HOW DOES TAX POLICY INFLUENCE CORPORATE BEHAVIOR?

EVIDENCE FROM THE RESTAURANT INDUSTRY

A Dissertation

by

SCOTT GREGORY RANE

Submitted to the Office of Graduate and Professional Studies of
Texas A&M University
in partial fulfillment of the requirements for the degree of

DOCTOR OF PHILOSOPHY

Chair of Committee,	Sean T. McGuire
Committee Members,	Sarah C. Rice
	John R. Robinson
	Connie D. Weaver
	Christa H.S. Bouwman
Head of Department,	James J. Benjamin

December 2019

Major Subject: Business Administration

ABSTRACT

This study examines the influence of tax policy on corporate behavior. Changes in tax policy can provide cash windfalls to firms in the form of cash tax savings. However, identifying a setting with a tax-related cash windfall that is distinct from other tax policy changes is difficult. I exploit a unique natural experiment and examine whether the implementation of the Tip Credit in the restaurant industry influenced corporate investment and payout behavior. I find that Tip Credit firms increased investment and were more likely to increase payout to shareholders following the implementation of the credit, relative to control firms. I also find that firms increased payout in the form of repurchases. Further analysis suggests that the relation between the implementation of the Tip Credit and firm behavior varies with firm financial constraints. Overall, these findings suggest that tax policy changes can have an important impact on business decisions.

DEDICATION

I dedicate this dissertation to my family. To my wife, Lacy, for embracing this adventure with me and for her unwavering love and support. To my children, for their daily hugs when I came home. And to my parents, for their incredible examples.

ACKNOWLEDGEMENTS

I am extremely grateful for the guidance and support of my dissertation committee: Sean McGuire (chair), Connie Weaver, Sarah Rice, John Robinson, and Christa Bouwman. I am also thankful for helpful comments from Anwer Ahmed, Jennifer Glenn, Timothy Halverson, Bradford Hepfer, Abigail Needles, Senyo Tse, and workshop participants at Texas A&M University, the University of Florida, Southern Methodist University, Arizona State University, and the University of Kansas.

I also thank Mike Kinney, my fellow doctoral students, and the professors in the Department of Accounting at Texas A&M University for selflessly giving of their time and energy to facilitate my success. I especially thank my cohorts, Mehmet Kara and Tara Vakil, who have become lifelong friends.

Finally, I appreciate generous financial support from Mays Business School and the Texas A&M James Benjamin Department of Accounting.

CONTRIBUTORS AND FUNDING SOURCES

Contributors

This work was supervised by a dissertation committee consisting of Professors Sean T. McGuire (chair), Sarah C. Rice, John R. Robinson, and Connie D. Weaver of the Department of Accounting and Professor Christa H.S. Bouwman of the Department of Finance.

All work conducted for the dissertation was completed by the student independently.

Funding Sources

Graduate study was supported by a research fellowship from Mays Business School and the Texas A&M Department of Accounting.

TABLE OF CONTENTS

Page

ABSTRACT ii

DEDICATION iii

ACKNOWLEDGEMENTS iv

CONTRIBUTORS AND FUNDING SOURCES v

TABLE OF CONTENTS vi

LIST OF TABLES viii

1. INTRODUCTION 1

2. TIP CREDIT INSTITUTIONAL DETAILS 9

3. PRIOR LITERATURE AND HYPOTHESIS DEVELOPMENT 12

3.1. Prior Literature 12
3.2. Hypothesis Development 14
3.2.1. Tip Credit and Investment 14
3.2.2. Tip Credit and Payout 15

4. SAMPLE SELECTION AND RESEARCH DESIGN 17

4.1. Sample Selection 17
4.2. Research Design 19
4.2.1. Investment Tests 19
4.2.2. Payout Policy Tests 21
4.2.3. Descriptive Statistics 22

5. EMPIRICAL RESULTS 26

5.1. Tests of Hypothesis 1: Investment 26
5.2. Tests of Hypothesis 2: Payout Policy 29
5.3. Additional Analyses 32
5.3.1. Financial Constraints 32
5.4. Parallel Trends Assumption 41
5.4.1. Pre-Period Trend 41

5.4.2. Falsification Tests....41
5.5. Robustness Tests....42
5.5.1. Entropy Balancing....42
5.5.2. Generalized Difference-in-Differences Model with Fixed Effects....46
5.5.3. Alternative Estimation Techniques....50
5.5.4. Studentized Residuals....52
5.5.5. Unexpected Investment....54
5.6. Tax Policy and Debt....56
5.7. Stock Market Test....58

6. CONCLUSIONS....60

REFERENCES....62

APPENDIX VARIABLE DEFINITIONS....67

LIST OF TABLES

Page

Table 1 Tax Rate Reconciliation 11

Table 2 Sample Selection Procedure 19

Table 3 Descriptive Statistics 24

Table 4 Tip Credit and Investment 28

Table 5 Tip Credit and Payout 31

Table 6 Tip Credit, Investment, and Financial Constraint 35

Table 7 Tip Credit, Payout, and Financial Constraint 39

Table 8 Entropy Balanced Tests of H1 and H2 44

Table 9 Tip Credit and Investment - Fixed Effects Model 47

Table 10 Tip Credit and Payout - Fixed Effects Model 49

Table 11 Tip Credit and Investment - Alternative Tests 51

Table 12 Tip Credit and Investment - Studentized Residuals 53

Table 13 Tip Credit and Unexpected Investment 55

Table 14 Tip Credit and Debt 57

Table 15 Tip Credit Announcement - Market Test 59

1. INTRODUCTION

Changes in tax policy, either through a reduction in statutory tax rates or targeted tax incentives, can result in substantial cash windfalls for corporations in the form of cash tax savings. Because the behavior of firms following a tax policy change can impact both firm value and economic growth, the influence of tax policy on corporate decision-making is a topic of considerable interest among policymakers and academics (Graham 2003; Hanlon and Heitzman 2010; Wolfers 2018). This study exploits a unique natural experiment to examine how a cash windfall from a change in tax policy influences corporate financial decisions. Specifically, I investigate how the implementation of the Tip Credit in the restaurant industry influenced investment and shareholder payout.

Understanding how tax-related cash windfalls influence investment and payout is critical for evaluating and developing tax policy because these decisions can impact a broad range of stakeholders (Graham 2003). However, governments often change multiple components of tax law simultaneously, making it difficult to identify the influence of a tax benefit that generates a significant cash windfall. Consistent with this notion, prior research finds conflicting corporate responses to changes in tax policy. For example, prior research examines broad changes in tax policy (e.g. statutory tax rate change) and finds that firms' responses are inconsistent, suggesting that tax policy changes have only a limited impact on firm investment practices (Desai and Goolsbee 2004; Gale, Krupkin, and Rueben 2015). More recent studies find that the relation

between tax incentives and investment is weak or only exists in certain circumstances (Edgerton 2010; Yagan 2015; Dobridge 2016; Zwick and Mahon 2017; Luchs, Plesko, and Utke 2018). Likewise, evidence on the influence of tax-related cash windfalls on shareholder payout is limited to firms' response to a temporary repatriation tax holiday (Blouin and Krull 2009).

The Tip Credit provides a powerful setting to examine the influence of a tax-related cash windfall on corporate financial decisions. The credit was implemented unexpectedly as part of the Omnibus Budget Reconciliation Act of 1993 (OBRA 1993) to encourage firms in the restaurant industry to more accurately report the tip wages of their employees (Koenig 2015).[1] The Tip Credit provides a tax credit to firms for payroll taxes paid on tip wages received by restaurant employees. As a result, the credit provides a significant tax benefit to restaurant owners with tipped employees. For example, among firms that disclose the tax benefit associated with the Tip Credit, the average expected cash savings was roughly six percent of pre-tax income.[2] In contrast, restaurant firms without tipped employees do not benefit from the Tip Credit. Thus, the implementation of the Tip Credit allows me to observe the responses of restaurant

[1] The Omnibus Budget Reconciliation Act of 1993 (H.R. 2264), P.L. 103-66 was signed on August 10, 1993 as part of the first session of the 103rd Congress. Through conversations with practitioners and a review of news articles during the time period, it appears that the Tip Credit was not anticipated by the industry prior to the passage of the Omnibus Budget Reconciliation Act. See Section 5 for a stock market test.

[2] Not all firms eligible for the Tip Credit disclose the amount or tax rate benefit of the credit. I calculate the six percent of pre-tax income from a hand collected sample of firms that disclose the Tip Credit benefit in 1994. Anecdotal evidence corroborates the significance of the credit as well as arguments surrounding the consideration of the repeal of the Tip Credit as part of proposed federal budgets (Committee for a Responsible Federal Budget 2013). In an untabulated analysis, I perform a multivariate test of the change in the cash effective tax rates (Cash ETR) for Tip Credit firms and find results similar to the hand collected amount; a roughly six percent decrease in Cash ETR.

corporations following a shock to cash due to a tax policy change holding other changes in tax policy constant.[3]

Using the implementation of the Tip Credit to identify a change in tax policy, I first examine the impact of tax policy on corporate investment decisions. An increase in cash induced by a change in tax policy can influence investment by increasing access to internal capital. Theory suggests that internal capital is cheaper than external capital and therefore the first choice for funding new projects (Myers and Majluf 1984).[4] The Tip Credit provides a potentially significant increase in current and future available internal capital for restaurant firms with tipped employees. Therefore, I predict that Tip Credit firms will increase investment following the implementation of the credit. However, unlike other targeted tax benefits (e.g., the Research and Development Credit or accelerated depreciation), the Tip Credit does not provide a specific incentive to invest. Consequently, it is possible that the Tip Credit will not alter the established investment strategies of affected restaurant firms.

I also investigate whether the Tip Credit influences firms' payout policy. A cash windfall can create significant agency conflicts because managers may use the funds for personal gain or to invest in suboptimal projects (Richardson 2006). From a theoretical perspective, Jensen (1986) predicts that firms will payout at least a portion of a cash

---

[3] Several other tax policy changes were a part of OBRA 1993, including an increase in the corporate tax rate from 34% to 35%. However, I do not expect these changes to affect my treatment and control firms differently.

[4] Theory also suggests that taxes are related to investment because taxes affect the total cost of the investment (Hall and Jorgenson 1967). I acknowledge that the reduction in the marginal tax rate of future investments is an alternative or additional explanation for an increase in investment spending following an ongoing cash windfall.

windfall to mitigate the agency conflicts associated with free cash flow. The implementation of the Tip Credit provided an ongoing cash windfall for eligible firms without any specific restrictions on the use of the new cash. Thus, I expect that Tip Credit firms will increase shareholder payout following the implementation of the credit. However, it is not obvious that the cash windfall from the Tip Credit will be associated with an increase in shareholder payout. Brav, Graham, Harvey, and Michaely (2005) find that an increase in shareholder payout is not a first-order concern for managers, which suggests that the Tip Credit may not alter firms' payout policy.

To examine whether the Tip Credit influenced investment and payout, I use a difference-in-differences design surrounding the implementation of the Tip Credit with a sample of restaurant firms with publicly available data. I find that Tip Credit firms increase investment spending relative to control firms, in the form of capital expenditures. This finding is consistent with the prediction that, on average, firms use increased internal capital from a tax policy change for additional investment spending. Specifically, this result suggests that even in the absence of investment incentives tax policy changes can influence investment decisions. I also find that Tip Credit firms were more likely to increase payout to shareholders relative to control firms following the implementation of the Tip Credit. This result suggests that Tip Credit firms paid out at least some of the tax savings to shareholders, consistent with an effort to mitigate the

agency conflicts associated with free cash flow. [5]

To investigate further, I also examine whether Tip Credit firms increase payout in the form of dividends or repurchases. Unlike many other tax policies (e.g., repatriation tax holiday or R&D credit), the Tip Credit provides a potential ongoing reduction in taxes independent of firm investment or operating decisions. In the presence of a recurring cash windfall, theory suggests that firms may increase dividends (Jensen 1986). Interestingly, I find that Tip Credit firms were more likely to increase repurchases relative to control firms, but I do not find a change in the likelihood of a dividend increase. These results suggest an inclination toward repurchases consistent with firm avoiding the pre-commitment to future payouts associated with dividends (Guay and Harford 2000; Jagannathan, Stephens, and Weisbach 2000). Further, this finding is consistent with recent anecdotal evidence that suggests that a surge in stock buybacks are a result of the recent corporate tax rate cut as part of the Tax Cuts and Jobs Act of 2017 (TCJA) (Marron 2018).

To provide further insight into the relation between tax policy and firm behavior, I investigate whether financial constraints influence a firm's investment and payout response to the implementation of the Tip Credit. I find that financially constrained firms increase investment to a greater extent when compared with firms with relatively low

[5] Although restaurant firms with tipped and non-tipped employees represent an advantageous treatment and control group, I did not randomly assign these firms and treatment and control firms have some differences in observable characteristics. In order to mitigate concerns that sample selection bias effects my results, I employ entropy balancing and propensity score matching and find that the results are robust to these sample selection and weighting procedures. See Section 5 for details.

financial constraints. This finding is consistent with the theory that additional cash flow allows financially constrained firms to investment in previously unfunded projects (Fazzari, Hubbard, and Petersen 1988). I also find that the relation between the Tip Credit and shareholder payout is concentrated among firms with low financial constraints. These additional findings suggest that a firm's response to a cash windfall from a tax policy change is conditional on its financial position. Overall, these findings suggest that tax policy changes can have an important impact on business decisions and that this relation varies with firm characteristics.

