{\mathbf{J}(i,t)}$ and the error term η_{it} are independent.
>
> **Assumption 4 [Stationarity]**: The covariance between any two error terms, $\text{Cov}\left(\epsilon_{it}, \epsilon_{is}\right)$, equals $\sigma^2_{\epsilon,|t-s|}$.

We use these assumptions to isolate the preparer variance from the within-firm covariance structure by differentiating between pairs of observations that do and do not share the same preparer.

$$\begin{aligned} \sigma^{j=k}_{|t-s|} &= \text{Cov}\left(T_{ijt}, T_{iks}\right) &= \sigma^2_{\mu} + \sigma^2_{\epsilon,|t-s|} \\ \sigma^{j\neq k}_{|t-s|} &= \text{Cov}\left(T_{ijt}, T_{iks}\right) &= \sigma^2_{\epsilon,|t-s|} \end{aligned} \tag{1.9}$$

We find the preparer variance by taking the difference between the two equations above.

We make a strong assumption with the independence condition. It may not hold because sophisticated clients likely hire sophisticated preparers. But if the firm error term and the preparer effect are positively correlated as we suspect, our estimate for the preparer variance provides an upper bound for the true variance. In this sense, our estimate is useful as a metric for the potential magnitude of the unobserved preparer effect.

Figure 1.7 plots our estimates of the within-firm covariances separately for observations that do and do not share the same preparer. Figure 1.8 plots our estimate for the standard deviation of the preparer effect. We also report the point estimates and standard errors in Table 1.10. We block bootstrap the standard errors by firm.[31]

We find drift in both series of covariance estimates in Figure 1.7. The covariances decline as the lapsed time between observations increases. This pattern could arise if client unobservables, such as management practices, slowly change over time. Our estimates for the preparer variance comes from the difference between the two series in Figure 1.7. Despite their overall decline, the difference remains relatively stable over time.

The standard deviation of the preparer effect in Figure 1.8 equals the square root of the difference between the within-firm covariances. The point estimates in the series range between 0.1303 and 0.1659. Most are statistically indistinguishable. Their weighted average equals 0.1472.[32] This magnitude is larger than any of our estimated effects of the individual preparer covariates.

We benchmark our result by comparing it to the predicted effect from firm observables $W_{it}\widehat{\pi}$. We find similar variance estimates for the unobserved preparer effect and the prediction from firm observables. Both estimates equal 9 percent of the variance in take-up of the carryback refunds.

Alternatively, we can also interpret our result by calculating the implied change in take-up if a client moved from the 10th percentile to the 90th percentile in preparer effects. Assuming that the preparer effects follow a normal distribution, take-up would increase by 38 percentage points. This change would be larger than the baseline take-up rate of 37 percent in the population. These estimates imply that preparers potentially play a substantial role in determining whether their clients claim the carryback refund.

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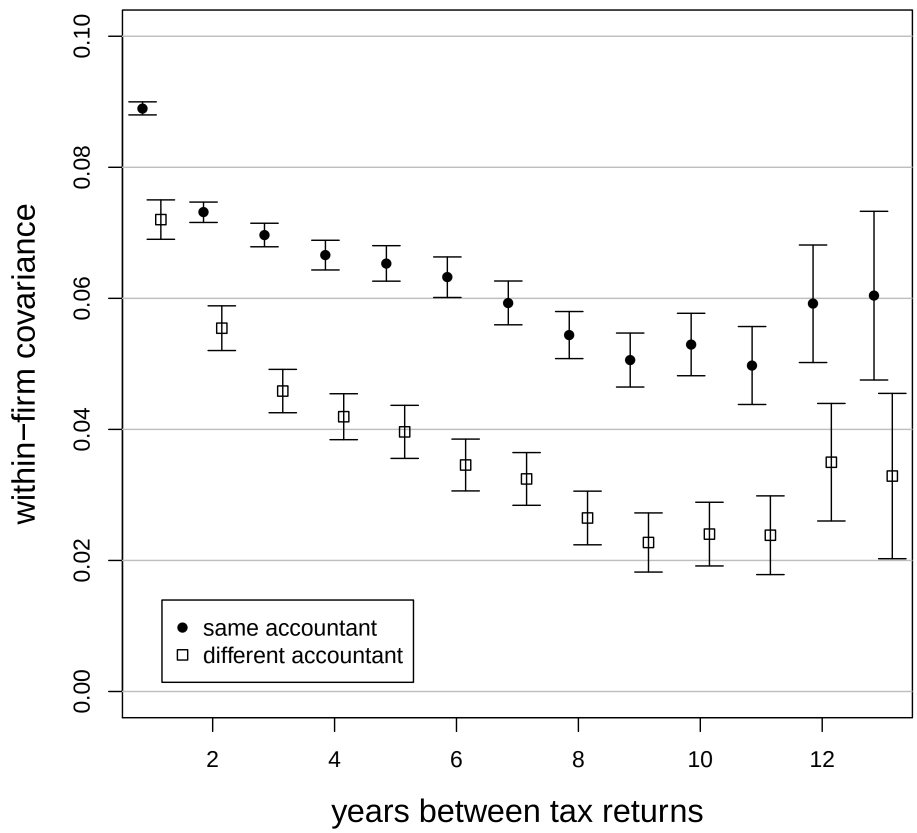
[31] We bootstrap with 1,000 replications.

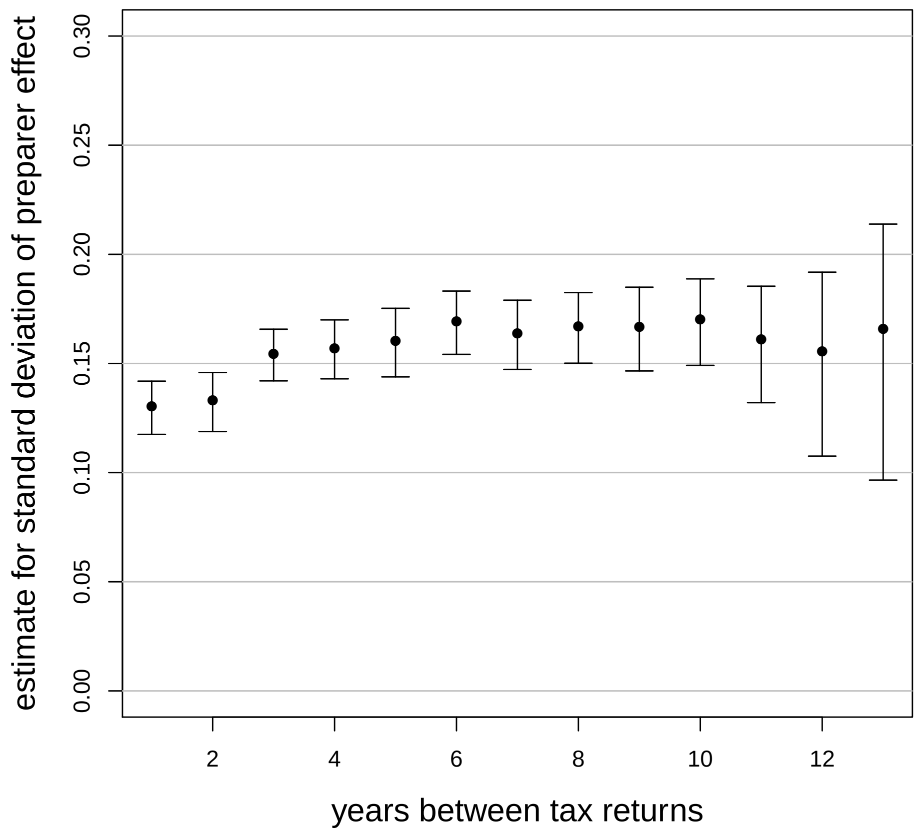
[32] The weights equal the number of pairs of observations at each time interval, as defined by the length of time between observations.

Figure 1.7: *Estimates of within-firm covariances of residual carryback take-up*



Notes: This figure reports the within-firm covariances of residual carryback take-up. Standard errors are block bootstrapped with 1,000 replications. The estimates are based on the residual $T_{ijt} = I(\text{carryback take-up}) - W_{it}\pi$ where W_{it} are client observables. The coefficients π are estimated from a regression of take-up on client observables and a preparer fixed effect. Client observables include deciles in the eligible carryback refund, deciles in revenue, deciles in assets, deciles in payroll, state-year fixed effects, and industry-year fixed effects. We estimate covariances for pairs of observations from the same firm. We differentiate between pairs by the length of time between observations and by whether the observations share the same preparer. Please see Section 1.4.3 for further details.

Figure 1.8: *Estimates for standard deviation of preparer effects*



Notes: This figure plots estimates of the standard deviation of the preparer effect. Standard errors are block bootstrapped with 1,000 replications. We estimate the variance of the preparer effect by taking the difference of the within-firm covariances for pairs of observations that do and do not share the same preparer. (Please see Figure 1.7.) We then take the square root of the difference to estimate the standard deviation. Please see Section 1.4.3 for further details.

Table 1.10: *Estimates of within-firm covariances*

Years between tax returns	Same preparer		Different preparers		Estimate of preparer variance		Estimate of preparer std. dev.
1	0.0890	(0.0005)	0.0720	(0.0015)	0.0170	(0.0016)	0.1303
2	0.0731	(0.0008)	0.0554	(0.0017)	0.0177	(0.0018)	0.1330
3	0.0697	(0.0009)	0.0459	(0.0017)	0.0238	(0.0019)	0.1543
4	0.0666	(0.0012)	0.0419	(0.0018)	0.0247	(0.0022)	0.1570
5	0.0653	(0.0014)	0.0396	(0.0021)	0.0257	(0.0026)	0.1603
6	0.0632	(0.0016)	0.0346	(0.0020)	0.0287	(0.0025)	0.1693
7	0.0593	(0.0017)	0.0324	(0.0021)	0.0269	(0.0026)	0.1639
8	0.0544	(0.0018)	0.0265	(0.0021)	0.0279	(0.0027)	0.1671
9	0.0506	(0.0021)	0.0227	(0.0023)	0.0278	(0.0032)	0.1669
10	0.0530	(0.0024)	0.0240	(0.0025)	0.0289	(0.0034)	0.1701
11	0.0498	(0.0030)	0.0238	(0.0031)	0.0259	(0.0043)	0.1609
12	0.0592	(0.0046)	0.0350	(0.0046)	0.0242	(0.0064)	0.1555
13	0.0604	(0.0066)	0.0329	(0.0064)	0.0275	(0.0093)	0.1659
weighted average					0.0217		0.1472

Notes: This table reports the estimates from an analysis of variance of carryback take-up. Standard errors are block bootstrapped with 1,000 replications and are reported in parentheses. The estimates are based on the residual T_{ijt} = I(carryback take-up) $-$ $W_{it}\pi$ where W_{it} are client observables. The coefficients π are estimated from a regression of take-up on client observables and a preparer fixed effect. Client observables include deciles in the eligible carryback refund, deciles in revenue, deciles in assets, deciles in payroll, state-year fixed effects, and industry-year fixed effects. We estimate covariances for pairs of observations from the same firm. We differentiate between pairs by the length of time between observations and by whether the observations share the same preparer. We estimate the preparer variance as the difference between covariances for observations that do and do not share the same preparer. We estimate the preparer standard deviation as the square root of the preparer variance. We compute the weighted average based on the number of pairs per covariance estimate. Please see Section 1.4.3 for further details.

1.5 Conclusion

Our study highlights the mediating role that preparers play between the tax code and taxpayers by showing that preparers influence tax claiming decisions. We use firms that change preparers to demonstrate that preparer characteristics predict take-up of a corporate tax refund for losses. We validate our results by focusing on a subsample of events where the prior preparer either dies or relocates to a new zip code at least 75 miles away. In these cases, we find it more plausible that client unobservables remain unchanged before and after the switching event. We also estimate the variance of an unobserved preparer term and find that it potentially explains as much of the variation in take-up as the prediction from firm observables.

The results suggest that investing in better take-up of tax benefits could be as important as changing the tax code itself. Take-up is important for policy design because it directly impacts the effectiveness of tax benefits. Fiscal stimulus measures in particular often rely on the introduction of new and temporary tax benefits. For example, the American Reinvestment and Recovery Act of 2009 distributed 36 percent of its stimulus dollars through 55 different tax benefits (The Recovery Accountability and Transparency Board 2014). Future research should consider whether targeting preparers with informational materials or training programs can improve the take-up of tax benefits.

Chapter 2

Do Financial Frictions Amplify Fiscal Policy? Evidence from Business Investment Stimulus[1]

2.1 Introduction

Going back to Hall and Jorgenson (1967), economists have asked how taxes affect investment. The answer is central to the design of countercyclical fiscal policy, since policymakers often use tax-based investment incentives to spur growth in times of economic weakness. Such policies often coincide with disruptions in capital markets, so it is natural to ask how taxes affect investment in the presence of financial frictions. However, the standard theoretical and empirical treatments assume perfect capital markets.[2] This paper uses recent episodes of investment stimulus to study whether the effect of taxes on investment accords with

[1] Co-authored with Eric Zwick

[2] Key theoretical studies include Hall and Jorgenson (1967), Tobin (1969), Hayashi (1982), Abel and Eberly (1994), and Caballero and Engel (1999). Abel (1990) presents a unifying synthesis of the early theoretical literature. Key empirical work includes Summers (1981), Auerbach and Hassett (1992), Cummins, Hassett and Hubbard (1994), Goolsbee (1998), Chirinko, Fazzari and Meyer (1999), Desai and Goolsbee (2004), Cooper and Haltiwanger (2006), and House and Shapiro (2008). Edgerton (2010) relaxes the frictionless assumption but, in contrast to our study, finds mixed results.

the standard, frictionless model. We find that, by ignoring financial frictions, the standard analysis overlooks a crucial driver of firm responses to tax policy.

The policy we study, "bonus" depreciation, accelerates the schedule for when firms can deduct from taxable income the cost of investment purchases. Bonus alters the timing of deductions but not their amount, so the economic incentive created by bonus works because future deductions are worth less than current deductions. That is, bonus works because of discounting: firms judge the benefits of bonus by the present discounted value of deductions over time.[3] Speeding up the timing of deductions reduces short term taxes, but at the expense of higher taxes in the future. With a reasonable risk-adjusted discount rate, bonus depreciation generates a modest subsidy, so the frictionless model predicts a small effect of bonus on investment. But in the presence of financial frictions, firms sharply discount future deductions. Thus financial frictions make bonus more appealing, since the difference in today's tax benefits dwarfs the present value comparison that matters in theory.

We study two episodes of bonus depreciation using a difference-in-differences methodology to estimate the effect of this policy. We present three empirical findings. First, bonus depreciation has a substantial effect on investment, much larger than past estimates and much stronger than the conventional wisdom predicts. Estimates of how tax changes affect investment vary, but the consensus prediction is that bonus depreciation has a small positive effect.[4] In contrast, we find that bonus depreciation raised eligible investment by 17.3 percent on average between 2001 and 2004 and 29.5 percent between 2008 and 2010. We estimate a user cost elasticity of approximately -1.6, outside the range of estimates of -0.5 to -1 surveyed by Hassett and Hubbard (2002) and more than double the consensus point

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[3]Summers (1987) states this most clearly: "It is *only* because of discounting that depreciation schedules affect investment decisions. . . "

[4]Cummins, Hassett and Hubbard (1994) study many corporate tax reforms and public company investment data and conclude that tax policy has a strong effect on investment. Using similar data and a different empirical methodology, Chirinko, Fazzari and Meyer (1999) argue that tax policy has a small effect on investment and that Cummins, Hassett and Hubbard (1994) misinterpret their results. Hassett and Hubbard (2002) survey empirical work and conclude that the range of estimates for the user cost elasticity has narrowed to between -0.5 and -1. Surveying this and more recent work, Bond and Van Reenen (2007) decide "it is perhaps a little too early to agree with Hassett and Hubbard (2002) that there is a new 'consensus' on the size and robustness of this effect."

estimate.[5]

The first part of the paper details this finding and a litany of robustness tests. The research design compares firms at the same point in time whose benefits from bonus differ. Our strategy exploits technological differences between firms in narrowly defined industries. Firms in industries with most of their investment in short duration categories act as the "control group" because bonus only modestly alters their depreciation schedule. This natural experiment separates the effect of bonus from other economic shocks happening at the same time. If the parallel trends assumption holds—if investment growth for short and long duration industries would have been similar absent the policy—then the experimental design is valid.

The key threat to this design is that time-varying industry shocks may coincide with bonus. This risk is limited for four reasons. First, graphical inspection of parallel trends indicates smooth pretrends and a clear, steady break for short and long duration firms during both the 2001 to 2004 and 2008 to 2010 bonus periods. The effects are the same size in both periods, though different industries suffered in each recession. Second, the estimates are stable across many specifications and after including firm-level cash flow controls, industry Q, and flexible industry trends. Controlling for industry-level co-movement with the macroeconomy actually increases our estimates. Third, the estimates pass a placebo test: the effect of bonus on ineligible investment is indistinguishable from zero. Last, for firms making eligible investments, bonus take-up rates (i.e., do firms fill in the bonus box on the tax form?) are indeed higher in long duration industries. For these reasons, spurious factors are unlikely to explain the large effect of bonus.

Firms respond to bonus depreciation as if they apply implausibly high discount rates to investment decisions. This finding is inconsistent with a frictionless model of firm behavior. In the second part of the paper, we explore alternative models that generate high effective discount rates by adding financial frictions.[6] One alternative is costly external finance,

[5] In Appendix Section B.3, we collect estimates from past studies of tax reforms. The average user cost elasticity across these studies is -0.69.

[6] We use the term *financial frictions* as an umbrella term over a class of models that generate high effective

which raises the total discount rate firms apply to evaluate projects. Another alternative is managerial myopia, which raises effective discount rates by sharply discounting the future relative to the present. Both models prove useful in explaining our findings.

Our second empirical finding is that, consistent with the costly external finance story, financial constraints amplify the effects of investment stimulus. Nearly all prior empirical tests of financial constraints use public firm data, which is problematic because public firms have the best collateral, the strongest banking relationships and broad access to equity and bond markets.[7] In contrast, we work with an analysis sample of more than 120,000 public and private companies drawn from two million corporate tax returns. Half the firms in our sample are smaller than the smallest firms in Compustat.[8] Our baseline estimate therefore averages over substantial heterogeneity in firm type, including many firms likely to face financial constraints.

The largest firms in our sample, those most like the firms in past studies, yield estimates in line with the Hassett and Hubbard (2002) range. In contrast, small and medium-sized firms, previously unstudied, show much stronger responses. Building on the differential response by firm size, we perform a split sample analysis using several markers of ex ante financial constraints. In addition to small firms, non-dividend payers and firms with low cash holdings are 1.5 to 2.6 times more responsive than their unconstrained counterparts. Moreover, we find that firms respond by borrowing and cutting dividends. These facts do not match the frictionless model of investment behavior, in which firms divided by financial constraint markers do not respond differently to bonus.

Firms with tax losses must wait to realize the benefits of tax breaks. Because many firms in our sample are in a tax loss position when a policy shock occurs, we can ask whether firms

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discount rates. Some of these—such as managerial myopia and agency theory—are not about external finance per se, but refer instead to organizational frictions.

[7] Kaplan and Zingales (1997) find that very few of Fazzari, Hubbard and Petersen's (1988*a*) most constrained firms appear constrained by other measures.

[8] When aggregated, these small firms account for a large amount of economic activity. According to Census tabulations in 2007 (`http://www.census.gov/econ/susb/data/susb2007.html`), firms with less than $100 million in receipts (around the 80th percentile in our data) account for more than half of total employment and one third of total receipts.

value *future* cash windfalls, namely, the larger deductions bonus depreciation provides them in later years. Our third empirical finding is that, consistent with the managerial myopia story, firms *only* respond to investment incentives when the policy immediately generates cash flows. This finding holds even though firms can carry forward unused deductions to offset future taxes, and it cannot be explained by differences in growth opportunities. Furthermore, this fact contradicts a simple model of costly external finance, because firms neglect how the policy affects borrowing in the future.

To confirm the myopia story, we study a second component of the depreciation schedule. Firms making small investment outlays face a permanent kink in the tax schedule, which creates a discontinuous change in marginal investment incentives. This sharp change in incentives induces substantial investment bunching, with many firms electing amounts within just a few hundred dollars of the kink. And when legislation raises the kink, the bunching pattern follows. Consistent with myopia, bunching strongly depends on a firm's current tax status: firms just in positive tax position are far more likely to bunch than firms on the other side of the discontinuity. For a different group of firms and a different depreciation policy, we again find that firms ignore future tax benefits.

These facts do not match the predictions of a frictionless model, which cannot account for the large baseline response, the differential response for constrained firms or the nonresponse for nontaxable firms. The facts point instead toward models in which costly finance matters and current benefits outweigh future benefits. We use an investment model to clarify these findings. The model incorporates costly external finance and managerial myopia into a general model in which the frictionless model of Hayashi (1982) is a special case. These alternative theories make predictions about the discount rate firms apply to future cash flows. The model shows how to combine reduced form estimates to distinguish the frictionless benchmark from costly external finance and managerial myopia.

The general model yields a set of theoretical moments—one comparing constrained and unconstrained firms and one comparing taxable and nontaxable firms—which we can combine with our empirical findings to measure financial frictions. With these comparisons we can compute the shadow cost of external funds and an implied present versus future discount factor. We estimate the shadow cost of external funds to be between \$0.63 and \$1.61 per dollar and an implied discount factor of 0.82. Combining these results, financially constrained firms act as if \$1 next year is worth just 38 cents today, yielding a total discount rate of 97 percent. Thus accounting for the effect of bonus depreciation on investment requires a major role for financial frictions.

Our paper sits at the intersection of several strands in the economics and finance literatures. Most directly, the paper relates to studies of the effect of taxes on business investment. Our data improve on past studies by including two periods of bonus depreciation; a granular breakdown of eligible investment; a large sample of small, private firms; and better tax variables. Earlier studies pool the effects of different tax reforms, which include depreciation changes, tax rate changes and rule changes regarding corporate form. We focus on one specific policy, bonus depreciation, and carefully dissect how firms respond.[9] In the literature on salience and taxation, our study offers an example of a strong tax policy effect on economic behavior.[10]

Cummins, Hassett and Hubbard (1995) and Edgerton (2010) note that tax losses will reduce the incentive of firms to respond to tax changes. The former study uses a sample of sixty loss firms to conclude that losses reduce the effect of tax breaks on investment. The latter maps financial accounting data to a tax account and finds mixed evidence that losses matter.[11] With our data, we can precisely measure whether a firm's current tax position means that the next dollar of investment affects this year's tax bill. Our sample of loss firms includes almost two hundred thousand loss year observations.

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[9]House and Shapiro (2008) study the first episode of bonus depreciation using aggregate investment data.

[10]Our evidence is consistent with the strong behavioral response to and salience of the Earned Income Tax Credit (Chetty, Friedman and Saez, 2013). It stands, for instance, in contrast to evidence that individuals react incompletely to obscure taxes (Chetty, Looney and Kroft, 2009) and that business investment does not react to changes in the dividend tax (Yagan, 2013).

[11]Recent work documents large differences between "book" and tax accounts, which introduces the risk of measurement error into such a mapping (see, e.g., Mills, Newberry and Trautman (2002)). Edgerton (2010) is very careful with this procedure, but acknowledges that "[one] cannot rule out, however, the possibility that difficulties in measuring firms' taxable status drive the relative unimportance of taxable status observed in the Compustat data."

The paper also relates to the literature on financial constraints.[12] We use depreciation changes as a plausibly exogenous financial constraint shock.[13] Unlike past studies, our instrument also changes the relative price of investment. We use this feature and an investment model to compute the implied shadow price of internal funds. Our findings imply that incorporating financial frictions adds much explanatory power to neoclassical investment theory.

The outline of the paper is as follows. Section 2.2 formalizes intuition about how bonus works and develops a set of testable hypotheses, which guide the empirical analysis. Section 2.3 describes the corporate tax data, variable construction and sample selection process. Section 2.4 describes the main empirical strategy for studying bonus depreciation, the identification assumptions and presents results and robustness tests. Section 2.5 uses split sample tests by markers of financial constraints and by tax position to show financial frictions can account for the large baseline effect of bonus. We then develop a set of theoretical moments and combine the empirical results to compute implied discount rates. Section 2.6 discusses policy implications and avenues for future research.

2.2 Hypothesis Development

To direct our empirical analysis, we develop a simple model of investment in the presence of depreciation incentives, financial constraints and heterogeneous tax positions. We modify the neoclassical investment model with adjustment costs (Abel, 1982; Hayashi, 1982) by introducing an external finance wedge and managerial myopia. Here, we focus on the intuition of the model and the mapping from theory to empirical objects and tests. We use a

[12]Fazzari, Hubbard and Petersen (1988*a*) argue that, if firms more likely to be financially constrained respond more strongly to cash flow shocks, then financial constraints are responsible. Subsequent studies make this argument while identifying quasi-experimental variation in cash flows or credit supply (Lamont, 1997; Rauh, 2006; Chaney, Sraer and Thesmar, 2012). We apply this insight to the case of bonus depreciation. Stein (2003) surveys models in which financial frictions influence investment decisions.

[13]See the conclusion of Fazzari, Hubbard and Petersen (1988*a*) and Fazzari, Hubbard and Petersen (1988*b*) for a discussion of how taxes might affect investment in the presence of financial constraints. They focus on average tax rates more generally and do not perform an empirical analysis along these lines.

simple one-shot static investment model with a reduced form credit wedge.[14] In Appendix Section B.1, we derive the hypotheses in an infinite horizon setting with adjustment costs and a dynamic leverage constraint.

Consider a firm making a one shot investment decision. The firm begins with initial profits π_0 and chooses a level of investment I to determine the capital stock and hence future profits. Future profits are given by $\pi(I)$, taxed at the proportional corporate tax rate τ. The firm discounts future flows at risk-adjusted rate r.

The tax code permits the firm to write off the cost of investment over time. The value of these deductions depends on the tax rate and how the schedule interacts with the firm's discount rate. We collapse the stream of future depreciation deductions owed for investment:

$$z^0(\beta) = D_0 + \beta \sum_{t=1}^{T} \frac{1}{(1+r)^t} D_t \tag{2.2.1}$$

where D_t is the allowable deduction per dollar of investment in period t and T is the class life of investment. $z^0(\beta)$ measures the present discounted value of one dollar of investment deductions before tax. If the firm can immediately deduct the full dollar, then z^0 equals one. Because of discounting, z^0 is lower for longer lived items (i.e., items with greater T), which forms the core of our identification strategy.

In general, the stream of future deductions depends on future tax rates and discount rates. Our empirical analysis assumes the effective tax rate does not change over time, except when the firm is nontaxable.[15] For discount rates, we apply a risk-adjusted rate of seven percent for r to compute z^0 in the data, which enables comparison to past work. In Section 2.5.3 we compute the implied additional discount firms apply to future deductions because of costly finance. β is an additional discount term between zero and one, which

[14]This wedge is a reduced form model of a set of capital market frictions, which might reflect, for example, costly monitoring problems or adverse selection (Stein, 2003).

[15]We use the top statutory tax rate in the set of specifications requiring a tax rate. This is an upper bound on the more realistic effective marginal tax rate, which in turn depends on tax rate progressivity and the level of other expenses relative to taxable income. See, e.g., Graham (1996, 2000) for a method tracing out the marginal tax benefit curve. The policies we study will increase the use of investment as a tax shield regardless of where the firm is on this marginal benefit curve. Except when current and all future taxes are zero, bonus increases the marginal tax benefit of investment.

reflects the possibility of myopia.[16] We use our heterogeneity analysis to identify this term separately.

Bonus depreciation allows the firm to deduct a per dollar bonus, θ, at the time of the investment and then depreciate the remaining $1-\theta$ according to the normal schedule:

$$z(\beta) = \theta + (1-\theta)z^0(\beta) \tag{2.2.2}$$

At different points in time, Congress has set θ equal to 0, 0.3, 0.5 or 1. We use these policy shocks to identify the effect of bonus depreciation on investment. Industries differ by average z^0 prior to bonus, providing the basis for identification in a difference-in-differences setup with continuous treatment.

We further generalize z by incorporating a nontaxable state. When the next dollar of investment does not affect this year's tax bill, then the firm must carry forward the deductions to future years.[17] Our general z reflects this case:

$$z(\beta,\gamma) = \gamma z(\beta) + (1-\gamma)\beta\phi z(1), \tag{2.2.3}$$

where $\gamma \in \{0,1\}$ is an indicator for current tax state and ϕ is a discounter that reflects both the expected arrival time of the taxable state and the discount rate applied to the future and subsequent periods when the firm switches. Note that for the nontaxable firm, β applies to all future deductions. Even when β equals one, ϕ is less than one, so the value of these deductions are lower when the firm is nontaxable. We measure ϕ in the data and apply our split sample results to determine whether we can justify our findings in a model without myopia.

External finance matters for all investment exceeding current cash flow. During the

[16] The myopia model is closer to Akerlof (1991) and Laibson (1997) than it is to the model of managerial myopia in Stein (1989). Stein's (1989) model of managerial myopia specifically refers to the incentive to boost current earnings as a way of signaling high quality to the stock market. We use the term to reflect any motive to boost current earnings and neglect projects with long term payoffs and short term costs.

[17] This assumes that "carrybacks"—in which firms apply unused deductions this year against past tax bills—have been exhausted or ignored. Relaxing this assumption complicates notation without altering the puzzle of a low response for nontaxable firms.

investment period, the firm faces an external finance wedge that is linear in expenses net of cash flows, that is,

$$c(I) = \lambda\left[(1-\tau z)I - (1-\tau)\pi_0\right], \tag{2.2.4}$$

where λ can be thought of as the shadow price on a borrowing constraint that may or may not bind now or in the future. Thus a dollar of cash inside the firm is worth $1+\lambda$.[18] We include z in the net expense term rather than the first year deduction to capture the influence of depreciation deductions on future taxes and thus future borrowing. While bonus depreciation relaxes the current constraint through reducing this year's tax bill, it does so at the expense of higher future taxes. The net effect is to reduce the present discounted borrowing costs for the firm. However, if myopia plays a role (that is, for low β), then only the current year change will matter. The two models thus yield different predictions for constrained, nontaxable firms: constrained, myopic firms respond much less to bonus when nontaxable than do constrained, farsighted firms. This is the feature we use to distinguish costly external finance from myopia models.

Though the problem occurs over time, we can write it as a static one shot investment problem by discounting future flows to the present. Discarding elements not involving investment, the firm's objective is

$$\max_I \left\{ \frac{(1-\tau)\pi(I)}{1+r} - (1-\tau z)I - \lambda(1-\tau z)I \right\} \tag{2.2.5}$$

We assume π is weakly concave, which ensures that the problem yields a unique interior solution.

The first order condition for optimal investment is

$$(1-\tau)\pi'(I^*) = (1+r)(1+\lambda)(1-\tau z). \tag{2.2.6}$$

Intuitively, the investment decision trades off the after-tax future benefits of the marginal

[18] Note that because we have assumed a linear external finance function, there will be no direct effect of cash flows on investment, that is, the investment-cash flow sensitivity is zero. Because each dollar of investment can only generate at most 35 cents of cash back, these policies cannot operate mainly through a direct cash windfall channel.

dollar of investment against its price (normalized to one) and the marginal external finance cost, less the marginal benefit due to depreciation deductions. Deductions lower the hurdle rate for investment both through their net present value and through relaxing the external finance constraint. With costly external finance, optimal investment is strictly lower than in the frictionless case or when inside cash can cover all investment expenses (i.e., when $\lambda = 0$).

We derive three testable hypotheses from the model. The first concerns the average effect of bonus depreciation on investment, while the latter two concern heterogeneous effects by the presence of costly external finance and by tax position. Bonus depreciation increases the present value of deductions, reducing the price of investment. Thus bonus depreciation should increase investment. Each hypothesis builds on the comparative static with respect to the bonus parameter θ. In the appendix, we show that investment is increasing in θ.

Hypothesis 1 *Investment responds more strongly to bonus depreciation for industries with more investment in longer lived eligible items. That is,* $\partial^2 I / \partial\theta\partial z^0 < 0$.

Bonus depreciation works through increasing θ. Hypothesis one concerns the basic effect of this policy on investment. The more delayed the normal depreciation schedule is, the more generous bonus will be. Longer lived items like telephone lines and heavy manufacturing equipment have a more delayed baseline schedule than short lived items like computers (i.e., $z^0_{\text{Long}} < z^0_{\text{Short}}$). Thus industries that buy more long lived equipment see a larger relative price cut when bonus happens.

Our second hypothesis concerns how the investment response varies with costly external finance.

Hypothesis 2 *Investment responds more strongly to bonus depreciation for financially constrained firms. That is,* $\partial^2 I / \partial\theta\partial\lambda > 0$.

For financially constrained firms, bonus depreciation both reduces the price of investment and reduces how much they have to borrow. The effective price change is thus larger for constrained firms. We use several proxies for ex ante financial constraints—firm size, dividend payment activity and liquid asset positions—to test for a difference in elasticities between constrained and unconstrained firms. If financial constraints are unimportant, then we should not find a consistent, systematic difference in elasticities for groups of firms based on these proxies. We can also use the difference in coefficients between constrained and unconstrained firms to infer the implied external finance spread. We formalize and implement this intuition in Section 2.5.3.

Our third hypothesis concerns how the investment response varies with the firm's current tax position.

Hypothesis 3 *Investment responds more strongly to bonus depreciation for firms with current-year taxable income. That is,* $\partial I/\partial\theta|_{\gamma=1} > \partial I/\partial\theta|_{\gamma=0}$.

Hypothesis three emerges in any model with some positive discounting, since future benefits are worth less than immediate benefits. The main value of the comparison between taxable and nontaxable groups derives from the calibration it offers. We can calibrate the expected arrival of the taxable state for nontaxable firms and ask whether the difference between elasticities for taxable and nontaxable firms requires some myopia (i.e., $\beta < 1$).

2.3 Business Tax Data

The analysis in this paper uses the most complete dataset yet applied to study business investment incentives.[19] The data include detailed information on equipment and structures investment, offering a finer breakdown than previously available for a broad class of industries. The sample includes many small, private firms and all of the largest US firms, which enables the heterogeneity analysis we use to document financial constraints. Because the data come from corporate tax returns, we can separate firms based on whether the next dollar of investment affects this year's taxes. This allows a split sample analysis that can distinguish the myopia model from a simple model of costly external finance. In this section,

[19] Yagan (2013) uses these data to study the 2003 dividend tax cut. Kitchen and Knittel (2011) use these data to describe general patterns in bonus and Section 179 take-up.

we describe where these data come from and the analysis sample, as well as how we map the theory into empirical objects.

Sampling Process. Each year, the Statistics of Income (SOI) division of the IRS Research, Analysis and Statistics unit produces a stratified sample of approximately 100,000 unaudited corporate tax returns.[20] Stratification occurs by form type,[21] total assets, and proceeds. SOI uses these samples to generate annual publications documenting income characteristics. The BEA uses them to finalize national income statistics. In addition, the Treasury's Office of Tax Analysis (OTA) uses the sample to perform policy analysis and revenue estimation.

In 2008, the sample represented about 1.8 percent of the total population of 6.4 million C and S corporation returns. Any corporation selected into the sample in a given year will be selected again the next year, providing it continues to fall in a stratum with the same or higher sampling rate. Shrinking firms are resampled at a lower rate, which introduces sampling attrition. We address this attrition in several ways, including a nonparametric reweighting procedure for figures and through assessing the robustness of our results in a balanced panel. Each sample year includes returns with accounting periods ending between July of that year and the following June. When necessary, we recode the tax year to align with the implementation of the policies studied in this paper.

Analysis Samples, Variable Definitions and Summary Statistics. We create a panel by linking the cross sectional SOI study files using firm identifiers.[22] The raw dataset has 1.84 million rows covering the years from 1993 to 2010. There are 355 thousand distinct firms in this dataset, 19,711 firms with returns in each year of the sample and 62,478 firms with at least 10 years of returns. Beginning with the sample of firms with valid data for each of the

[20] Details come from `http://www.irs.gov/pub/irs-soi/08cosec3ccr.pdf`.

[21] For example, C corporations file form 1120 and S corporations file form 1120S. Other form types include real estate investment trusts, regulated investment companies, foreign corporations, life insurance companies, and property and casualty insurance companies. We focus on 1120 and 1120S, which cover the bulk of business activity in industries making equipment investments.

[22] We thank Jason Debacker and Rich Prisinzano for providing the data crosswalk.

main data items analyzed, we keep firm-years satisfying the following criteria: (a) having non-zero total deductions or non-zero total income[23] and (b) having an attached investment form.[24] In addition, we exclude partial year returns, which occur when a firm closes or changes its fiscal year. To analyze bonus depreciation, we exclude firms potentially affected by Section 179, a small firm investment incentive which we analyze separately. Our main bonus analysis sample consists of all firms with average eligible investment greater than \$100,000 during years of positive investment.[25] This sample consists of 820,769 observations for 128,151 distinct firms.

We describe the economic concepts underlying the variables we study. **Eligible investment**, our main variable of interest, includes expenditures for all equipment investment put in place during the current year for which bonus and Section 179 incentives apply.[26] We conduct separate analyses for intensive and extensive margin responses. The intensive margin variable is the logarithm of eligible investment. The extensive margin variable is an indicator for positive eligible investment. We aggregate this indicator at the industry level and transform it into a log odds ratio[27] for our empirical analyses. In some specifications, we use an alternative measure of investment, which is eligible investment divided by lagged **capital stock**. Capital stock is the reported book value of all tangible, depreciable assets. **Sales** equals operating revenue and **assets** equals total book assets. **Total debt** equals the sum of non-equity liabilities excluding trade credit. **Liquid assets** equals cash and other liquid securities. **Payroll** equals non-officer wage compensation. **Rents** equals lease and

[23]Knittel et al. (2011) use a similar "de minimus" test to select business entities that engage in "substantial" business activity.

[24]Form 4562 is the tax form that corporations attach to their return to claim depreciation deductions on new and past investments. An entity that claims no depreciation deductions need not attach form 4562. It is likely that these firms do not engage in investment activity, and so their exclusion should not affect the interpretation of results.

[25]The relevant threshold for Section 179 was \$25,000 until 2003, when it increased to \$100,000. In 2008, it increased to \$250,000 and then to \$500,000 in 2010. Using alternative thresholds in the range from \$50,000 to \$500,000 does not alter the results.

[26]Section 179 and bonus rules differ slightly, in that Section 179 also applies to used equipment purchases, while bonus only applies to new equipment. The form does not require firms to list used purchases separately.

[27]We use $\log(\frac{p}{1-p})$ as our measure of the extensive margin.

rental expenses. **Interest** equals interest payments.

Our main policy variable of interest, $\mathbf{z_{N,t}}$, is the present discounted value of one dollar of deductions for eligible investment. In each non-bonus year, we compute the share of eligible investment a firm reports in each category.[28] We use these shares and the present value of one dollar of eligible investment for each category to construct a weighted average, firm-level z. Category z's come from applying a seven percent discount rate to the pertinent deduction schedule,[29] while assuming a six-month convention for the purchase year.[30] We compute z_N at the four-digit NAICS industry level as the simple average of firm-level z's across non-bonus years prior to 2001.[31] In bonus years, we adjust z by the size of the bonus. If θ is the additional expense allowed per dollar of investment (e.g., $\theta = .3$ for 2001), then $z_{N,t|\theta_t} = \theta_t + (1 - \theta_t) \times z_N$. The interaction between the time series variation in θ and the cross sectional variation in z_N delivers the identifying variation we use to test our three hypotheses.

Table 2.1 collects summary statistics for the sample in our bonus depreciation analysis. The average observation has \$6.8 million in eligible investment, \$180 million in sales and \$27 million in payroll. The size distribution of corporations is skewed, with median eligible investment of just \$370 thousand and median sales of \$26 million. The average net present value of depreciation allowances, $z_{N,t}$, is 0.88 in non-bonus years, implying that eligible investment deductions for a dollar of investment are worth eighty-eight cents to the average firm. $z_{N,t}$ increases to an average of 0.94 during bonus years. Cross sectional differences in

[28]Specifically, 3-, 5-, 7-, 10-, 15-, and 20-year Modified Accelerated Cost Recovery System (MACRS) property and listed property.

[29]The category deduction schedules are available in IRS publication 946. We use a seven percent rate as a frictionless benchmark that is likely larger than the rate firms should be using, which will tend to bias our results downward. Summers (1987) argues that firms should apply a discount rate close to the risk-free rate for depreciation deductions. Seven percent is the largest discount rate House and Shapiro (2008) apply when computing the value of bonus depreciation.

[30]The six-month convention is applied because on average the property is in place for only half of the first year.

[31]Like Cummins, Hassett and Hubbard (1994) and Edgerton (2010), we proxy for the firm-level benefit of bonus depreciation with an industry measure of policy benefits. Unlike these studies, our measure derives directly from tax data, reducing measurement error. It is possible to apply the same strategy at the firm level. This approach does not alter our findings.

$z_{N,t}$ are similar in magnitude to the change induced by bonus, with $z_{N,t}$ varying from 0.87 at the tenth percentile to 0.94 at the ninetieth. The first year deduction, $\theta_{N,t}$, increases from an average of 0.18 in non-bonus years to 0.58 in bonus years.

The difference in z's over time of just six cents per dollar before tax translates into a benefit of just over two cents after tax, which is why some authors claim the effect of bonus on investment should be small. However, if the discount rate firms apply to future deductions includes a large external finance wedge or myopia, then this two cent difference can increase to as much as the fourteen cent difference in average after-tax θs.

It is helpful to give a sense of the groups being compared, because our identification will be based on assuming that industry-by-year shocks are not confounding the trends between industry groups. The five most common three-digit industries (NAICS code) in the bottom three z_N deciles are: motor vehicle and parts dealers (441), food manufacturing (311), real estate (531), telecommunications (517), and fabricated metal product manufacturing (332). In the top three deciles are: professional, scientific and technical services (541), specialty trade contractors (238), computer and electronic product manufacturing (334), durable goods wholesalers (423), and construction of buildings (236). Neither group of industries appears to be skewed toward a spurious relative boom in the low z group. The telecommunications industry suffered unusually during the early bonus period as did real estate in the later period. Both industries are in the group for which we observe a larger investment response due to bonus.

2.4 The Effect of Bonus Depreciation on Investment

We begin with a test of Hypothesis 1, which predicts that investment responds more strongly to bonus depreciation for industries with more investment in longer lived eligible items. In both bonus periods we study, we estimate large responses to bonus depreciation. The estimates are similar in both periods. We assess the key risk of this design—that time-varying industry shocks confound our estimates—using a variety of specifications, a placebo test and differences in policy salience across space.

Table 2.1: *Statistics: Bonus Analyses*

	Mean	P10	Median	P90	Count
Investment Variables					
Investment (000s)	6,786.87	0.81	367.59	5,900.17	818,576
log(Investment)	6.27	4.10	6.14	8.81	735,341
Investment/Lagged Capital Stock	0.10	0.00	0.05	0.27	637,243
Δ log(Capital Stock)	0.08	-0.05	0.05	0.33	637,278
log(Odds Ratio$_N$)	1.28	0.54	1.34	2.05	818,107
Other Outcome Variables					
Δ log(Debt)	0.04	-0.37	0.03	0.56	642,546
Δ log(Rent)	0.08	-0.38	0.04	0.66	574,305
Δ log(Wage Compensation)	0.06	-0.21	0.05	0.40	624,918
log(Structures Investment)	5.02	2.13	4.98	8.10	389,232
Policy Variables					
$z_{N,t}$	0.90	0.87	0.89	0.94	818,576
Characteristics					
Assets (000s)	403,597.2	3,267.96	24,274.82	327,301.6	818,576
Sales (000s)	180,423.8	834.65	25,920.92	234,076.1	818,576
Capital Stock (000s)	89,977.09	932.00	7,214.53	80,122.69	818,576
Net Income Before Depreciation (000s)	15,392.59	-2,397.92	1,474.65	17,174.55	818,576
Profit Margin	0.17	-0.07	0.05	0.68	777,968
Wage Compensation (000s)	26,826.36	372.09	4,199.88	38,526.46	818,576
Cash Flow/Lagged Capital Stock	0.05	-0.09	0.03	0.26	647,617

Notes: This table presents summary statistics for analysis of bonus depreciation. To preserve taxpayer anonymity, "percentiles" are presented as means of all observations in the $(P-1, P+1)$th percentiles. Investment is bonus eligible equipment investment. $z_{N,t}$ is the weighted present value for a dollar of eligible investment expense at the four-digit NAICS level, with weights computed using shares of investment in each eligible category. The odds ratio is defined at the four-digit NAICS level as the fraction of firms with positive investment divided by the fraction with zero investment. Cash flow is net income before depreciation after taxes paid. Ratios are censored at the one percent level. Appendix Table B.2 presents more detailed investment statistics, allowing comparison of our sample to past work.

Policy Background. House and Shapiro (2008) provide a detailed discussion of the baseline depreciation schedule and legislative history of the first round of bonus depreciation. Kitchen and Knittel (2011) provide a brief legislative history of the second round.[32] Appendix Section B.2 summarizes the relevant legislation for our sample frame.

In 2001, firms buying qualified investments were allowed to immediately write off 30 percent of the cost of these investments. The bonus increased to 50 percent in 2003 and expired at the end of 2004. In 2008, 50 percent bonus depreciation was reinstated. It was later extended to 100 percent bonus for tax years ending between September 2010 and December 2011. The policies applied to equipment and excluded most structures.[33]

Consider a firm buying \$1 million worth of computers. The firm owes corporate taxes on income net of business expenses. For expenses on nondurable items such as wages and advertising, the firm can immediately deduct the full cost of these items on its tax return. Thus an extra dollar of spending on wages reduces the firm's taxable income by a dollar and reduces the firm's tax bill by the tax rate. But for investment expenses the rules differ.

Usually, the firm follows the regular depreciation schedule in the top panel of Table 2.2. The first year deduction is \$200 thousand, which provides an after-tax benefit of \$70 thousand. Over the next five years, the firm deducts the remaining \$800 thousand. The total undiscounted deduction is the \$1 million spent and the total undiscounted tax benefit is \$350 thousand. With bonus depreciation the situation changes. Assume 50 percent bonus. The firm can now deduct a \$500 thousand bonus before following the normal schedule for the remaining amount, so the total first year deduction rises to \$600 thousand. Each subsequent deduction falls by half.

The total amount deducted over time does not change. However, the accelerated schedule does raise the present value of these deductions. Applying a seven percent discount rate yields \$311 thousand for the present value of cash back in normal times. Bonus raises this

[32] See also the Treasury's "Report to The Congress on Depreciation Recovery Periods and Methods" (2000).

[33] These provisions coincided with an increase in the Section 179 allowance for small investments from \$24,000 to \$100,000 in 2003, from \$125,000 to \$250,000 in 2008, and from \$250,000 to \$500,000 in 2010.

Table 2.2: *Regular and Bonus Depreciation Schedules for Five Year Items*

Normal Depreciation							
Year	0	1	2	3	4	5	Total
Deductions (000s)	200	320	192	115	115	58	1000
Tax Benefit ($\tau = 35\%$)	70	112	67.2	40.3	40.3	20.2	350
Bonus Depreciation (50%)							
Year	0	1	2	3	4	5	Total
Deductions (000s)	600	160	96	57.5	57.5	29	1000
Tax Benefit ($\tau = 35\%$)	210	56	33.6	20.2	20.2	10	350

Notes: This table displays year-by-year deductions and tax benefits for a $1 million investment in computers, a five year item, depreciable according to the Modified Accelerated Cost Recovery System (MACRS). The top schedule applies during normal times. It reflects a half-year convention for the purchase year and a 200 percent declining balance method (2X straight line until straight line is greater). The bottom schedule applies when 50 percent bonus depreciation is available. See IRS publication 946 for the recovery periods and schedules applying to other class lives.

present value by $20 thousand, just two percent of the original purchase price. This small present value payoff is why some authors conclude that bonus provides little stimulus for short-lived items (Desai and Goolsbee, 2004).[34]

In a frictionless model, a firm will judge the benefits of bonus by comparing these present value payoffs. Note however the large difference in the initial deduction, which translates into $140 thousand of savings in the investment year. Such a difference will matter if firms must borrow to meet current expenses and external finance is costly. Or it will matter if managers are myopic in the sense that they will aggressively use bonus to reduce current taxes even at the expense of higher future taxes. In short, when firms use higher effective discount rates to evaluate bonus, they will respond more than the frictionless model predicts.

The policies were intended as economic stimulus. In the words of Congress, "increasing

[34]See also Steuerle (2008), Knittel (2007) and House and Shapiro (2008). In his comment on Desai and Goolsbee (2004), Kevin Hassett argues that the temporary nature of these policies increases the stimulus through intertemporal shifting, and that the authors' results are consistent with a large response; see also Cohen, Hansen and Hassett (2002). The intertemporal shifting story cannot explain our heterogeneity results and predicts patterns which we do not observe.

and extending the additional first-year depreciation will accelerate purchases of equipment, promote capital investment, modernization, and growth, and will help to spur an economic recovery" (Committee on Ways & Means, 2003, p. 23). To avoid encouraging firms to delay investment until the policy came online, legislators announced that the policy would apply retroactively to include the time when the policy was under debate. Although the first bonus legislation passed in early 2002, firms anticipating policy passage would have begun responding in the fourth quarter of 2001. We therefore include firm-years with the tax year ending within the legislated window in our treatment window.

Whether firms perceived the policy as temporary or permanent is a subject of debate. The initial bill branded the policy as temporary stimulus, slating it to expire at the end of 2004, which it did. For this reason, House and Shapiro (2008) assume firms treat the policy as temporary. In contrast, Desai and Goolsbee (2004) cite survey evidence indicating that many firms expected the provisions to continue, and our empirical analysis in Section 2.4 offers little evidence of intertemporal shifting. Expecting the policy to be temporary is important for House and Shapiro (2008), because their exercise relies upon how policies approximated as instantaneous interact with the duration of investment goods approximated as infinitely lived. Our design relies less on this assumption. In our model, costly external finance and myopia amplify the effects of both temporary and permanent policies. And our cross sectional identification also relies less on the response of the longest lived investment goods.

Empirical Setup. Bonus depreciation provides a temporary reduction in the price and a temporary increase in the first year deduction for eligible investment goods. Eligible items are classified for deduction profiles over time based on their useful life. Identification builds upon the idea that some industries benefited more from these cuts by virtue of having longer duration investment patterns, that is, by having more investment in longer class life categories. This cross-sectional variation permits a within-year comparison of investment growth for firms in different industries.[35] The policy variation is at the industry-

[35]This methodological approach was first applied in Cummins, Hassett and Hubbard (1994). See also Cummins, Hassett and Hubbard (1996), Desai and Goolsbee (2004), House and Shapiro (2008) and Edgerton

by-year level, so the key identifying assumption is that the policies are independent of other industry-by-year shocks. Several robustness tests validate this assumption.

The regression framework implements the difference-in-differences (DD) specification,

$$f(I_{it}, K_{i,t-1}) = \alpha_i + \beta g(z_{N,t}) + \gamma X_{it} + \delta_t + \varepsilon_{it} \tag{2.4.1}$$

where $z_{N,t}$ is measured at the four-digit NAICS industry level and increases temporarily during bonus years. The specific additive form we adopt in (2.4.1) for the unobserved firm-level components, α_i, can only be valid for a particular class of investment functions. For example, if valid in levels, the design cannot be valid in logs. The investment data summarized in Table 2.1 is highly skewed with a mean of \$6.8 million and a median of just \$368 thousand. Thus a multiplicative unobserved effect (that is, $I_i = A_i I^*(z)$) is the most likely empirical model for investment levels. This delivers an additive model in logarithms, which is the approach we pursue below. Because approximately eight percent of our observations for eligible investment are equal to zero, we supplement the intensive margin logs approach with a log odds model for the extensive margin. We measure the log odds ratio as $\log(P[I > 0]/(1 - P[I > 0]))$ at the four-digit industry level.[36]

Studies often use an alternative empirical specification for $f(I, K)$, where investment is scaled by lagged assets or lagged capital stock. We prefer log investment for four reasons. First, small firms are not always required to disclose balance sheet information, so requiring reported assets would reduce our sample frame. Second, and related to the first reason, requiring two consecutive years of data for a firm-year reduces our sample by fifteen percent. Third, there is some concern that balance sheet data on tax accounts are not reported correctly for consolidated companies due to failure to net out subsidiary elements.[37] Measurement error in the scaling variable introduces non-additive measurement

(2010).

[36]An alternative specification, with the odds ratio replaced by $P[I > 0]$, works as well. However, the logs odds ratio has better statistical properties (e.g., a more symmetric distribution).

[37]Mills, Newberry and Trautman (2002) analyze balance sheet accounting in tax data and document difficulties in reconciling these accounts with book accounts.

error into the dependent variable. Last, with multiple types of capital, the scaling variable might not remove the unobserved firm effect from the model. This is especially a concern because we cannot measure a firm's stock of eligible capital and because firms vary in the share of total investments made in eligible categories.[38] While we prefer the log investment model, we also report results using investment scaled by lagged capital stock, which allows comparison to past studies.

Graphical Evidence. Figure 2.1 presents a visual implementation of this research design. To allow a comparison that matches a regression analysis with fixed effects and firm-level covariates, we construct residuals from a two-step regression procedure. First, we nonparametrically reweight (i.e., Dinardo, Fortin and Lemieux (1996) reweight) the group-by-year distribution within ten size bins based on assets crossed with ten size bins based on sales.[39] This procedure addresses sampling frame changes over time, which cause instability in the aggregate distribution.[40] In the second step, we run cross sectional regressions each year of the outcome variable on an indicator for treatment group—either long duration or short duration—and a rich set of controls, including ten-piece splines in assets, sales, profit margin and age. We plot the residual group means from these regressions.[41]

We compare mean investment in calendar time for the top and bottom three deciles of the investment duration distribution.[42] Long duration industries show growth well above that of the short duration industries, with this difference only appearing in the bonus years. The difference between the slopes of these two lines in any year gives the difference-in-differences estimate between these groups in that year. The other years provide placebo

[38] Abel (1990) notes that this issue and other violations of linear homogeneity can lead to spurious conclusions (e.g., a reversed investment-Q relationship).

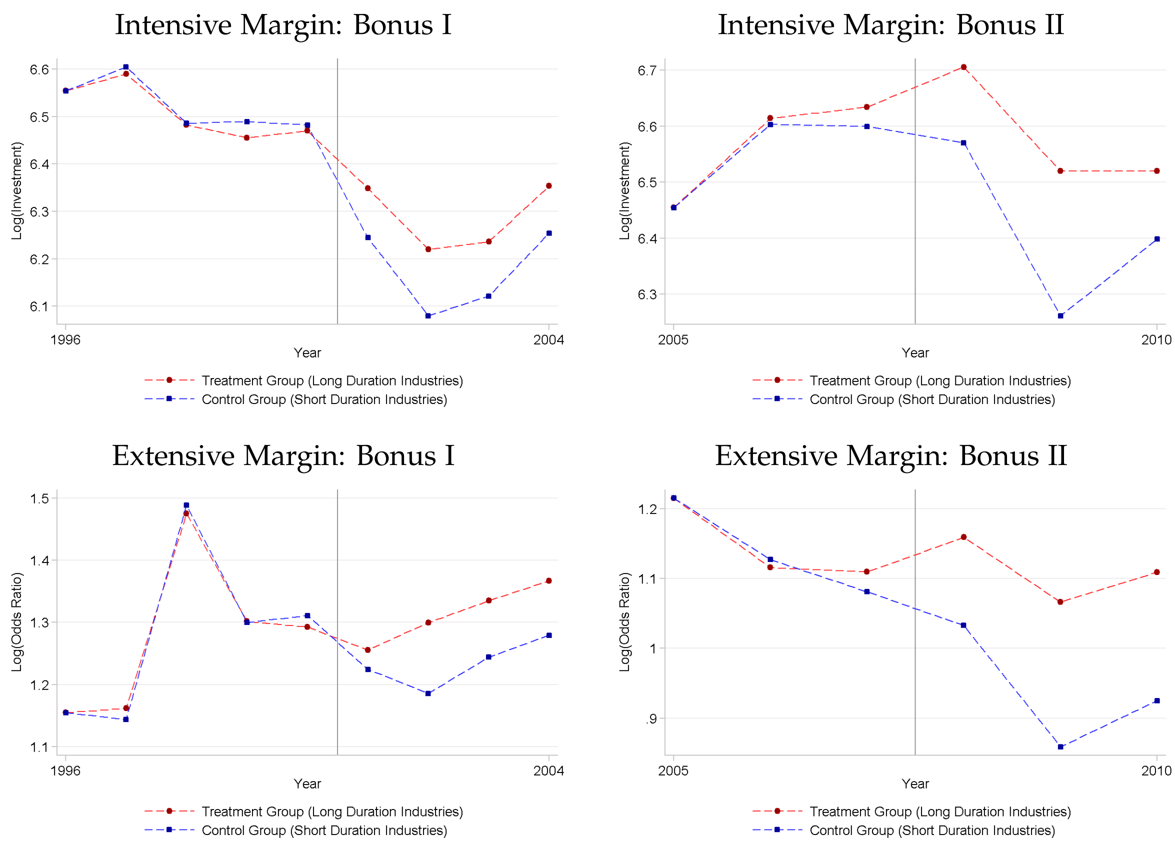
[39] The bins are set based on the size distribution in 2000.

[40] During the period we study, the size of the sample frame changed twice due to budgetary constraints.

[41] To align the first year of each series and ease comparison of trends, we subtract from each dot the group mean in the first year and add back the pooled mean from the first year. All means are count weighted.

[42] Deciles are computed at the industry level.

Figure 2.1: *Calendar Difference-in-Differences*



Notes: The top graphs plot the average logarithm of eligible investment over time for groups sorted according to their industry-based treatment intensity. Treatment intensity depends on the average duration of investment, with long duration industries (treatment groups) seeing a larger average price cut due to bonus than short duration industries (control groups). The bottom graphs plot the industry-level log odds ratio for the probability of positive eligible investment, thus offering a measure of the extensive margin response. The treatment years for Bonus I are 2001 through 2004 and 2008 through 2010 for Bonus II. In these years, the difference between changes in the red and the blue lines provides a difference-in-differences estimator for the effect of bonus in that year for those groups. The earlier years provide placebo tests and a demonstration of parallel trends. The averages plotted here result from a two-step regression procedure. First, we nonparametrically reweight the group-by-year distribution (i.e., Dinardo, Fortin, and Lemieux (1996) reweight) within ten size bins based on assets crossed with ten size bins based on sales to address sampling frame changes over time. Second, we run cross sectional regressions each year of the outcome variable on an indicator for treatment group and a rich set of controls, including ten-piece splines in assets, sales, profit margin and age. We plot the residual group means from these regressions. To align the first year of each series and ease comparison of trends, we subtract from each dot the group mean in the first year and add back the pooled mean from the first year. All means are count weighted.

tests of the natural experiment and indicate no false positives.

Statistical Results and Economic Magnitudes. Table 2.3 presents regressions of the form in (2.4.1), where $f(I_{it}, K_{i,t-1})$ equals $\log(I_{it})$ in the intensive margin model, $\log(P_N[I_{it} > 0]/(1 - P_N[I_{it} > 0]))$ in the extensive margin model, and $I_{it}/K_{i,t-1}$ in the user cost model; and $g(z_{N,t})$ equals $z_{N,t}$ in the intensive and extensive margin models and $(1 - \tau z_{N,t})/(1 - \tau)$ in the user cost model.[43] The baseline specification includes year and firm fixed effects. Standard errors are clustered at the firm level in the intensive margin and user cost models.[44] Because log odds ratios are computed at the industry level, standard errors in the extensive margin model are clustered at the industry level.