This study contributes to several streams of literature. First, this study contributes to the literature that investigates the relation between tax policy and corporate investment. Decades of research is aimed at addressing the connection between tax policy and investment with inconsistent results (Desai and Goolsbee 2004; Hassett and Newmark 2008). This study contributes to this debate by addressing whether a targeted tax benefit influences investment even in the absence of specific investment incentives (unlike the R&D credit or bonus depreciation). Further, the inconsistent results in prior research may be influenced by other economic and policy factors that changed simultaneously. The implementation of the Tip Credit improves identification of the influence of tax policy changes because it allows for a comparison of Tip Credit and non-Tip Credit firms after holding constant other changes in tax policy that are likely to influence firm behavior.

Second, this study contributes to the payout policy literature. Although the payout policy literature is extensive (DeAngelo, DeAngelo, and Skinner 2009), there is

limited evidence on whether corporate cash windfalls influence payout policy beyond broad economic studies. In a tax-related setting, Blouin and Krull (2009) find that firms increase repurchases following a temporary repatriation tax holiday. I extend Blouin and Krull (2009) by providing evidence that firms also modify their payout policy in response to an ongoing reduction in taxes where the tax savings are not required to be used for specific investment activities.[6] Additionally, the results of the financial constraint tests suggest that the impact of tax policy changes varies widely based on certain firm characteristics.

Third, this study contributes to the cash windfall literature, which is largely limited to studies that examine endogenous cash windfalls (Bates 2005; Blouin and Krull 2009; Faulkender and Petersen 2012; von Beschwitz 2018). This study contributes to this literature by investigating a cash windfall that is independent from major corporate decisions, such as divestitures, acquisitions, or repatriation. This study adds the descriptive evidence of Blanchard, Lopez-de-Silanes, and Shleifer (1994) by examining a case in which an increase in cash was given to only some firms in the industry, providing a plausible exogenous shock to cash flow.

Finally, my results more generally provide evidence on the consequences of a change in tax policy. The Tip Credit was intended to increase compliance related to the reporting of tip wages, however the evidence in this study suggests that it also had a

[6] Although no tax law change in the U.S. is necessarily ongoing or permanent, the Tip Credit was not attached to a sunset provision and was not scheduled to expire. Discussions of the repeal of the Tip Credit was not reported until 2013.

significant influence on corporate investment and payout policy. Although the Tip Credit is a unique tax policy in a specific industry, the behavioral consequences of increased cash flow from a tax policy change are broadly applicable. Recently, U.S. corporations experienced a dramatic decrease in their income tax rate as part of the TCJA.[7] However, multiple confounding factors make it difficult to identify the effect of the increase in cash from this rate change. Although I do not test the general-equilibrium effects of the Tip Credit, the unique attributes of this setting provide insights about the influence of a targeted tax benefit on firm behavior.

[7] The Tax Cuts and Jobs Acts (TCJA) was passed by Congress on December 22, 2017, which implemented major changes to the corporate tax code, including a reduction in the statutory tax rate from 35% to 21%.

2. TIP CREDIT INSTITUTIONAL DETAILS

The total amount of tip wages in the U.S. is significant. Based on 2017 estimates published by the National Restaurant Association (NRA), the restaurant industry in the U.S. receives $799 billion in total sales and employs 10% of the domestic work force with the average hourly pay of servers with tips at $12.15 (NRA 2017).[8] Although estimates on total tip wages received is limited, recent data suggest that average tip wages equals between 14 and 17% of sales (Ferdman 2014). The IRS classifies tip wages as income that is subject to both income tax and payroll taxes. However, since tips are not paid by restaurant owners, the full amount of tip wages may not be reported by either the employee or employer. The IRS requires restaurant firms with tipped employees to file monthly informational returns detailing tip wages. These forms require the reporting of tip wages of at least 8% of sales (Form 8027). Not surprisingly, prior to the implementation of the Tip Credit, some firms reported tip wages near this threshold even if their tip wages were much higher. Thus, a substantial amount tip wages went unreported, which reduced income tax, Social Security, and Medicare payments (Robertson, Quinn, and Carr 2006).

To encourage more complete and voluntary tip reporting through employers, the Tip Credit was included as part of the Omnibus Budget Reconciliation Act of 1993. The

[8] Hourly wage data from the Bureau of Labor Statistics at https://www.bls.gov/oes/current/oes353031.htm.

inclusion of this credit was largely unanticipated by the market.[9] The credit equals the amount of the employer's Social Security and Medicare (FICA) taxes associated with tip wages and provides a substantial business tax credit for employers in the food and beverage industry (Haffer 2015).[10] The Tip Credit applies to restaurants with servers that receive tips. More specifically, the credit is available for firms that have employees who receive tips from customers for providing, delivering, or serving food or beverages and pay or incur employer FICA taxes on these tips after December 31, 1993 (IRC section 45B).[11] Importantly, restaurant firms that do not have tipped employees do not receive a similar tax benefit even though they are required to report and pay payroll taxes on the wages of their employees.[12]

The Tip Credit created a non-trivial reduction in expected cash taxes in both the year of implementation and in subsequent years. As an example, Table 1 shows a summary of the 1993 and 1994 tax rate reconciliation for Max & Erma's Restaurants (Max and Erma's). When the Tip Credit became effective in 1994, Max and Erma's estimated that the Tip Credit reduced its effective tax rate by 5.0%. In other words, Max and Erma's expected cash taxes due (assuming no book-tax differences) was reduced by

---

[9] This conjecture is based on practitioner discussions and a search of news reports during the time period. See Section 5 for a market reaction analysis.

[10] The credit does not apply to the amount of tips needed to bring the individual employee's wages up to the statutory minimum wage (Form 8846).

[11] "Tip Credit" is a common name used in the industry to describe this credit. Industry professionals also refer to the Tip Credit as the "45B Credit" or the "FICA Credit".

[12] In repeated letters to Congress, the National Restaurant Association (NRA) defended the credit and listed the retention of the credit as one of its highest priorities (Koenig 2015). However, the Obama Administration proposed the repeal of the Tip Credit as part of the fiscal-year 2016 Revenue Proposal. Opponents of the Tip Credit argue that the loss of tax revenue and the inequity of the availability of the credit outweigh the benefits associated with increase tip reporting (Hofmann 2015).

5.0% of pre-tax income because of the Tip Credit. This tax reduction from the Tip Credit for Max and Erma's continued at similar levels in subsequent years and is similar to other firms with tipped employees.[13]

Table 1
Tax Rate Reconciliation

Max & Erma's Restaurants, Inc.	1994		1993	
Pre-tax book income (*in thousands*)		2,815		2,085
Federal income tax at statutory rates	34.0%	957	34.0%	709
Increase (decrease) to income tax expense:				
State income taxes, net of federal benefit	4.4%	124	5.6%	117
Targeted Jobs Credits	-5.6%	(158)	-4.9%	(103)
FICA tip tax credit (net)	***-5.0%***	***(142)***	***0.0%***	***-***
Other	1.2%	35	-0.5%	(10)
Income taxes	29.0%	816	34.2%	713

This table shows a summary of the 1993 and 1994 tax rate reconciliation for Max & Erma's Restaurants from its 1994 10-K filing.

[13] The actual cash savings is difficult to estimate because not all firms disclose the size of the Tip Credit. Anecdotally, firms estimate the Tip Credit at roughly 1% of sales. The Tip Credit is also eligible to be carried back or carried forward to other tax years the extent it cannot be utilized in the current year.

3. PRIOR LITERATURE AND HYPOTHESIS DEVELOPMENT

3.1. Prior Literature

Research regarding the influence of tax policy on corporate investment is extensive, ranging from the effects of broad tax policy changes to targeted incentives (Hassett and Newmark 2008). Examining broad tax policy changes, some researchers have predicted that the Tax Reform Act of 1986 would lead to an increase in investment activity in the United States and there has been some evidence consistent with this prediction (Auerbach, Hassett, and Slemrod 1993; Swenson 1994).[14] However, it is difficult to identify the influence of reductions in tax rates because many other important economic and policy factors changed simultaneously (Hassett and Hubbard 2002).

Governments often use short-term tax policy changes (i.e., accelerated depreciation) to influence investment. However, these changes may only succeed in shifting investment into advantageous periods or to advantageous types of assets rather than generating an overall increase in investment (Desai and Goolsbee 2004; House and Shapiro 2008).[15] Additionally, prior research finds that the influence of accelerated depreciation on investment is concentrated among firms that are small, profitable, and have more immediate cash benefits from the incentive (Edgerton 2010; Zwick and Mahon 2017). Dobridge (2016) finds that firms' response to net operating loss carryback

[14] In an international setting Djankov, Ganser, McLiesh, Ramalho, and Shleifer (2010) find that foreign tax rates are negatively associated with investment in certain industries.

[15] Recent research examines the relation between tax policy changes and competition and suggests that tax differences can influence competitive behavior and firm performance (Kim, Nessa, and Wilson 2018; Donohoe, Jang, and Lisowsky 2018).

rules varies with each recession period, suggesting that other factors influence the impact of these tax policies. Luchs et al. (2018) examine the influence of a targeted tax benefit in the steel industry and fail to find that firms used the increase in cash for investment or shareholder payout. In general, these studies find that even when there is a specific incentive to invest, the relation between these incentives and investment is weak or only exists in certain circumstances.

More broadly, some studies investigate the behavior of firms following a cash windfall that is not related to tax policy. Blanchard et al. (1994) examine eleven firms that received cash windfalls in the form of legal settlements and fail to find evidence that these firms use their cash windfalls to increase investment, but they do find limited evidence of increases in shareholder payout. Bates (2005) investigates the payout behavior of firms after major divestitures and finds that corporate payout following these cash inflows is conditional on firm investment opportunities. In a tax-related setting, Blouin and Krull (2009) investigate a pseudo-cash windfall in the form of cash that was repatriated into the United States as part of the one-time repatriation tax holiday under the American Jobs Creation Act of 2004.[16] They find firms that repatriated foreign cash as part of the repatriation holiday increased shareholder payout in the form of share repurchases.[17]

---

[16] Blouin and Krull (2009) also find that firms that chose to repatriate earnings under the repatriation holiday exhibited lower investment opportunities and higher free cash flows relative to firms that did not repatriate foreign cash. Thus, in totality, Blouin and Krull's (2009) findings are consistent with corporations' response to a cash windfall being conditional on firms' investment opportunities (Bates 2005).

[17] Edwards, Schwab, and Shevlin (2015) propose that firms can use tax avoidance to increase internal funds. Using a cross-country setting, Green and Kerr (2016) find that firms that avoid the most tax tend to

Another stream of literature investigates the relation between cash windfalls and investment in the context of pension contribution requirements. Rauh (2006) finds that capital expenditures decrease after pension contributions increase. However, Bakke and Whited (2012) find that these results are largely driven by severely underfunded firms and with a refined research design they fail to find a relation between pension funding and investment on average. Dambra (2018) also fails to find that cash flow from pension funding requirements is related to investment or dividends, but acknowledges endogeneity concerns associated with these tests.[18]

3.2. Hypothesis Development

3.2.1. Tip Credit and Investment

Changes in tax policy can influence cash flow through reductions in the statutory tax rate or targeted tax incentives such as accelerated depreciation, tax credits, or special deductions.[19] Indeed, theoretical work links tax policy and investment by acknowledging that tax policy can influence investment by providing a cash windfall to certain firms and thus increasing internal capital (Fazzari et al. 1988). Myers and Majluf (1984) argue that internal capital is preferable to external capital due to the cost of borrowing

---

increase investment and decrease dividend payout. My study is distinct from these studies because I investigate an exogenous increase to cash flow from a tax policy change rather than tax avoidance behavior.

[18] In an investigation of acquisition behavior and cash windfalls, von Beschwitz (2018) finds that firms benefiting from a German tax reform are more likely to engage in acquisitions.

[19] I acknowledge that the reduction in the marginal tax rate of future investments is as an alternative or additional explanation for an increase in investment spending following an ongoing cash windfall (Hall and Jorgenson 1967).

additional funds. This theory suggests that an increase in new capital may be used for new investment because this increase in internal capital provides cheaper funding for potential new projects.

The Tip Credit provided a potentially significant increase in current and future available internal capital by reducing cash tax payments and thus increasing after-tax cash flow in both current and future periods. However, as discussed previously, the Tip Credit only benefits firms with tipped employees (Tip Credit firms) and not firms without tipped employees (non-Tip Credit firms). To the extent that the cash tax savings associated with the Tip Credit increases available internal capital, firms may be able to invest in additional projects. Accordingly, I expect that Tip Credit firms will increase investment relative to non-Tip Credit firms. I state my first hypothesis in the alternative form:

> ***Hypothesis 1: Tip Credit firms increase investment relative to other restaurant firms after the implementation of the credit.***

Despite the clarity of my first hypothesis, it is uncertain whether Tip Credit firms will increase investment. Unlike other targeted tax benefits (e.g., R&D Credit or accelerated depreciation), the Tip Credit does not provide a specific incentive to invest. Consequently, it is possible that the Tip Credit will not alter the established financial strategies of affected restaurant firms.