The first column reports an intensive margin semi-elasticity of investment with respect to z of 3.7, an extensive margin semi-elasticity of 3.8 and a user cost elasticity of -1.6. The average change in $z_{N,t}$ was 4.7 cents during the early bonus period and 8 cents during the later period, implying average investment increases of $17.3 (= 3.69 \times 4.7)$ and $29.5 (= 3.69 \times 8)$ log points, respectively. These predictions should not be confused with the aggregate effect of the policy, because they are based on equal-weighted regressions which include many small firms. They only provide an informative aggregate prediction under the strong assumption that the semi-elasticity is independent of firm size.

In the second column, including a control for contemporaneous cash flow scaled by lagged capital does not alter the estimates. Columns three and four show a similar semi-elasticity for both the early and late episodes. Column five controls for fourth order polynomials in each of assets, sales, profit margin and firm age, as well as industry average Q measured from Compustat at the four-digit level. Column six adds quadratic time trends interacted with two-digit NAICS industry dummies, which causes the estimated

[43] τ is set to 35 percent, the top statutory tax rate for all firms.

[44] This is consistent with recent work (e.g., Desai and Goolsbee (2004), Edgerton (2010), Yagan (2013)) and enables us to compare our confidence bands to past estimates. The implicit assumption that errors within industries are independent is strong, for the same reason that Bertrand, Duflo and Mullainathan (2004) criticize papers that cluster at the individual level when studying state policy changes. Our results in this section are robust to industry clustering, as are the tax splits in the next section. We are not aware of other studies that restrict inference in this way and still show that taxes affect investment.

Table 2.3: *Investment Response to Bonus Depreciation*

	Intensive Margin: LHS Variable is log(Investment)					
	(1)	(2)	(3)	(4)	(5)	(6)
$z_{N,t}$	3.69***	3.78***	3.07***	3.02***	3.73***	4.69***
	(0.53)	(0.57)	(0.69)	(0.81)	(0.70)	(0.62)
$CF_{it}/K_{i,t-1}$		0.44***				
		(0.016)				
Observations	735341	580422	514035	221306	585914	722262
Clusters (Firms)	128001	100883	109678	63699	107985	124962
R^2	0.71	0.74	0.73	0.80	0.72	0.71
	Extensive Margin: LHS Variable is $\log(P(\text{Investment} > 0))$					
	(1)	(2)	(3)	(4)	(5)	(6)
$z_{N,t}$	3.79**	3.87**	3.12	3.59**	3.99*	4.00***
	(1.24)	(1.21)	(2.00)	(1.14)	(1.69)	(1.13)
$CF_{it}/K_{i,t-1}$		0.029**				
		(0.0100)				
Observations	803659	641173	556011	247648	643913	803659
Clusters (Industries)	314	314	314	274	277	314
R^2	0.87	0.88	0.88	0.93	0.90	0.90
	User Cost: LHS Variable is Investment/Lagged Capital					
	(1)	(2)	(3)	(4)	(5)	(6)
$\frac{1-t_c z}{1-t_c}$	-1.60***	-1.53***	-2.00***	-1.42***	-2.27***	-1.50***
	(0.096)	(0.095)	(0.16)	(0.13)	(0.14)	(0.10)
$CF_{it}/K_{i,t-1}$		0.043***				
		(0.0023)				
Observations	637243	633598	426214	211029	510653	631295
Clusters (Firms)	103890	103220	87939	57343	90145	103565
R^2	0.43	0.43	0.48	0.54	0.45	0.44
Controls	No	No	No	No	Yes	No
Industry Trends	No	No	No	No	No	Yes

Notes: This table estimates regressions of the form

$$f(I_{it}, K_{i,t-1}) = \alpha_i + \beta g(z_{N,t}) + \gamma X_{it} + \delta_t + \varepsilon_{it}$$

where I_{it} is eligible investment expense and $z_{N,t}$ is the present value of a dollar of eligible investment computed at the four-digit NAICS industry level, taking into account periods of bonus depreciation. Column (2) augments the baseline specification with current period cash flow scaled by lagged capital. Column (3) focuses on the early bonus period and column (4) focuses on the later period. Column (5) controls for four-digit industry average Q for public companies and quartics in assets, sales, profit margin and firm age. Column (6) includes quadratic time trends interacted with two-digit NAICS industry dummies. Ratios are censored at the one percent level. All regressions include firm and year fixed effects. Standard errors clustered at the firm level are in parentheses (industry level for the extensive margin models).

semi-elasticity to increase.[45] These alternative control sets do not challenge our main finding: the investment response to bonus depreciation is robust across many specifications.[46]

Appendix Section B.3 collects from other studies estimates that we can compare to our user cost model. Like our study, each one uses tax reforms crossed with industry characteristics to estimate the effect of taxes on investment. Panel (a) of Table 2.2 plots the estimates from these studies with confidence bands, highlights the consensus range, and compares them to our estimate. The average user cost elasticity across these studies is -0.69, which falls within Hassett and Hubbard (2002)'s consensus range of -0.5 to -1, but is less than half our estimate of 1.60. In an investment model, the elasticity of investment with respect to the net of tax rate, $1 - \tau z$, equals the price elasticity and interest rate elasticity, derived in Appendix Section B.1. Our empirical model delivers a large elasticity of 7.2. Thus by several accounts, bonus depreciation has a substantial effect on investment, much larger than past estimates and much stronger than the conventional wisdom predicts.

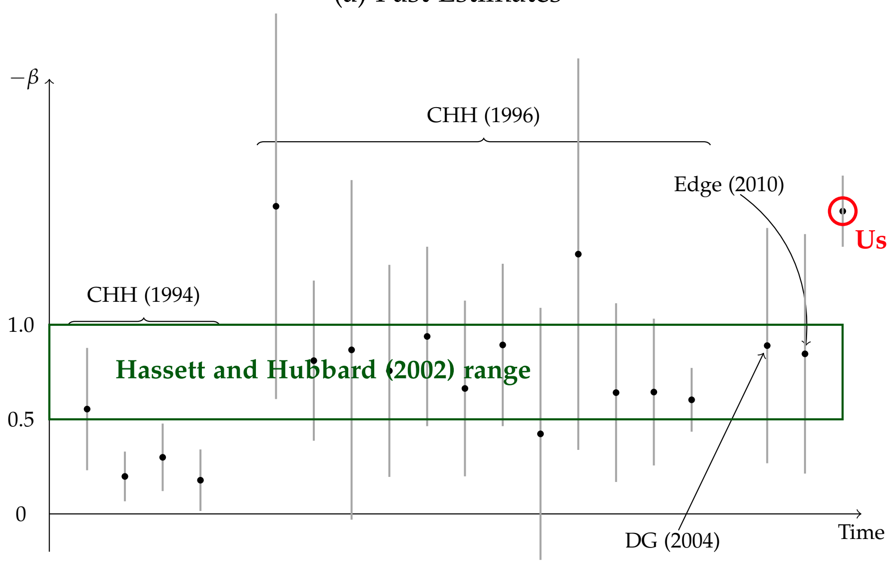
Additional Robustness. The calendar time plot in Figure 2.1 provides several visual placebo tests through inspection of the parallel trends assumption in non-bonus years. Because bonus depreciation excludes very long lived items (i.e., structures), we can use ineligible investment as an alternative intratemporal placebo test. The first two columns of Table 2.4 present two specifications of the intensive margin model, which replace eligible investment with structures investment. The first specification is the baseline model, and the second includes two-digit industry dummies interacted with quadratic time trends. We cannot distinguish the structures investment response from zero. Thus the results pass this

[45] We can replace the quadratic time trends with increasingly nonlinear trends or two digit industry-by-time fixed effects. We can also replace the time trends with two-digit industry interacted with log GDP or GDP growth. In each case, the estimates increase. This suggests that omitted industry-level factors bias our estimates downward. Consistent with this story, Dew-Becker (2012) shows that long duration investment falls more during recessions than short duration investment.

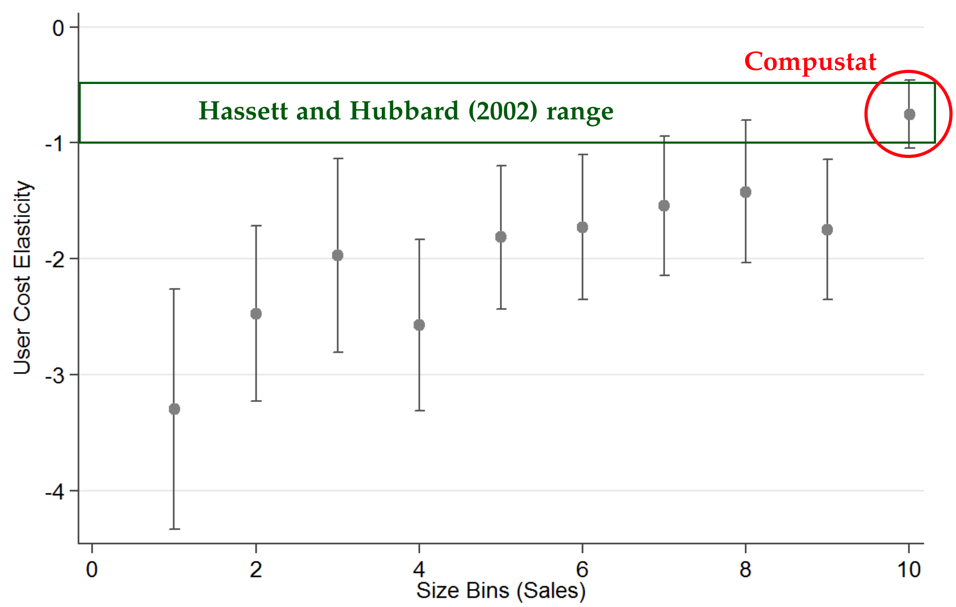
[46] We have confirmed these results in a balanced panel and for the sample of firms with enough observations to compute firm-level zs, which allows inclusion of four-digit industry-by-time fixed effects. Results for these additional specifications are available upon request.

Figure 2.2: *Heterogeneous Effects by Firm Size*

(a) Past Estimates



(b) Estimates by Firm Size, Bonus Sample



Notes: These figures plot coefficients and confidence bands from user cost specifications (see the third row of Table 2.3) for past studies of tax reforms and our sample. The sources for the coefficients in Panel (a) are in Appendix Section B.3. Panel (b) splits the sample into deciles based on mean pre-policy sales. The average firm in Compustat during this time period falls in the tenth size bin (with sales equal to \$1.8B), which coincides with the Hassett and Hubbard (2002) survey range of user cost elasticity estimates (-0.5 to -1).

Table 2.4: *Investment Response to Bonus Depreciation: Robustness*

	Structures		Net Investment		Has Bonus		Salience Split	
	Basic	Trends	Basic	Trends	Basic	Trends	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$z_{N,t}$	0.52	1.10	1.07***	0.78***	0.95***	1.15***	5.09***	1.56
	(0.78)	(0.98)	(0.23)	(0.22)	(0.13)	(0.16)	(0.87)	(0.94)
Observations	389232	381921	637278	631680	818576	804128	211390	215205
Clusters (Firms)	92351	90166	103447	103147	128150	125534	29627	30836
R^2	0.59	0.60	0.27	0.27	0.61	0.61	0.70	0.70
Industry Trends	No	Yes	No	Yes	No	Yes	No	No

Notes: This table estimates regressions of the form

$$Y_{it} = \alpha_i + \beta z_{N,t} + \delta_t + \varepsilon_{it}$$

where Y_{it} is either the logarithm of structures investment (columns (1) and (2)), log growth in capital stock (columns (3) and (4)), an indicator for take-up of bonus depreciation ((5) and (6)), or the logarithm of eligible investment ((7) and (8)). $z_{N,t}$ is the present value of a dollar of eligible investment computed at the four-digit NAICS industry level, taking into account periods of bonus depreciation. Columns (1), (3), (5), (7) and (8) implement the baseline specification in table 2.3. Columns (2), (4) and (6) include quadratic time trends interacted with two-digit NAICS industry dummies. Columns (7) and (8) split the sample into the top and bottom three deciles according to local geographic salience of the depreciation schedule. We proxy for local salience using frequency of bunching by small firms at the Section 179 kink point in the depreciation schedule. All regressions include firm and year fixed effects. Standard errors clustered at the firm level are in parentheses.

placebo test.[47]

Another concern with our results is that they may merely reflect a reporting response, with less real investment taking place. The third and fourth columns of Table 2.4 provide a reality check. We replace our measure of investment derived from form 4562 with net investment, which is the difference in logarithms of the capital stock between year t and year $t-1$. Both the baseline and industry trend regressions confirm our gross investment results with net investment responding strongly as well.

Columns five and six of Table 2.4 offer a sanity check of our findings. Here, the dependent variable is an indicator for whether the firm reports depreciation expense in the specific form item applicable to bonus. Effectively, this is a test for bonus depreciation

[47]This placebo test is valid if structures are neither complements nor substitutes for equipment. Without this assumption, the structures test remains useful because observing a structures response equal in magnitude or larger than the equipment response would indicate that time-varying industry shocks drive our results.

take-up. The table indicates that the probability of taking up bonus is strongly increasing in the strength of the incentive.

Policy Salience. We now present direct evidence that firms take the tax code into account when making investment decisions. With respect to equipment investment, they pay special attention to the depreciation schedule and the nonlinear incentives it creates. These nonlinear budget sets should induce *bunching* of firms at rate kinks. Consistent with this logic, we find sharp bunching at depreciation kink points. This evidence supports our claim that temporary bonus depreciation incentives were also salient.

We study a component of the depreciation schedule, Section 179, which applies mainly to smaller firms. Under Section 179, taxpayers may elect to expense qualifying investment up to a specified limit. With the exception of used equipment,[48] all investment eligible for Section 179 expensing is eligible for bonus depreciation. Focusing on Section 179 thus serves as an out of sample test of policy salience that remains closely linked to the bonus incentives at the core of the paper.

Each tax year, there is a maximum deduction and a threshold over which Section 179 expensing is phased out dollar for dollar. The kink and phase-out regions have increased incrementally since 1993. When the tax schedule contains kinks and the underlying distribution of types is relatively smooth, the empirical distribution should display excess mass at these kinks (Hausman, 1981; Saez, 2010). Figure 2.3 shows how dramatic the bunching behavior of eligible investment is in our setting. These figures plot frequencies of observations in our dataset for eligible investment grouped in $250 bins. Each plot represents a year or group of years with the same maximum deduction, demarcated here by a vertical line. The bunching within $250 of the kink tracks the policy shifts in the schedule exactly and reflects a density five to fifteen times larger than the counterfactual distribution nearby.[49]

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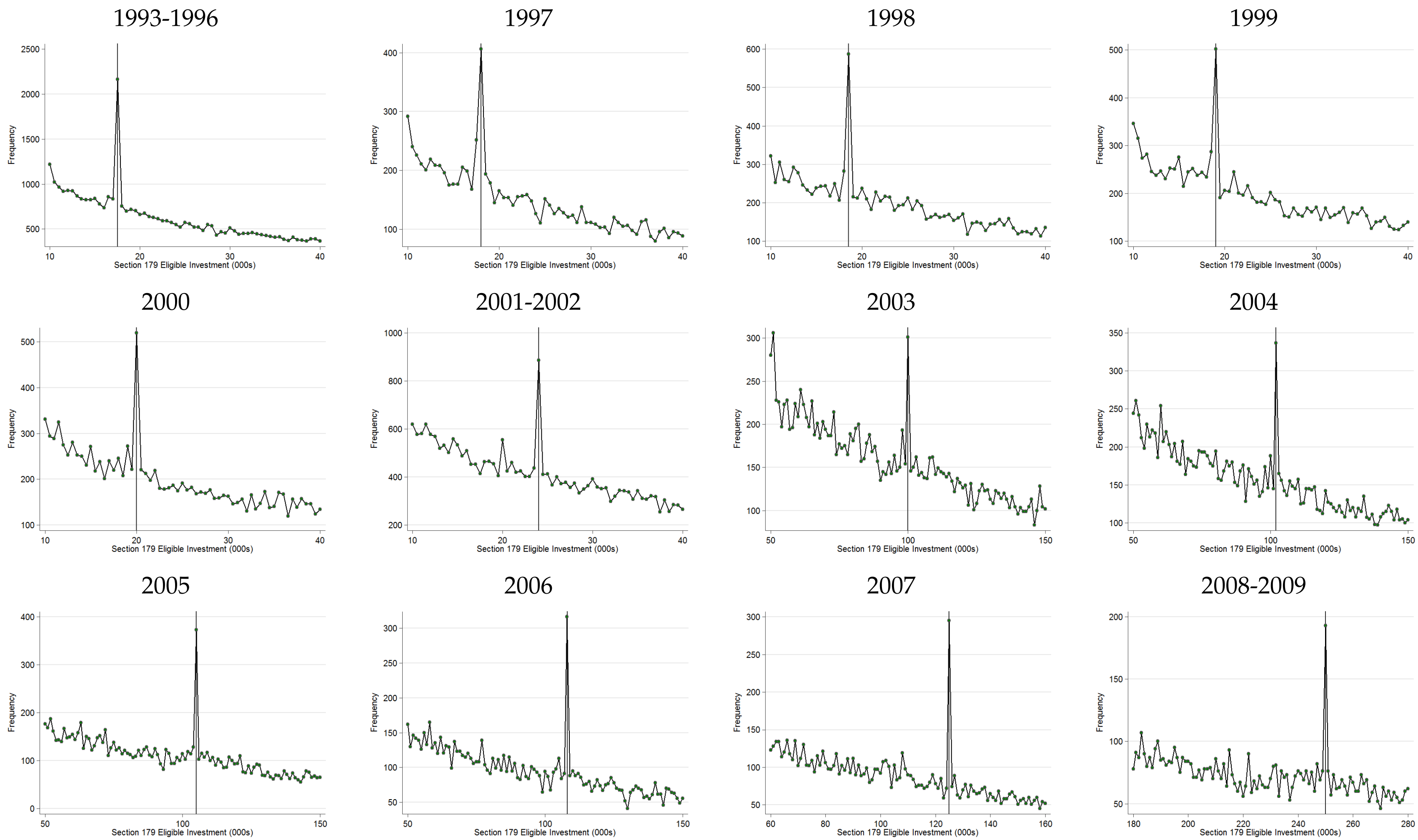
[48]Used equipment accounts for approximately six percent of equipment investment (Kitchen and Knittel, 2011).

[49]Excess mass ratios are computed using the algorithm and code in Chetty et al. (2011).

Figure 2.3: *Depreciation Schedule Salience*



Notes: These figures illustrate the salience of nonlinearities in the depreciation schedule. They show sharp bunching of Section 179 eligible investment around the depreciation schedule kink from 1993 through 2009. Each plot is a histogram of eligible investment in our sample in the region of the maximum deduction for a year or group of years. Each dot represents the number of firms in a $250 bin. The vertical lines correspond to the kink point for that year or group of years. Bunching behavior by geography serves as a proxy for tax code sophistication or state conformity with federal depreciation rules.

In general, evidence of bunching at kink points reflects a mix of reporting and real responses.[50] The bunching evidence is informative in either case because these are both behavioral responses, which show whether firms understand and respond to the schedule. In the next section, we study managerial myopia by comparing bunching activity across different groups of firms. This test does not depend on whether the response is real or reported.

We can interact the bunching evidence with the basic regression model identifying the response to bonus. The design of the test generates control and treatment groups from the notion that firms differ in their tax code knowhow.[51] We compute geographic proxies of investment schedule sophistication through measuring the local propensity to bunch at the Section 179 kink point. We use the low information areas as cross-sectional counterfactuals for the high information areas. We then separately estimate the baseline model for each group, effectively providing a difference-in-difference-in-differences estimate of the bonus response.

We group firms by two-digit ZIP code, which is the lowest level of aggregation that permits a reliable measure of bunching. For each ZIP-2, we pool all years and compute the fraction of firms within \$10,000 of the kink who bunch within \$250 of it. This provides the sorting variable. In this design, more bunching in a region indicates more awareness of the tax code for that region. So, we should expect the growth in investment during bonus periods to be increasing in the level of bunching. Columns seven and eight of Table 2.4

[50]See Saez (2010) for a discussion of this point. The bonus difference-in-differences (DD) design is less vulnerable to misreporting. In that design, we can confirm the response by looking at other outcomes. In addition, the DD estimator is much less sensitive to misreporting by a small fraction of total investment. Moreover, the sample contains many firms who use external auditors, for whom misreporting investment entails substantial risk and little benefit. Last, our conversations with tax preparers and corporate tax officers suggest that misreporting investment is an inferior way to avoid taxes. This is because investment purchases are typically easily verifiable, require receipts when audited, and usually reduce current taxable income by just a fraction of each dollar claimed as spent. In the case of investment expenses depreciated over multiple years, the audit risk of misreporting is also extended over the entire depreciation schedule.

[51]This test follows the design of Chetty, Friedman and Saez (2013), who use geographic differences in individual bunching at a kink in the Earned Income Tax Credit schedule to study the labor supply response to taxes. An alternative explanation for the differences we observe is state differences in conformity with federal bonus rules (Kitchen and Knittel, 2011).

show that indeed the high bunching areas display a stronger response to bonus than do the low bunching areas.[52]

Substitution Margins and External Finance. We ask whether increased investment involves substitution away from payroll or equipment rentals, how firms finance their additional investment, and whether the increased investment reflects intertemporal substitution or new investment. Understanding substitution margins is critical for assessing the macroeconomic impact of these policies and provides further indication of whether the observed response is real. Studying external finance responses helps us understand how firms paid for new investments.

Table 2.5 presents estimates of the intratemporal and intertemporal substitution margins. These regressions follow the baseline specification in equation 2.4.1, with a different left hand side variable. For rents, payroll and debt, we focus on flows (namely, differences in logs) as outcomes that match investment most closely. For payouts, we study an indicator for whether dividends are non-zero.

How flexible is the rent-versus-own margin for equipment investment? If firms simply shift away from leasing to take advantage of the tax benefits of buying, then the aggregate impact of these policies will be minimal. In their tax returns, firms separately report rental payments for computing net income. Unfortunately, this item does not permit decomposition into equipment and structures leasing. Acknowledging this limitation, we ask what effect bonus depreciation had on changes in rental payments. The first column of Table 2.5 shows that growth in rental payments did not slow due to bonus, but rather increased somewhat. Thus we do not find evidence of substitution away from equipment leasing. The second column of Table 2.5 reports the effect of bonus on growth in non-officer payrolls. Again, we find no evidence of substitution, but rather coincident growth of payroll. Finding limited substitution in both leasing and employment makes it more likely that bonus incentives caused more output.

[52]Specifically, we compare the top and bottom three deciles of local bunching.

Table 2.5: *Substitution Margins and External Finance*

	Dependent Variable				
	ΔRents	ΔPayroll	ΔDebt	Payer?	Investment
	(1)	(2)	(3)	(4)	(5)
$z_{N,t}$	0.75**	1.49***	1.84***	-0.36***	4.22***
	(0.26)	(0.20)	(0.21)	(0.089)	(0.62)
$z_{N,t-2}$					-0.86
					(0.69)
Observations	574305	624918	642546	818576	476734
Clusters (Firms)	98443	102043	103868	128150	84777
R^2	0.18	0.23	0.20	0.68	0.76

Notes: This table estimates regressions of the form

$$Y_{it} = \alpha_i + \beta z_{N,t} + \gamma X_{it} + \delta_t + \varepsilon_{it}$$

where Y_{it} equals the difference in the logarithm of the dependent variable in columns (1) through (3). In column (4), the dependent variable is an indicator for positive dividend payments. $z_{N,t}$ is the present value of a dollar of eligible investment computed at the four-digit NAICS industry level, taking into account periods of bonus depreciation. Column (5) includes contemporaneous and twice lagged $z_{N,t}$. All regressions include firm and year fixed effects. Standard errors clustered at the firm level are in parentheses.

While increased depreciation deductions do allow firms to reduce their tax bills and keep more cash inside the firm, they must still raise adequate financing to make the purchases in the first place. This point is especially critical if firms thought to be in tight financial positions respond more. Here, we test whether bonus incentives affect net issuance of debt and payout policy. Columns (3) and (4) of Table 2.5 provide some insight. Increased equipment investment appears to coincide with significantly expanded borrowing and reduced payouts.

We assess the extent of intertemporal substitution using a model that includes both contemporaneous z and lagged z. In the case of temporary incentives, the shifting of investment from the high tax future to the low tax present offers a potential source of amplification (Abel, 1982; House and Shapiro, 2008). Our data often do not include the fiscal year month, so it is possible that we are marking some years as t when they should be $t-1$ or $t+1$. For most of our tests, this issue introduces an attenuation but no systematic bias. However, when testing for intertemporal substitution, we want to be sure that lagged

z measures past policy changes. Thus column (6) of Table 2.5 includes regressions with twice lagged z added to the baseline bonus model. The coefficient on lagged z is negative but not distinguishable from zero and including lagged z does not alter the coefficient on contemporaneous z. This implies limited intertemporal shifting of investment, which further motivates our study of amplification through financial frictions.

Summary. Bonus depreciation has a large effect on investment, and spurious time-varying industry factors cannot explain this fact. Such factors would cause parallel trends to fail in the years prior to bonus. They would lead to different estimates in recessions marked by weakness in different industries. They would lead ineligible investment to expand. They would attenuate the estimated effect when regressions include flexible industry-by-time controls. And they would lead to a similar response across geographies where firms pay more and less attention to the depreciation schedule. The facts do not match these predictions. Section 2.5, which presents heterogeneous effects by firm size and tax position, further contradicts the omitted industry factor story.

These investment responses directly correspond to take-up of depreciation incentives—bonus take-up rates rise with the policy's generosity and many firms sharply bunch around the Section 179 kink point—in contrast to recent work on partial salience of sales taxes (Chetty, Looney and Kroft, 2009) and the nonresponse of investment to dividend tax changes (Yagan, 2013). Net investment responds to bonus depreciation as well, even though the reported balance sheet items do not affect taxable income. Debt issuance increases because of bonus depreciation and payroll and dividend payments—which are double reported—respond as well. Thus the observed response is a policy response that does not reflect a mere reporting response, but rather reflects real economic behavior.

2.5 Explaining the Large Response with Financial Frictions

The large response of investment to bonus depreciation is not consistent with a frictionless model of firm behavior: the magnitudes imply implausibly high discount rates. In this section, we explore alternative models that can generate high effective discount rates and thus reconcile our estimates with past work.

One alternative is costly external finance, which raises the total discount rate firms apply to evaluate projects. Our rich data environment enables us to study how the investment response to tax incentives interacts with costly external finance. We perform a series of split sample tests, using several common markers of ex ante financial constraints.[53] Consistent with this story, firms more likely to depend on costly external finance—small firms, non-dividend payers and firms with low levels of cash—respond more strongly to bonus.

Another alternative model is managerial myopia, which raises effective discount rates by sharply discounting the future relative to the present. Consistent with this story, firms only respond to investment incentives when the policy immediately generates after-tax cash flows. For firms with positive taxable income before depreciation, expanding investment reduces this year's tax bill and returns extra cash to the firm today. Firms without this immediate incentive can still carry forward the deductions incurred but must wait to receive the tax benefits.[54] We present evidence that, for both Section 179 and bonus depreciation, this latter incentive is weak, and differences in growth opportunities cannot explain this fact.

2.5.1 Heterogeneous Responses by Ex Ante Financial Constraints

We divide the sample along several markers of ex ante financial constraints used elsewhere in the literature. Even for private unlisted firms, we can still measure size, payout frequency and proxies for balance sheet strength. Panel (b) of Figure 2.2 plots elasticities and confidence

[53] See Fazzari, Hubbard and Petersen (1988*a*) for an early application of this methodology and Almeida, Campello and Weisbach (2004) and Chaney, Sraer and Thesmar (2012) for recent examples.

[54] In the code, current loss firms have the option to "carry back" losses against past taxable income. The IRS then credits the firm with a tax refund. Our logic assumes that firms have limited loss carryback opportunities because, in the data, we find low take-up rates of carrybacks. Furthermore, carrybacks create a bias against our finding a difference between taxable and nontaxable firms, because carrybacks create immediate incentives for the nontaxable group.

bands from regressions run for each of ten deciles based on average sales.[55] The smallest firms in the sample show the largest response to bonus. These estimates help us reconcile our findings with past studies. Larger firms show user cost elasticities in line with the findings surveyed in Hassett and Hubbard (2002). It is only the smaller firms, for whom data were previously unavailable, that yield estimates outside the consensus range.

Table 2.6 presents a statistical test of the difference in elasticities across three markers of ex ante constraints. For the sales regressions, we split the sample into deciles based on average sales and compare the bottom three to the top three deciles.[56] The average semi-elasticity for small firms is twice that for large firms and statistically significantly different with a p-value of 0.03.[57] The second two columns present separate estimates for firms who paid a dividend in any of the three years prior to the first round of bonus depreciation.[58] The non-paying firms are significantly more responsive.

Our third sample split is based on whether firms enter the bonus period with relatively low levels of liquid assets. We run a regression of liquid assets on a ten-piece linear spline in total assets plus fixed effects for four-digit industry, time, and corporate form. We sort firm-year observations based on the residuals from this regression lagged by one year, and then report in the last two columns of Table 2.6 separate estimates for the top and bottom three deciles. Note that this sort is uncorrelated with firm size by construction. The estimates are reported in the last two columns of Table 2.6. The results using this marker of liquidity parallel those in the size and dividend tests, with the low liquidity firms yielding an estimate of 7.2 as compared to 2.8 for the high liquidity firms.

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[55] Specifically, we use average sales from the three years before each bonus period. We use as many of these six years as are available for each firm.

[56] When we measure size with total assets or payroll, the size results are unchanged.

[57] Cross equation tests are based on seemingly unrelated regressions with a variance-covariance matrix clustered at the firm level.

[58] We only use the first round of bonus for the dividend split. The dividend tax cut of 2003, which had a strong effect on corporate payouts (Yagan, 2013), may have influenced the stability of this marker for the later period.

Table 2.6: *Heterogeneity by Ex Ante Constraints*

	Sales		Div Payer?		Lagged Cash	
	Small	Big	No	Yes	Low	High
$z_{N,t}$	6.29***	3.22***	5.98***	3.67***	7.21***	2.76**
	(1.21)	(0.76)	(0.88)	(0.97)	(1.38)	(0.88)
Equality Test	$p = .030$		$p = .079$		$p = .000$	
Observations	177620	255266	274809	127523	176893	180933
Clusters (Firms)	29618	29637	39195	12543	45824	48936
R^2	0.44	0.76	0.69	0.80	0.81	0.76

Notes: This table estimates regressions from the baseline intensive margin specification presented in Table 2.3. We split the sample based on pre-policy markers of financial constraints. For the size splits, we divide the sample into deciles based on the mean value of sales, with the mean taken over years 1998 through 2000. Small firms fall into the bottom three deciles and big firms fall into the top three deciles. For the dividend payer split, we divide the sample based on whether the firm paid a dividend in any of the three years from 1998 through 2000. The dividend split only includes C corporations. The lagged cash split is based on lagged residuals from a regression of liquid assets on a ten piece spline in total assets and fixed effects for four-digit industry, year and corporate form. The comparison is between the top three and bottom three deciles of these lagged residuals. All regressions include firm and year fixed effects. Standard errors clustered at the firm level are in parentheses.

These constraint markers are imperfect.[59] First, they do not directly measure the external finance cost faced by new firms. This concern would tend to bias any differences existing between groups toward zero, and thus against the results we present. A second concern with sample splitting is that the splitting criteria are correlated with the investment error term and so may bias the estimated coefficient of interest (Bond and Van Reenen, 2007). This issue is important for investment-cash flow sensitivity tests because cash flow is likely correlated with other components of the investment error term. Because our setting features plausibly exogenous policy variation at the industry level, this concern is less important here. The key assumption we make is that interacting our splitting criterion, measured prior to the policy change, with the policy variable and the year effects enables a valid difference-in-differences design for each group.

[59]Criticism of split sample markers dates back to Poterba's comments in Fazzari, Hubbard and Petersen (1988*a*). See Farre-Mensa and Ljungqvist (2013) for a more recent assessment of their value in samples of public and private companies.

2.5.2 Heterogeneous Responses by Tax Position

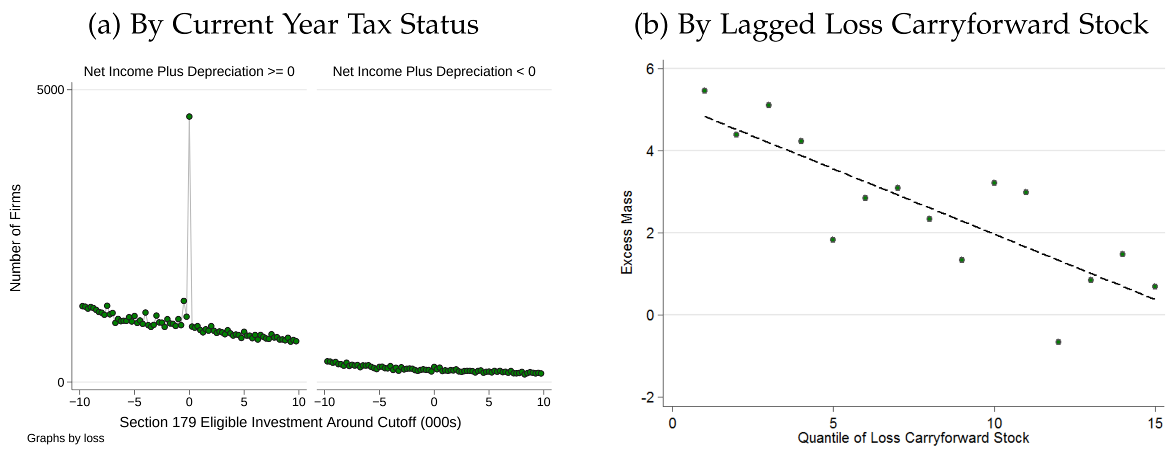
The Section 179 bunching environment offers an ideal setting for documenting the immediacy of investment responses to depreciation incentives. The simple idea is to separate firms based on whether their investment decisions will fully offset current year taxable income, or whether deductions will have to be carried forward to future years. We choose net income before depreciation expense as our sorting variable. Firms for which this variable is positive have an immediate incentive to invest and reduce their current tax bill. If firms for which this variable is negative show an attenuated investment response and these groups are sufficiently similar, we can infer that the immediate benefit accounts for this difference.

The panels of Figure 2.4 starkly confirm this intuition. In Panel (a), we pool all years in the sample, recenter eligible investment around the year's respective kink, and split the sample according to a firm's taxable status. Firms in the left graph have positive net income before depreciation and firms in the right graph have negative net income before depreciation. For firms below the kink on the left, a dollar of Section 179 spending reduces taxable income by a dollar in the current year. Retiming investment from the beginning of next fiscal year to the end of the current fiscal year can have a large and immediate effect on the firm's tax liability. For firms below the kink on the right, the incentive is weaker because the deduction only adds to current year losses, deferring recognition of this deduction until future profitable years. As the figure demonstrates, firms with the immediate incentive to bunch do so dramatically, while firms with the weaker, forward-looking incentive do not bunch at all.[60]

One objection to the taxable versus nontaxable split is that nontaxable firms have poor growth opportunities and so are not comparable to taxable firms. We address this objection in two ways. First, we restrict the sample to firms very near the zero net income before depreciation threshold to see whether the difference persists when we exclude firms with large losses. Panel (a) of Appendix Figure B.1 plots bunch ratios for taxable and nontaxable

[60] On average, half of the nontaxable firms transition to taxable status in the next year. Thus this provides further evidence against amplification through intertemporal substitution, in which nontaxable firms expecting higher future taxes should also respond.

Figure 2.4: *Bunching Behavior and Tax Incentives*



Notes: These figures illustrate how bunching behavior responds to tax incentives. Firms bunch less when eligible investment provides less cash back now. Panel (a) splits the sample based on whether firm net income before depreciation is greater than or less than zero. Firms with net income before depreciation less than zero can carry back or forward deductions from eligible investment but have no more current taxable income to shield. Panel (b) groups firms with current year taxable income based on the size of their prior loss carryforward stocks. The x-axis measures increasing loss carryforward stocks relative to current year income. The y-axis measures the excess mass at the kink point for that group. Firms with more alternative tax shields find investment a less useful tax shield and therefore bunch less.

firms, estimated within a narrow bandwidth of the tax status threshold. The difference in bunching appears almost immediately away from zero, with the confidence bands separating after we include firms within \$50 thousand dollars of the threshold. For loss firms, the observed pattern cannot be distinguished from a smooth distribution, even for firms very close to positive tax position. The bunching difference for nontaxable firms is not driven by firms making very large losses.

Table 2.7 replicates the tax status split idea in the context of bonus depreciation. We modify the intensive margin model from Table 2.3 by interacting all variables with a taxable indicator based on whether net income before depreciation is positive or negative.[61] According to these regressions and consistent with bunching results, the positive effect of bonus depreciation on investment is concentrated exclusively among taxable firms. The semi-elasticity is statistically indistinguishable from zero for nontaxable firms, while it is 3.8 for taxable firms. In Panel (b) of Appendix Figure B.1, we repeat the narrow bandwidth test for bonus depreciation. The figure plots the coefficients on the interaction of taxable and nontaxable status with the policy variable. The difference in coefficients in Table 2.7 emerges within \$50 thousand of the tax status threshold, and these coefficients are statistically distinguishable within \$100 thousand of the threshold. Here as well, the results are not driven by differences for firms far from positive tax positions.

To further address the concern about nontaxable firms, Panel (b) of Figure 2.4 uses differences within the group of taxable firms. This plot shows again that bunching is due to tax planning with regard to the immediate potential benefit. Here, we divide profitable firms by their stock of loss carryforwards in the previous year. Each dot in this plot represents a bunching histogram where the y-axis measures the degree of bunching using the excess mass estimator in Chetty et al. (2011). The groups are sorted according to the ratio of lagged loss carryforward stock to current year net income before depreciation, which proxies for the availability of alternative tax shields. The scatter clearly indicates a negative relationship

[61] That is, we interact z, any controls, and the time fixed effects with the taxable indicator. We do not interact the firm effects with the taxable indicator.

Table 2.7: *Heterogeneity by Tax Position*

	LHS Variable is Log(Investment)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Taxable $\times z_{N,t}$	3.83***	3.08***	1.95*	6.43***	4.32***	4.15***	
	(0.79)	(0.93)	(0.92)	(1.46)	(0.96)	(0.82)	
$z_{N,t}$	-0.15	0.60	0.38	-3.03*	-0.69	0.88	5.68***
	(0.90)	(1.05)	(1.06)	(1.55)	(1.15)	(0.94)	(1.70)
Medium LCF $\times z_{N,t}$							-2.56
							(1.46)
High LCF $\times z_{N,t}$							-3.70*
							(1.55)
$CF_{it}/K_{i,t-1}$		0.14***					
		(0.028)					
Taxable $\times CF_{it}/K_{i,t-1}$		0.27***					
		(0.035)					
Observations	735341	580422	514035	221306	585914	722262	119628
Clusters (Firms)	128001	100883	109678	63699	107985	124962	40282
R^2	0.71	0.74	0.74	0.80	0.73	0.72	0.84
Controls	No	No	No	No	Yes	No	No
Industry Trends	No	No	No	No	No	Yes	No

Notes: This table estimates regressions from each intensive margin in columns (1) through (6) specification presented in Table 2.3. For each firm year, we generate an indicator based on whether a firm is in taxable position prior to depreciation expense. We fully interact this indicator with all controls and the time effects. Column (7) splits taxable firms into three groups based on the size of their lagged loss carryforward stocks relative to net income before depreciation. We interact these group indicators with $z_{N,t}$ and the time effects. Only firms with nonzero stocks of lagged loss carryforwards are included. All regressions include firm and year fixed effects. Standard errors clustered at the firm level are in parentheses.

between the presence of this alternative tax shield and the extent of eligible investment manipulation.

We confirm this pattern in the bonus setting. Column (7) of Table 2.7 focuses on the group of taxable firms with non-zero stocks of lagged loss carryforwards. We split this group into three subgroups based on the size of their carryforward stock. Firms with large stocks of loss carryforwards display a semi-elasticity with respect to z of 2 compared to a semi-elasticity of 5.7 for firms with low loss carryforward stocks.

The finding for nontaxable firms contradicts a simple model of costly external finance, because firms neglect how the policy affects borrowing in the future. On the other hand, firms cannot be too myopic because the investment decision itself only pays off in the future. Thus for myopia to be the explanation, firms must use different accounts to think about investment decisions and the tax implications. Moreover, the myopia story must also explain the finding for financially constrained firms—are small firms, non-dividend payers and firms with low levels of cash more myopic?

The facts presented in this section—the stronger response for financially constrained firms and the nonresponse for nontaxable firms—do not match the predictions of a frictionless model. The facts point instead toward models in which costly external finance matters and current benefits outweigh future benefits, with neither alternative being obviously redundant.

2.5.3 Discount Rates and the Shadow Cost of Funds

Taken together, our empirical findings emphasize a financial frictions channel for how investment incentives work. We use a standard investment model to quantify the importance of this channel. Specifically, we ask what is the marginal value of cash, λ, implied by our financial constraint split sample analysis, and what is the discount term, β, implied by our tax status split sample analysis. The answers allow us to summarize our findings through an implied discount rate that firms seem to apply in evaluating investment incentives.

In Appendix Section B.1, we derive the comparative static for investment with respect to the bonus depreciation term θ:

$$I \cdot \varepsilon_{I,\theta} \equiv \frac{\partial I}{\partial \theta} = \frac{(1+\lambda)p_I}{\psi_{II}} \frac{\partial z}{\partial \theta} > 0 \tag{2.5.1}$$

where $\varepsilon_{I,\theta}$ is the semi-elasticity of investment with respect to θ, p_I is the price of investment, ψ_{II} is the second derivative of the adjustment cost function, and z is defined as in (2.2.3). In the Appendix, we state assumptions under which $I \cdot \psi_{II}$ will be equal across groups.[62] Under these assumptions, we can derive two empirical moments that combine our estimates for constrained and unconstrained firms and for taxable and nontaxable firms and yield simple formulas for λ and β.

The first empirical moment we use compares the estimated response with respect to bonus for constrained and unconstrained firms. Assuming constrained firms face shadow price λ_C and unconstrained firms face shadow price λ_U, we take the ratio of comparative statics:

$$\frac{\varepsilon^C_{I,z}}{\varepsilon^U_{I,z}} \equiv m_1 = \frac{\partial I/\partial\theta|_{\lambda_C}}{\partial I/\partial\theta|_{\lambda_U}} = \frac{1+\lambda_C}{1+\lambda_U} = 1 + \frac{\Delta\lambda}{1+\lambda_U}, \tag{2.5.2}$$

which reveals an implied credit spread between constrained and unconstrained firms. Table 2.8 presents m_1 for each pair of estimates in Table 2.6. λ is the shadow price of relaxing the firm's borrowing constraint. An alternative interpretation is that every after-tax dollar inside the firm is worth $1+\lambda$ dollars outside the firm. Our estimates reveal that, for financially constrained firms, a dollar inside the firm is worth \$2.06 on average outside the firm.

Is this estimate reasonable? There are not many existing benchmarks. Faulkender and Wang (2006) attempt a calculation with a very different methodology, but that ultimately arrives at a similar conclusion. They estimate the value of changes in cash in excess return regressions, while attempting to control for a host of omitted factors. They find that for low payout firms and for small firms the value of a dollar of after-tax cash is worth \$1.67 and \$1.62, respectively. For these firms' unconstrained counterparts, a dollar is only worth \$1.07 and \$1.12. The spreads in their study are comparable to ours, especially considering their

[62] That is, we assume linear homogeneity of the marginal adjustment cost function. Nearly all studies in the literature make this assumption, which is necessary for example for marginal q to equal average Q.

Table 2.8: *Calibrated Moments*

Shadow Cost of Funds Calibration				
	Mean Sales	Dividend Payers	Lagged Cash	Average
m_1	1.95	1.63	2.61	2.06
$\lambda_C\|_{\lambda_U=0}$	0.95	0.63	1.61	1.06
Discount Factor Calibration				
	p	ϕ	r	β
High ϕ	0.51	0.88	0.07	0.82
Medium ϕ	0.31	0.82	0.07	0.84
Low ϕ	0.05	0.42	0.07	0.97

Notes: This table computes empirical estimates for m_1 and m_2, as defined in the text. m_1 reveals an implied credit spread between constrained and unconstrained firms. m_2 reveals the discount factor firms apply to all future cash flows relative to current flows.

exercise operates within a group of firms we consider to be relatively unconstrained.[63]

We define a second empirical moment that compares taxable and nontaxable firms:

$$\frac{\varepsilon_{I,z}^{\gamma=0}}{\varepsilon_{I,z}^{\gamma=1}} \equiv m_2 = \frac{\partial I/\partial\theta|_{\gamma=0}}{\partial I/\partial\theta|_{\gamma=1}} = \beta\phi\frac{1-z_t^0(1)}{1-z_t^0(\beta)} \tag{2.5.3}$$

where ϕ is a discounter that reflects the average arrival of the taxable status event for nontaxable firms. We proxy for ϕ by assuming a fixed transition probability p for nontaxable firms and an infinite horizon for realizing carryforwards.[64] This implies $\phi = p/(p+r)$.[65] We calibrate p using tax status transitions in the data. Specifically, we measure the probability that a currently nontaxable firm has sufficient income in the next year for depreciation deductions to affect next year's tax bill. In our data, this probability is 0.51 if future loss carryforwards are not used and 0.31 if all future carryforwards are deducted prior to

[63]Similarly, Koijen and Yogo (2012) find that a relaxed borrowing constraint for life insurers during the financial crisis is worth $2.32 per dollar of inside capital.

[64]The actual expiration period for carryforwards is twenty years.

[65]That is, the expected arrival is $p/(1+r) + (1-p)p(1+r)^{-2} + (1-p)^2p(1+r)^{-3} + \cdots = p/(1+r) \cdot [1/(1-(1-p)/(1+r))] = p/(p+r)$.

considering investment incentives.[66] We also consider an extreme transition probability of 0.05.

Note the external finance wedge falls out of this expression. This is true as long as average shadow costs are the same across taxable and nontaxable groups. To maintain this assumption, we use our loss carryforward group estimates to calibrate m_2. That is, we estimate semi-elasticities within the group of taxable firms sorted according to their past stocks of alternative tax shields. For firms with large loss carryforward stocks relative to current income, the marginal dollar of investment is unlikely to affect this year's tax bill. At the same time, we have less reason to believe these firms face substantially worse growth opportunities or tighter financial constraints. This biases our estimates of β toward the neoclassical benchmark of β equal to one.

Applying the estimates from the last column of Table 2.7 yields a value for m_2 of $0.35 (= (5.68 - 3.7)/5.68)$. For $p = 0.51$, this maps to an implied discount factor (β) of 0.82.[67] The more conservative $p = 0.31$ hardly affects the calculation; only a counterfactual, extreme expectation of loss persistence can justify a β near the neoclassical benchmark. Ignoring for the moment the other discount terms, β equal to 0.82 implies a discount rate of approximately 20 percent. We are not aware of studies that attempt to measure discount factors such as this for firms. Prior studies on individual decision making have found similar magnitudes for short term discount rates in both lab and field experiments.[68]

The discounting implied by β says that one dollar next year is worth 82 cents, before taking into account risk or the shadow cost of funds. If we then apply the assumed risk adjusted rate of 7 percent and the estimated shadow cost of funds of 1.06, we find that a dollar next year is worth approximately 38 cents today for the financially constrained

[66]Auerbach and Poterba (1987*b*) note more persistence of nontaxable positions than we do. Our measure is based on net income before depreciation, to capture the state of having the next dollar of investment affect this year's tax bill. Their measure is based on whether firms exhaust their carryforward stocks.

[67]We evaluate $z_t^0(1)$ at the sample average of 0.88 and use the sample average first year deduction of 0.18 to set $z_t^0(\beta) = 0.18 + \beta \times 0.7$.

[68]Laibson D. Repetto and Tobacman (2007) estimate short term discount rates of 40 percent in the context of individual saving decisions. In a more general model, they estimate a short run discount rate of 15 percent and a long run rate of 3 percent.

firms in our sample. This substantial discount is not surprising, given the starkness of the reduced form empirical results: nontaxable firms seem to ignore the future benefits and small, financially constrained firms seem to value highly the immediate cash back due to bonus depreciation. In the model, we use costly external finance and myopia to describe the observed deviations from a rational benchmark, but the exercise performed here provides just one of several plausible calibrations of this basic fact. In general, models of firm behavior that do not generate high discount rates are unlikely to fit the data for most firms.

2.6 Conclusion

This paper combines methods from public and applied economics with insights from finance to answer a first order macroeconomic question: how do taxes affect investment behavior in the presence of financial frictions? We find that firms respond strongly to incentives that directly target investment decisions. Our heterogeneity results—that the investment response is larger for financially constrained firms, but only when the benefit is immediate—show that financial frictions are critical for understanding investment behavior.

The results point toward a set of models in which costly external finance matters and firms place more weight on current benefits than they would in a frictionless model. Whether the high implied discount rate reflects an external finance wedge, managerial myopia, agency considerations or a mix of these is an important question for future research. Further study of the external finance mechanism would be valuable. A deeper study of the employment effects of these policies is of direct interest to macroeconomists and policymakers.

A related question for future research concerns the effects of tax planning. How do tax preparers affect the decision to take up these policies? More generally, do firms focus on minimizing current taxes at the possible expense of future payoffs? The answer to these questions might shed light on the role of agency problems and firm learning about optimal management practices.

The empirical results imply that policies which target investment directly and yield immediate payoffs are most likely to influence investment activity. Policies that target financial constraints, such as direct loans, might have a similar effect if conditional on the investment decision. In comparison to studies of consumer durable goods, we find less evidence of intertemporal shifting, but more work on this question is needed. Data from the period following the recent stimulus, once available, will be very useful.

Chapter 3

Do the Victors Share the Spoils? Evidence from US House Elections, 1982-2006

3.1 Introduction

How does US congressional politics affect the distribution of federal spending across voters? A substantial body of research has focused on which legislators receive more spending for their districts, but less is known about the allocation of federal spending across constituents within districts. Theoretical models of elections where candidates propose spending allocations across a distribution of voters on a left-right ideological dimension make competing predictions over whether swing, co-partisan, or both types of voters will benefit disproportionately. I test between these models within a sub-sample of congressional districts from the US House of Representatives by measuring the within-district distribution of per-capita federal spending across counties. I find that neither counties with co-partisan voters nor swing voters receive disproportionately more spending.

Much of the distributive politics literature of US federal spending has focused on which legislators receive the most spending for their districts. Theoretically, legislative bargaining models have explored how recognition rules among legislators affect the equilibrium distribution of benefits when legislators engage in repeated rounds of voting over proposed spending allocations (Baron and Ferejohn 1989, McCarty 2000, McKelvey and Riezman 1992). In these models, the first legislator recognized to propose an allocation has an advantage due to the costs of legislative delay. Consequently, the empirical literature has measured how predictors of legislator proposal power affect the distribution of US federal spending across congressional districts and states, such as partisan alignment with congressional majorities (Albouy 2013, Balla et al. 2002, Bickers and Stein 2000) and the president (Berry, Burden and Howell 2010, Larcinese, Rizzo and Testa 2006), congressional committees (Alvarez and Lowry 1997, Carsey and Rundquist 1999, Ferejohn 1974, Heitshusen 2001, Levitt and Poterba 1999, Knight 2005), and bicameralism (Lauderdale 2008, Shepsle et al. 2009). Most recently, Berry, Burden and Howell (2010) finds larger effects for partisan alignment than congressional committees on the distribution of federal spending aggregates across congressional districts.

All of these studies assume that legislators want to capture federal spending for their constituents, but how should they distribute that spending among their constituents? Are all constituents political equals? A theoretical literature in distributive politics considers this question in the context of two-candidate elections. When voters differ in their ideological orientation on a left-right dimension, alternative models make competing predictions for whether candidates propose spending allocations that disproportionately benefit co-partisan or swing voters (Dixit and Londregan 1996, Hirano, Snyder and Ting 2009, Lindbeck and Weibull 1987). By comparing the within-district distribution of spending and voting patterns across counties, my empirical exercise tests whether the predictions from those models bear out in meaningful ways for federal spending aggregates in the context of the US House of Representatives.

Within individual congressional districts and each Congress, I estimate the difference in per-capita federal spending across counties using a 26-year panel for a range of spending measures. Although legislators could use geography and demography in a variety of strategies to target federal spending to specific groups of voters,[1] there is a substantial amount of spending that they are unlikely to manipulate for this purpose, such as broad-based entitlement programs. I thus report results for a range of spending measures from broad aggregates to place-specific grants and contracts to manage this trade-off between broad and manipulable spending. In the discussion of my results that follow, I will focus on my non-entitlement measure which excludes the largest entitlement programs, Social Security, Medicare, and Medicaid, from the total spending measure.

To characterize the distribution of spending within congressional districts, I use a rural sub-sample of districts because I can only reliably measure broad aggregates of federal spending at the county-level and many urban counties are split across multiple congressional districts. Largely because I exclude districts that do not entirely contain at least three counties, my sample includes about 39 percent of all congressional districts.[2] While my main estimates are representative for a rural sub-sample of US congressional districts, the same electoral pressures that motivate the distributive politics models that I test are present in both rural and urban districts. Furthermore, I replicate my tests when possible in the context of the US Senate where I can include urban counties.

For the within-district distribution of voters, I define partisan county quantiles using the county mean and standard deviation of the Republican vote share in presidential elections across the entire panel. Counties are defined as either co-partisan or opposition and as either swing or base. Intuitively, these relative within-district measures define counties as co-partisan when they tend to vote for the winning candidate's party and as swing when their Republican vote share exhibits greater variability from election to election.[3] While spending may be targeted to voters at a narrower geographic level, the per-capita county

[1]For example, programs such as low-income housing and agricultural subsidies that are allocated by formula can provide benefits to specific demographic groups. Alternatively, grants and contracts such as academic research awards or bridge construction can be allocated to specific places.

[2]Figure 3.1 presents two maps that depict my sample for the 98th and 110th Congresses.

[3]Within individual congressional districts and each Congress, I evenly divide all counties into Republican and Democratic halves using the panel mean, and into swing and base halves using the panel standard deviation of the Republican vote share in presidential elections. Then, I define counties as co-partisan when they share the same partisanship as their House representative and conversely for opposition counties.

means should reflect any bias in the aggregate distribution of spending across voters.

For my main results, I estimate differences in per-capita federal spending between partisan county quantiles within congressional districts of the US House of Representatives. My specifications include district-congress[4] fixed effects to focus on the relative allocation of spending across counties within each district-congress. I also include county fixed effects to absorb county-level spending that is unrelated to changes in the political environment. In this framework, my estimates of the differences between counties within each district are identified from party switches in elections and redistricting. Party switches allow me to identify the effect for co-partisan counties because the two parties have different core supporters. It cannot, however, identify the effect for swing counties because both parties have the same swing voters. Because redistricting changes the composition of counties within districts, I can use it to identify both co-partisan and swing counties as the partisan county quantiles are relative definitions among the counties within each district-congress.

I report results from three alternative strategies to estimate the within-district differences across counties. In all cases, I find reasonably precise null effects. First, I estimate the differences in log per-capita spending across partisan county quantiles in the full panel, including district-congress and county fixed effects as described above. Second, I implement a regression discontinuity design (RDD) on two-party elections in order to address concerns that congressional districts where Republicans and Democrats are elected may differ in important ways that also affect their within-district distribution of spending. Because this method relies on party switches, it can only identify the co-partisan counties. Third, I report first-difference estimates where I restrict the sample to the periods before and after redistricting where the party of the House representative also remains unchanged. Here, I isolate redistricting as the source of identification and focus on its immediate impact on the within-district distribution of spending.

For the non-entitlement measure using the full panel, I find a point estimate of -0.0064 with a standard error of 0.0091 for the co-partisan county and a point estimate of 0.0003 with

[4]Unique pair of congressional district and biannual Congress.

a standard error of 0.0158 for the swing county. To place these magnitudes in perspective, Berry, Burden and Howell (2010) finds a point estimate of 0.040 for the effect of a partisan match between a House representative and the president on a district's log federal spending in their most basic specification. Using their estimate as a baseline, I find magnitudes that are 16 percent and 0.75 percent as large. Furthermore, the estimates from the three methods that I implement respectively allow me to rule out differences in per-capita spending between co-partisan counties and base opposition counties greater than 1.1, 3.5, and 0.2 percentage points[5] under a 95 percent confidence interval. Similarly, I can rule out differences between swing counties and base opposition counties greater than 3.1 and 3.3 percentage points from the full panel and first-difference estimates.

While I find a flat distribution of spending across counties for a sub-sample of congressional districts, my results could be criticized for largely excluding urban districts if the greater overall levels of federal spending received by urban areas present more opportunities for political manipulation. Furthermore, my results could mask heterogeneous treatment effects if the capacity of House representatives to influence federal spending depends on the cooperation of other political actors or legislative seniority. In particular, previous papers find that partisan alignment increases overall levels of federal spending received by congressional districts and states (Albouy 2013, Berry, Burden and Howell 2010, Larcinese, Rizzo and Testa 2006). To address these concerns, I replicate the full panel estimates within the context of the US Senate, where I can include urban counties, and with interactions for partisan alignment with the House majority and the president, congressional committee assignments, and the bicameral nesting of House and Senate districts in the US Congress. These considerations do not substantively change my main results.

Within my sub-sample of congressional districts, I find limited evidence of substantial distortions to the within-district distribution of federal spending aggregates that favors broad groups of voters for electoral purposes. This result holds even when focusing on the most influential House representatives. None of the standard two-candidate distributive

[5] Small differences in log points approximate differences in percentage points.

politics models with ideologically heterogeneous voters predict this flat within-district distribution of spending across counties.[6] The existing literature provides at least three suggestions to understand this result. First, legislators may have chosen to adopt checks and balances in the federal budget process to prevent themselves from biasing spending toward one group of voters over another. Although individually preferable, the aggregate welfare cost from this behavior may be so great that legislators commit themselves to preventive procedures. Second, narrow self-interest may not guide voting behavior. The negligible probability that voters have of being pivotal in an election suggest that no one would vote purely on instrumental grounds. Third, voters may have imperfect information about the distribution of federal spending. If poorly informed, they may neither be able to effectively reward nor punish House representatives on this basis in elections. Instead, legislators may focus on funneling concentrated benefits to well-informed special interests.

Within the US House of Representatives, this paper is the first to my knowledge that tests two-candidate election models of distributive politics. While Berry, Burden and Howell (2010) and Martin (2003) have constructed similar data sets matching county-level federal spending to congressional districts, those papers do not capture the relative distribution of spending and voters within each district because they do not include district-congress fixed effects. Outside of the congressional context, Ansolabehere and Snyder (2006) have implemented a similar design to look at the effect of party control of US State governments on the county share of federal transfers to the state government. They find evidence that co-partisan counties receiving greater spending levels, but not swing counties.

My findings also complement previous research about the distribution of spending and voters across the United States from the perspective of the president and political parties (Larcinese, Rizzo and Testa 2006, Larcinese, Snyder and Testa 2013, Levitt and Snyder 1995). While these papers present valuable empirical facts, my context of US congressional districts provides more opportunities for causal inference. In this setting, I am able to

[6] Assuming an ideologically homogeneous set of voters, Myerson (1993) and Laslier and Picard (2002) find conditions under which two-candidate electoral competition results in uniform or near uniform balanced-budget proposals across voters.

implement a RDD on two-party elections and a first-differences design on redistricting. Furthermore, the two-candidate distributive politics models do not exactly apply to their empirical setting because the winner-takes-all feature of the electoral college and legislator districts changes the incentives faced by presidential candidates and political parties. In particular, an additional vote in a safe state does not increase a presidential candidate's count in the electoral college in presidential elections. Examining the distribution of federal spending within congressional districts allows for more plausibly causal estimates and more direct testing of the two-candidate distributive politics models.

This paper proceeds as follows: Section 3.2 reviews predictions from the theoretical distributive politics literature on the incentives that two-candidate elections create for legislators in allocating spending across voters. Section 3.3 describes in greater detail the panel construction and discusses some descriptive statistics. Section 3.4 presents my main results for the full panel, RDD, and first-difference estimates. Section 3.5 presents my robustness checks for the US Senate, partisan alignment, congressional committees, and bicameralism. Section 3.6 concludes.