3.2.2. Tip Credit and Payout

A cash windfall can create significant agency conflicts. For example, managers may use the increase in cash for personal gain or to invest in suboptimal projects (Richardson 2006). Theory predicts that firms that receive a significant cash windfall

may increase shareholder payout in an effort to mitigate the agency costs associated with free cash flow (Jensen 1986).[20] The implementation of the Tip Credit provided an ongoing cash windfall for eligible firms. Also, the Tip Credit does not place any specific restrictions on the use of cash generated from the credit.[21] Given that the Tip Credit provides an ongoing, unrestricted source of cash, I expect that at least some of the cash tax savings from the Tip Credit will be paid out to shareholders. Accordingly, I predict that Tip Credit firms will respond to this increase in cash and increase shareholder payout. I state my second hypothesis in the alternative form:

Hypothesis 2: Tip Credit firms are more likely to increase shareholder payout relative to other restaurant firms after the implementation of the credit.

However, the effect of the implementation of the Tip Credit on firm payout policy is uncertain. Brav et al. (2005) find that managers view an increase in payout policy of second-order importance. This finding suggests that even if the Tip Credit supplies an increase in cash, it may not be the case that firms increase shareholder payout as a result.

---

[20] Given the potential for misdirected funds in response to an increase in free cash flow, a test on executive compensation could be appropriate. However, this information is largely unavailable before 1992 making it difficult to estimate a pre/post test during this time period. Archived 10-K filings are available in microfiche format for 12 firms in my sample. However, executive compensation data was not a required part of form 10-K during that time period, but were reserved for the Proxy Statements. These statements are not readily available before 1996. Unfortunately, given these data constraints, I am unable to adequately test the effect of the Tip Credit on executive compensation.

[21] In an untabulated test, I confirm the conjecture that Tip Credit firms experience a cash savings by testing changes in cash effective tax rates using the difference-in-differences method discussed in Section 4 with traditional controls for cash effective tax rates. I find that Tip Credit firms had a 0.06 greater reduction in cash effective tax rates compared to control firms, after the implementation of the Tip Credit ($p < 0.10$).

4. SAMPLE SELECTION AND RESEARCH DESIGN

4.1. Sample Selection

My sample is limited to the restaurant industry (Compustat – SIC 58 – Retail eating and drinking places) because the Tip Credit affects only restaurant firms. This research design choice allows me compare firms that benefit from the Tip Credit to a control group of firms in the same industry that do not benefit from the Tip Credit. My sample period is centered on the Tip Credit and extends from 1987 to 1999. I chose this sample period to maximize my sample size without overlapping with the change in tax law of 1986. I exclude 1993 because the Tip Credit was announced in August of 1993 and therefore that year may include some activity in response to the Tip Credit.[22] Finally, I require firms in my initial sample to have assets greater than one million and a gross profit percentage greater than zero. Based on these requirements, my initial sample consists of 266 firms and 1,639 firm-year observations.

I read the annual reports for these firms to determine whether each firm is eligible for the Tip Credit. I consider a firm a Tip Credit (treatment) firm if either it discloses the Tip Credit in the tax rate reconciliation, discusses its Tip Credit in the income tax footnote, or describes the business as a full-service restaurant company in its company description. During this hand collection process, I identify and remove 110 firms that are either non-taxable entities, non-food and beverage firms, or firms with

[22] The results are robust to the inclusion of 1993 in the pre-period.

insufficient data to make a determination of Tip Credit status. Non-Tip Credit restaurant firms are my control firms. I remove 292 firm-year observations that do not have the required dependent or control variables. I also impose a more balanced sample and require that firms have at least one observation in the pre- and post-Tip Credit implementation period, which reduces the firm-year observations by 211. This sample selection procedure leaves 73 individual firms and 687 firm-year observations. Of these firms, I identify 45 as Tip Credit firms and 28 as non-Tip Credit or control firms. Of these firm-year observations 302 are in the pre-period (1987 to 1992) and 385 are in the post-period (1994 to 1999). Table 2 describes my sample selection procedure in greater detail.

Table 2
Sample Selection Procedure

Difference-in-differences sample	Firms	Obs.
Firms in Compustat with 2-digit SIC=58, *fyears* from 1987-1992 (pre-period) or 1994-1999 (post period), assets > 1 million & gross profit percentage > 0.	266	1639
Non-taxable entity, non-food and beverage firm, or insufficient data	(110)	(449)
Missing dependent variables or controls	(20)	(292)
Missing an observation in the pre-period (for a more balanced panel)	(63)	(211)
Total	73	687
Treatment Group	45	405
Control Group	28	282
Total Firms & Observations	73	687

	Pre	Post	Total
Treatment Group	181	224	405
Control Group	121	161	282
Total	302	385	687

This table describes the sample selection procedure using data available in Compustat.

4.2. Research Design

4.2.1. Investment Tests

To examine whether a change in tax policy influences investment, I estimate the following OLS regression:

$$(1)\ Investment_{i,t} = \beta_0 + \beta_1 TIP_i + \beta_2 POST_t + \beta_3 TIP_i * POST_t + \beta_k Controls_{k,t-1} + \varepsilon_{i,t,}$$

where *Investment* is a proxy for a firm's investment expenditures in year *t*. Because a

firm's investment can take different forms, I calculate *Investment* in three ways: *Total Investment*, capital expenditures (*Capex*), and non-capital investment expenditures (*Non-Capex*) (research and development and acquisition expenses). I deflate *Total Investment* and *Non-Capex* by lagged assets and I deflate *Capex* by total property, plant, and equipment (Biddle, Hilary, and Verdi 2009).

Equation (1) utilizes a difference-in-differences design to examine whether the Tip Credit influences firms' level of investment relative to a control group of restaurant firms that do not benefit from the Tip Credit. *TIP* is an indicator variable equal to one for firms that are eligible for the Tip Credit and zero for restaurant firms that are not eligible for the Tip Credit. *POST* is an indicator variable equal to one for firm-year observations from 1994 to 1999, during which time the Tip Credit was effective (post-Tip Credit period), and zero for firm-year observations from 1987 to 1992 before the Tip Credit became effective (pre-Tip Credit period). To test H1, I examine the coefficient on the interaction between *TIP* and *POST*. H1 predicts that Tip Credit firms will increase investment following the implementation of the Tip Credit relative to non-Tip Credit firms. Accordingly, I expect a positive coefficient on the interaction between *TIP* and *POST.*

In addition to my variables of interest, I include several control variables used in prior literature to examine firm financial decisions (Dambra 2018). I include *Operating Cash Flow* to control for cash from operations that may influence firm financial decisions. I include *Cash* and *Leverage* to control for a firm's liquidity. I include firm size (*Size*) as the natural log of assets to control for access to capital and economies of

scale. I include the market-to-book ratio (*MTB*) to control for firm growth opportunities. I measure these control variables in the year *t*-1 to mitigate the concern that these characteristics are determined jointly with investment.[23] I also include the natural log of *Age* (average) to control for firm life-cycle and maturity, which prior literature shows is associated with firm financial decisions (DeAngelo, DeAngelo, and Stulz 2006).

4.2.2. Payout Policy Tests

To examine whether a change in tax policy influences shareholder payout, I estimate the following logistic regression:

(2) $\Pr(Payout\ Inc._{i,t}) = F(\beta_1 TIP_i + \beta_2 POST_t + \beta_3 TIP_i * POST_t + \beta_k Controls_{k,t-1})$,

where *Payout Increase* is equal to one in year *t* if the firm increased total shareholder payout from year *t*-1 to year *t,* zero otherwise. I examine payout increases because increases in shareholder payout communicate economically meaningful information about expected future cash flows, which is important for firm valuation (Guay and Harford 2000; Brown, Liang, and Weisbenner 2007). Because a firm's payout can take different forms, I also separately examine the likelihood of a *Dividend Increase* and *Repurchase Increase*.[24]

Equation (2) utilizes a difference-in-differences design to examine whether the Tip Credit influences the likelihood that a firm increases shareholder payout relative to the control group, where *TIP* and *POST* are defined previously. To test H2, I examine

---

[23] If a control variable is missing in year *t-1* but is not missing in year *t*, I use the value from year *t* in the regression in order to preserve sample. However, if I exclude observations with missing control variables in year *t-1* my inferences remain the same.

[24] Firms may also choose to increase cash holdings. In an untabulated test, I fail to find a difference in the change in cash holdings for Tip Credit firms versus non-Tip Credit firms.

the coefficient on the interaction of *TIP* and *POST*. H2 predicts that Tip Credit firms will be more likely to increase shareholder payout relative to non-Tip Credit firms. Accordingly, I expect a positive coefficient on the interaction between *TIP* and *POST*. As defined above, I include *Operating Cash Flow*, *Cash*, *MTB*, *Size*, *Leverage*, and *Age* as control variables.

4.2.3. Descriptive Statistics

Table 3, Panel A presents descriptive statistics for the full sample of restaurant firms in my hypothesis tests. Panel B compares variables for Tip Credit firms and control firms in the pre-period and Panel C presents variable correlations. Panel A shows that firms in my sample have mean (median) investment expenditures of 0.195 (0.136) of assets. These investment figures are higher than recent large sample studies (Biddle et al. 2009), and reflects the high growth nature of the restaurant industry. However, *Non-Capex* is zero for most firms because R&D is not a common component of the restaurant industry. Similar to prior research (Brown et al. 2007), I find that, on average, 27.8 percent of firm-years have a payout increase. Firms in my sample have relatively high average market-to-book ratios of 3.241, high average operating cash flow of 0.134, and are younger on average compared to firms in recent large sample studies (Biddle et al. 2009).[25]

Panel B shows that in the pre-Tip Credit period, Tip Credit firms are different on average from non-Tip Credit firms. Tip Credit firms are younger and smaller than the

[25] All continuous variables are winsorized at 1 percent and 99 percent.

control firms on average. Tip Credit firms also have lower market-to-book ratios, higher leverage, and lower operating cash flow when compared with control firms. These differences highlights the importance of controlling for firm characteristics in the multivariate analyses. In supplemental analyses, I employ entropy balancing and propensity score matching procedures to help mitigate concerns that differences in observable characteristics bias the results. I also perform analyses to test the parallel trends assumption. Section 5 discusses these analyses in detail.

Panel C shows that the control variables are correlated with the dependent variables. *Capex* is not correlated with *Non-Capex* and a *Dividend Increase* is not correlated with *Repurchase Increase*, suggesting that these dependent variables capture unique aspects of investment spending and payout increase, respectively.

Table 3
Descriptive Statistics

Panel A: Descriptive Statistics - Full Sample

Variables	N	Mean	Std. Dev.	25th Pctl.	Median	75th Pctl.
Total Investment	687	0.195	0.200	0.066	0.136	0.268
Capital Expenditures	687	0.163	0.126	0.073	0.132	0.211
Non-Capex Investment	687	0.018	0.066	0.000	0.000	0.000
Payout Increase	687	0.278	0.448	0.000	0.000	1.000
Dividend Increase	687	0.135	0.342	0.000	0.000	0.000
Repurchase Increase	687	0.215	0.411	0.000	0.000	0.000
Operating Cash Flow	687	0.134	0.113	0.068	0.135	0.204
Cash	687	0.094	0.106	0.020	0.054	0.130
Size	687	4.096	1.660	2.766	4.086	5.268
Market-to-book	687	3.241	5.606	1.242	2.092	3.348
Leverage	687	0.281	0.225	0.101	0.242	0.411
Natural Log of Age	687	2.450	0.554	1.946	2.375	2.938
Financial Constraint	687	-2.678	0.804	-3.247	-2.758	-2.131

Panel B: Covariate Comparison Tip vs Non-Tip	Control (Non-Tip) N	Control (Non-Tip) Mean	Treatment (Tip) N	Treatment (Tip) Mean	Mean Diff.	
Total Investment	121	0.233	181	0.186	0.047	*
Capital Expenditures	121	0.185	181	0.157	0.028	*
Non-Capex Investment	121	0.021	181	0.009	0.012	*
Payout Increase	121	0.339	181	0.188	0.151	***
Dividend Increase	121	0.256	181	0.088	0.168	***
Repurchase Increase	121	0.231	181	0.127	0.104	**
Operating Cash Flow	121	0.159	181	0.125	0.034	**
Cash	121	0.101	181	0.100	0.001	
Size	121	4.250	181	3.406	0.844	***
Market-to-book	121	4.035	181	2.407	1.628	***
Leverage	121	0.220	181	0.314	-0.094	***
Natural Log of Age	121	2.639	181	2.431	0.208	***
Financial Constraint	121	-2.711	181	-2.270	-0.441	***

Table 3 (continued)

Panel C: Correlation Table	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
(1) *Total Investment*		**0.78**	**0.48**	0.04	0.01	0.02	**0.37**	**0.23**	**-0.17**	**0.14**	**-0.18**	**-0.22**	**0.13**
(2) *Capital Expenditures*	**0.83**		**0.08**	0.03	-0.05	0.02	**0.27**	**0.35**	**-0.21**	**0.15**	**-0.22**	**-0.31**	**0.21**
(3) *Non-Capex Investment*	**0.25**	0.07		0.01	0.02	0.00	0.07	0.02	0.01	0.05	**0.12**	-0.01	-0.04
(4) *Payout Increase*	**0.11**	0.05	0.03		**0.44**	**0.81**	**0.13**	-0.03	**0.15**	-0.03	0.00	**0.10**	**-0.18**
(5) *Dividend Increase*	0.03	-0.04	0.02	**0.44**		0.05	0.05	-0.04	**0.20**	-0.03	-0.01	**0.27**	**-0.27**
(6) *Repurchase Increase*	**0.09**	0.05	0.03	**0.81**	0.05		**0.13**	-0.05	**0.13**	-0.04	-0.01	0.07	**-0.16**
(7) *Operating Cash Flow*	**0.55**	**0.43**	**0.10**	**0.18**	**0.08**	**0.17**		0.00	**0.19**	-0.02	**-0.29**	0.03	**-0.26**
(8) *Cash*	**0.22**	**0.26**	-0.01	-0.06	-0.04	**-0.09**	0.06		**-0.33**	**0.10**	**-0.21**	**-0.16**	**0.31**
(9) *Size*	-0.04	**-0.08**	**0.21**	**0.16**	**0.21**	**0.14**	**0.21**	**-0.36**		-0.05	0.07	**0.33**	**-0.89**
(10) *Market-to-book*	**0.34**	**0.37**	**0.09**	-0.01	0.04	-0.04	**0.33**	**0.12**	0.05		**0.11**	0.01	0.01
(11) *Leverage*	**-0.25**	**-0.24**	**0.11**	0.00	-0.02	0.01	**-0.33**	**-0.26**	0.05	-0.07		0.03	-0.01
(12) *Natural Log of Age*	**-0.18**	**-0.30**	0.00	**0.10**	**0.25**	**0.08**	0.03	**-0.13**	**0.29**	**-0.11**	0.05		**-0.60**
(13) *Financial Constraint*	0.03	**0.12**	**-0.18**	**-0.19**	**-0.27**	**-0.16**	**-0.23**	**0.32**	**-0.91**	-0.05	-0.01	**-0.55**	

Panel A shows the descriptive statistics for the pre/post Tip Credit sample from 1987 to 1999 (excluding 1993). Panel B shows the covariate comparison of means for the descriptive statistics between the Treatment and Control groups in the pre-period (1987 to 1992). I use t-tests for the tests of difference in means. ***, **, and * denote significance at a one, five, and ten percent level (two-tailed). Panel C shows the correlations, with Pearson coefficients reported above the diagonal and Spearman coefficients reported below the diagonal. Significant correlations at the 0.05 level are in bold. Appendix A provides a description of the variables in this table.