3.2 Motivation

In two-candidate election models of distributive politics, candidates compete over a distribution of voters on a left-right dimension by proposing alternative spending allocations across voters under a balanced-budget rule. Voters then select a candidate on the basis of their ideological tastes and the spending allocation promised to them. Whether co-partisan or swing voters disproportionately benefit from candidate proposals depends on the model set-up. In their most basic setting, Lindbeck and Weibull (1987) find that both candidates will propose identical, uni-modal spending programs that disproportionately benefit swing voters because they are ideologically indifferent between the two candidates. In contrast, Dixit and Londregan (1996) find that co-partisan voters benefit disproportionately when they assume that candidates are better at delivering the spending programs preferred by their co-partisans. Ongoing political machines with a standing network of beneficiaries could motivate this assumption, or alternatively candidates may have a better understanding of the preferences of their co-partisans. Hirano, Snyder and Ting (2009)consider another wrinkle by adding primary elections and forward-looking voters to the basic set-up. They find that candidates will target both co-partisan and swing voters with spending because primary voters elect candidates who (i) promise them spending and (ii) can win general elections. Exploiting these conflicting predictions, I propose to test which of these models is most empirically relevant for the distribution of federal spending within a sub-sample of congressional districts.

In particular, I measure the per-capita distribution of federal spending across partisan county quantiles defined in order to capture the sub-areas of each congressional district with relatively high shares of co-partisan and swing voters. To limit endogeneity concerns about voting behavior responding to spending patterns within congressional districts, I use returns from presidential elections averaged over the entire panel. Using the panel mean and the panel standard deviation of the presidential Republican vote share, I respectively evenly sort counties into Republican and Democratic halves and into base and swing halves within each district-congress. Then, I identify counties as co-partisan when they share the same partisanship as their House representative and conversely for opposition counties.[7] Intuitively, co-partisan counties have vote shares that tend to favor the party of the winning candidate and swing counties have vote shares that vary from election to election, relative to the other counties within their district-congress. While this paper focuses on estimates that use this definition for the partisan county quantiles, I find similar results when I consider three alternatives in Appendix Section C.3 based on House elections and on a rolling window that excludes the concurrent election.[8]

With equation 3.2.1, I characterize the within-district distribution of spending across

[7]This procedure results in four types of counties: base co-partisan, swing co-partisan, swing opposition, and base opposition.

[8]Appendix Section C.3 replicates my main analysis using three alternative vote share statistics to define partisan county quantiles: moving statistics from presidential elections, panel statistics from House elections, and moving statistics from House elections. The two moving statistics include the three previous presidential elections and the five previous House elections respectively. Both exclude the concurrent election.

partisan county quantiles:

$$\begin{aligned}\log\left(\text{spending}_{cdt}\right) &= \beta_1\text{co-partisan county}_{cdt} + \beta_2\text{swing county}_{cdt} \\ &\quad +\beta_3\left(\text{co-partisan county}_{cdt} \times \text{swing county}_{cdt}\right) + \delta_c + \xi_{dt} + \epsilon_{cdt}\end{aligned} \tag{3.2.1}$$

where opposition county and base county are the omitted categories, δ_c is a fixed effect for county c, and ξ_{dt} is a fixed effect for district d-congress t. Equation 3.2.1 includes district-congress fixed effects to focus on the relative distribution of spending across counties within each district-congress. The county fixed effects absorb county-level spending that is unrelated to changes in the political environment. I also include an interaction between co-partisan and swing counties to allow for the possibility that spending in these counties, such as on local public goods, has more political value than if it only had relatively large numbers of one or the other voter type. In this specification, the variation that identifies the coefficients β_1, β_2, and β_3 are switches in the party of the House representative and redistricting that changes the collection of counties in the district. Using the log of spending, the coefficients can also be interpreted as percentage point differences with the omitted categories.

Assuming that the partisan county quantiles identify the counties with relatively high shares of co-partisan and swing voters, the alternative distributive politics models make competing predictions about the signs of the coefficients in equation 3.2.1. The basic model in Lindbeck and Weibull (1987) predicts that $\beta_1 = 0$, $\beta_2 > 0$ and $\beta_3 = 0$ because it finds that both parties disproportionately reward swing voters. Dixit and Londregan (1996) predict that $\beta_1 > 0$, $\beta_2 = 0$, and $\beta_3 = 0$ when they assume that each party has an advantage in targeting their own co-partisans. Hirano, Snyder and Ting (2009) predict that $\beta_1 > 0$, $\beta_2 > 0$, and $\beta_3 > 0$ because, while primary voters want spending for themselves, they also want spending for swing voters in order to ensure winning the general election.

In addition to estimating equation 3.2.1, I also implement (i) a RDD on two-party elections and (ii) a first-differences version of equation 3.2.1 on a sample limited to the periods before and after redistricting where the party of the House representative also remains unchanged. I use the RDD to address concerns that Republicans and Democrats may get elected to congressional districts where the within-district distribution of spending differs for reasons other than politics. With a first-differences approach, I can focus on the immediate impact of redistricting and provide estimates less subject to long-term time-series confounds.

While the theoretical distributive politics literature makes competing predictions about the results that I should find, my test does not capture all the ways in which House representatives could target spending to particular constituents. First, politicians may target constituents very narrowly, such as key interest groups or large donors, instead of the broad distribution of voters. Second, politicians may respond to voter preferences over the type of spending, which may or may not have a geographic incidence that matches the distribution of voters.[9] Third, my identification strategies do not distinguish between whether the spending allocation is determined by electoral incentives or by politician preferences. But despite these limitations, my empirical exercise reveals whether the within-district patterns of broad aggregates of federal spending across counties matches the predictions from the two-candidate distributive politics models.

3.3 Data and descriptive statistics

3.3.1 US counties panel

The panel for this paper matches 1982-2006 US House elections to 1984-2009 county-level federal spending measures. It is indexed by biannual Congresses from the 98th to the 110th US Congress. Elections and federal spending have this staggered match because elections take place the November before a new Congress starts and each congressional session passes annual appropriations legislation for the following fiscal year.[10] I also digitized biannual

[9] Albouy (2013), for example, finds that Republican legislators tend to receive more defense spending and Democratic legislators tend to receive more education and housing and urban development spending for their districts.

[10] Annual appropriations acts include a small number of multi-year appropriations. For acts passed in the years 1995-2003, multi-year appropriation amounts accounted on average for 6.1 percent of the net total (Senate

congressional district maps to match counties to congressional districts and to identify changes in congressional district boundaries.

To measure per-capita federal spending,[11] I compiled county-level spending from the *Consolidated Federal Funds Report* (CFFR) and annual county population estimates from the US Census. The CFFR tabulates domestic federal expenditures/obligations to geographic areas, using data submitted by federal agencies. It includes grants, payroll, procurement contracts, and direct payments to individuals and other entities. I construct per-capita spending measures to control for federal formulas that sometimes use population to determine allocation of federal spending across geographic areas. In Appendix Section C.3, I find similar results when I replicate the full panel estimates on spending measures divided by voter turnout in presidential and House elections.

To test how comprehensively the CFFR covers federal expenditures, I compared this measure to the historical federal expenditure totals from the Office of Management and Budget (OMB).[12] Appendix Section C.2 reports aggregate CFFR spending by Congress as a share of the OMB's historical totals. Coverage varies between 0.76 and 0.85 for the total spending measure.

In addition to total federal spending, I construct four alternative measures to exclude spending programs that House representatives are unlikely to be able to influence and target to specific groups of voters. Non-entitlement spending excludes Social Security, Medicare, and Medicaid–the largest broad-based entitlement programs–from the total measure. Discretionary spending uses an approximation to exclude mandatory spending[13] from the total measure by weighting executive agency-level spending by historical discretionary shares as

1995-2003).

[11]All spending measures are inflation-indexed to 2000 US dollars.

[12]See Appendix Section C.2 for details about the comparability of the CFFR and OMB federal spending measures.

[13]Broadly, federal spending can be divided into two categories: discretionary and mandatory spending. Discretionary spending is annually approved by the appropriations committee while mandatory spending is enacted by permanent statute and recurs regularly. House representatives may have more opportunities to influence discretionary spending because it is subject to the annual federal budget process.

reported by the OMB.[14] Grants are place-specific transfers to state and local governments and to private organizations that are distributed either by formula or are allocated for specific projects, such as research, evaluation, technical assistance, and construction. Contracts captures spending on place-specific procurement, such as construction and equipment purchases (Bureau 2010). In reporting my results, I focus on the non-entitlement measure because it provides a comprehensive measure of federal spending while excluding the largest and least likely spending programs that House representatives would manipulate to target voters. Alternatively, previous research by Lee (2003) argues that US House politics should affect the grants measure the most because House representatives can credibly claim credit for the resources received by their congressional district through this spending mechanism.[15]

The panel includes county and congressional district-level voting data for presidential and House elections. Vote share variables are normalized to the Republican margin of favor and only include votes for Democratic and Republican candidates. I also incorporated congressional committee assignment data.

3.3.2 Sample selection

The main sample for this paper excludes counties that do not uniquely match to a congressional district. These observations cannot be used in the analysis because I can only reliably measure aggregates of federal spending at the county-level and I am interested in characterizing the distribution of federal spending within congressional districts. In particular, I limit my sample to congressional districts that (i) entirely contain at least three counties (ii) hold a House election where the two front-running candidates represent the

[14] This measure is only available for the 103rd-110th US Congresses because the CFFR only started reporting spending by executive agency in the 1993 fiscal year.

[15] Albouy (2013) reports results using the grants measure. For their county analysis, Berry, Burden and Howell (2010) also limits federal spending to grants.

Republican and Democratic parties.[16] These restrictions limit my sample to about 39 percent of all US congressional districts.

Restricting my sample to congressional districts that entirely contain at least three counties results in a sample of counties less densely populated than the US average.[17] This sample exclusion could raise concerns if differential amounts of within-district targeting takes place within urban and rural districts. While the greater overall levels of per-capita federal spending received by urban districts may create more opportunities for targeting, their smaller geographic size may also make it more difficult. Furthermore, the two-candidate models that I test emphasize the influence of the same electoral incentives–present in both rural and urban districts–on the distribution of within-district spending. Although my results are only representative for a largely rural sub-sample of US congressional districts, they provide a valid test of the two-candidate distributive politics models for a subset of House representatives.

Figure 3.1 depicts the counties included in the sample for the 98th and the 110th US Congresses by shading them in light gray. The dark gray lines represent the county boundaries and the black lines represent congressional district boundaries. The map indicates that while most counties are included under the sample definition, urban counties are largely excluded because they tend to be split across multiple congressional districts. In addition, the map includes cases where large rural districts are excluded because a Republican and a Democrat did not compete in the House election for that Congress.

3.3.3 Descriptive statistics

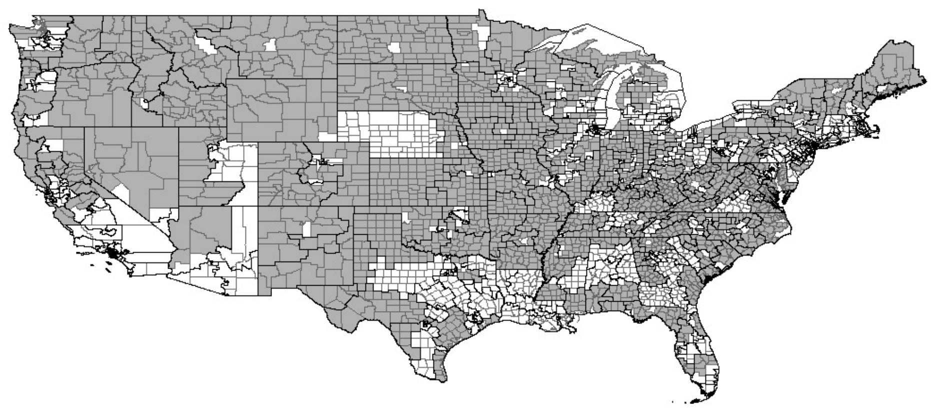
Panels A and B of Table 3.1 presents the underlying within-district variation in the presidential election measures captured by the co-partisan and swing county indicators. Panel C of Table 3.1 reports county sample summary statistics for the log per-capita federal spending

[16] Counties that contain state capitals are also excluded because a significant amount of federal spending is mechanically allocated to them for accounting purposes, and then redistributed across the state.

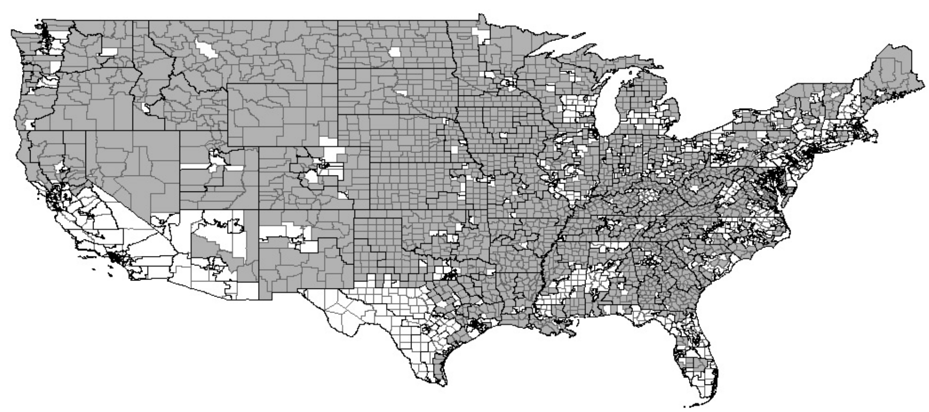
[17] Appendix Section C.2 reports mean county demographics for the United States and my sample.

Figure 3.1: *County sample maps for the 98th and 110th Congresses*

Panel A: 98th Congress



Panel B: 110th Congress



Notes: County boundaries are in dark gray. Congressional district boundaries are in black. Counties included in the sample are shaded light gray. Sample is limited to counties that uniquely match one congressional district and to districts that entirely contain at least three counties and where the two leading candidates in the House election are from the Democratic and Republican parties. Counties that contain state capitals are also excluded because federal transfers to the state government are often mechanically assigned to the state capital and ultimately reallocated across the state.

measures by base co-partisan, swing co-partisan, swing opposition, and base opposition county quantiles. All panels report summary statistics demeaned by district-congress and include the average within-district standard deviations across counties.

The two tables exhibit stark contrasts in their differences across partisan county quantile means relative to their average within-district standard deviations. Whereas the maximum of this ratio is about two among the political variables, it is at most one-quarter for the spending measures. This comparison implies that there is a lot of variation in spending measures that is unrelated to the partisan county quantiles.

Furthermore, a comparison of the partisan county quantile means for the spending measures does not suggest obvious favoritism for either co-partisan or swing counties that is consistent across spending measures. For further illustration, Figure 3.2 graphically depicts the within-district distribution of non-entitlement and grants spending using a kernel density by partisan county quantile.

3.4 Results

3.4.1 Full panel

Panel A of Table 3.2 reports estimates of equation 3.2.1. Under the non-entitlement spending measure, I find an estimate of -0.0064 with a standard error of 0.0091 for the co-partisan county and an estimate of 0.0003 with a standard error of 0.0158 for the swing county. Using a 95 percent confidence interval, these coefficients allow me to rule out differences in per-capita spending with the base opposition counties greater than 1.1 percent and 3.1 percent respectively in per-capita federal spending. The estimates for the interaction and the other spending measures are also insignificant. They have similar magnitudes and precision, with the exception of the contracts measure. For broad measures of federal spending, these results reject that either co-partisan or swing counties receive a substantially more favorable allocation of spending.

Although it is hard to sign the net bias, selection could confound these results because

Table 3.1: *Sample summary statistics of log (per-capita federal spending) demeaned by district-congress*

Panel A: County panel average in Republican vote share from presidential elections (in percentage points)

	party of the congressional representative	
	Democrat	Republican
co-partisan counties	-5.61	4.79
	(5.40)	(4.24)
opposition counties	5.16	-5.21
	(4.45)	(5.08)
mean within district-congress std. dev	6.63	6.00
	(2.95)	(2.76)
mean no. of counties per district-congress	12.96	12.88
	(10.34)	(10.79)
no. of county-congresses	12,588	15,936
no. of district-congresses	971	1,237
no. of states	45	45

Panel B: County panel standard deviation in Republican vote share from presidential elections (in percentage points)

	party of the congressional representative	
	Democrat	Republican
base counties	-1.21	-1.18
	(1.08)	(1.01)
swing counties	1.12	1.08
	(1.02)	(0.98)
mean within district-congress std. dev	1.38	1.36
	(0.57)	(0.52)
mean no. of counties per district-congress	12.96	12.88
	(10.34)	(10.79)
no. of county-congresses	12,588	15,936
no. of district-congresses	971	1,237
no. of states	45	45

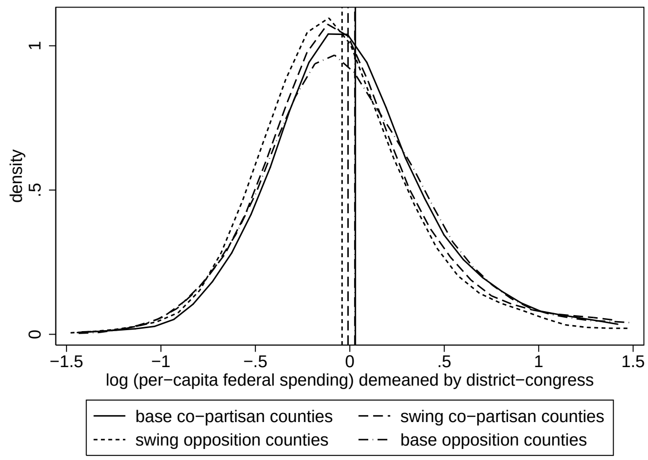
Panel C: Log (per-capita federal spending)

	total	non-entitlement	discretionary	grants	contracts
base co-partisan counties	0.0065	0.0291	0.0498	0.0352	0.0678
	(0.3072)	(0.4829)	(0.5436)	(0.4699)	(1.2197)
swing co-partisan counties	0.0050	-0.0094	-0.0312	-0.0258	-0.1102
	(0.3171)	(0.4955)	(0.5429)	(0.4628)	(1.1546)
swing opposition counties	-0.0090	-0.0410	-0.0572	-0.0431	-0.1179
	(0.3208)	(0.4902)	(0.5097)	(0.4341)	(1.1780)
base opposition counties	-0.0020	0.0268	0.0473	0.0406	0.1863
	(0.3216)	(0.5145)	(0.5287)	(0.4544)	(1.2663)
mean within district-congress std. dev.	0.2966	0.4753	0.5138	0.4103	1.2040
	(0.1651)	(0.2407)	(0.2769)	(0.1928)	(0.4579)
no. of county-congresses	28,524	28,524	17,815	28,524	28,524
no. of district-congresses	2,208	2,208	1,355	2,208	2,208
no. of states	47	47	47	47	47

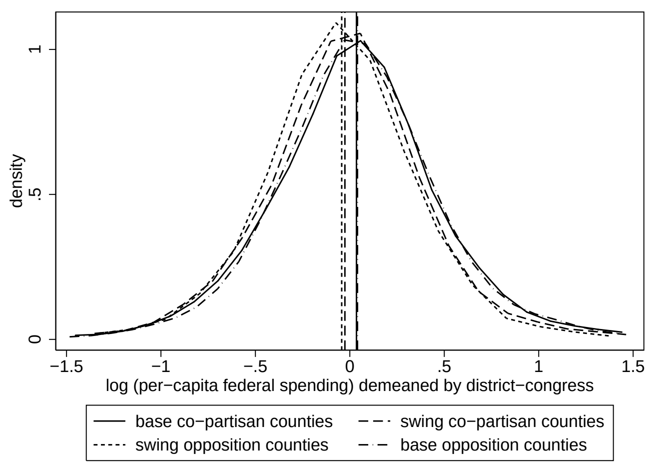
Notes: Observations reported by county quantile. Standard deviations in parentheses. All observations demeaned by district-congress. Within each district-congress and using the Republican vote share in presidential elections, counties are evenly divided into either Republican or Democratic halves by their panel average and into either swing or base halves by their panel standard deviation. Counties are defined as co-partisan when their partisanship matches their House representative in the current Congress and are defined as opposition under the converse. Non-entitlement spending excludes Social Security, Medicare, and Medicaid. Discretionary spending is approximated using historical executive agency-level shares of discretionary spending from the Office of Management and Budget and are limited to the $103-110$ *Congresses. Grants are transfers either to state and local governments or to private organizations. Contracts include only spending on procurement. See text for additional details.*

Figure 3.2: *Density plots of log per-capita spending demeaned by district-congress by county quantile*

Panel A: Non-entitlement spending



Panel B: Grants spending



Notes: Density estimates use the Epanechnikov kernel and the Silverman bandwidth $h = 1.06 \cdot \sigma \cdot N^{-1/5}$*. See text for additional details.*

Table 3.2: *US House: relative log per-capita spending across counties within congressional districts*

	log (per-capita federal spending)				
	total	non-entitlement	discretionary	grants	contracts
Panel A: Full panel					
co-partisan county	-0.0051	-0.0064	0.0089	0.0093	-0.0160
	(0.0061)	(0.0091)	(0.0122)	(0.0101)	(0.0245)
swing county	0.0071	0.0003	-0.0120	-0.0100	-0.0683*
	(0.0090)	(0.0158)	(0.0243)	(0.0136)	(0.0394)
co-partisan county	0.0045	0.0078	-0.0155	-0.0012	0.0564*
×swing county	(0.0062)	(0.0110)	(0.0133)	(0.0124)	(0.0321)
county FE	X	X	X	X	X
district-congress FE	X	X	X	X	X
no. of county-congresses	28,524	28,524	17,815	28,524	28,524
no. of district-congresses	2,208	2,208	1,355	2,208	2,208
no. of states	47	47	47	47	47
Panel B: Regression discontinuity					
Republican county	-0.0034	-0.0267	-0.0192	-0.0531	-0.1011
×Republican district	(0.0204)	(0.0315)	(0.0529)	(0.0450)	(0.1043)
county FE	X	X	X	X	X
district-congress FE	X	X	X	X	X
no. of county-congresses	5,304	5,304	3,351	5,304	5,304
no. of district-congresses	383	383	232	383	383
no. of states	45	45	43	45	45
Panel C: First-differences					
Δ co-partisan county	-0.0195*	-0.0395*	-0.0327	-0.0226	-0.0452
	(0.0106)	(0.0205)	(0.0438)	(0.0147)	(0.0481)
Δ swing county	0.0029	-0.0015	-0.0032	0.0039	-0.0036
	(0.0097)	(0.0171)	(0.0306)	(0.0236)	(0.0510)
Δ (co-partisan county	0.0092	0.0196	0.0146	0.0308*	0.0025
×swing county)	(0.0132)	(0.0240)	(0.0335)	(0.0170)	(0.0611)
district-congress FE	X	X	X	X	X
no. of county-congresses	3,717	3,717	1,886	3,716	3,715
no. of district-congresses	350	350	177	350	350
no. of states	37	37	33	37	37

*Notes: Standard errors in parentheses and clustered by state. Opposition county and base county are the omitted categories in Panels A and C. Democratic county, base county, and Democratic district are the omitted categories in Panel B. Panel A estimates equation 3.2.1, Panel B estimates equation 3.4.1, and Panel C estimates equation 3.4.2. Within each district-congress and using the Republican vote share in presidential elections, counties are equally sorted into either Republican or Democatic halves by their panel average and into either swing or base halves by their panel standard deviation. Counties are defined as co-partisan when their partisanship matches their House representative in the current Congress and are defined as opposition otherwise. Panel B uses the full sample to estimate the county fixed effects and limits the local linear regression to elections where the leading candidate wins by less than a 5 percentage point margin. Panel C restricts the sample to periods before and after redistricting where the party of the House representative also remains the same. See text for additional details. * - p-value < 0.10, ** - p-value < 0.05, *** - p-value < 0.01*

(i) Republican counties tend to differ on demographics from Democratic counties (ii) Republicans and Democrats tend to win elections in different contexts. For example, Republicans may be elected when commodity markets are strong and farmers receive fewer payouts from federal agricultural insurance programs. If Republican counties tend to be more agricultural, then this effect would generate a downward bias on the coefficient for the co-partisan county. Alternatively, Democrats may be elected in periods of high unemployment. If Democratic counties have larger rates of unemployment, then unemployment benefits would create an upwards bias on the coefficient for the co-partisan county. I next provide estimates from a RDD to address this concern for the coefficient on the co-partisan county.

3.4.2 Regression discontinuity design

A recent literature has developed applying RDDs to elections in order to identify the local treatment effect of the winner.[18] Two-candidate elections are a suitable context for this methodology because the winner is determined by a well-defined cutoff point, one-half, in a continuous forcing variable, the vote share. Lee (2008) both formalized the conditions necessary for a valid RDD and then applied his framework to identify incumbency effects in US House elections. Intuitively, this approach assumes that the other covariates that determine the outcome of interest are continuous at the cutoff point of the forcing variable. Thus, estimating the effect of the discontinuity in the election outcome identifies its effect on the outcome of interest. Because few observations typically exist exactly at the discontinuity, this technique requires including observations within a bandwidth around the cutoff point and specifying a polynomial in the forcing variable that flexibly controls for unobservables to the left and right of the discontinuity. While RDDs have the limitation that they only identify a local treatment effect, focusing on two-party elections provides a stronger test of the two-candidate distributive politics models than safe elections because politicians face

[18] Lee and Lemieux (2010) provides an excellent review of the empirical literature applying RDDs.

stronger electoral pressures to respond to voter demands.[19]

The RDD cannot be directly implemented on equation 3.2.1 because the covariates are not election outcomes. Instead, I estimate equation 3.4.1:

$$\begin{aligned}
\log\left(\text{spending}_{cdt}\right) \quad = \quad & \beta_1 \text{ Republican county}_{cdt} \\
& + \beta_2 \left(\text{Republican county}_{cdt} \times \text{Republican district}_{dt}\right) \\
& + \beta_3 \left(\text{Republican county}_{cdt} \times v_{dt}\right) \\
& + \beta_4 \left(\text{Republican county}_{cdt} \times \text{Republican district}_{dt} \times v_{dt}\right) \\
& + \beta_5 \text{ swing county}_{cdt} \\
& + \beta_6 \left(\text{Republican county}_{cdt} \times \text{swing county}_{cdt}\right) + \delta_c + \xi_{dt} + \epsilon_{cdt}
\end{aligned} \tag{3.4.1}$$

where v_{dt} represents the Republican margin of favor in the congressional district's House election[20] and where Democratic counties, base counties, and Democratic districts are the omitted categories. The variable Republican district$_{dt}$ indicates a congressional district where the Republican party won the House election for the current Congress. As before, δ_c is a fixed effect for county c and ξ_{dt} is a fixed effect for district d-congress t. To focus on estimating the coefficient on Republican county precisely at the discontinuity, I interact it with the vote share v_{dt} to the left and right of the cutoff where Republicans win the election.

In this specification, β_2 directly estimates the discontinuity at the cutoff. It is a difference-in-difference estimate of per-capita spending between Republican and Democratic counties under Republican and Democratic legislators. Alternatively, it can be interpreted as a local estimate of the coefficient on the co-partisan county. For the implementation, I use the full panel to estimate the county fixed effects but I restrict the covariates to equal zero when elections are won by more than a 5 percentage point margin. Because my design

[19]Caughey and Sekhon (2011) and Grimmer et al. (2011) have raised objections to the internal validity of the RDD for US House elections, emphasizing that incumbency, campaign resources, and partisan match with the state governor or the state electoral administrator predict winners in two-party elections. I do not, however, find sorting around the cutoff on county demographic characteristics and lagged spending measures in my sample, the most serious confounds in my context that the RDD is intended to address. See Appendix Section C.3 for the estimates.

[20]The vote share measure v_{dt} only includes votes for the Republican and Democratic candidates.

includes district-congress fixed effects, the county covariates only pick-up the within-district differences for the observations within the bandwidth. In Appendix Section C.3, I also report estimates for bandwidths of 7.5 and 2.5 percentage points with similar results.[21] In all cases, I find null effects with the smallest standard errors under the largest bandwidth and the largest standard errors under the smallest bandwidth.

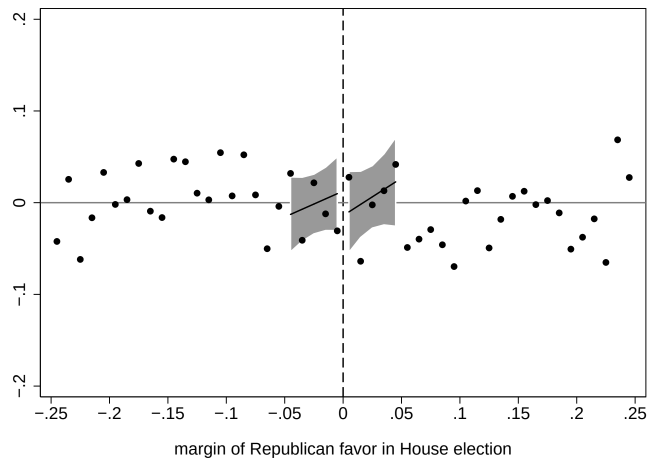
Figure 3.3 illustrates the implementation of the RDD. For each percentile of the Republican margin of favor in the House election, this graph plots the mean difference between Republican and Democratic counties for all congressional districts where the vote share is within that percentile. It also plots the linear polynomial from the RDD to the left and right of the cutoff within the 5 percentage point bandwidth. The vertical difference at the cutoff between these linear polynomials equals β_2. The y-axis has been scaled to roughly equal one within-district standard deviation of the spending measure. By this benchmark, I find small point estimates at the discontinuity. Furthermore, the full panel beyond the 5 percentage point bandwidth exhibits limited differences between congressional districts with small and large electoral margins in their within-district spending patterns.