5. EMPIRICAL RESULTS

5.1. Tests of Hypothesis 1: Investment

Table 4 reports results of the difference-in-differences test of the implementation of the Tip Credit on Investment. The dependent variable is measured as *Total Investment* in Column 1, *Capex* in Column 2, and *Non-Capex* in Column 3. In Column 1, the coefficient on *TIP* is insignificant which indicates that levels of investment spending during the pre-period are indistinguishable between the treatment and control groups ($p > 0.10$). The coefficient on *POST* is insignificant, which indicates that the change in *Total Investment* from the pre- to post-period for control firms is indistinguishable from zero ($p > 0.10$). Consistent with Hypothesis 1, I find a positive and significant coefficient on *TIP*POST* ($p < 0.10$), suggesting that Tip Credit firms increased total investment spending to a greater extent than control firms following the implementation of the credit. In terms of economic significance, my results suggest that the incremental increase in Tip Credit firms' investment equals 5.9 percent of assets.[26]

The results in Column 2, when the dependent variable is *Capex*, are similar to the results in Column 1. The coefficient estimate for *TIP*POST* is positive and significant ($p < 0.05$), and suggests that, relative to control firms, Tip Credit firms increase their

[26] The economic significance of these tests is quite large. Cash generated by the Tip Credit is unlikely to be greater than 1.5% of sales based on the calculation of the credit. It is possible that outliers in this sample are contributing to the larger than expected coefficients. Alternative tests in Section 5 show significantly more reasonable coefficients when incorporating procedures to limit the effect of outliers on these tests. However, the results of these tests do not change the inferences of the main tests. It is also possible that the increase in cash from the Tip Credit provides enough increased liquidity in order to increase investment beyond the specific cash received.

capital expenditures by 4.5 percent of assets. In Column 3, I estimate the influence of the Tip Credit on non-capital expenditure investments (R&D and acquisitions). The coefficient for the *TIP*POST* interaction for *Non-Capex* expenditures is not statistically different from zero ($p > 0.10$). This suggests that Tip Credit firms did not change *Non-Capex* spending following the implementation of the Tip Credit, on average. This result is not surprising since the average *Non-Capex* investment spending of firms in my sample is 0.018 of total assets, which suggests that non-capex spending (e.g., research and development) is not substantial among restaurant firms.[27]

Overall, these results suggest that Tip Credit firms increase their investment spending following the implementation of the Tip Credit. These results are consistent with my first hypothesis and suggest that, on average, firms respond to a cash windfall and increase investment. This finding is important because it suggests that increased internal cash flow from a tax policy change can increase investment even when the change is not conditional on investment spending.

[27] I cluster standard errors by firm for each of my hypothesis tests. However too few treated clusters may increase the likelihood of over-rejection or under-rejection, particularly within a difference-in-differences design (MacKinnon and Webb 2016). My inferences remain the same when I do not cluster standard errors by firm and use robust standard errors to correct for heteroscedasticity.

Table 4
Tip Credit and Investment

Variables	*Prediction*	(1) Total Investment	(2) Capex	(3) Non-Capex
β_1 - *TIP*		-0.039	-0.025	-0.015*
		(-1.45)	(-1.63)	(-1.69)
β_2 - *POST*		-0.041	-0.035*	0.003
		(-1.38)	(-1.68)	(0.26)
β_3 - TIP * POST	+	***0.059****	***0.045*****	***0.007***
		(1.66)	***(1.84)***	***(0.61)***
β_4 - *Operating Cash Flow*		0.672***	0.293***	0.076**
		(5.13)	(4.05)	(2.23)
β_5 - *Cash*		0.249*	0.292***	0.024
		(1.96)	(3.22)	(0.69)
β_6 - *Size*		-0.017**	-0.006	-0.001
		(-2.23)	(-1.24)	(-0.57)
β_7 - *Market-to-book*		0.004	0.003	0.000
		(1.33)	(1.45)	(0.41)
β_8 - *Leverage*		-0.034	-0.049	0.053***
		(-0.55)	(-1.58)	(2.88)
β_9 - *Age*		-0.060***	-0.059***	-0.001
		(-3.21)	(-4.88)	(-0.24)
β_0 - *Constant*		0.320***	0.288***	0.003
		(5.07)	(7.91)	(0.17)
Observations		687	687	687
R-squared		0.262	0.307	0.041

This table reports the difference-in-differences OLS regressions of Hypothesis 1, in which a measure of investment is the dependent variable and *TIP * POST* is the difference-in-differences estimator. In Column (1), the dependent variable is *Total Investment,* in Column (2) the dependent variable is *Capex*, and in Column (3) the dependent variable is *Non-Capex*. I cluster standard errors by firm and *p*-values are one-tailed on the variable of interest with a directional prediction, two tailed otherwise. ***, **, and * denote significance at a one, five, and ten percent level. Appendix A provides a description of the variables in this table.

5.2. Tests of Hypothesis 2: Payout Policy

Table 5 reports the results of my test of Hypothesis 2. I estimate the likelihood of an increase in total payout in Column 1. The coefficient on *TIP* is negative and significant which indicates that treatment firms were less likely to increase payout in the pre-period relative to control firms ($p < 0.10$). The insignificant coefficient estimate on *POST* indicates there was no distinguishable change in the likelihood of a payout increase for control firms from the pre- to post-period ($p > 0.10$). Consistent with Hypothesis 2, the positive and significant coefficient for the interaction, *TIP*POST*, indicates that the change in likelihood of a payout increase for Tip Credit firms from the pre- to post-period is more positive relative to control firms ($p < 0.05$).[28] This result suggests that Tip Credit firms paid out at least some of the tax savings to shareholders, consistent with an effort to mitigate the agency costs associated with free cash flow (Jensen 1986).

To better understand the influence of the Tip Credit on payout, I separately estimate the likelihood of a dividend or repurchase increase. Because the Tip Credit provides a recurring cash windfall, theory suggests that firms will increase dividends (Jensen 1986). However, firms may be inclined towards repurchases to avoid a pre-commitment to future payouts associated with dividends. Column 2 of Table 5 presents the results of my analysis that examines the likelihood that a firm increases dividends. I

[28] In untabulated tests, I use OLS regressions to test whether payout levels are associated with the implementation of the Tip Credit. In the test of total payout, the coefficient on *TIP*POST* is positive but not statistically significant. However, the test of repurchases is positive and weakly significant. These inconsistent results may be due to a lack of power since many firm-year observations have zero payout.

find that the coefficient for the *TIP*POST* interaction is positive, but not statistically different from zero ($p > 0.10$). This indicates that the change in likelihood of a dividend increase for Tip Credit firms from the pre- to post-period is indistinguishable from control firms. This result is somewhat surprising given the non-transitory nature of the Tip Credit. Column 3 presents the results of tests that examine the likelihood that a firm increases repurchases. I find that the coefficient on *TIP*POST* is positive and significant ($p < 0.05$), which indicates that, relative to control firms, Tip Credit firms were more likely to increase repurchases after the implementation of the credit.[29]

Together, these findings suggest that Tip Credit firms increased payout following the implementation of the credit, which is consistent with Hypothesis 2. However, firms appear to choose share repurchases as their form of payout. Repurchases traditionally are not associated with a future payout commitment, which allows firms to provide payout and maintain some flexibility regarding future payout policy (Guay and Harford 2000; Jagannathan et al. 2000). These results extend Blouin and Krull (2009) by showing that firms experiencing cash windfalls from tax policy increase repurchases, but not dividends even when the tax policy change is ongoing. Although surprising, these results are consistent with research that suggests that firms may be shifting their payout policy from dividends to repurchases even after considering the transitory nature of earnings (Grullon and Michaely 2002).

[29] Researchers do not agree on the interpretation of interaction coefficients in nonlinear models (Ai and Norton 2003; Greene 2010; Kolasinski and Siegel 2010). Therefore, I interpret the sign of the coefficients. Additionally, my inferences remain the same if I reestimate the model using an OLS regression.

Table 5
Tip Credit and Payout

Variables	*Prediction*	(1) Payout Increase	(2) Dividend Increase	(3) Repurchase Increase
β_1 - *TIP*		-0.635*	-0.960*	-0.585
		(-1.76)	(-1.91)	(-1.60)
β_2 - *POST*		-0.048	-0.567	0.243
		(-0.17)	(-1.27)	(0.98)
β_3 - TIP * POST	+	***0.616****	***0.640***	***0.570****
		(1.74)	***(1.17)***	***(1.80)***
β_4 - *Operating Cash Flow*		2.912**	1.074	3.441***
		(2.43)	(0.70)	(2.59)
β_5 - *Cash*		0.334	0.695	-0.531
		(0.30)	(0.38)	(-0.31)
β_6 - *Size*		0.104	0.195	0.071
		(1.33)	(1.50)	(0.86)
β_7 - *Market-to-book*		-0.026	-0.023	-0.038
		(-0.93)	(-1.22)	(-0.83)
β_8 - *Leverage*		0.558	0.346	0.528
		(1.00)	(0.50)	(0.85)
β_9 - *Age*		0.307	1.108**	0.256
		(1.47)	(2.55)	(1.18)
β_0 - *Constant*		-2.496***	-5.229***	-2.732***
		(-3.84)	(-4.86)	(-4.18)
Observations		687	687	687
Pseudo R-squared		0.043	0.127	0.047

This table reports the difference-in-differences logistic regressions of Hypothesis 2, in which an indicator of an increase in payout is the dependent variable and *TIP * POST* is the difference-in-differences estimator. In Column (1), the dependent variable is equal to 1 if payout increased from year *t*-1 to year *t*, in Column (2) the dependent variable equal to 1 if dividends increased from year *t*-1 to year *t*, and in Column (3) the dependent variable equal to 1 if repurchases increased from year *t*-1 to year *t*, zero otherwise. I cluster standard errors by firm and *p*-values are one-tailed on the variable of interest with a directional prediction, two tailed otherwise. ***, **, and * denote significance at a one, five, and ten percent level. Appendix A provides a description of the variables in this table.

5.3. Additional Analyses

5.3.1. Financial Constraints

My primary results suggest that Tip Credit firms increase investment and are more likely to increase shareholder payout relative to firms that do not benefit from the Tip Credit. However, it is likely that the influence of changes in tax policy on corporate decisions varies across firms. Accordingly, I examine whether a firm's financial constraints influence the relation between the implementation of the Tip Credit and firms' investment and payout policy. Fazzari et al. (1988) note that taxes can create a cash windfall and predict that financially constrained firms may increase investment following a reduction in tax rates because these firms are likely to have previously unfunded projects. Consistent with this theory, I predict that the positive relation between the Tip Credit and investment will be greater for financially constrained firms. In contrast, firms that are not financially constrained may be more likely to increase payout since their available investment opportunities are likely exhausted. Agency theory posits that firms without positive net present value projects should payout increased cash flow to shareholders (Jensen 1986). Therefore, I predict that the positive relation between the Tip Credit and the likelihood of a payout increase will be greater for low financial constraint firms.

To test this theory in my setting, I investigate whether firm financial constraints influence a firm's investment and payout response to the implementation of the Tip Credit. I reestimate the tests of Hypothesis 1 and 2 for low financial constraint firms and high financial constraint firms separately. I measure financial constraints using the size-

age (SA) index created by Hadlock and Pierce (2010).[30] The SA index is based on linear functions of size and age and a quadratic function of size, which Hadlock and Pierce (2010) show to be reliable factors when estimating financial constraints. I consider high financial constraint firms as firms with above the median SA index and low financial constraint firms at or below the median SA index.