Panel B of Table 3.2 reports the RDD estimates. Under the non-entitlement measure, I find a point estimate of -0.0267 with a standard error of 0.0315 for the difference between Republican and Democratic counties at the discontinuity. While not as precise as the results from the full panel, it can still rule out increases in the difference in per-capita spending between Republican and Democratic counties greater than 3.5 percentage points when a congressional district switches from Democratic to Republican control under a 95 percent confidence interval. The other specifications also find insignificant results with negative point estimates, although the discretionary and contracts measures have larger upper bounds.

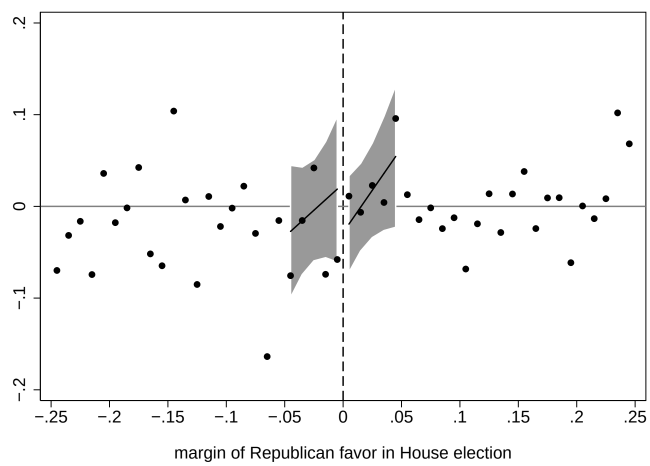
[21] Appendix Section C.3 compares RDD estimates using margins of 7.5, 5, and 2.5 percentage points.

Figure 3.3: *Regression discontinuity: mean within-district difference in log per-capita spending between Republican and Democratic counties by percentile of congressional district Republican vote share*

Panel A: Non-entitlement spending



Panel B: Grants spending



Notes: Vote share only includes votes for the Republican and Democratic candidates and is normalized to represent the Republican margin of favor. Estimates use district-congress and county fixed effects. Bandwidth restricted to close elections within a 5 *percentage point margin. Figures include a local linear regression separately fitted to the left and right of the cutoff at zero. The difference at the cutoff between the two linear fits equals the difference-in-difference estimate reported in Panel B of Table3.2. The y-axis is scaled to roughly equal an average within-district standard deviation of the log per-capita spending measure as reported in Table 3.1. See text for additional details.*

3.4.3 First-differences design

To isolate the identification from redistricting, I implement a first-differences version of equation 3.2.1. By limiting the sample to only periods before and after redistricting where the party of the House representative also remains unchanged, I focus on the immediate changes in the within-district distribution of spending that follow redistricting. A potential confound to this approach is that redistricting sometimes occurs in response to gradual demographic changes, such as to balance population shifts or to protect minority districts.[22] The distribution of federal spending may also respond to these demographic changes because some federal formulas use US Census demographic estimates to determine their geographic allocation. My design partially accounts for this issue by focusing on the immediate within-district change and by using a per-capita spending measure based on the annual US Census population estimates.

Equation 3.4.2 implements the first-differences design:

$$\begin{aligned}\Delta \log\left(\text{spending}_{cdt}\right) \quad = \quad & \beta_1\, \Delta\left(\text{co-partisan county}_{cdt}\right) + \beta_2\, \Delta\left(\text{swing county}_{cdt}\right) \\ & +\beta_3\, \Delta\left(\text{co-partisan county}_{cdt} \times \text{swing county}_{cdt}\right) + \Delta\xi_{dt} + \Delta\epsilon_{cdt}\end{aligned} \tag{3.4.2}$$

where, as before, ξ_{dt} is a fixed effect for district d-congress t. Here, I have omitted the fixed effect δ_c for county c because first-differences eliminates time-invariant variables.

Figure 3.4 displays estimates of the coefficient for the swing county. I also include lead and lag estimates of this indicator as placebo tests for redistricting and as checks for pre- and post-trends.[23] The y-axis has been scaled to roughly equal one within-district standard deviation of the spending measure. By this benchmark, the first-difference specification

[22] Redistricting can be politically motivated as well, particularly to protect friendly incumbents or to unseat opposing ones. Redistricting that creates a safer district could result in less targeting because legislators feel less pressure to respond to voter demands. Conversely, redistricting that results in a more competitive district could result in more targeting because legislators feel more electoral pressure. On net, it is unclear whether this bias is positive or negative, although Figure 3.3 for the RDD suggests that congressional districts with small and large electoral margins have similar within-district spending patterns.

[23] In Figure 3.4, the sample is only restricted to periods where the party remains unchanged in order to estimate the leads and lags in addition to the redistricting treatment. Because party switches are excluded, identification still solely comes from redistricting.

estimates a reasonably precise zero effect. The leads and lags do not suggest an obvious pre- or post-trend that could be attributed to demographic changes. In addition, the standard errors on the estimates are similar across treatment, leads, and lags.

Panel C of Table 3.2 reports estimates of equation 3.4.2. Under the non-entitlement spending measure, I find an estimate of -0.0015 with a standard error of 0.0171 for the swing county. This estimate rules out differences in per-capita spending with the base opposition counties greater than 3.3 percentage points under a 95 percent confidence interval. The results for the other spending measures and the co-partisan county are also not strongly significant.

3.5 Robustness

The null effects found in the previous section could be criticized for largely excluding urban counties. Because they receive greater overall levels of spending, legislators representing urban areas may have more opportunities to influence its within-district distribution. Furthermore, my main results ignore variation in institutional context that may affect the capacity of legislators to manipulate federal spending. In response, I provide estimates of the within-state distribution of spending for the US Senate, where I can include all counties, and I replicate my full panel estimates to account for (i) partisan alignment with the House majority and the president, (ii) congressional committee assignments, and (iii) the bicameral nesting of House and Senate districts in the US Congress. These considerations do not substantively change my previous findings.

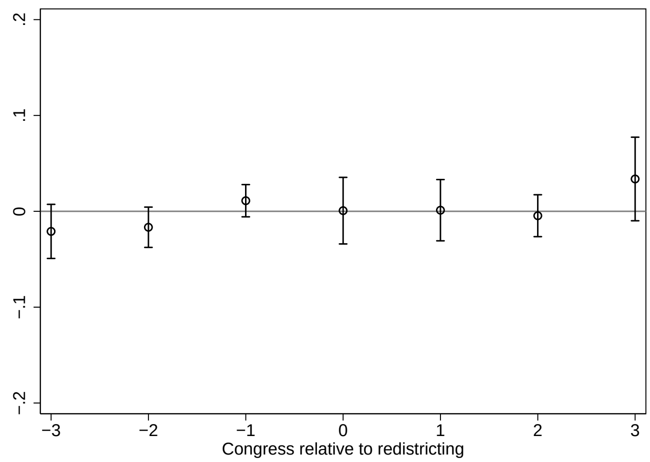
3.5.1 US Senate

Replicating my analysis using the US Senate allows me to include nearly all counties because senators represent entire states.[24] I report county sample summary statistics de-meaned

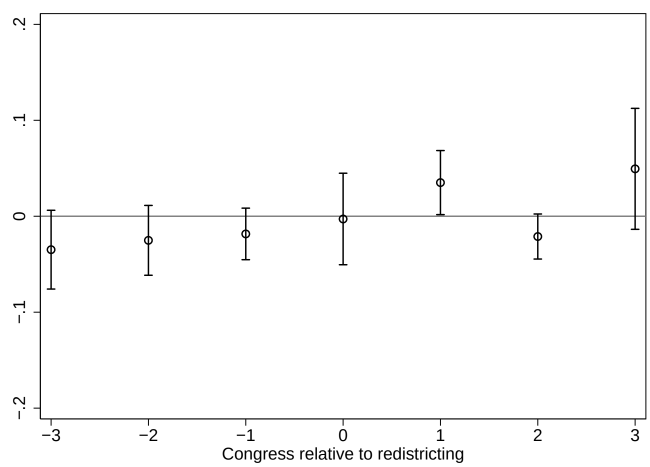
[24] This sample still excludes counties that contain a state capital because a significant amount of federal spending is mechanically allocated to them for accounting purposes, and then ultimately redistributed across the state.

Figure 3.4: *First-differences: lead and lag estimates of changes in log per-capita spending for swing counties*

Panel A: Non-entitlement spending



Panel B: Grants spending



Notes: Error bars indicate a 95 percent confidence interval. Plotted estimates from equation 3.4.2, including three leads and lags. The sample is restricted to periods where the party of the House representative also remains unchanged. The y-axis is scaled to roughly equal an average within-district standard deviation of the log per-capita spending measure as reported in Table 3.1. See text for additional details.

by state-congress and estimates that use the full panel and that include state-congress and county fixed effects. Here, only counties under Democratic and Republican Senate delegations are defined as either co-partisan or opposition.[25] Furthermore, I exclude swing counties from the analysis when including county fixed effects because all Senate delegations share the same swing voters and states do not change their boundaries.

With these considerations, I estimate equation 3.5.1:

$$\log\left(\text{spending}_{cst}\right) = \beta \text{ co-partisan county}_{cst} + \delta_c + \xi_{st} + \epsilon_{cst} \tag{3.5.1}$$

where opposition county is the omitted category, δ_c is a fixed effect for county c, and ξ_{st} is a fixed effect for state s-congress t. Here, β captures the mean within state-congress difference between co-partisan and opposition counties.

Panel A of Table 3.3 reports partisan county quantile means for the spending measures. All variables are de-meaned by state-congress. I do not report means for the bi-partisan county quantiles because the two-candidate distributive politics models do not make clear predictions for these cases. Similar to the patterns within congressional districts, swing counties receive less spending than base opposition counties under all spending measures. Furthermore, the differences between quantile means are a small fraction of the mean within state-congress standard deviations. As before, the three models reviewed earlier do not predict the observed patterns across partisan county quantile means.

Panel B of Table 3.3 reports estimates of equation 3.5.1. Under the non-entitlement spending measure, I find a point estimate of -0.0080 with a standard error of 0.0093. Using a 95 percent confidence internal, I can rule out differences between the co-partisan county and the opposition county greater than 1.0 percentage points. I find similarly precise null effects for the other spending measures. Including a broader sample of counties under the US Senate delivers similar results as in the previous section for whether co-partisan counties receive disproportionately more spending.

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[25] I define counties under bi-partisan Senate delegations as bi-partisan. I cannot identify an effect on this variable because there is no within state-congress variation.

Table 3.3: *US Senate: relative log per-capita spending across counties within states*

	log (per-capita federal spending)				
	total	non-entitlement	discretionary	grants	contracts
Panel A: Summary statistics demeaned by state-congress					
base co-partisan counties	0.0204	0.0732	0.0852	0.0318	0.1381
	(0.3751)	(0.5919)	(0.6535)	(0.5508)	(1.3420)
swing co-partisan counties	-0.0139	-0.0457	-0.0687	-0.0873	-0.1485
	(0.3429)	(0.5549)	(0.5995)	(0.4850)	(1.2448)
swing opposition counties	-0.0104	-0.0449	-0.0633	-0.0395	-0.1720
	(0.3504)	(0.5509)	(0.5528)	(0.4930)	(1.2269)
base opposition counties	0.0078	0.0339	0.0568	0.0636	0.1807
	(0.3648)	(0.5766)	(0.6068)	(0.5284)	(1.3588)
mean within district-congress std. dev.	0.3501	0.5681	0.6060	0.4842	1.2900
	(0.1172)	(0.1581)	(0.1790)	(0.1357)	(0.2727)
Panel B: Full panel					
co-partisan county	-0.0121	-0.0080	0.0087	0.0107	-0.0371
	(0.0079)	(0.0093)	(0.0132)	(0.0151)	(0.0263)
county FE	X	X	X	X	X
state-congress FE	X	X	X	X	X
no. of county-congresses	36,640	36,640	22,069	36,640	36,640
no. of state-congresses	585	585	352	585	585
no. of states	48	48	48	48	48

*Notes: Standard errors in parentheses and clustered by state. Panel A does not report county quantile means for bi-partisan counties. Panel B estimates equation 3.5.1 where opposition county is the omitted category. Bi-partisan counties are not identified because the specification includes state-congress fixed effects. Within each state-congress, counties are equally sorted into Republican and Democratic halves by their panel mean Republican vote share in presidential elections. Under state senate delegations with either two Republicans or two Democrats, I define counties as co-partisan when their partisanship matches their state's senate delegation and as opposition otherwise. Under bi-partisan senate delegations, I define all counties as bi-partisan. See text for additional details. * - p-value < 0.10, ** - p-value < 0.05, *** - p-value < 0.01*

3.5.2 Partisan alignment

Next, I replicate estimates for equation 3.2.1 with interactions for whether the House representative shares the same party as the House majority and as the president. Identification comes from the partisan switches in the House majority and the president in the time-series. As before, I find null results with similar precision.

Table 3.4 reports the estimates. Under the non-entitlement measure for the interaction with the House majority, I estimate a coefficient of 0.0065 with a standard error of 0.0125 for the co-partisan county and a coefficient of 0.0156 with a standard error of 0.0103 for the swing county. These estimates allow me to rule out differences with base opposition counties greater than 3.1 and 3.6 percentage points respectively using a 95 percent confidence interval. For the interactions with the president's party, I find a point estimate of 0.0163 with a standard error of 0.0119 on the co-partisan county and a point estimate of 0.0004 with a standard error of 0.0133 on the swing county. Similarly, I can rule out differences with base opposition counties greater than 3.9 and 2.7 percentage points respectively. Estimates are also generally insignificant and precise for the grants spending measure. Although one coefficient is marginally significant on the interaction between the swing county and the House majority party, the p-values are not adjusted for multiple hypothesis tests. These results do not suggest that the within-district allocations under House representatives substantially differ under the partisan alignments tested here.

3.5.3 Congressional committees

I also consider interactions for committee chairs, ranking minority members, membership on the Appropriations Committee, and membership on the Ways & Means Committee. Chairs and ranking minority members are the most senior members of the committee from the majority and minority parties respectively. The Appropriations Committee drafts the annual appropriations bills for discretionary spending. The Ways & Means Committee writes major tax legislation. These positions are generally considered influential and desirable among House members. They serve as proxies to identify legislators with greater ability to

Table 3.4: *Partisan alignment: relative log per-capita spending estimates using the full panel*

	log(per-capita federal spending)			
	non-entitlement	grants	non-entitlement	grants
co-partisan county	-0.0104	-0.0025	-0.0142	0.0001
	(0.0113)	(0.0133)	(0.0109)	(0.0112)
$\times$House majority party	0.0065	0.0179		
	(0.0125)	(0.0126)		
$\times$president's party			0.0163	0.0188
			(0.0119)	(0.0140)
swing county	-0.0093	-0.0237*	0.0004	-0.0166
	(0.0193)	(0.0133)	(0.0145)	(0.0157)
$\times$House majority party	0.0156	0.0226*		
	(0.0103)	(0.0121)		
$\times$president's party			0.0004	0.0132
			(0.0133)	(0.0118)
co-partisan county$\times$swing county	0.0230	0.0083	0.0015	0.0031
	(0.0165)	(0.0167)	(0.0140)	(0.0155)
$\times$House majority party	-0.0238	-0.0148		
	(0.0174)	(0.0201)		
$\times$president's party			0.0127	-0.0087
			(0.0201)	(0.0156)
county FE	X	X	X	X
district-congress FE	X	X	X	X
no. of county-congresses	28,524	28,524	28,524	28,524
no. of district-congresses	2,208	2,208	2,208	2,208
no. of states	47	47	47	47

*Notes: Standard errors in parentheses and clustered by state. Estimates equation 3.2.1, with added interactions for when the House representative shares the same party as the House majority and the president. Within each district-congress and using the Republican vote share in presidential elections, counties are equally sorted into either Republican or Democratic halves by their panel average and into either swing or base halves by their panel standard deviation. Counties are defined as co-partisan when their partisanship matches their House representative in the current Congress and are defined as opposition otherwise. See text for additional details. * - p-value < 0.10, ** - p-value < 0.05, *** - p-value < 0.01* influence federal spending.

Table 3.5 reports estimates for the committee interactions. Under the non-entitlement measure, I estimate large and significantly negative coefficients when considering committee chairs and ranking minority members. Among the committee chair interactions, I find a point estimate of -0.0652 with a standard error of 0.0323 for the co-partisan county and a point estimate of -0.0304 with a standard error of 0.0284 for the swing county. Using a 95 percent confidence interval, these estimates allow me to rule out differences with base opposition counties greater than -0.2 percentage points and 2.5 percentage points. For the ranking minority member interaction, I estimate a coefficient of -0.0770 with a standard error of 0.0385 on the co-partisan county and a coefficient of -0.0708 with a standard error of 0.0267 on the swing county. Similarly, I can rule out differences with base opposition counties greater than -0.1 and -1.9 percentage points respectively. I neither, however, replicate these results for the grants measure nor for the other committee interactions. In these cases, I instead estimate reasonably precise zero effects as before.

The significant negative coefficients that I find are hard to interpret under the two-candidate distributive politics models. Because I still estimate precise zeroes for the main effects of the co-partisan and swing counties, these negative interactions imply that opposition counties receive the most spending under committee chairs and that base opposition counties receive the most spending under ranking minority members. The models that I test do not predict that the most influential legislators would target opposition voters with spending.

3.5.4 Bicameralism

Finally, I account for the bicameral nesting of House districts within Senate districts in the US Congress. Although a district-congress fixed effect will pick-up a common effect from the state's Senate delegation, senators may also be engaged in differential targeting across counties. This interaction could matter if (i) House and Senate members substitute between each other's spending or (ii) House and Senate members cooperate to direct

Table 3.5: *Committees: relative log per-capita spending estimates using the full panel*

	log(per-capita federal spending)			
	non-entitlement	grants	non-entitlement	grants
co-partisan county	-0.0027	0.0097	-0.0054	0.0165*
	(0.0098)	(0.0105)	(0.0089)	(0.0094)
×committee chair	-0.0652**	0.0090		
	(0.0323)	(0.0269)		
×ranking minority member	-0.0770**	-0.0197		
	(0.0385)	(0.0285)		
×Appropriations Committee			-0.0136	-0.0303
			(0.0188)	(0.0265)
×Ways & Means Committee			0.0036	-0.0503
			(0.0258)	(0.0347)
swing county	0.0046	-0.0080	-0.0033	-0.0081
	(0.0158)	(0.0138)	(0.0175)	(0.0162)
×committee chair	-0.0304	0.0091		
	(0.0284)	(0.0284)		
×ranking minority member	-0.0708***	-0.0458*		
	(0.0267)	(0.0246)		
×Appropriations Committee			0.0177	-0.0142
			(0.0267)	(0.0284)
×Ways & Means Committee			0.0162	-0.0064
			(0.0237)	(0.0365)
co-partisan county×swing county	0.0057	-0.0007	0.0124	-0.0066
	(0.0115)	(0.0124)	(0.0116)	(0.0116)
×committee chair	0.0449	-0.0288		
	(0.0435)	(0.0476)		
×ranking minority member	0.0365	0.0116		
	(0.0408)	(0.0375)		
×Appropriations Committee			-0.0061	0.0271
			(0.0297)	(0.0352)
×Ways & Means Committee			-0.0577	0.0275
			(0.0390)	(0.0585)
county FE	X	X	X	X
district-congress FE	X	X	X	X
no. of county-congresses	28,524	28,524	28,524	28,524
no. of district-congresses	2,208	2,208	2,208	2,208
no. of states	47	47	47	47

*Notes: Standard errors in parentheses and clustered by state. Estimates equation 3.2.1, with added interactions for committee positions held by the House representative. Committee chair and ranking minority member indicate the most senior members of the committee from the majority and minority parties respectively. The interactions for the Appropriations Committee and the Ways & Means Committee identify their members. These committees respectively draft the annual appropriations bills for discretionary spending and write tax legislation. Within each district-congress and using the Republican vote share in presidential elections, counties are equally sorted into either Republican or Democratic halves by their panel average and into either swing or base halves by their panel standard deviation. Counties are defined as co-partisan when their partisanship matches their House representative in the current Congress and are defined as opposition otherwise. See text for additional details. * - p-value < 0.10, ** - p-value < 0.05, *** - p-value < 0.01* spending to geographic areas they both favor. For the same rationales expressed in Section 3.5.1, I only consider Senate co-partisan counties under Democratic and Republican Senate delegations. In estimation, I include main effects for House co-partisan, House swing, and Senate co-partisan counties, their interactions, and district-congress and county fixed effects.

Table 3.6 reports the results accounting for bicameralism. The estimates are generally insignificant and precise as before. For the main effects on the House co-partisan and swing counties under the non-entitlement measure, I find point estimates of 0.0009 and 0.0117 with standard errors of 0.0099 and 0.0180 respectively. The upper bounds of a 95 percent confidence interval allow me rule out differences with base opposition counties greater than 2.0 and 4.7 percentage points. For their interactions with the Senate co-partisan county, I report point estimates of -0.0263 and -0.0401 with standard errors of 0.0203 and 0.0216 respectively. I can rule out differences with base opposition counties greater than 1.3 and 0.2 percentage points. Although the negative interactions suggest substitution, they are not strongly significant. Furthermore, I estimate reasonably precise zeroes for the main effects. Accounting for nested districts in the bicameral legislature has little impact on my initial results.

3.6 Conclusion

This paper finds that neither the co-partisan nor swing counties within a sub-sample of congressional districts receive substantially more federal spending when broad measures are considered in a 1982-2006 panel of US House elections. I find similar results when I implement a RDD on two-party elections and a first-differences design on redistricting. Furthermore, I estimate reasonably precise null effects when I consider the US Senate, partisan alignment, congressional committees, and the nested districts of the bicameral US Congress. Standard distributive politics models do not predict the flat distribution of spending that I find across geographic clusters of voters. At least three alternative explanations emerges from previous research.

First, checks and balances within the federal budget process may prevent legislators from

Table 3.6: *Bicameralism: relative log per-capita spending estimates using the full panel*

	log (per-capita federal spending)				
	total	non-entitlement	discretionary	grants	contracts
House co-partisan county	-0.0008	0.0009	0.0134	0.0130	0.0084
	(0.0063)	(0.0099)	(0.0150)	(0.0109)	(0.0239)
House swing county	0.0123	0.0117	-0.0009	-0.0030	-0.0584
	(0.0098)	(0.0180)	(0.0275)	(0.0150)	(0.0468)
House co-partisan county×	-0.0003	-0.0006	-0.0178	-0.0053	0.0368
House swing county	(0.0062)	(0.0104)	(0.0167)	(0.0112)	(0.0374)
Senate co-partisan county	0.0076	0.0279	0.0365	0.0167	0.0384
	(0.0108)	(0.0176)	(0.0282)	(0.0187)	(0.0407)
House co-partisan county×	-0.0148	-0.0263	-0.0178	-0.0138	-0.0821
Senate co-partisan county	(0.0109)	(0.0203)	(0.0312)	(0.0225)	(0.0608)
House swing county×	-0.0189*	-0.0401*	-0.0371	-0.0246	-0.0360
Senate co-partisan county	(0.0111)	(0.0216)	(0.0316)	(0.0188)	(0.0540)
House co-partisan×House	0.0173	0.0308	0.0086	0.0155	0.0645
swing×Senate co-partisan	(0.0143)	(0.0233)	(0.0335)	(0.0292)	(0.0877)
county FE	X	X	X	X	X
district-congress FE	X	X	X	X	X
no. of county-congresses	28,524	28,524	17,815	28,524	28,524
no. of district-congresses	2,208	2,208	1,355	2,208	2,208
no. of states	47	47	47	47	47

*Notes: Standard errors in parentheses and clustered by state. Estimates equation 3.2.1, with a main effect and interactions added for the Senate co-partisan counties. House co-partisan and swing counties are defined relative to other counties within each district-congress and depend on the partisan match with their House representative. Senate co-partisan counties are defined relative to other counties within each state-congress and depend on the partisan match with their state's Senate delegation. Both use the Republican vote share in presidential elections to sort counties into Republican and Democratic halves by their panel mean and into swing and base halves by the panel standard deviation. Counties are defined as co-partisan when they match the partisanship of their legislator(s) and conversely for opposition counties. See text for additional details. * - p-value < 0.10, ** - p-value < 0.05, *** - p-value < 0.01* steering resources toward co-partisan and swing voters. The welfare costs from allowing legislators to manipulate federal spending for electoral purposes may be so large that legislators prefer to commit themselves to impartial budgetary rules. A large literature (Alesina and Perotti 1996, Alt and Lowry 1994, Besley and Case 2003, Ferejohn and Krehbiel 1987, Poterba 1996) has explored the effects of alternative budgetary institutions on fiscal policy outcomes. Although I partially account for this issue by accounting for partisan alignment, congressional committees, and bicameralism to identify legislators with greater capacity to influence federal spending, there could still be particulars about the rules regulating the budget process that prevent spending aggregates from benefiting one group of voters over another.

Second, the direct personal impact of candidate policy positions may not determine whom voters choose to support. Meehl (1977)argues it unlikely that voters choose between candidates on the basis of personal material benefit because any individual voter has a negligible probability of being pivotal in an election. Rather, social altruism may provide a more plausible basis for voting behavior. Under this assumption, ideologically heterogeneous tastes can still play a role among voters in their preferred programmatic composition of government spending

Third, imperfect information among voters about public policy may break the predictions from the two-candidate distributive politics models (Lupia 1992). If voters are largely ignorant about federal spending, then they can neither reward nor punish politicians for manipulating its distribution. Legislators may instead expend their effort on securing federal benefits for well-informed special interests at the expense of the general public (Lohmann 1998).

While this paper finds evidence that the victors do indeed share the spoils, future research is needed to understand why. My empirical results cannot distinguish between the three alternative interpretations discussed here. Previous positive results for the relevance of politics in the distribution of federal spending suggest against the checks and balances interpretation. For the distributive politics literature in particular, a flat distribution of federal spending aggregates could mask either partisan differences in the programmatic composition of spending, narrow targeting of spending to interest groups, or an emphasis on highly salient government spending projects for political campaigning.

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Appendix A

Appendix to Chapter 1

A.1 Simulation of Tax Refunds for the Carryback Election

The IRS does not automatically compute the eligible carryback refund each time a corporation files an income tax return that reports a net operating loss. Instead, the IRS requires firms to provide documentation that details the computation of their refund when they file for it. To determine whether firms are eligible for carryback refunds, we simulate eligible refunds based on each firm's reported loss, their history of taxes paid in prior tax years, and the policy rules detailed in Table 1.1. We validate the accuracy of our simulated refunds by comparing them to claimed refunds.

Specifically, we identify each corporate tax return that reports a net operating loss. For each firm, we pull their tax liability history and incorporate any post-filing adjustments to their past tax returns. We then infer each firm's past taxable income using their past tax liabilities. This assumption ignores adjustments to tax liability from Schedule J of Form 1120, such as the application of tax credits and the alternative minimum tax. We next apply the reported loss against our simulation of past taxable income according to the policy rules in Table 1.1. We start by deducting the loss against the earliest eligible tax year and progress in calendar order, until we have exhausted either the loss or all eligible taxable income. We then re-compute the firm's tax liability based on their new taxable income in each past tax year. Finally, we calculate the carryback refund by taking the difference in the firm's tax liability before and after applying the carryback deduction.

We check the credibility of our simulation of eligible carryback refunds by comparing them to the observed refunds received by firms that claim the carryback. Appendix Figure A.1 plots means of $\log(\text{claimed refunds})$ by vigintiles of $\log(\text{simulated refunds})$. A univariate regression of $\log(\text{claimed refunds})$ on $\log(\text{simulated refunds})$ yields a coefficient of 0.9636 and a R-squared of 0.9336. These results imply that we simulate the eligible carryback refunds with a high degree of accuracy.

A.2 Simulation of Carryforward Deductions

We compute the net present values of the carryback and carryforward elections by simulating future carryforward deductions. For each firm, we use the observed taxable income in future tax periods. We first compute the amount of tax loss available for carryforward deductions. For firms that elect the carryback, this amount equals the remaining amount of deductions that could not be applied to past taxable income. For firms that elect the carryforward, this amount equals the full loss reported at time $t = 0$.

We then simulate the claiming of the carryforward deduction against future taxable income. We assume that firms deduct the carryforwards as soon as possible. Our simulation can also account for firms that have an existing stock of carryforwards prior to time $t = 0$. In these cases, we first deduct from the stock of pre-existing carryforwards before we deduct the carryforwards generated by the tax loss at time $t = 0$. This treatment increases the delayed realization of the tax benefits from the carryforwards.

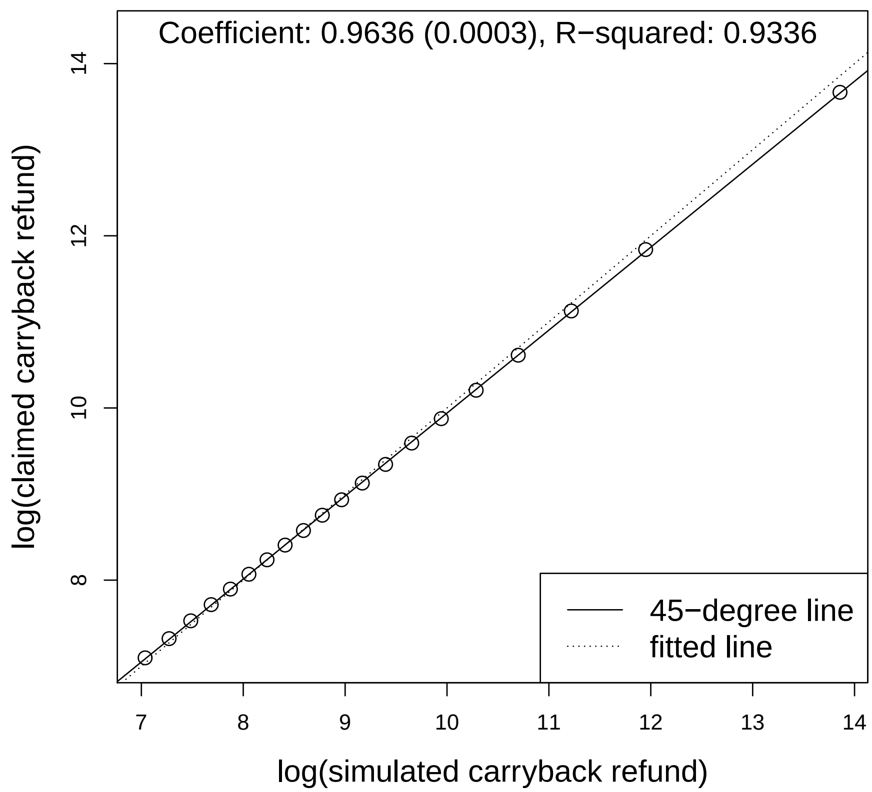
A.3 Variable Definitions from the Business Tax Data

We pull line items from the income tax return, Form 1120, to describe characteristics of the tax loss firms in our sample. Here, we explain how we construct the variables reported in Table 1.2. All dollar amounts are normalized to 2013 price levels. Revenue equals total income (line 11) plus cost of goods sold (line 2). Assets is reported in box D on the front page of Form 1120. Payroll is the sum of W-2 and 1099-MISC wage statements. Firms issue these statements to employees and contractors respectively. EBITDA is defined as total income (line 11) minus total deductions (line 27) plus compensation of officers (line 12) plus interest (line 19) plus charitable contributions (line 19) plus depreciation (line 20) plus depletion (line 21) plus domestic production activities deduction (line 25).

We identify the paid preparer that assisted a corporation with filing their tax return from Form 1120. On the bottom of the front page of the tax form, preparers hired to file an income tax return must self-identify themselves and their employing firm. This field does not include internal employees that prepare their employer's income tax return.

We construct measures of preparers using the identifiers reported on Form 1120. We match these identifiers to their individual income tax returns. We obtain our age variable from a social security file that records the date of birth. We also compute average client characteristics and the number of clients from the population of C and S corporations by preparer and tax year. Similarly, we match tax firm identifiers to income tax returns for sole-proprietorships, partnerships, and corporations to construct our measures for tax firms.

Appendix Figure A.1: *Claimed carryback refunds vs. simulated carryback refunds*



Notes: This figure compares claimed carryback refunds to simulated carryback refunds. The sample includes all firms that (i) report a net operating loss, (ii) have simulated eligible refunds of at least $1,000, and (iii) claim (and receive) a carryback refund. The figure plots mean log(claimed carryback refund) by vigintiles in log(simulated carryback refund). It also reports the slope coefficient and the R-squared from a regression of log(claimed carryback refund) on log(simulated carryback refund). The simulation of eligible carryback refunds are based on each firm's tax loss, historical tax liability, and the policy rules for carryback refunds. Please see Appendix Section A.1 for further details.

Appendix B

Appendix to Chapter 2

B.1 Investment with Adjustment Costs and a Borrowing Constraint

We develop an infinite horizon, non-stochastic investment model, deriving the testable hypotheses in Section 2.2 and the empirical moments for calibration in Section 2.5.3. The model nests the standard neoclassical investment model with adjustment costs (Hayashi, 1982), a model with credit constraints and a model with managerial myopia.

B.1.1 General Setup

We begin with a discrete time version of Hayashi (1982). Firm value, V_0, is given by an infinite series of discounted net receipts, R_t. The discount rate, r_t, is risk-adjusted and possibly time varying. The expression for firm value is

$$V_0 = \sum_{t=0}^{\infty} \frac{1}{\Pi_{s=0}^{t}(1+r_s)} R_t. \tag{B.1.1}$$

Net receipts in each period reflect net revenues after taxes, investment costs, adjustment costs and depreciation deductions for current and past investments:

$$R_t = [1-\tau_t]\,\pi_t - [1-k_t]\,p_{I,t}I_t - \psi_t(I_t, K_t) + \tau_t \sum_{x=0}^{\infty} D_{t-x}(x)p_{I,t-x}I_{t-x}, \tag{B.1.2}$$

where τ_t is the corporate tax rate, π_t is pretax profits, $p_{I,t}$ is the price of investment goods, k_t is the investment tax credit, I_t is investment, ψ_t is adjustment costs and $D_{t-x}(x)$ is the depreciation deduction for capital of age x, based on the schedule from time $t-x$. Pretax profits are π_t, which equals gross revenues, $p_t F_t(K_t, N_t)$, with capital, K_t, and labor, N_t, inputs, less the cost of labor inputs. Net revenues are thus given by

$$\pi_t = p_t F_t(K_t, N_t) - w_t N_t. \tag{B.1.3}$$

Firms are price takers so output prices, p_t, and wages, w_t, are exogenous. F_t is weakly concave. The firm maximizes (B.1.1) subject to a capital accumulation law of motion:

$$K_{t+1} = K_t - \delta K_t + I_t, \tag{B.1.4}$$

where δ is the rate of economic depreciation. The adjustment cost function is convex and reflects after-tax resource losses due to production disruptions and installation.[1]

It is useful to have an expression for the stream of future depreciation deductions owed for investment in time t:

$$z_t^0(\beta) = \tau_t D_0 + \beta \sum_{x=1}^{\infty} \frac{1}{\Pi_{s=1}^{x}(1 + r_{t+s})} \tau_{t+x} D_t(x). \tag{B.1.5}$$

$z_t^0(\beta)$ reflects the present discounted value of one dollar of investment deductions after tax.[2] If the firm can immediately deduct the full dollar, then z_t^0 equals τ_t. In general, the stream of future deductions will depend on future tax rates and interest rates. β is an additional discount term between zero and one, which reflects the possibility of myopia. We use our heterogeneity analysis to identify this term separately.

[1]Hayashi (1982) models adjustment costs through influencing the law of motion in (B.1.4), rather than as a net receipts flow. Abel (1982) models adjustment costs through augmenting pretax profits in (B.1.3). There is no strong a priori argument for one versus the other. We adopt this notation to simplify the borrowing constraint in our calibration exercise. Intuitively, it means adjustment costs are not verifiable and thus the firm cannot borrow to offset them. It makes sense to further assume that such costs would not be deductible as well. The hypotheses we derive do not depend on the assumption.

[2]In the main text, we define z without incorporating the tax rates, in order to isolate the direct effect of bonus. Here, we define z with tax rates because it matches Hayashi (1982)'s notation and highlights the general dependence of the term on future tax rates.

Bonus depreciation, the policy we study in our empirical analysis, allows the firm to deduct a per dollar bonus, θ_t, at the time of the investment and then depreciate the remaining $1-\theta_t$ according to the normal schedule:

$$z_t(\beta) = \tau_t\theta_t + (1-\theta_t)z_t^0(\beta) \tag{B.1.6}$$

At different points in time, Congress set θ_t equal to 0.3, 0.5 or 1. We use these policy shocks to identify the effect of bonus depreciation on investment.

We further generalize z_t by incorporating a nontaxable state. When the next dollar of investment does not affect this year's tax bill, then the firm must carry forward the deductions to future years. Our general z_t reflects this case:

$$z_t(\beta,\gamma) = \gamma z_t(\beta) + (1-\gamma)\beta\phi z_t(1), \tag{B.1.7}$$

where $\gamma \in \{0,1\}$ is an indicator for current tax state and ϕ is a discounter that reflects both the expected arrival time and the discount rate, r_T, applied to the future period when the firm switches. Note that for the nontaxable firm, β will apply to all future deductions.[3]

Hayashi (1982) considers the case with β and γ equal to one. We consider this case first. Define $z_t \equiv z_t(1,1)$. We can rewrite the objective in (B.1.1) as

$$V_0 = \sum_{t=0}^{\infty} \frac{1}{\Pi_{s=0}^{t}(1+r_s)} \left[(1-\tau_t)\pi_t - \psi_t(I_t,K_t) - (1-k_t-z_t)p_{I,t}I_t\right] + A_0, \tag{B.1.8}$$

where A_0 is the present value of depreciation deductions on past investments.[4] We assume r is fixed over time and that k equals zero, since the investment tax credit is not active during our sample frame. We isolate the terms where period t investment enters and rewrite the

[3] This formula is not exactly correct because additional periods will lead to additional accumulated losses for subsequent deductions. The firm will deduct these at an accelerated rate relative to the schedule in $z_t(1)$. This formulation simplifies the algebra and biases our empirical findings toward the neoclassical benchmark.

[4] The A_0 term is important for Hayashi (1982) because it influences the average value of the firm and one purpose of his study is to show when average Q and marginal q are equal. A_0 does not affect the investment decision problem.

relevant part of the problem:

$$\max_I \left\{ -\psi(I, K) - (1-z)p_I I + \frac{q_{t+1} I}{1+r} \right\}, \tag{B.1.9}$$

where q_{t+1} is the multiplier on the law of motion for capital.

We write the first order condition for investment as

$$q_{t+1} = (1+r)\left[\psi_I + (1-z)p_I\right], \tag{B.1.10}$$

which emphasizes that optimal investment equates the marginal product of capital, q_{t+1}, with the hurdle rate $(1+r)$ applied to the marginal costs of investment. These costs include adjustment costs and the price of investment less the value of investment as a tax shield. q_{t+1} is the marginal value of a unit of capital, which accumulates over many future periods. We can apply the envelope condition and differentiate $V_0(K_t) = \max_I V_0(K_t, I)$ to show that

$$q_t = \sum_{s=t}^{\infty} \frac{1}{\Pi_{v=t}^{s}(1+r_v+\delta)} \left[(1-\tau_s)\pi_{K,s} - \psi_{K,s}\right], \tag{B.1.11}$$

which says that q_t includes the present discounted value of future after-tax marginal products for capital, accounting for the rate of economic depreciation.[5] In a two period model without adjustment costs, we could rewrite (B.1.10) as

$$r = \left(\frac{1-\tau}{1-z}\right) \frac{\pi_{K,t+1}}{p_I} - 1, \tag{B.1.12}$$

which shows that the general condition is just a dynamic statement of the simple idea that optimal investment should equate returns and the risk-adjusted discount rate.[6]

We augment the problem to introduce the possibility of imperfect capital markets, which leads to a generalized version of (B.1.10). Firms face a credit limit on gross borrowing, B_t, which accumulates according to

$$B_{t+1} = B_t + (1-\tau_t)\pi_t - (1-z_t)p_{I,t}I_t. \tag{B.1.13}$$

[5] Note that capital also has an effect on future adjustment costs.

[6] Also, note that with immediate expensing, $z = \tau$ and so taxes do not affect investment. This also holds in certain versions of the more general model. See Abel (1982).

Firms must borrow to cover tax obligations and investment outlays, to the extent these exceed current cash flows. Note that z_t and not just $\tau\theta_t$ enters here. This is because future borrowing constraints also matter.

From Summers (1981) to Edgerton (2010), modern empirical studies of investment apply a parameterized version of (B.1.10), typically under the conditions shown in Hayashi (1982) to yield marginal q equal to average Q.[7] The financial constraint augmented first order condition is

$$q_{t+1} = (1+r)\left[\psi_I + (1+\lambda)(1-z)p_I\right], \tag{B.1.14}$$

where $\lambda \geq 0$ is the shadow price associated with the borrowing constraint (B.1.13).[8] The shadow price on the borrowing constraint works in this model much like a discount rate. To see this, note that without adjustment costs and in the one shot model we can rewrite (B.1.12) as

$$r + \lambda = \left(\frac{1-\tau}{1-z}\right)\frac{\pi_{K,t+1}}{p_I} - 1, \tag{B.1.15}$$

where we have assumed for illustration that $r\lambda$ is small. The hurdle rate for an investment project reflects both the discount rate and the borrowing spread. In our empirical analysis, we assume that firms use the same r but may differ in λ, in order to back out an implied λ spread between constrained and unconstrained firms.[9]

B.1.2 Testable Hypotheses

We can derive the three testable hypotheses outlined in Section 2.2. Each hypothesis results from defining optimal investment in (B.1.14) as a function of an exogenous parameter, a, and then implicitly differentiating. The general condition is

$$\psi_{II}\frac{\partial I}{\partial a} + \frac{\partial q}{\partial a} = (1+\lambda)p_I\frac{\partial z}{\partial a}, \tag{B.1.16}$$

[7] These assumptions include making firms price takers in all markets and linear homogeneity for production (i.e., constant returns to scale) and adjustment costs.

[8] The general version of (B.1.9) is $\max_I \left\{-\psi(I,K) - (1-z)p_I I + \frac{q_{t+1}I}{1+r} - \lambda(1-z)p_I I\right\}$.

[9] When thinking about the discount rates firms apply to depreciation tax shields, this assumption feels appropriate. In general, our estimated λ spread will also include discount rate differences.

where z includes nontaxable states and possibly myopia, as in (B.1.7) and q now satisfies the general version of (B.1.11):

$$q_t = \sum_{s=t}^{\infty} \frac{1}{\Pi_{v=t}^{s}(1 + r_v + \delta)} \left[(1 + \lambda_s)(1 - \tau_s)\pi_{K,s} - \psi_{K,s}\right]. \tag{B.1.17}$$

The only difference between (B.1.11) and (B.1.17) is that increasing capital leads to higher future after-tax profits, which relax future financial constraints.

We consider comparative statics with respect to θ, z_t^0, λ, and γ. Except for λ, none of these terms directly affect q. They only affect q through investment's effect on future capital. We assume this latter effect is negligible. While nontrivial, this assumption is justified for two reasons. First, while the policies we study have a substantial temporary effect on investment, the change in investment is small relative to the existing capital stock. Thus the long run marginal product of capital, which q measures, is likely unaffected.[10] The second reason is that nearly all empirical studies of investment incentives assume that production exhibits constant returns to scale and linear homogeneity in adjustment costs, which leads to constant q as a function of capital.[11]

Given the assumption that $\partial q / \partial \theta = 0$, our testable hypotheses build on the comparative static with respect to the bonus parameter θ:

$$\frac{\partial I}{\partial \theta} = \frac{(1 + \lambda) p_I \left[\gamma(\tau - z_t^0(\beta)) + (1 - \gamma)\beta\phi(\tau - z_t^0(1))\right]}{\psi_{II}} > 0. \tag{B.1.18}$$

Bonus depreciation increases the present value of deductions, reducing the price of investment. Thus bonus depreciation should increase investment. Alternatively, we could study the effect of a general increase in z. The comparative static here is

$$\frac{\partial I}{\partial z} = \frac{(1 + \lambda) p_I}{\psi_{II}} > 0, \tag{B.1.19}$$

[10] This is the assumption House and Shapiro (2008) make to replace short run approximations to capital and q with their steady state values. (See p. 740.)

[11] Bond and Van Reenen (2007) survey the investment literature and argue that "conclusive evidence that linear homogeneity should be abandoned in the investment literature has not yet been presented." This is because the assumption has both theoretical appeal and fits with evidence that changes in firm size are hard to predict (implying that firms do not have a sharp, optimal firm size).

which yields a useful equivalence between the depreciation elasticity, the price elasticity and the interest rate elasticity. In particular, $\varepsilon_{I,1-z} = \varepsilon_{I,p_I} \leq \varepsilon_{I,1+\lambda}$, where $\varepsilon_{I,x} = (\partial I/\partial x)(x/I)$ and the last inequality reflects the fact that $\partial q/\partial \lambda \geq 0$. We begin our empirical analysis by estimating different versions of (B.1.19), enabling easier comparisons to past work.

Hypothesis one concerns the differential effect of bonus depreciation on long and short duration industries. Long duration industries will have more delayed baseline deduction schedules and hence lower z_t^0. The hypothesis thus derives from the cross partial of (B.1.18) with respect to z_t^0:

$$\frac{\partial^2 I}{\partial \theta \partial z_t^0} = -\frac{(1+\lambda)p_I\left[\gamma + (1-\gamma)\beta\phi\right]}{\psi_{II}} < 0. \tag{B.1.20}$$

Bonus results in relatively more acceleration for long lived items and so the investment response should be greater for these items. Note this is *not* a statement about the relative price elasticities for goods of different durations, which depend on the curvature of production and adjustment cost functions. Rather, it is a statement that bonus mechanically leads to larger price reductions for long duration items, even holding underlying technologies constant.

Hypothesis two concerns the differential effect of bonus depreciation for constrained and unconstrained firms. This depends on the cross partial of (B.1.18) with respect to λ:

$$\frac{\partial^2 I}{\partial \theta \partial \lambda} = \frac{\gamma(\tau - z_t^0(\beta)) + (1-\gamma)\beta\phi(\tau - z_t^0(1))}{\psi_{II}} p_I > 0. \tag{B.1.21}$$

For constrained firms (i.e., when $\lambda > 0$), bonus both reduces the price of investment goods and relaxes the borrowing constraint. This is true even if the investment-cash flow sensitivity is zero, that is, if cash flow does not affect the marginal external finance cost, λ. The logic is similar to the foregoing logic about long versus short duration goods. The effective price cut due to bonus is larger for constrained firms, even if the cost of borrowing does not change. Under fairly general conditions therefore, financial constraints tend to amplify the effects of bonus.

Hypothesis three concerns the differential effect of bonus by taxable status. We can compare the elasticities for γ equal to zero and γ equal to one:

$$\left.\frac{\partial I}{\partial \theta}\right|_{\gamma=1} - \left.\frac{\partial I}{\partial \theta}\right|_{\gamma=0} = (1+\lambda)p_I \frac{(\tau - z_t^0(\beta)) - \beta\phi(\tau - z_t^0(1))}{\psi_{II}} > 0 \tag{B.1.22}$$

Because nontaxable firms must wait to take bonus deductions, bonus is less valuable to them. This might be due to neoclassical reasons. Namely, taking into account a possibly long delay and applying a reasonable discount rate might lead the response for nontaxable firms to be quite low, even without myopia (i.e., with $\beta = 1$). We use the empirical distribution of loss transition probabilities to calibrate ϕ in the model and ask whether the results still require $\beta < 1$.

B.1.3 Empirical Moments for Calibration

We perform a calibration exercise to distinguish between models, based on their predictions about the external finance wedge, λ, and the discount rate applied to future flows, β. This exercise requires comparing estimates across subgroups. For this comparison to be useful, we need to make certain homogeneity assumptions about technologies across these groups. In particular, we want the curvature of adjustment costs to be equal across groups.

One way to satisfy this requirement is to make second derivatives effectively constant across groups. We make a weaker assumption, based on the common quadratic form used elsewhere in the literature. One feature of relying on this assumption is that nearly all other empirical studies of investment do so as well. Specifically, we write the adjustment cost function as

$$\psi(I, K) = \frac{\alpha}{2} \left[\log(I) - \log(\delta K)\right]^2 p_I I, \tag{B.1.23}$$

so that adjustment costs are increasing quadratically as investment deviates from the replacement rate. As long as α is constant and average investment equals δK across groups, then the following results will hold.[12]

The first empirical moment we use compares the estimated response with respect to

[12] With our functional form for adjustment costs, we have $I\psi_{II} = \alpha p_I(1 + \log(I/\delta K))$, which is equal across groups under these assumptions.

bonus for constrained and unconstrained firms. Define the semi-elasticity of investment with respect to θ as $\varepsilon_{I,\theta} \equiv (\partial I/\partial\theta)(1/I)$, where $\partial I/\partial\theta$ is defined in (B.1.18). Assuming constrained firms face shadow price λ_C and unconstrained firms face shadow price λ_U, we take the ratio of semi-elasticities:

$$\frac{\varepsilon^C_{I,\theta}}{\varepsilon^U_{I,\theta}} \equiv m_1 = \frac{1+\lambda_C}{1+\lambda_U} = 1 + \frac{\Delta\lambda}{1+\lambda_U}. \tag{B.1.24}$$

We estimate m_1 and solve (B.1.24) for $\Delta\lambda/(1+\lambda_U)$, which can be viewed as an implied credit spread. Our empirical analysis estimates the semi-elasticity with respect to z, rather than θ. Because z is linear in θ (see (B.1.6)), the ratio of z semi-elasticities equals the ratio of θ semi-elasticities.

We define a second empirical moment analogously by comparing taxable and nontaxable firms:

$$\frac{\varepsilon^{\gamma=0}_{I,\theta}}{\varepsilon^{\gamma=1}_{I,\theta}} \equiv m_2 = \beta\phi\frac{\tau - z^0_t(1)}{\tau - z^0_t(\beta)} \tag{B.1.25}$$

Note the external finance wedge falls out of this expression. This is true as long as average shadow costs are the same across taxable and nontaxable groups.[13] Under a constant τ assumption, we can drop tax rates from this formula, which we do in Section 2.5.3. We estimate m_2 and calibrate ϕ in order to estimate β.

B.2 Legislative Background

This appendix describes legislation affecting the bonus and Section 179 depreciation provisions studied in this paper.

[13]We can relax this assumption, since we expect nontaxable firms to be more constrained on average. Alternatively, we can narrow our taxable/nontaxable comparison to groups that differ only by how likely it is for the next dollar of investment to affect this year's taxes. We pursue this latter approach and use the stock of alternative tax shields to sort firms.

Economic Recovery Tax Act of 1981

The act set the Section 179 allowance at $5,000 and established a timetable for gradually increasing the allowance to $10,000 by 1986. Few firms took advantage of the allowance initially. Some attributed the low response to limitations on the use of the investment tax credit. A business taxpayer could claim the credit only for the portion of an eligible asset's cost that was not expensed; so the full credit could be used only if the company claimed no expensing allowance. For many firms, the tax savings from the credit alone outweighed the tax savings from combining the credit with the allowance.[14]

Depreciation Policies Affected – Section 179

Date Signed – August 13, 1981

Bill Number – H.R. 4242

Deficit Reduction Act of 1984

The act postponed from 1986 to 1990 the scheduled increase in the Section 179 allowance to $10,000. Use of the allowance rose markedly following the repeal of the investment tax credit by the Tax Reform Act of 1986.

Depreciation Policies Affected – Section 179

Date Signed – July 18, 1984

Bill Number – H.R. 4170

Omnibus Budget Reconciliation Act of 1993

The act increased the Section 179 allowance from $10,000 to $17,500, as of January 1, 1993.

Depreciation Policies Affected – Section 179

[14] Source: `http://www.section179.org/stimulus_acts.html`

Date Introduced – May 25, 1993

Date of First Passage Vote – May 27, 1993

Date Signed – August 10, 1993

Bill Number – H.R. 2264

Small Business Job Protection Act of 1996

The act increased the Section 179 allowance and established scheduled annual (with one exception) increases over six years. Specifically, the act raised the maximum allowance to $18,000 in 1997, $18,500 in 1998, $19,000 in 1999, $20,000 in 2000, $24,000 in 2001 and 2002, and $25,000 in 2003 and thereafter.

Depreciation Policies Affected – Section 179

Date Introduced – May 14, 1996

Date of First Passage Vote – May 22, 1996

Date Signed – August 20, 1996

Bill Number – H.R. 3448

Job Creation and Worker Assistance Act of 2002

The act created the first bonus depreciation allowance, equal to 30 percent of the adjusted basis of new qualified property acquired after September 11, 2001, and placed in service no later than December 31, 2004. A one-year extension of the placed-in-service deadline was available for certain property with a MACRS recovery period of 10 or more years and for transportation equipment.

Depreciation Policies Affected – Bonus Depreciation

Date Introduced – October 11, 2001

Date of First Passage Vote – October 24, 2001

Date Signed – March 9, 2002

Bill Number – H.R. 3090

Jobs and Growth Tax Relief Reconciliation Act of 2003

The act (JGTRRA) raised the bonus allowance to 50 percent for qualified property acquired after May 5, 2003, and placed in service before January 1, 2005. The act raised the Section 179 allowance to $100,000 (as of May 6, 2003), set it to stay at that amount in 2004 and 2005, and then reset in 2006 and beyond at its level before JGTRRA ($25,000). JGTRRA also raised the phase out threshold to $400,000 from May 2003 to the end of 2005, indexed the regular allowance and the threshold for inflation in 2004 and 2005, and added off-the-shelf software for business use to the list of depreciable assets eligible for expensing in the same period.

The American Jobs Creation Act of 2004 extended the Section 179 changes made by JGTRRA through the end of 2007. The Tax Increase Prevention and Reconciliation Act of 2005 extended the changes in the allowance under JGTRRA through 2009.

Depreciation Policies Affected – Bonus Depreciation and Section 179

Date Introduced – February 27, 2003

Date of First Passage Vote – May 9, 2003

Date Signed – May 28, 2003

Bill Number – H.R. 2

US Troop Readiness, Veterans' Care, Katrina Recovery, and Iraq Appropriations Act of 2007

Congress extended the changes in the allowance made by JGTRRA through 2010, raised the maximum allowance to $125,000 and the phaseout threshold to $500,000 for 2007 to 2010, and indexed both amounts for inflation in that period.

Depreciation Policies Affected – Section 179

Date Introduced – May 8, 2007

Date of First Passage Vote – May 10, 2007

Date Signed – May 25, 2007

Bill Number – H.R. 2206

Economic Stimulus Act of 2008

The act provided for 50 percent bonus depreciation. To claim the allowance, a taxpayer had to acquire qualified property after December 31, 2007 and place it in service before January 1, 2009. The previous $125,000 limit on the Section 179 allowance was increased to $250,000, and the $500,000 limit on the total amount of equipment purchased became $800,000.

Depreciation Policies Affected – Bonus Depreciation and Section 179

Date Introduced – January 28, 2008

Date of First Passage Vote – January 29, 2008

Date Signed – February 13, 2008

Bill Number – H.R. 5140

American Recovery and Reinvestment Act of 2009

The act extended the deadlines by one year, to the end of 2009, for the 50 percent bonus depreciation allowance.

Depreciation Policies Affected – Bonus Depreciation

Date Introduced – January 26, 2009

Date of First Passage Vote – January 28, 2009

Date Signed – February 17, 2009

Bill Number – H.R. 1

Small Business Jobs Act of 2010

The act extended the 50 percent bonus depreciation to qualifying property purchased and placed in service during the 2010 tax year. The act increased the amount a business could expense under Section 179 from $250,000 to $500,000 of qualified capital expenditures. These deductions were subject to a phase-out for expenditures exceeding $2,000,000. The provision covered tax years for 2010 and 2011.

Depreciation Policies Affected – Bonus Depreciation and Section 179

Date Introduced – May 13, 2010

Date of First Passage Vote – June 17, 2010

Date Signed – September 27, 2010

Bill Number – H.R. 5297

Tax Relief, Unemployment Compensation Reauthorization, and Job Creation Act of 2010

The bonus depreciation allowance increased to 100 percent for qualified property acquired after September 8, 2010, and placed in service before January 1, 2012. The act also established a 50 percent allowance for property acquired and placed in service in 2012.

Depreciation Policies Affected – Bonus Depreciation

Date Introduced – March 16, 2010

Date Signed – September 27, 2010

Bill Number – H.R. 5297

B.3 Past User Cost Estimates

Cummins, Hassett, and Hubbard (1994)

- Estimating Equation – $\frac{I}{K} = \beta_0 + \beta_1 Q$
- Estimation Details – first-differences; firm and year FEs; robust SE
- Data – US public firm panel (Compustat), 1953-88
- Estimates
  - Pooled reforms: 0.083(0.006), Table 4 (OLS, all years), p. 28
  - 1962 reform: 0.554(0.165), Table 4 (OLS, 1962), p. 28
  - 1972 reform: 0.198(0.067), Table 4 (OLS, 1972), p. 28
  - 1981 reform: 0.299(0.091), Table 4 (OLS, 1981), p. 28
  - 1986 reform: 0.178(0.083), Table 4 (OLS, 1986), p. 28

Cummins, Hasset, and Hubbard (1996)

- Estimating Equation – $\frac{I}{K} = \beta_0 + \beta_1 Q$
- Estimation Details – difference between observed and forecasted variables; forecasting based on lagged $\frac{I}{K}$, lagged $\frac{CF}{K}$, time-trend, and firm FE; robust SE
- Data – International public firm panel (Global Vantage), 1982-92
- Estimates
  - AUS 1988: 0.647(0.238), Table 6 (AUS 1988, top), p. 254
  - BEL 1990: 1.626(0.520), Table 6 (BEL 1990, top), p. 254
  - CAN 1988: 0.810(0.216), Table 6 (CAN 1988, top), p. 254
  - DNK 1988: 0.867(0.458), Table 6 (DNK 1990, top), p. 254
  - FRA 1990: 0.756(0.286), Table 6 (FRA 1990, top), p. 254

- GER 1990: 0.938(0.242), Table 6 (GER 1990, top), p. 254
- ITA 1992: 0.663(0.237), Table 6 (ITA 1992, top), p. 254
- JPN 1989: 0.893(0.219), Table 6 (JPN 1989, top), p. 254
- NLD 1989: 0.423(0.340), Table 6 (NLD 1989, top), p. 254
- NOR 1992: 1.373(0.528), Table 6 (NOR 1992, top), p. 254
- SPN 1989: 1.485(1.378), Table 6 (SPN 1989, top), p. 254
- SWE 1990: 0.641(0.241), Table 6 (SWE 1990, top), p. 254
- UK 1991: 0.644(0.198), Table 6 (UK 1991, top), p. 254
- USA 1987: 0.603(0.086), Table 6 (USA 1987, top), p. 254

Desai and Goolsbee (2004)

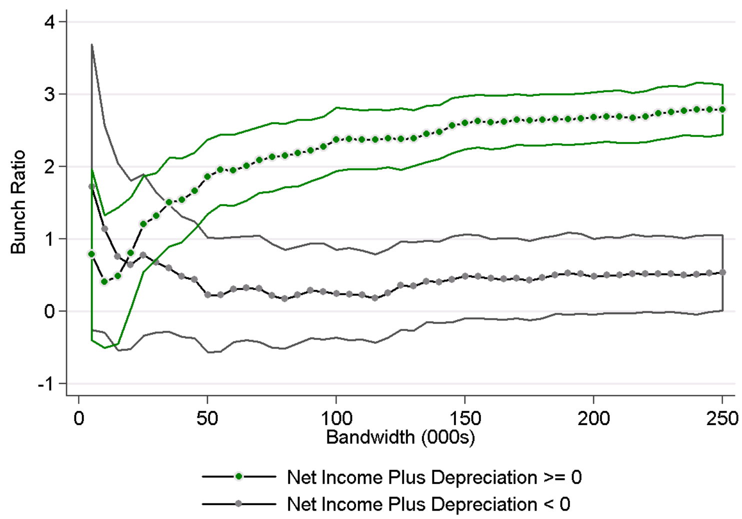
- Estimating Equation – $\frac{I}{K} = \beta_0 + \beta_1 \frac{1-\tau z - ITC}{1-\tau} + \beta_2 \frac{q}{1-\tau} + \beta_2 \frac{CF}{K}$
- Estimation Details – year and firm FEs; SE clustered at firm-level
- Data – US public firm panel (Computstat), 1962-03
- Estimates – -0.890(0.317), Table 8 (baseline), p. 314

Edgerton (2010)

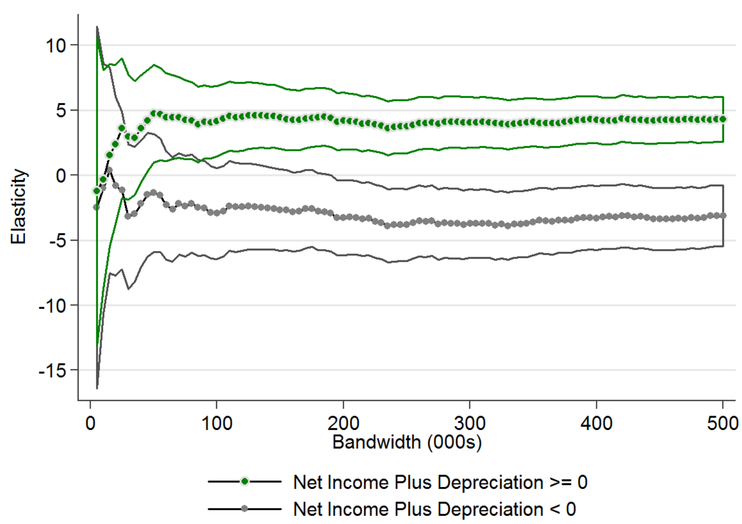
- Estimating Equation – $\frac{I}{K} = \beta_0 + \beta_1 \frac{1-\tau z - ITC}{1-\tau} + \beta_2 \frac{q}{1-\tau}$
- Estimation Details – year and firm FEs; SE clustered at firm-level; includes dummy and interaction for non-taxable firms
- Data – US public firm panel (Computstat), 1967-05
- Estimates – -0.846(0.323), Table 3 (2), p. 945

Appendix Figure B.1: *Investment Behavior and Tax Incentives: Narrow Bandwidth*

(a) Bunching with Narrow Bandwidth



(b) Bonus with Narrow Bandwidth



Notes: These figures replicate the taxable position splits in the bunch and bonus settings, while restricting the sample to within a narrow bandwidth of the tax status threshold. Panel (a) replicates the analysis in panel (a) of Figure 2.4, which compares bunching behavior for taxable and nontaxable firms. Panel (b) replicates the regression in column (1) of Table 2.7, which estimates separate coefficients with respect to bonus incentives for taxable and nontaxable firms.