Table 6 presents the results of the test of Hypothesis 1 for firms with low and high financial constraint. The dependent variable is measured as *Total Investment* in Columns 1 and 2. The coefficient on *TIP*POST* for the low financial constraint firms (Column 1) is insignificant ($p > 0.10$), which suggests that the Tip Credit did not influence investment for firms that were not financially constrained. The coefficient on *TIP*POST* for the high financial constraint firms (Column 2) is positive and significant ($p < 0.01$), which suggests that Tip Credit firms increased investment for firms that were financially constrained. The coefficients on *TIP*POST* in Columns 1 and 2 are statistically different when tested using seemingly unrelated estimation ($p < 0.01$). The dependent variable is measured as *Capex* in Columns 3 and 4. The coefficient on *TIP*POST* for the low financial constraint firms (Column 3) is insignificant ($p > 0.10$), which suggests that the Tip Credit did not influence capital expenditures for firms that

[30] Hadlock and Pierce (2010) outline the details of their measure and its advantages over prior measures. In my study, it is important to use a measure of financial constraint that does not rely on measures or investment or payout. Accordingly, I use the SA index instead of the KZ index (Kaplan and Zingales 1997) or the Whited and Wu (2006) measures of financial constraint. When using these alternative proxies for financial constraint my inferences remain the same, however the results are less consistent when testing shareholder payout. I still include Size and Age in the models for these tests to account for the relation between size and age separately on investment and payout. However, the results are robust to the exclusion of these controls.

were not financially constrained. The coefficient on *TIP*POST* for the high financial constraint firms (Column 4) is positive and significant ($p < 0.05$), which suggests that Tip Credit firms increased capital expenditures for firms that were financially constrained. The coefficients on *TIP*POST* in Columns 3 and 4 are statistically different when tested using seemingly unrelated estimation ($p < 0.10$). The dependent variable is measured as *Non-Capex* in Columns 5 and 6. The coefficient on *TIP*POST* for the low financial constraint firms (Column 5) is insignificant ($p > 0.10$), which suggests that the Tip Credit did not influence non-capital investment expenditures for firms that were not financially constrained. The coefficient on *TIP*POST* for the high financial constraint firms (Column 6) is positive and significant ($p < 0.01$), which suggests that Tip Credit firms increased non-capital investment expenditures for firms that were financially constrained. The coefficients on *TIP*POST* in Columns 5 and 6 are statistically different when tested using seemingly unrelated estimation ($p < 0.05$).

These results suggest that although Tip Credit firms increased investment, on average, following the implementation of the credit, the increase in investment was concentrated among financially constrained firms. Overall, these findings are consistent with the theory that additional cash flow allows financially constrained firms to investment in previously unfunded projects (Fazzari et al. 1988).

Table 6
Tip Credit, Investment, and Financial Constraint

		(1)	(2)	(3)	(4)	(5)	(6)
		Total Investment		Capex		Non-Capex	
Variables	*Pred.*	Low Constraint	High Constraint	Low Constraint	High Constraint	Low Constraint	High Constraint
β_1 - *TIP*		0.015	-0.089**	0.014	-0.059**	-0.004	-0.024*
		(0.51)	(-2.12)	(0.82)	(-2.57)	(-0.26)	(-1.88)
β_2 - *POST*		0.032	-0.144**	0.009	-0.084*	0.015	-0.020*
		(1.40)	(-2.59)	(0.80)	(-1.70)	(1.01)	(-1.77)
β_3 - TIP * POST	+	***0.010***	***0.180****	***0.017***	***0.099****	***-0.009***	***0.032****
		(0.26)	***(2.79)***	***(0.70)***	***(1.80)***	***(-0.48)***	***(2.46)***
β_4 - *Operating Cash Flow*		0.982***	0.627***	0.523***	0.250***	0.086	0.068**
		(6.94)	(5.22)	(8.25)	(3.36)	(0.93)	(2.45)
β_5 - *Cash*		0.039	0.230	0.086	0.360***	0.015	0.027
		(0.27)	(1.60)	(0.93)	(3.28)	(0.22)	(0.85)
β_6 - *Size*		-0.016**	-0.034*	-0.007	-0.002	-0.003	-0.002
		(-2.20)	(-1.96)	(-1.61)	(-0.17)	(-1.10)	(-0.56)
β_7 - *Market-to-book*		-0.003**	0.008**	-0.001*	0.005**	-0.000	0.001
		(-2.55)	(2.57)	(-1.91)	(2.07)	(-1.05)	(1.11)
β_8 - *Leverage*		0.115*	-0.094	0.038	-0.050	0.061***	0.043*
		(1.92)	(-1.15)	(1.04)	(-1.16)	(2.77)	(1.76)
β_9 - *Age*		-0.000	-0.165***	-0.010	-0.133***	-0.004	0.003
		(-0.01)	(-3.87)	(-0.85)	(-6.98)	(-0.65)	(0.25)

Table 6 (continued)

		(1)	(2)	(3)	(4)	(5)	(6)
		Total Investment		Capex		Non-Capex	
Variables	*Pred.*	Low Constraint	High Constraint	Low Constraint	High Constraint	Low Constraint	High Constraint
β_0 - *Constant*		0.045	0.646***	0.087*	0.459***	0.012	0.004
		(0.73)	(4.44)	(1.84)	(6.10)	(0.40)	(0.11)
β_3 - *for High Constraint >* β_3 - *for Low Constraint*	+	**0.170*** (5.54)**		**0.082* (2.02)**		**0.041** (3.14)**	
Observations		344	343	344	343	344	343
R-squared		0.263	0.343	0.309	0.377	0.045	0.061

This table reports the separate difference-in-differences OLS regressions of Hypothesis 1 for firms with low financial constraints versus high financial constraints. Investment is the dependent variable and *TIP * POST* is the difference-in-differences estimator. In Column (1) & (2), the dependent variable is *Total Investment,* in Column (3) & (4) the dependent variable is *Capex*, and in Column (5) & (6) the dependent variable is *Non-Capex*. Low Constraint firms have at or below the median Size-Age Index measure of financial constraint and High Constraint firms are above the median (Hadlock and Pierce 2010). I cluster standard errors by firm and *p*-values are one-tailed on the variable of interest with a directional prediction, two tailed otherwise. ***, **, and * denote significance at a one, five, and ten percent level. The regressions are then estimated simultaneously using seemingly unrelated estimation, and β3 is tested for statistical difference. For each test of differences between coefficients, the difference in estimates is represented with the Chi square statistic below and ***, **, and * denote significance at a one, five, and ten percent level, one-tailed tests. Appendix A provides a description of the variables in this table.

Table 7 presents the results of the test of Hypothesis 2 for firms with low and high financial constraint. The dependent variable is measured as *Payout Increase* in Columns 1 and 2. The coefficient on *TIP*POST* for the low financial constraint firms (Column 1) is positive and significant ($p < 0.05$), which suggests that Tip Credit firms were more likely to increase shareholder payout among firms that were not financially constrained. The coefficient on *TIP*POST* for the high financial constraint firms (Column 2) is insignificant ($p > 0.10$), which suggests that the Tip Credit did not influence payout for these firms. The coefficients on *TIP*POST* in Columns 1 and 2 are statistically different when tested using seemingly unrelated estimation ($p < 0.10$). The dependent variable is measured as *Dividend Increase* in Columns 3 and 4. The coefficients on *TIP*POST* for the low financial constraint firms (Column 3) and high financial constraint firms (Column 4) are insignificant ($p > 0.10$), which suggests that the Tip Credit were not more likely to increase dividends among either low or high financial constraint firms. The dependent variable is measured as *Repurchase Increase* in Columns 5 and 6. The coefficient on *TIP*POST* for the low financial constraint firms (Column 5) is positive and significant ($p < 0.01$), which suggests that Tip Credit firms were more likely to increase repurchases among firms that were not financially constrained. The coefficient on *TIP*POST* for the high financial constraint firms (Column 6) is insignificant ($p > 0.10$), which suggests that the Tip Credit did not influence repurchases among financially constrained firms. The coefficients on

*TIP*POST* in Columns 5 and 6 are statistically different when tested using seemingly unrelated estimation ($p < 0.05$).

These results suggest that although Tip Credit firms were more likely to increase payout (total payout and repurchases), on average, following the implementation of the Tip Credit, the likelihood of a payout increase was concentrated among firms with lower financial constraints. This finding is consistent with the notion that low financial constraint firms may have already funded their available investment projects and therefore will payout at least a portion of a cash windfall to mitigate the associated agency conflicts (Jensen 1986). Overall, these findings regarding investment and payout suggest that tax policy changes may have an important impact on business decisions, but these decisions vary with firm characteristics.

Table 7
Tip Credit, Payout, and Financial Constraint

		(1)	(2)	(3)	(4)	(5)	(6)
		Payout Increase		Dividend Increase		Repurchase Increase	
		Low	High	Low	High	Low	High
Variables	*Pred.*	Constraint	Constraint	Constraint	Constraint	Constraint	Constraint
β_1 - *TIP*		-1.088*	-0.180	-1.443**	0.360	-1.240**	-0.272
		(-1.92)	(-0.37)	(-2.37)	(0.40)	(-2.13)	(-0.55)
β_2 - *POST*		-0.330	0.279	-0.800	-0.720	0.090	0.519
		(-1.00)	(0.58)	(-1.51)	(-0.57)	(0.29)	(1.25)
β_3 - TIP * POST	+	***1.169****	***0.098***	***0.655***	***1.059***	***1.342*****	***-0.065***
		(1.99)	***(0.17)***	***(0.97)***	***(0.77)***	***(2.38)***	***(-0.12)***
β_4 - *Operating Cash Flow*		4.770***	2.345*	1.837	0.031	11.189***	2.737**
		(2.96)	(1.79)	(0.87)	(0.01)	(4.34)	(2.10)
β_5 - *Cash*		-1.682	1.950	-1.042	2.669	-3.689	1.939
		(-1.01)	(1.13)	(-0.34)	(1.17)	(-1.58)	(0.97)
β_6 - *Size*		-0.044	0.196	-0.024	0.747***	0.130	-0.031
		(-0.40)	(1.00)	(-0.15)	(2.82)	(1.15)	(-0.16)
β_7 - *Market-to-book*		-0.090	0.007	-0.034	0.013	-0.560***	0.005
		(-1.49)	(0.21)	(-1.47)	(0.45)	(-3.33)	(0.15)
β_8 - *Leverage*		1.282**	0.120	0.927	-1.538	1.437**	0.616
		(2.08)	(0.11)	(1.08)	(-0.89)	(2.03)	(0.52)
β_9 - *Age*		0.293	0.355	1.148**	0.271	0.233	0.557
		(1.26)	(0.80)	(2.47)	(0.34)	(0.79)	(1.39)

Table 7 (continued)

Variables	*Pred.*	(1) Payout Increase Low Constraint	(2) Payout Increase High Constraint	(3) Dividend Increase Low Constraint	(4) Dividend Increase High Constraint	(5) Repurchase Increase Low Constraint	(6) Repurchase Increase High Constraint
β_0 - *Constant*		-1.561	-3.357**	-3.924***	-6.028***	-2.880***	-3.633***
		(-1.60)	(-2.45)	(-3.00)	(-2.63)	(-2.95)	(-2.66)
β_3 - *for Low Constraint >*	+	**1.071***		**-0.404**		**1.407****	
β_3 - *for High Constraint*		**(1.67)**		**(0.07)**		**(2.90)**	
Observations		344	343	344	343	344	343
Pseudo R-squared		0.043	0.033	0.128	0.087	0.112	0.037

This table reports the separate difference-in-differences logistic regressions of Hypothesis 2 for firms with low financial constraints versus high financial constraints. An indicator variable of payout increase is the dependent variable and *TIP * POST* is the difference-in-differences estimator. In Column (1) & (2), the dependent variable is one if the firm had a Payout Increase (zero otherwise), in Column (3) & (4) the dependent variable is one if the firm had a dividend increase (zero otherwise), and in Column (5) & (6) the dependent variable is one if the firm had a repurchase increase (zero otherwise). Low Constraint firms have at or below the median Size-Age Index measure of financial constraint and High Constraint firms are above the median (Hadlock and Pierce 2010). I cluster standard errors by firm and p-values are one-tailed on the variable of interest with a directional prediction, two tailed otherwise. ***, **, and * denote significance at a one, five, and ten percent level. The regressions are then estimated simultaneously using seemingly unrelated estimation, and $\beta 3$ is tested for statistical difference. For each test of differences between coefficients, the difference in estimates is represented with the Chi square statistic below and ***, **, and * denote significance at a 1, 5, and 10 percent level, one-tailed tests. Appendix A provides a description of the variables in this table.

5.4. Parallel Trends Assumption

5.4.1. Pre-Period Trend

An important assumption of my difference-in-differences research design is that the trends in the outcome variables are the same for the treatment and control group prior to the treatment (Roberts and Whited 2013). If the trends in the outcome variables are not parallel, it would suggest that my results are potentially due to differences in firm characteristics as opposed to the implementation of the Tip Credit. To test the parallel trends assumption, I examine the growth rates of each dependent variable in the pre-treatment period and find the growth rates of *Investment* ($p = 0.33$) and *Total Payout* ($p = 0.91$) are not statistically different between Tip Credit and non-Tip Credit firms. These results provide some comfort that my tests do not violate the parallel trends assumption for a valid difference-in-differences test (Roberts and Whited 2013).[31]

5.4.2. Falsification Tests

In an untabulated analysis, I perform a falsification test by repeating my difference-in-differences test in the pre-Tip Credit period. Using a sample of 258 firm-year observations from the pre-period years of 1986 to 1992, I estimate my difference-in-differences test and exclude observations in 1989 as the false treatment year. I consider years 1990 to 1992 as the post-period for purposes of this falsification test and years 1986 to 1988 as the pre-period. I estimate Equations 1 and 2 using this new sample period. The coefficient on *TIP*POST* is not statistically significant ($p > 0.10$) across all

[31] The parallel trends assumption is also met when separately testing growth rates in capital expenditure, non-capital expenditures, dividends, and repurchases.

of the tests of my hypotheses. This suggests that the observed change in firm behavior in my main tests is likely due to the treatment effect (Roberts and Whited 2013).

5.5. Robustness Tests

5.5.1. Entropy Balancing

Although restaurant firms with tipped and non-tipped employees represent an advantageous treatment and control group, I did not randomly assign each firm and the descriptive statistics in Table 3 suggest that Tip Credit and non-Tip Credit firms differ on several dimensions. To address the concern that an unbalanced treatment and control group may yield biased results (Roberts and Whited 2013), I employ entropy balancing. Entropy balancing achieves covariate balance between the treatment and control groups by weighting control sample units (Hainmueller 2012). One advantage of this method over matching procedures, particularly in a small sample study, is that all sample units are preserved and provide information in the model estimation (Hainmueller 2012). As a result, I am able to maintain a consistent sample and also achieve covariate balance for the independent variables on the mean, variance, and skewness. Table 8, Panel A shows the differences in mean, variance, and skewness between the treatment and control groups for the control variables, prior to entropy balancing. Panel B demonstrates the covariate balance between treatment and control groups after entropy balancing.

Utilizing this weighting procedure, I estimate each of my hypothesis tests and present the results in the remaining panels of Table 8.[32] Panel C presents the results of

[32] I have withheld the control variables from this presentation for brevity.

my tests of Hypothesis 1 using entropy balancing. Consistent with my initial tests, the coefficient on *TIP* POST* is positive and significant ($p < 0.05$), indicating that the change in *Investment* and *Capex* for Tip Credit firms from the pre- to post-period is more positive relative to control firms. As with my initial tests, I fail to find that the Tip Credit is related to *Non-Capex*, on average. In Panel D, I present the results of my entropy balanced test of Hypothesis 2. Using a logistic regression, the coefficient on *TIP*POST* when the dependent variable is *Payout Increase* and *Repurchase Increase* is positive and significant ($p < 0.10$). As with my initial tests, I fail to find that the Tip Credit is related to the likelihood of dividend increases. Overall, these results help mitigate the concern that differences in observable characteristics between Tip Credit and non-Tip Credit firms bias my results.[33]

[33] Another method of addressing endogeneity is propensity score matching, which prior accounting research has used extensively. The objective of propensity score matching is to attain covariate balance and thereby relax the functional form assumptions between variables (Shipman, Swanquist, and Whited 2016). One weakness of this method is that results may be sensitive to myriad design choices and is dependent on correct model specification (Hainmueller 2012; DeFond, Erkens, and Zhang 2016). However, in untabulated tests I estimate my hypothesis tests using this method. I first estimate a model predicting *TIP* using the control variables. My one-to-one propensity score matched sample matches on year, and I use the nearest neighbor within 0.05 caliper without replacement. With the exception of a few characteristics, the matched sample achieves covariate balance. My inferences remain the same using this matching procedure.

Table 8
Entropy Balanced Tests of H1 and H2

Panel A: Covariate Balance before Entropy Balancing	Treatment			Control		
Variables	mean	variance	skewness	mean	variance	skewness
Operating Cash Flow	0.134	0.012	-0.253	0.134	0.014	-0.289
Cash	0.090	0.010	1.994	0.101	0.013	1.925
Size	3.821	2.251	0.344	4.490	3.229	0.443
Market-to-book	3.143	35.130	7.196	3.381	26.190	7.219
Leverage	0.303	0.055	0.833	0.250	0.043	0.799
Natural Log of Age	2.375	0.265	0.627	2.558	0.347	0.178
Panel B: Covariate Balance after Entropy Balancing	Treatment			Control		
Variables	mean	variance	skewness	mean	variance	skewness
Operating Cash Flow	0.134	0.012	-0.253	0.134	0.012	-0.253
Cash	0.090	0.010	1.994	0.090	0.010	1.994
Size	3.821	2.251	0.344	3.821	2.251	0.345
Market-to-book	3.143	35.130	7.196	3.143	35.130	7.196
Leverage	0.303	0.055	0.833	0.303	0.055	0.833
Natural Log of Age	2.375	0.265	0.627	2.375	0.265	0.627

Table 8 (continued)

Panel C: Investment	*Prediction*	(1) Total Investment	(2) Capex	(3) Non-Capex
β_1 - *TIP*		-0.072*	-0.040***	-0.027*
		(-1.87)	(-2.65)	(-1.90)
β_2 - *POST*		-0.042	-0.025	0.000
		(-1.23)	(-1.15)	(0.02)
β_3 - TIP * POST	+	***0.075****	***0.041****	***0.014***
		(1.87)	***(1.69)***	***(0.85)***
Controls		Yes	Yes	Yes
Observations		687	687	687
R-squared		0.242	0.320	0.115

Panel D: Payout	*Prediction*	Payout Increase	Dividend Increase	Repurchase Increase
β_1 - *TIP*		-0.561	-0.543	-0.641
		(-1.53)	(-0.94)	(-1.50)
β_2 - *POST*		0.031	-0.212	0.210
		(0.15)	(-0.42)	(0.79)
β_3 - TIP * POST	+	***0.508****	***0.260***	***0.558****
		(1.61)	***(0.43)***	***(1.58)***
Controls		Yes	Yes	Yes
Observations		687	687	687
Pseudo R-squared		0.043	0.127	0.043

This table reports the difference-in-differences regressions of each hypothesis using entropy balancing weighting procedures, which achieves covariate balance between the treatment and control groups on mean, variance, and skewness through a weighting process (Hainmueller 2012). Panel A shows the covariate balances prior to entropy balancing and Panel B shows the covariate balances after entropy balancing for each control variable on mean, variance, and skewness. As with each hypothesis test, *TIP * POST* is the difference-in-differences estimator. Panel C shows the reestimation of my first hypothesis test using entropy balancing. Panel D shows the reestimation of my second hypothesis test using entropy balancing. I cluster standard errors by firm and p-values are one-tailed on the variable of interest with a directional prediction, two tailed otherwise. ***, **, and * denote significance at a one, five, and ten percent level. Appendix A provides a description of the variables in this table. Control variables have been removed for brevity.

5.5.2. Generalized Difference-in-Differences Model with Fixed Effects

My tests of Hypothesis 1 and 2 rely on a single event, the implementation of the Tip Credit, which allows me to interact the TIP with POST as my difference-in-difference estimator. However, an alternative difference-in-differences model specification may estimate these tests differently. Tables 9 and 10 present the results of the tests of Hypothesis 1 and 2, respectively, using a generalized difference-in difference model with firm and year fixed effects. In this model, *TREAT* is equal to one for Tip Credit firm in the treatment period (1994 – 1999), and zero otherwise. The inclusion of firm and year fixed effects holds constant the effects across firm and time that are unobservable.

The results of these tests are similar to the main tests of Hypothesis 1 and 2. In Table 9, the dependent variable is measured as *Total Investment* in Column 1, *Capex* in Column 2, and *Non-Capex* in Column 3. Consistent with Hypothesis 1, I find a positive and significant coefficient on *TREAT* ($p < 0.10$), suggesting that Tip Credit firms increased total investment spending to a greater extent than control firms during the treatment period. The results in Column 2, when the dependent variable is *Capex*, are similar to the results in Column 1. The coefficient estimate for *TREAT* is positive and significant ($p < 0.05$), and suggests that, relative to control firms, Tip Credit firms increase their capital expenditures during the treatment period. In Column 3, I estimate the influence of the Tip Credit on non-capital expenditure investments (R&D and acquisitions). The coefficient for the *TREAT* for *Non-Capex* expenditures is not statistically different from zero ($p > 0.10$).

Table 9
Tip Credit and Investment - Fixed Effects Model

Variables	*Prediction*	(1) Total Investment	(2) Capex	(3) Non-Capex
β_1 - TREAT	+	***0.054****	***0.047*****	***0.006***
		(1.40)	***(1.87)***	***(0.43)***
β_2 - Operating Cash Flow		0.632***	0.250***	0.086**
		(5.08)	(2.78)	(2.28)
β_3 - Cash		0.004	0.002	0.001
		(1.05)	(1.07)	(1.01)
β_4 - Size		0.117	0.256***	0.033
		(1.07)	(3.45)	(0.81)
β_5 - Market-to-book		0.091	0.024	0.040
		(1.13)	(0.51)	(1.43)
β_6 - Leverage		-0.126***	-0.056***	-0.012
		(-4.78)	(-3.82)	(-1.62)
β_7 - Age		-0.416*	-0.309**	0.007
		(-1.78)	(-2.24)	(0.09)
β_0 - Constant		1.174**	0.845***	-0.005
		(2.38)	(3.00)	(-0.03)
Firm Fixed Effects		Yes	Yes	Yes
Year Fixed Effects		Yes	Yes	Yes
Observations		687	687	687
R-squared		0.507	0.556	0.234

This table reports the generalized difference-in-differences OLS regressions of Hypothesis 1, in which a measure of investment is the dependent variable and *TREAT* is the difference-in-differences estimator. In Column (1), the dependent variable is *Total Investment,* in Column (2) the dependent variable is *Capex*, and in Column (3) the dependent variable is *Non-Capex*. *TREAT* equals for 1 for treatment firm-years after 1993 and 0 otherwise. I include firm and year fixed effects. I cluster standard errors by firm and *p*-values are one-tailed on the variable of interest with a directional prediction, two tailed otherwise. ***, **, and * denote significance at a one, five, and ten percent level. Appendix A provides a description of the variables in this table.

Table 10 reports the results of my test of Hypothesis 2 using the generalized difference-in-differences model with firm and year fixed effects. I estimate the likelihood of an increase in total payout in Column 1. Consistent with Hypothesis 2, the positive and significant coefficient for *TREAT* indicates that the likelihood of an increase in payout is greater for treatment firms during the treatment period ($p < 0.05$).

Column 2 of Table 10 presents the results of my analysis that examines the likelihood that a firm increases dividends. I find that the coefficient for the *TREAT* is positive, but not statistically different from zero ($p > 0.10$). Column 3 presents the results of tests that examine the likelihood that a firm increases repurchases. I find that the coefficient on *TREAT* is positive and significant ($p < 0.05$), which indicates that Tip Credit firms were more likely to increase repurchases during the treatment period.[34] Overall, these results are consistent with the finding of my main hypothesis tests and provide some comfort that the original results are not sensitive to alternative difference-in-differences models.

[34] The samples for these tests are significantly reduced because firm-fixed effects requires some variation in the dependent variable by firm over time. Many firms, particularly regarding a dividend increase, never have an increase in payout over the time period.

Table 10
Tip Credit and Payout - Fixed Effects Model

Variables	*Prediction*	(1) Payout Increase	(2) Dividend Increase	(3) Repurchase Increase
β_1 - TREAT	+	***0.587****	***0.319***	***0.840****
		(1.36)	***(0.39)***	***(2.08)***
β_2 - Operating Cash Flow		1.122	-4.150	2.444
		(0.62)	(-1.10)	(1.16)
β_3 - Cash		-0.005	-0.049	-0.008
		(-0.11)	(-1.44)	(-0.14)
β_4 - Size		2.874*	0.791	4.125**
		(1.86)	(0.30)	(1.98)
β_5 - Market-to-book		0.202	-1.070	2.555*
		(0.16)	(-0.62)	(1.92)
β_6 - Leverage		0.729**	0.429	0.614**
		(2.14)	(0.84)	(2.00)
β_7 - Age		11.052	13.008	-33.259***
		(1.46)	(1.22)	(-3.81)
β_0 - Constant		-23.284*	-23.665	54.350***
		(-1.72)	(-1.33)	(3.67)
Firm Fixed Effects		Yes	Yes	Yes
Year Fixed Effects		Yes	Yes	Yes
Observations		552	314	519
R-squared		0.154	0.217	0.213

This table reports the generalized difference-in-differences logistic regressions of Hypothesis 2, in which an indicator of an increase in payout is the dependent variable and *TREAT* is the difference-in-differences estimator. In Column (1), the dependent variable is equal to 1 if payout increased from year t-1 to year t, in Column (2) the dependent variable equal to 1 if dividends increased from year t-1 to year t, and in Column (3) the dependent variable equal to 1 if repurchases increased from year t-1 to year t, zero otherwise. *TREAT* equals for 1 for treatment firm-years after 1993 and 0 otherwise. I include firm and year fixed effects. I cluster standard errors by firm and p-values are one-tailed on the variable of interest with a directional prediction, two tailed otherwise. ***, **, and * denote significance at a one, five, and ten percent level. Appendix A provides a description of the variables in this table.

5.5.3. Alternative Estimation Techniques

This study utilizes a relatively small sample of firm-year observations to test the hypotheses. I am therefore, naturally concerned with the influence of outliers in my multivariate tests. To alleviate this concern, I restimate my generalized difference-in-difference tests of Hypothesis 1 using a robust regression (Leone, Minutti-Meza, and Wasley 2013). Table 11 presents the results of these tests. In Columns 1 and 2, consistent with my previous tests of *Total Investment* and *Capex*, I find positive and significant coefficients on *TREAT* ($p < 0.05$; $p < 0.10$). However, the coefficient estimates appear smaller than the original tests, suggesting that outliers may be influencing the coefficient estimate. For example, in Column 2 the coefficient estimate using the robust regression when testing *Capex* is 0.015 compared with 0.047 from the OLS model. However, my inference that the Tip Credit increased investment remains the same.