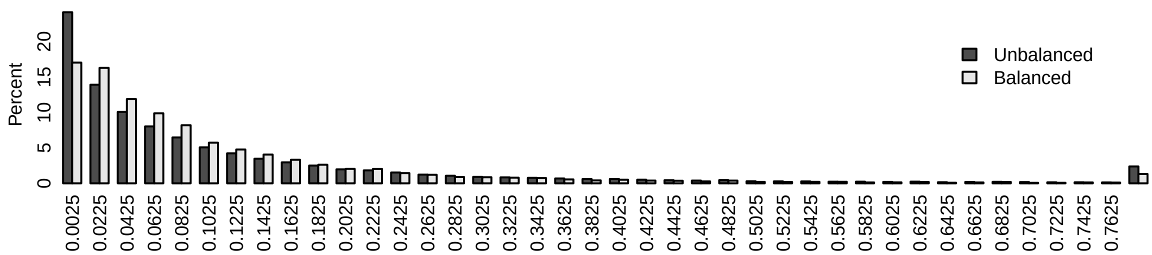
Appendix Table B.1: *Section 179 and Bonus Depreciation Policy Changes*

Year	S179 Max Value	S179 Phase-out Region	Bonus
1993-96	$17,500	$200,000-$217,500	
1997	$18,000	$200,000-$218,000	
1998	$18,500	$200,000-$218,500	
1999	$19,000	$200,000-$219,000	
2000	$20,000	$200,000-$220,000	
2001-02	$24,000	$200,000-$224,000	30% Tax years ending after 9/10/01
2003	$100,000	$400,000-$500,000	50% Tax years ending after 5/3/03
2004	$102,000	$410,000-$512,000	50%
2005	$105,000	$420,000-$525,000	
2006	$108,000	$430,000-$538,000	
2007	$125,000	$500,000-$625,000	
2008-09	$250,000	$800,000-$1,050,000	50% Tax years ending after 12/31/07
2010-11	$500,000	$2,000,000-$2,500,000	100% Tax years ending after 9/8/10

a. 2008 was retroactive.

Appendix Table B.2: *Detailed Investment Statistics (1998-2010)*

(a) Investment Rate Distribution



(b) Summary Statistics

Variable	Unbalanced	Balanced
Average investment rate	11.9% (0.20, 3.23, 12.7)	10.4% (0.16, 3.60, 17.6)
Inaction rate	30.2%	23.7%
Spike rate	17.4%	14.4%
Serial correlation of investment rates	0.38	0.40
Aggregate investment rate	7.7%	6.9%
Spike share of aggregate investment	25.1%	24.4%

(c) Summary Statistics over Time and Correlation with Aggregate Investment (Unbalanced)

Variable	1998	1999	2000	2001	2002	2003	2004	2005
Average investment rate (%)	15.1	15.7	13.9	12.1	11.3	12.0	13.0	12.7
Std. dev. investment rate	0.221	0.234	0.213	0.195	0.189	0.205	0.209	0.209
Inaction rate (%)	22.9	21.9	25.7	28.5	28.7	29.3	26.2	27.4
Spike rate (%)	22.9	23.9	21.3	17.9	16.6	16.8	18.8	18.5
Aggregate investment rate (%)	11.7	8.7	8.8	7.5	7.0	6.4	7.2	7.0
	2006	2007	2008	2009	2010		σ	β_{Agg}
Average investment rate (%)	12.8	11.3	10.4	7.1	7.0		0.026	0.74
Std. dev. investment rate	0.208	0.189	0.180	0.140	0.129		0.030	0.64
Inaction rate (%)	28.7	31.2	34.0	41.5	40.5		0.059	-0.68
Spike rate (%)	19.2	15.5	15.5	9.0	9.2		0.045	0.76
Aggregate investment rate (%)	8.3	7.5	7.5	6.3	6.0		0.015	

(d) Investment Rates by Firm Characteristics (Unbalanced)

Sorting Variable	Investment	Inaction	Spike		Investment	Inaction	Spike
Size by Mean Sales Decile (Unweighted)							
$< 0.9M$	11.2% (0.23)	53.8%	16.5%	$[23.1M, 33.5M]$	11.4% (0.17)	17.3%	16.1%
$[0.9M, 3.7M]$	13.0% (0.21)	32.0%	20.2%	$[33.5M, 48.8M]$	10.6% (0.16)	17.4%	13.7%
$[3.7M, 8.7M]$	12.0% (0.19)	23.3%	17.2%	$[48.8M, 77.4M]$	10.5% (0.16)	16.3%	13.3%
$[8.7M, 15.4M]$	11.0% (0.16)	20.3%	15.6%	$[77.4M, 164M]$	10.7% (0.16)	14.8%	13.5%
$[15.4M, 23.1M]$	11.3% (0.18)	19.5%	15.7%	$> 164M$	10.0% (0.14)	14.3%	11.7%
Dividend Payer							
Yes	8.9% (0.14)	20.2%	10.3%				
No	12.0% (0.20)	30.6%	17.6%				

Notes to Appendix Table B.2: This exhibit provides detailed investment statistics to enable comparison to past work. The investment rate is bonus eligible investment divided by lagged depreciable assets. All statistics are weighted by sampling weights from SOI. The unbalanced sample includes all firms used in the bonus analysis. The balanced sample includes only those firms in the sample for the entire sample frame. Figure (a) plots investment rate densities with intervals labeled by right end points. Table (b) follows Table 1 of Cooper and Haltiwanger (2006)*. Inaction is defined by investment below 1%. Spikes are defined by investment above 20%. Aggregate investment is total eligible investment divided by total lagged capital. The spike share of aggregate investment is total eligible investment due to spikes divided by total eligible investment. Table (c) presents these statistics over time for the unbalanced panel. σ is the standard deviation of a statistic over time. β_{Agg} is the correlation of a statistic with the aggregate investment rate. Table (d) presents investment rate statistics for the unbalanced panel with firms sorted by firm characteristics. Standard deviations, skewness and kurtosis are in parentheses for investment rates. Standard deviations are in parentheses for all other statistics.*

Appendix C

Appendix to Chapter 3

C.1 Data construction

C.1.1 Data sources

Consolidated Federal Funds Report (CFFR) files for 1983-1992 are from a US Census CD-ROM. The CFFR files for 1993-2009 were downloaded from the US Census. County-level presidential, Senate, and House election returns and district-level House election returns were collected by James Snyder. His files were supplemented with records from the *Congressional Quarterly Voting and Elections Collection*, David Leip's Election Atlas, and Polidata. Electronic maps of congressional district boundaries were obtained from the US Census and from digitizing the *Congressional District Atlas*. A mapping of Federal Information Processing Standards (FIPS) counties from 1990 to 2000 was downloaded from *MABLE/Geocorr*, a product of the Missouri Census Data Center. Information about party leadership positions and congressional committee assignments was obtained from Charles Stewart III's website. Historical annual federal outlays and executive agency-level discretionary shares were compiled from the Public Budget Database provided by the Office of Management and Budget (OMB). The US Census provided annual county population and demographic estimates. The consumer price index was downloaded from the Bureau of Labor Statistics.

C.1.2 Geographic definitions

County boundaries are defined according to the 2000 FIPS definitions. This excludes only a few counties because of the rare number of boundary changes that took place over the panel's tenure.

C.1.3 Spending measures

I exclude from all federal spending measures any items identified as payments for insurance or loans because they are not directly comparable to other forms of expenditures/obligations. These categories report full contingent liabilities which can be far greater than the actual dollar amount disbursed by the government. To compute a per-capita measure of federal spending for each bi-annual Congress, I use the mean county population estimate for the two corresponding fiscal years.

C.1.4 Political variables

I defined redistricting as an event whenever the group of counties that uniquely matched a single congressional district changed based on a Congress-by-Congress ArcGIS mapping of counties to congressional districts. Partisan representation of congressional districts was defined on the basis of the general election outcome. Special elections and temporary appointments were ignored.

C.1.5 Comparability of OMB and CFFR outlays

I use outlay totals reported by the OMB twice in this paper: (i) to assess how comprehensively the CFFR covers federal spending and (ii) to create an approximate discretionary measure of federal spending. OMB and CFFR outlays are not, however, directly comparable because the OMB reports net outlays, which includes offsetting receipts such as license and user fees, whereas the CFFR reports gross outlays. To make them as comparable as possible, I computed the OMB totals and discretionary shares from the *Public Budget Database* after (i) dropping all receipt accounts, (ii) dropping net interest payments, (iii) dropping spending under the international affairs budget function code, and (iv) forcing to zero all negative line items.

C.2 Data description

C.2.1 Comprehensiveness of the spending measures

Appendix Table C.1 compares the CFFR spending measures to the historical federal outlays reported by the OMB to gauge how much federal spending is included in the CFFR. The table reports the CFFR measures as a share of the OMB total. These measures include all counties. Coverage for total spending ranges from 0.74 to 0.85. Coverage for the other spending measures is consistent with expectations.

C.2.2 Demographic comparison of sample to the United States

Appendix Table C.2 compares county-level demographics between the main sample and all counties in the United States consistent with the 2000 FIPS definition. As expected, the counties in the sample are considerably smaller in population and density than the average US county. The sample is more similar to the US in its population share of African-Americans, population share of individuals aged 65 years and older, and in per-capita income. Furthermore, the county quantiles do not exhibit dramatic differences in demographics because they are balanced within each congressional district.

C.3 Robustness

C.3.1 Model fit

Appendix Figure C.1 plots predicted versus observed measures of spending from the full panel, regression discontinuity, and first-difference estimates. Under all specifications, the predicted 95 percent confidence interval covers 93.5 to 95.5 percent of the observations.

Although the plots reveal some outliers, the predicted confidence intervals have about the right coverage of the observed data.

C.3.2 Alternative partisan county quantile definitions

Appendix Table C.3 replicates the full panel, regression discontinuity, and first-difference estimates using alternative definitions for partisan county quantiles. Instead of using the panel average Republican vote share in presidential elections, this panel considers quantiles based on House election returns and on a moving average over a rolling window. For the presidential moving average, the rolling window includes the three previous elections. For the House moving average, it includes the five previous elections. Both exclude the concurrent election. The House panel average quantile only includes observations for elections in years 1982-1994, 2002, and 2004 due to the limited availability of House election data by county. Similarly, the House moving average quantile only includes observations for the period 1982-1994.

There is one significant coefficient under the presidential moving average and the House moving average quantile definitions respectively. These results are not, however, consistent across specifications. The estimates found here do not substantively change the interpretation of the main analysis.

C.3.3 Spending variables divided by voter turnout

Appendix Table C.4 replicates the full panel estimates using spending variables divided by voter turnout. Panels A and B respectively use presidential and US House elections to measure voter turnout and to define partisan county quantiles based on panel average vote shares. I also smooth presidential voter turnout as an average of the previous two presidential elections because they only occur every other Congress. Furthermore, Panel B only includes observations for elections in years 1982-1994, 2002, and 2004 because of incomplete House election data at the county level. Because the discretionary spending measures only spans the period 1993-2008, Panel B excludes it.

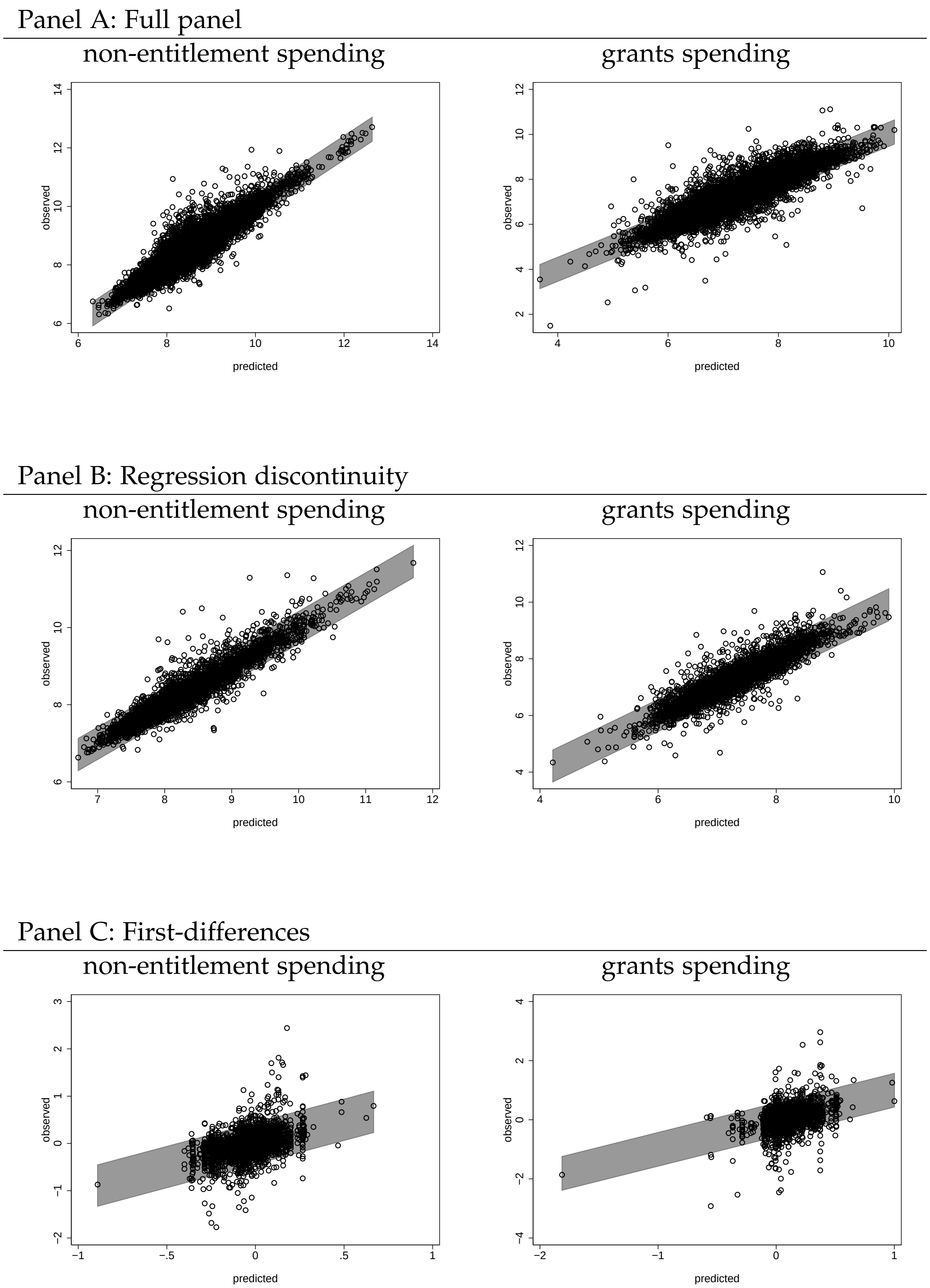
Panel A finds insignificant results with precision similar to the main analysis when accounting for turnout in presidential elections. Panel B also finds insignificant results when considering US House elections, although the standard errors are larger as expected due to the fewer available observations.

C.3.4 Validity of regression discontinuity design

Appendix Table C.5 checks for the smoothness of two important covariates with log per-capita spending in the RDD: county demographics and lagged spending variables. A non-null result here would suggest that, even in the RDD, the distribution of spending in the congressional districts represented by Republican and Democratic legislators differ for reasons other than politics. In all cases, I find insignificant results.

Appendix Table C.6 compares RDD estimates using bandwidths of 7.5, 5, and 2.5 percentage points. These estimates check the sensitivity of the RDD to the choice of bandwidth. For all spending measures, the results are statistically indistinguishable across bandwidths.

Appendix Figure C.1: *Predicted versus observed log per-capita federal spending from the full panel, regression discontinuity, and first-difference estimates*



Notes: Estimated specifications from the main analysis. Gray band represents the 95 percent confidence interval of the predicted values. See text for additional details.

Appendix Table C.1: *County-level federal spending measures from the Consolidated Federal Funds Report (CFFR) as a share of the historical Office of Management and Budget (OMB) aggregates*

	CFFR spending measures as a share of OMB aggregates				
Congress	total	non-entitlement	discretionary	grants	contracts
98	0.80	0.49		0.11	0.19
99	0.80	0.48		0.10	0.18
100	0.77	0.44		0.10	0.15
101	0.76	0.42		0.11	0.14
102	0.81	0.43		0.14	0.12
103	0.84	0.43	0.29	0.16	0.12
104	0.84	0.40	0.28	0.16	0.11
105	0.83	0.40	0.25	0.17	0.11
106	0.85	0.41	0.26	0.18	0.11
107	0.81	0.40	0.29	0.18	0.12
108	0.80	0.39	0.29	0.17	0.13
109	0.79	0.39	0.26	0.16	0.13
110	0.76	0.37	0.25	0.17	0.13

Notes: CFFR spending measures include all counties (in order to assess how comprehensively the CFFR covers federal spending). Non-entitlement spending excludes Social Security, Medicare, and Medicaid. Discretionary spending is approximated using historical executive agency-level shares of discretionary spending from the OMB. Grants are transfers either to state and local government or to private organizations. Contracts include only spending on procurement.

Appendix Table C.2: *Sample summary statistics of county demographics*

		partisan county quantiles			
	United States	base co-partisan	swing co-partisan	swing opposition	base opposition
population	85,708	42,322	26,762	29,212	48,403
	(281,769)	(61,161)	(35,434)	(44,369)	(63,300)
pop. per square mile	236	93	66	78	102
	(1,640)	(337)	(231)	(355)	(307)
pop. share black	0.09	0.09	0.06	0.06	0.09
	(0.14)	(0.17)	(0.12)	(0.12)	(0.14)
pop. share 65+ years	0.12	0.12	0.13	0.13	0.12
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
per-capita income	22,250	21,581	21,463	21,323	21,704
	(5,836)	(5,173)	(4,880)	(4,753)	(5,067)
no. of county-congresses	40,789	7,177	7,155	7,698	6,494

Notes: Standard deviations in parentheses. Summary statistics for the United States report means and standard deviations across counties and includes all counties consistent with the 2000 FIPS definition. Per-capita income is inflation-indexed to 2000 US dollars. Within each district-congress, counties are sorted into either Republican or Democratic quantiles by their panel mean Republican vote share in presidential elections and into either swing or base quantiles by their panel standard deviation Republican vote share in presidential elections. Counties are defined as co-partisan when their partisanship matches their House representative in the current Congress and are defined as opposition otherwise. See text for additional details.

Appendix Table C.3: *US House: full panel, regression discontinuity, and first-difference estimates on log per-capita federal spending under alternative partisan county quantiles*

	moving average presidential vote share		panel average House vote share		moving panel average House vote share	
	non-entitlement spending	grants spending	non-entitlement spending	grants spending	non-entitlement spending	grants spending
Panel A: Full panel						
co-partisan county	0.0001	0.0144	0.0104	0.0146	0.0080	0.0401**
	(0.0071)	(0.0088)	(0.0140)	(0.0160)	(0.0113)	(0.0176)
swing county	-0.0003	-0.0054	0.0100	0.0027	0.0006	0.0127
	(0.0062)	(0.0103)	(0.0185)	(0.0197)	(0.0120)	(0.0156)
co-partisan county	-0.0060	0.0047	-0.0075	0.0147	-0.0053	-0.0155
×swing county	(0.0077)	(0.0113)	(0.0131)	(0.0185)	(0.0136)	(0.0264)
county FE	X	X	X	X	X	X
district-congress FE	X	X	X	X	X	X
no. of county-congresses	28,512	28,512	17,195	17,195	10,681	10,681
no. of district-congresses	2,208	2,208	1,363	1,363	852	852
no. of states	47	47	47	47	46	46
Panel B: Regression discontinuity						
Republican county	-0.0132	-0.0006	-0.0041	0.0797	0.0192	0.0111
×Republican district	(0.0286)	(0.0341)	(0.0396)	(0.0716)	(0.0504)	(0.0668)
county FE	X	X	X	X	X	X
district-congress FE	X	X	X	X	X	X
no. of county-congresses	5,304	5,304	3,059	3,059	1,953	1,953
no. of district-congresses	383	383	226	226	151	151
no. of states	45	45	42	42	36	36
Panel C: First-difference						
Δ co-partisan county	0.0083	0.0062	-0.0013	0.0295	0.0889	0.1264
	(0.0142)	(0.0159)	(0.0207)	(0.0219)	(0.0505)	(0.0988)
Δ swing county	0.0337***	0.0218	-0.0256	0.0376	0.0089	0.0310
	(0.0096)	(0.0173)	(0.0167)	(0.0334)	(0.0369)	(0.0653)
Δ (co-partisan county	-0.0236	-0.0074	0.0090	-0.0365	-0.0057	0.0273
×swing county)	(0.0144)	(0.0255)	(0.0229)	(0.0398)	(0.0482)	(0.1046)
district-congress FE	X	X	X	X	X	X
no. of county-congresses	3,716	3,715	1,916	1,915	265	264
no. of district-congresses	350	350	313	313	25	25
no. of states	37	37	37	37	10	10

*Notes: Standard errors in parentheses and clustered by state. Opposition county and base county are the omitted categories in Panels A and C. Democratic county, base county, and Democratic district are the omitted categories in Panel B. Moving average captures the average of the election returns for the current and previous five Congresses. Panel average uses the average for the entire data set. Panel B implements the RDD by using the full sample to estimate the county fixed effects and by limiting a local linear regression to elections where the leading candidate wins by less than a 5 percentage point margin. Panel C restricts the sample to periods before and after redistricting where the party of the House representative also remains the same. See text for additional details. * - p-value < 0.10, ** - p-value < 0.05, *** - p-value < 0.01*

Appendix Table C.4: *US House: full panel estimates on spending variables divided by voter turnout*

	log (per-voter federal spending)				
	total	non-entitlement	discretionary	grants	contracts
Panel A: Turnout in presidential elections					
co-partisan county	-0.0048	-0.0061	0.0097	0.0096	-0.0158
	(0.0064)	(0.0094)	(0.0120)	(0.0104)	(0.0242)
swing county	0.0069	0.0001	-0.0154	-0.0103	-0.0687*
	(0.0103)	(0.0166)	(0.0223)	(0.0150)	(0.0386)
co-partisan county	-0.0002	0.0031	-0.0179	-0.0058	0.0517
×swing county	(0.0071)	(0.0115)	(0.0138)	(0.0127)	(0.0326)
county FE	X	X	X	X	X
district-congress FE	X	X	X	X	X
no. of county-congresses	28,522	28,522	17,815	28,522	28,522
no. of district-congresses	2,208	2,208	1,355	2,208	2,208
no. of states	47	47	47	47	47
Panel B: Turnout in US House elections					
co-partisan county	0.0042	0.0128		0.0148	0.0147
	(0.0138)	(0.0227)		(0.0244)	(0.0528)
swing county	0.0112	0.0196		0.0519	0.0001
	(0.0147)	(0.0243)		(0.0316)	(0.0744)
co-partisan county	-0.0104	-0.0251		-0.0207	0.0059
×swing county	(0.0180)	(0.0253)		(0.0321)	(0.0660)
county FE	X	X		X	X
district-congress FE	X	X		X	X
no. of county-congresses	12,952	12,952		12,952	12,952
no. of district-congresses	1,018	1,018		1,018	1,018
no. of states	46	46		46	46

*Notes: Standard errors in parentheses and clustered by state. Opposition county and base county are the omitted categories. Panel A uses presidential elections to measure voter turnout and to define the partisan county quantiles based on panel average vote shares. Similarly, Panel B uses US House elections to measure voter turnout and define the partisan county quantiles. The measure of turnout from presidential elections is an average of the two previous presidential elections because they only occur every other Congress. Within each district-congress, counties are sorted into either Republican or Democratic quantiles by their panel mean vote share and into either swing or base quantiles by their panel standard deviation vote share. Counties are defined as co-partisan when their partisanship matches their House representative in the current Congress and are defined as opposition otherwise. See text for additional details. * - p-value < 0.10, ** - p-value < 0.05, *** - p-value < 0.01*

Appendix Table C.5: *US House: regression discontinuity estimates on covariates with federal spending*

Panel A: County demographics

	log (total population)	log (pop per sq. mile)	pop. share African-American	pop. share 65+ years	log (per-capita income)
Republican county ×Republican district	-0.0049 (0.0140)	-0.0049 (0.0140)	-0.0022 (0.0015)	0.0004 (0.0020)	-0.0149 (0.0095)
county FE	X	X	X	X	X
district-congress FE	X	X	X	X	X
no. of county-congresses	5,304	5,304	5,304	5,304	5,242
no. of district-congresses	383	383	383	383	383
no. of states	45	45	45	45	45

Panel B: Lagged log (per-capita federal spending)

	total	non-entitlement	discretionary	grants	contracts
Republican county ×Republican district	0.0234 (0.0256)	0.0176 (0.0357)	0.0624 (0.0520)	0.0466 (0.0394)	-0.0548 (0.1027)
county FE	X	X	X	X	X
district-congress FE	X	X	X	X	X
no. of county-congresses	4,724	4,724	2,888	4,722	4,715
no. of district-congresses	342	342	198	342	342
no. of states	45	45	42	45	45

*Notes: Standard errors in parentheses and clustered by state. Democratic county, base county, and Democratic district are the omitted categories. Implements the RDD by using the full sample to estimate the county fixed effects and by limiting a local linear regression to elections where the leading candidate wins by less than a 5 percentage point margin. Per-capita income is inflation-indexed to 2000 US dollars. See text for additional details. * - p-value < 0.10, ** - p-value < 0.05, *** - p-value < 0.01*

Appendix Table C.6: *Comparison of regression discontinuity estimates on log per-capita federal spending under alternative bandwidths*

	bandwidth (in percentage points)		
	7.5	5	2.5
Panel A: Total spending			
Republican county ×Republican district	0.0103 (0.0186)	-0.0034 (0.0204)	0.0331 (0.0244)
no. of county-congresses	7,669	5,304	2,624
no. of district-congresses	570	383	195
no. of states	46	45	41
Panel B: Non-entitlement spending			
Republican county ×Republican district	-0.0058 (0.0314)	-0.0267 (0.0315)	0.0228 (0.0366)
no. of county-congresses	7,669	5,304	2,624
no. of district-congresses	570	383	195
no. of states	46	45	41
Panel C: Discretionary spending			
Republican county ×Republican district	0.0248 (0.0462)	-0.0192 (0.0529)	-0.0370 (0.0921)
no. of county-congresses	4,905	3,351	1,518
no. of district-congresses	357	232	114
no. of states	46	43	37
Panel D: Grants spending			
Republican county ×Republican district	-0.0154 (0.0372)	-0.0531 (0.0450)	-0.0184 (0.0675)
no. of county-congresses	7,669	5,304	2,624
no. of district-congresses	570	383	195
no. of states	46	45	41
Panel E: Contracts spending			
Republican county ×Republican district	-0.0976 (0.0840)	-0.1011 (0.1043)	-0.1014 (0.1756)
no. of county-congresses	7,669	5,304	2,624
no. of district-congresses	570	383	195
no. of states	46	45	41

Notes: Standard errors in parentheses and clustered by state. Dependent variable is log per-capita federal spending. Includes district-congress and county fixed effects. Implements the RDD by using the full sample to estimate the county fixed effects and by limiting a local linear regression to elections where the leading candidate wins by less than either a 7.5*,* 5*, or* 2.5 *percentage point margin. See text for additional details. * - p-value < 0.10, ** - p-value < 0.05, *** - p-value < 0.01*