My test of Hypothesis 1 using *Non-Capex* expenditures as the dependent variable may suffer from estimation bias because of a large number of zeros. Thus, in Column 3, I reestimate this test using a tobit regression model. The coefficient on *TREAT* is positive but not statistically different from zero ($p > 0.10$), which is consistent with the OLS test.

Table 11
Tip Credit and Investment - Alternative Tests

Variables	*Prediction*	(1) Total Investment	(2) Capex	(3) Non-Capex
β_1 - TREAT	+	***0.031****	***0.015****	***0.065***
		(2.02)	***(1.36)***	***(1.09)***
β_2 - Operating Cash Flow		0.499***	0.270***	0.319**
		(9.94)	(7.49)	(2.34)
β_3 - Cash		0.001	0.001*	0.003
		(0.86)	(1.70)	(1.32)
β_4 - Size		0.259***	0.291***	0.301
		(5.21)	(8.16)	(1.58)
β_5 - Market-to-book		0.038	0.022	0.249*
		(1.18)	(0.93)	(1.92)
β_6 - Leverage		-0.057***	-0.046***	-0.025
		(-7.03)	(-7.81)	(-0.87)
β_7 - Age		-0.189	-0.264*	0.049
		(-0.85)	(-1.66)	(0.12)
β_0 - Constant		0.582	0.742**	-1.036
		(1.39)	(2.47)	(-1.29)
Firm Fixed Effects		Yes	Yes	Yes
Year Fixed Effects		Yes	Yes	Yes
Robust Regression		Yes	Yes	No
Tobit Model		No	No	Yes
Observations		687	687	687
R-squared		0.666	0.728	0.804

This table reports the generalized difference-in-differences robust (Columns 1 & 2) and tobit (Column 3) regressions of Hypothesis 1, in which a measure of investment is the dependent variable and *TREAT* is the difference-in-differences estimator. In Column (1), the dependent variable is *Total Investment*, in Column (2) the dependent variable is *Capex*, and in Column (3) the dependent variable is *Non-Capex*. *TREAT* equals for 1 for treatment firm-years after 1993 and 0 otherwise. I include firm and year fixed effects. I cluster standard errors by firm and p-values are one-tailed on the variable of interest with a directional prediction, two tailed otherwise. ***, **, and * denote significance at a one, five, and ten percent level. Appendix A provides a description of the variables in this table.

5.5.4. Studentized Residuals

An alternative way to mitigate the effect of outliers is to exclude particularly influential observations. I do this by estimating the studentized residuals for my tests of Hypothesis 1. Table 12 presents the results of the tests of Hypothesis 1 when firm-year observations with studentized residuals greater than the absolute value of 2. This process excludes 36, 34, and 27 observations from Columns 1, 2, and 3 respectively. The results of these tests are similar the main tests of Hypothesis 1. I find a positive and significant coefficient on *TIP*POST* when testing Total *Investment* and *Capex* are positive and significant ($p < 0.10$; $p < 0.05$) and the test of *Non-Capex* remains insignificant ($p > 0.10$). However, the coefficient estimates appear smaller than the original tests, suggesting that outliers may be influencing the coefficient estimate. For example, the coefficient estimate after removing outliers when testing *Capex* is 0.028 compared with 0.047 from the original test. However, my inferences remain the same.

Table 12
Tip Credit and Investment - Studentized Residuals

Variables	*Prediction*	(1) Total Investment	(2) Capex	(3) Non-Capex
β_1 - *TIP*		-0.007	-0.011	-0.005*
		(-0.40)	(-0.92)	(-1.95)
β_2 - *POST*		-0.014	-0.027**	-0.002
		(-0.80)	(-2.16)	(-0.46)
β_3 - TIP * POST	+	***0.031****	***0.028*****	***0.005***
		(1.36)	***(1.67)***	***(1.23)***
β_4 - *Operating Cash Flow*		0.729***	0.431***	0.020**
		(10.46)	(8.52)	(2.38)
β_5 - *Cash*		0.002	0.002	0.000
		(1.17)	(1.28)	(1.06)
β_6 - *Size*		0.209**	0.289***	0.009
		(2.38)	(3.34)	(0.96)
β_7 - *Market-to-book*		-0.039	-0.036*	0.007*
		(-1.50)	(-1.75)	(1.70)
β_8 - *Leverage*		-0.008*	-0.002	0.001
		(-1.82)	(-0.52)	(1.01)
β_9 - *Age*		-0.038***	-0.050***	-0.003*
		(-3.45)	(-5.14)	(-1.89)
β_0 - *Constant*		0.184***	0.213***	0.006
		(5.11)	(7.13)	(1.54)
Observations		651	653	660
R-squared		0.414	0.442	0.034

This table reports the difference-in-differences OLS regressions of Hypothesis 1, in which a measure of investment is the dependent variable and *TIP * POST* is the difference-in-differences estimator. In these tests I have removed observations with studentized residuals with an absolute value greater than 2. In Column (1), the dependent variable is *Total Investment*, in Column (2) the dependent variable is *Capex*, and in Column (3) the dependent variable is *Non-Capex*. I cluster standard errors by firm and p-values are one-tailed for estimations with a directional prediction, two tailed otherwise. ***, **, and * denote significance at a one, five, and ten percent level. Appendix A provides a description of the variables in this table.

5.5.5. Unexpected Investment

In the main test of Hypothesis 1, I control for operating cash flow to mitigate the concern that the increase in investment for Tip Credit firms is correlated with broad operation changes that might affect free cash flow. For example, it is possible that economic conditions effected Tip Credit and non-Tip Credit firm differently. It is possible that Tip Credit firms (perhaps serving a superior good) enjoyed greater success in certain years because of overall economic improvement. In order to test whether this concern has merit, I estimate unexpected investment and use this estimate as an alternative test of Hypothesis 1. Unexpected investment is measured as a residual value of a regression of *Total Investment* on *Sales Growth* (Biddle et al. 2009). If economic conditions effected Tip Credit and non-Tip Credit firm differently, isolating the unexpected investment should help me test the effect of the Tip Credit on investment that is uncorrelated with sales growth.

Table 13 presents the results of the test of the effect of the Tip Credit of unexpected investment. The coefficient on *TIP*POST* is positive and significant ($p < 0.05$), suggesting that Tip Credit firms increased unexpected investment spending to a greater extent than control firms following the implementation of the credit. This test triangulates the earlier tests of Hypothesis 1.

Table 13
Tip Credit and Unexpected Investment

Variables	*Prediction*	(1) Unexpected Investment
β_1 - *TIP*	+	-0.023
		(-1.45)
β_2 - *POST*		-0.036*
		(-1.72)
β_3 - TIP * POST		***0.044****
		(1.76)
β_4 - *Operating Cash Flow*		0.290***
		(4.01)
β_5 - *Cash*		0.003
		(1.43)
β_6 - *Size*		0.305***
		(3.28)
β_7 - *Market-to-book*		-0.046
		(-1.49)
β_8 - *Leverage*		-0.005
		(-1.11)
β_9 - *Age*		-0.059***
		(-4.91)
β_0 - *Constant*		0.134***
		(3.64)
Observations		687
R-squared		0.305

This table reports the difference-in-differences OLS regressions of Hypothesis 1, in which a measure of investment is the dependent variable and *TIP * POST* is the difference-in-differences estimator. *Unexpected Investment* is equal to the residual value of a regression of Investment on *Sales Growth*. I include firm and year fixed effects. I cluster standard errors by firm and p-values are one-tailed on the variable of interest with a directional prediction, two tailed otherwise. ***, **, and * denote significance at a one, five, and ten percent level. Appendix A provides a description of the variables in this table.

5.6. Tax Policy and Debt

In addition to increasing investment or shareholder payout, firms may also choose to pay down debt after a cash windfall. However, it is also possible that firms increase debt following a cash windfall to use their greater debt capacity from an increase in cash (Blanchard et al. 1994). In order to examine these possibilities, I test the relation between the implementation of the Tip Credit and debt following the difference-in differences design of the previous tests. I measure the dependent variable, *Leverage*, as long-term debt deflated by assets in year *t*. I regress *Leverage*, on *TIP*, *POST*, *TIP*POST*, and a set of control variables identified in prior literature as influencing debt (Frank and Goyal 2009).

Table 14 reports results of the difference-in-differences test of the implementation of the Tip Credit on leverage. The negative and significant coefficient for the interaction of interest, *TIP*POST*, indicates that the change in *Leverage* of Tip Credit firms from the pre- to post-period is significantly more negative relative to control firms ($p < 0.05$). These findings are consistent with the conjecture that Tip Credit firms used some of the cash windfall from the credit to pay down debt and adjust their financial position. However, this finding is inconsistent with the descriptive findings in Blanchard et al. (1994), which finds an increase in debt following a legal settlement windfall.[35]

[35] I acknowledge that some of the decrease in debt following the implementation of the Tip Credit may be related to marginal tax rates, which has be shown to be related to debt usage (Graham 1996). The Tip Credit may act as a non-debt tax shield, making interest deductions from debt less valuable and therefore decrease the incentive to use debt financing (DeAngelo and Masulis 1980).

Table 14
Tip Credit and Debt

Variables	*Prediction*	(1) Leverage
β_1 - *TIP*		0.098*
		(1.95)
β_2 - *POST*		0.031
		(0.98)
β_3 - TIP * POST	***?***	***-0.066****
		(-1.70)
β_4 - *ROA*		-0.256**
		(-2.17)
β_5 - *Cash*		-0.497***
		(-3.34)
β_6 - *Size*		-0.007
		(-0.41)
β_7 - *Market-to-book*		0.005*
		(1.85)
β_8 - *NOL*		0.034
		(0.33)
β_9 - *Age*		0.026
		(0.72)
β_{10} - *Dividends*		-1.619*
		(-1.96)
β_{11} - *Capital Access*		0.201***
		(3.94)
β_0 - *Constant*		0.243**
		(2.52)
Observations		687
R-squared		0.22

This table reports the difference-in-differences OLS regressions in which *Leverage* is the dependent variable and *TIP * POST* is the difference-in-differences estimator. I calculate *Leverage* by deflating long-term debt by assets. I cluster standard errors by firm and *p*-values are two tailed. ***, **, and * denote significance at a one, five, and ten percent level. Appendix A provides a description of the variables in this table.

5.7. Stock Market Test

The Tip Credit was included as part of the Omnibus Budget Reconciliation Act of 1993. To my knowledge, the inclusion of the credit in the legislative negotiations was largely unknown to the general public. Thus, I conjecture that the passage of the Tip Credit was a relative surprise to the industry. In order to test this conjecture, I estimate the market reaction to the announcement of Tip Credit as part of the legislation passed on Tuesday, August 10, 1993. Table 13 presents the value-weighted, market adjusted returns for the three-day window (-1, 0, 1) surrounding the announcement of the credit. I find that Tip Credit firms received a significant positive return (0.00326) during the event window. This suggests a market reaction to positive news for Tip Credit firms. I also find a positive return (0.00179) return to non-Tip Credit restaurant firms. This suggests that the industry as a whole received an increase in market valuation as a result of the Tip Credit passage. Importantly, I also find that the market return for Tip Credit firms is significantly greater than for non-Tip Credit firms (0.00148), which suggests that the market understood, at least in part, the benefit of the Tip Credit to only certain firms ($p < 0.10$).

Table 15
Tip Credit Announcement - Market Test

	(1)	(2)	(3)
	Tip Credit Announcement Return	T-Stat	N (Number of firms)
Tip Credit Firm Return > 0	0.00326***	(5.56)	34
Non-Tip Credit Firm Return > 0	0.00179***	(4.86)	24
All Firm Return > 0	0.00265***	(6.87)	58
Tip Return > Non-Tip Return	0.00148*	(1.93)	58

This table reports the value weighted, market adjusted returns for the three-day window (-1, 0, 1) surrounding the announcement of the Tip Credit as part of the Omnibus Budget Reconciliation Act of 1993 on August 10, 1993. Column 1, reports the market adjusted returns. Column 2, reports the t-statistic when comparing whether the market adjusted return is different from zero or different between groups. Column 3, reports the total number firms in each test. Note that not all firms in the sample have available data to be included in this test. ***, **, and * denote significance at a one, five, and ten percent level.

6. CONCLUSIONS

This study examines how a cash windfall from a change in tax policy influences corporate financial decisions. Using the implementation of the Tip Credit in the restaurant industry to identify a change in tax policy, I examine the influence of tax policy on corporate investment and payout decisions. I find that Tip Credit firms increase investment spending relative to control firms following the implementation of the credit. This finding is consistent with the prediction that, on average, firms use increased internal capital from a tax policy change for additional investment spending. I also find that Tip Credit firms were more likely to increase payout to shareholders relative to control firms following the implementation of the Tip Credit. This results suggests that Tip Credit firms paid out at least some of the tax savings to shareholders, consistent with an effort to mitigate the agency costs associated with free cash flow (Jensen 1986). In separate analyses, I find that Tip Credit firms were more likely to increase repurchases, but I do not find a change in the likelihood of a dividend increase. These results are consistent with firms avoiding a pre-commitment to future payouts associated with dividends (Guay and Harford 2000; Jagannathan et al. 2000).

In additional analyses, I find that financially constrained firms increase investment to a greater extent when compared with firms with relatively low financial constraints. This finding is consistent with the theory that additional cash flow allows financially constrained firms to investment in previously unfunded projects (Fazzari et al. 1988). I also find that the relation between the Tip Credit and shareholder payout is concentrated among firms with low financial constraints. These additional findings

suggest that a firm's response to a cash windfall from a tax policy change is conditional on its financial position.

This study contributes to several streams of literature. First, this study contributes to the literature that investigates the relation between tax policy and corporate investment by addressing whether tax policy influences investment even in the absence of investment incentives. Second, this study contributes to the payout policy literature. I extend Blouin and Krull (2009) by providing evidence that firms also modify their payout policy in response to a permanent reduction in taxes where the tax savings are not required to be used for specific investment activities. Third, this study contributes to the cash windfall literature by investigating a cash windfall that is independent from major corporate decisions, such as divestitures, acquisitions, or repatriation. Finally, my results more broadly provide evidence on the consequences of tax policy. Although I examine a unique credit in a specific industry, the attributes of the setting provide some insight into corporate behavior following a reduction in taxes, which is applicable to a variety of tax policy changes.

REFERENCES

Ai, C., and E. C. Norton. 2003. Interaction terms in logit and probit models. *Economics Letters* 80 (1):123-129.

Auerbach, A. J., K. Hassett, and J. Slemrod. 1993. Taxation and foreign direct investment in the United States: a reconsideration of the evidence. In *Studies in International Taxation*: University of Chicago Press, 119-148.

Bakke, T. E., and T. M. Whited. 2012. Threshold events and identification: A study of cash shortfalls. *The Journal of Finance* 67 (3):1083-1111.

Bates, T. W. 2005. Asset sales, investment opportunities, and the use of proceeds. *The Journal of Finance* 60 (1):105-135.

Biddle, G. C., G. Hilary, and R. S. Verdi. 2009. How does financial reporting quality relate to investment efficiency? *Journal of Accounting and Economics* 48 (2-3):112-131.

Blanchard, O. J., F. Lopez-de-Silanes, and A. Shleifer. 1994. What do firms do with cash windfalls? *Journal of Financial Economics* 36 (3):337-360.

Blouin, J., and L. Krull. 2009. Bringing it home: A study of the incentives surrounding the repatriation of foreign earnings under the American Jobs Creation Act of 2004. *Journal of Accounting Research* 47 (4):1027-1059.

Brav, A., J. R. Graham, C. R. Harvey, and R. Michaely. 2005. Payout policy in the 21st century. *Journal of Financial Economics* 77 (3):483-527.

Brown, J. R., N. Liang, and S. Weisbenner. 2007. Executive financial incentives and payout policy: Firm responses to the 2003 dividend tax cut. *The Journal of Finance* 62 (4):1935-1965.

Committee for a Responsible Federal Budget. *The Tax Break-Down: th FICA Tip Credit* 2013 [cited. Available from http://www.crfb.org/blogs/tax-break-down-fica-tip-credit.]

Dambra, M. J. 2018. Stakeholder Conflicts and Cash Flow Shocks: Evidence from Changes in ERISA Pension Funding Rules. *The Accounting Review* 93 (1):131-159.

DeAngelo, H., L. DeAngelo, and D. J. Skinner. 2009. Corporate payout policy. *Foundations and Trends in Finance* 3 (2–3):95-287.

DeAngelo, H., L. DeAngelo, and R. M. Stulz. 2006. Dividend policy and the earned/contributed capital mix: a test of the life-cycle theory. *Journal of Financial Economics* 81 (2):227-254.

DeAngelo, H., and R. W. Masulis. 1980. Optimal capital structure under corporate and personal taxation. *Journal of Financial Economics* 8 (1):3-29.

DeFond, M., D. H. Erkens, and J. Zhang. 2016. Do client characteristics really drive the Big N audit quality effect? New evidence from propensity score matching. *Management Science* 63 (11):3628-3649.

Desai, M. A., and A. D. Goolsbee. 2004. Investment, overhang, and tax policy. *Brookings Papers on Economic Activity* 2004 (2):285-338.

Djankov, S., T. Ganser, C. McLiesh, R. Ramalho, and A. Shleifer. 2010. The effect of corporate taxes on investment and entrepreneurship. *American Economic Journal: Macroeconomics* 2 (3):31-64.

Dobridge, C. 2016. Fiscal stimulus and firms: A tale of two recessions. *Working Paper*.

Donohoe, M. P., H. Jang, and P. Lisowsky. 2018. Competitive Externalities of Tax Cuts. *Working Paper*.

Edgerton, J. 2010. Investment incentives and corporate tax asymmetries. *Journal of Public Economics* 94 (11-12):936-952.

Edwards, A., C. Schwab, and T. Shevlin. 2015. Financial constraints and cash tax savings. *The Accounting Review* 91 (3):859-881.

Faulkender, M., and M. Petersen. 2012. Investment and capital constraints: repatriations under the American Jobs Creation Act. *The Review of Financial Studies* 25 (11):3351-3388.

Fazzari, S., R. G. Hubbard, and B. C. Petersen. 1988. Financing constraints and corporate investment. *Brookings Papers on Economic Activity* 1:141-195.

Ferdman, R. A. 2014. Which US states tip the most (and least), as shown by millions of Square transactions. *Quartz*, 21 March 2014.

Frank, M. Z., and V. K. Goyal. 2009. Capital structure decisions: which factors are reliably important? *Financial Management* 38 (1):1-37.

Gale, W. G., A. Krupkin, and K. Rueben. 2015. The relationship between taxes and growth at the state level: new evidence. *National Tax Journal* 68 (4):919-942.

Graham, J. R. 1996. Debt and the marginal tax rate. *Journal of Financial Economics* 41 (1):41-73.

Graham. 2003. Taxes and corporate finance: A review. *The Review of Financial Studies* 16 (4):1075-1129.

Green, D., and J. N. Kerr. 2016. How Do Firms Use Cash Tax Savings: A Cross-Country Analysis. *Working Paper*.

Greene, W. 2010. Testing hypotheses about interaction terms in nonlinear models. *Economics Letters* 107 (2):291-296.

Grullon, G., and R. Michaely. 2002. Dividends, share repurchases, and the substitution hypothesis. *The Journal of Finance* 57 (4):1649-1684.

Guay, W., and J. Harford. 2000. The cash-flow permanence and information content of dividend increases versus repurchases. *Journal of Financial Economics* 57 (3):385-415.

Hadlock, C. J., and J. R. Pierce. 2010. New evidence on measuring financial constraints: Moving beyond the KZ index. *The Review of Financial Studies* 23 (5):1909-1940.

Haffer, L. 2015. Economic Considerations of Eliminating Restaurant Tips. *BDO*.

Hainmueller, J. 2012. Entropy balancing for causal effects: A multivariate reweighting method to produce balanced samples in observational studies. *Political Analysis* 20 (1):25-46.

Hall, R. E., and D. W. Jorgenson. 1967. Tax Policy and Investment Behavior. *The American Economic Review* 57 (0):391-414.

Hanlon, M., and S. Heitzman. 2010. A review of tax research. *Journal of Accounting and Economics* 50 (2):127-178.

Hassett, K. A., and R. G. Hubbard. 2002. Tax policy and business investment. In *Handbook of Public Economics*: Elsevier, 1293-1343.

Hassett, K. A., and K. Newmark. 2008. Taxation and business behavior: A review of the recent literature. *Fundamental Tax Reform: Issues, Choices and Implications*:191-214.

Hofmann, P. 2015. FICA Tip Credit under Fire on Capitol Hill and White House. *BDO*.

House, C. L., and M. D. Shapiro. 2008. Temporary investment tax incentives: Theory with evidence from bonus depreciation. *American Economic Review* 98 (3):737-768.

Jagannathan, M., C. P. Stephens, and M. S. Weisbach. 2000. Financial flexibility and the choice between dividends and stock repurchases. *Journal of Financial Economics* 57 (3):355-384.

Jensen, M. C. 1986. Agency cost of free cash flow, corporate finance, and takeovers. *Corporate Finance, and Takeovers. American Economic Review* 76 (2).

Kaplan, S. N., and L. Zingales. 1997. Do investment-cash flow sensitivities provide useful measures of financing constraints? *The Quarterly Journal of Economics* 112 (1):169-215.

Kim, J., M. Nessa, and R. Wilson. 2018. How do Reductions in Foreign Country Corporate Tax Rates Affect US Domestic Manufacturing Firms? *Working Paper*.

Koenig. 2015. National Restaurant Association - Letter to Committee on Finance. [cited. Available from https://www.restaurant.org/Downloads/PDFs/advocacy/-20150421_Depreciation_Working_Group_Letter.pdf]

Kolasinski, A. C., and A. F. Siegel. 2010. On the economic meaning of interaction term coefficients in non-linear binary response regression models. *Working Paper*.

Leone, A. J., M. Minutti-Meza, and C. E. Wasley. 2013. Influential observations and inference in accounting research. *The Accounting Review*.

Luchs, J., G. A. Plesko, and S. Utke. 2018. Market and Firm Reaction to Targeted Tax Benefits: Evidence From the Tax Reform Act of 1986. *Working Paper*.

MacKinnon, J. G., and M. D. Webb. 2016. Randomization Inference for Differences-in-differences with Few Treated Clusters: Carleton University, Department of Economics.

Marron, D. 2018. Three Things You Should Know About the Stock Buyback Furor. *Forbes*, 12 April 2018.

Myers, S. C., and N. S. Majluf. 1984. Corporate financing and investment decisions when firms have information that investors do not have. *Journal of Financial Economics* 13 (2):187-221.

NRA. 2017. State of the Industry Report: https://www.restaurant.org/News-Research/Research/Facts-at-a-Glance.

Rauh, J. D. 2006. Investment and financing constraints: Evidence from the funding of corporate pension plans. *The Journal of Finance* 61 (1):33-71.

Richardson, S. 2006. Over-investment of free cash flow. *Review of Accounting Studies* 11 (2-3):159-189.

Roberts, M. R., and T. M. Whited. 2013. Endogeneity in empirical corporate finance1. In *Handbook of the Economics of Finance*: Elsevier, 493-572.

Robertson, J., T. Quinn, and R. C. Carr. 2006. Unreported tip income: A taxing issue. *The CPA Journal* 76 (12):30.

Shipman, J. E., Q. T. Swanquist, and R. L. Whited. 2016. Propensity score matching in accounting research. *The Accounting Review* 92 (1):213-244.

Swenson, D. L. 1994. The impact of US tax reform on foreign direct investment in the United States. *Journal of Public Economics* 54 (2):243-266.

von Beschwitz, B. 2018. Cash windfalls and acquisitions. *Journal of Financial Economics* 128 (2):287-319.

Whited, T. M., and G. Wu. 2006. Financial constraints risk. *The Review of Financial Studies* 19 (2):531-559.

Wolfers, J. 2018. How to Think About Corporate Tax Cuts. *The New York Times*, 8 March 2018.

Yagan, D. 2015. Capital tax reform and the real economy: The effects of the 2003 dividend tax cut. *American Economic Review* 105 (12):3531-3563.

Zwick, E., and J. Mahon. 2017. Tax policy and heterogeneous investment behavior. *American Economic Review* 107 (1):217-248.

APPENDIX

VARIABLE DEFINITIONS

Variable	Definition
TIP	Equal to 1 for all *fyears* if firm is eligble for the Tip Credit determined by review of the Annual Report (Business Description, Income Tax Footnote, or Income Tax Rate Reconciliation), 0 otherwise
POST	Equal to 1 for *fyears* after 1993, 0 for years prior to 1994
Total Investment	Capital expeditures (capx) plus research and development expenditures (xrd) plus acquisitions (aqc) minus sale of property, plant, and equipment (sppe) deflated by lagged assets (at)
Capital Expenditures	Capital expeditures (capx) deflated by lagged assets (at)
Non-Capex Investment	Research and development expenditures (xrd) plus acquisitions (aqc) deflated by lagged assets (at)
Payout Increase	Equal to 1 if dividends (dvc + dvp) plus stock repurchases (prstkc) is greater than dividends plus repurchases in the prior year, 0 otherwise
Dividend Increase	Equal to 1 if dividends (dvc + dvp) is greater than dividends in the prior year, 0 otherwise
Repurchase Increase	Equal to 1 if stock repurchases (prstkc) is greater than repurchases in the prior year, 0 otherwise
Operating Cash Flow	Operating cash flow (oancf) deflated by lagged assets (at)
Cash	Cash (che) deflated by total assets (at)
Size	Natural log of sales (sale) plus 1
Market-to-book	Market value of equity (csho * prcc_f) deflated by book value of equity (ceq)
Leverage	Long-term debt (dltt) plus the current portion of long-term debt (dlc) deflated by total assets (at)
Age	Natural log of the average number of years since the firm's first year in Compustat
Financial Constraint	Size-age (SA) index as constructed by Hadlock and Pierce (2010), as $-0.737Size + 0.043Size^2 - 0.040Age$
TREAT	Equal to 1 for treatment firm *fyears* after 1993, 0 otherwise
Unexpected Investment	The residual value of a regression of *Total Investment* on *Sales Growth* (Biddle et al. 2009)

Variable	Definition
Sales Growth	Percent change in sales calculated as lagged sales minus sales deflated by lagged sales
Return on Assets	Operating income (oibdp) deflated by lagged assets (at)
Net Operating Loss	Tax loss carryforward (tlcf) deflated by lagged assets (at)
Dividends	Total dividends (dvc + dvp) deflated by lagged assets (at)
Capital Access	Equal to 1 if bond rating (splticrm, spsdrm, or spsticrm) is not missing in Compustat, 0 otherwise