The Influence of Firm Maturation on Firms' Tax-induced Financing and Investment Decisions

by

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The Influence of Firm Maturation on
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Abstract

Empirical evidence supporting the predicted positive relation between firms' financial leverage and their marginal tax rates has been elusive. Scholes and Wolfson (1989a) argue that re-financing costs that accumulate with age affect the time-series variation in firms' tax-induced financing and investment policies. They predict that as corporate capital structures gradually become more constrained over time, firms' financing decisions will become less sensitive to changes in their tax rates. Secondly, Scholes and Wolfson predict that as firms are increasingly impeded from adjusting their capital structures, they will resort to relying more on investment-related tax shields.

This thesis tests Scholes and Wolfson's (1989a) predictions using panel data spanning firms' first through ninth years since their initial public offerings. Strong, robust evidence on the evolution in firms' debt and asset tax shields is consistent with their predictions. The empirical testing in this thesis begins with the estimation of the time-series pattern in firms' rate of reversion to their optimal capital structures, which suggests that adjusting leverage becomes more difficult over time. The longitudinal evidence also indicates that the positive relation between leverage and marginal tax rates subsides as firms mature. Further, firms are observed to gradually shift more toward investment-related tax shields when re-financing costs increasingly constrain their capital structures.

This thesis contributes to the literature by developing a research design that represents a richer empirical characterization of the capital structure problem by isolating the influence of dynamic adjustment costs on tax-induced financing and investment decisions. In addition, this thesis provides the first evidence on the time-series properties of firms' responses to stable tax shield incentives. Extant capital structure research examines either cross-sectional variation or time-series variation relating to changes in certain macroeconomic conditions such as tax laws.

Finally, this thesis answers Slemrod and Shobe's (1990) and Shevlin's (1999) call for empirical research that more precisely models non-tax costs and the time-series behavior of firms' reactions

to tax incentives, respectively. This is largely achieved by exploiting the features of panel data to control for unobserved firm-specific effects to avoid omitted variable bias and to refine the estimation of within-firm dynamics.

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In memory of my father and grandmother.

Table of Contents

ABSTRACT iv

ACKNOWLEDGEMENTS vi

DEDICATION viii

TABLE OF CONTENTS ix

LIST OF TABLES AND FIGURES xi

CHAPTER 1 – INTRODUCTION 1
1.1 Motivation and Summary of the Thesis 1
1.2 Thesis Organization 4

CHAPTER 2 – REVIEW OF EXTANT LITERATURE, HYPOTHESES DEVELOPMENT, AND THESIS CONTRIBUTIONS 6
2.1 Introduction 6
2.2 Time-series Variation in Tax-induced Financing Decisions 7
2.3 Time-series Variation in Tax-induced Investment Decisions 16
2.4 Thesis Contributions 17
2.5 Conclusions 19

CHAPTER 3 – TIME-SERIES VARIATION IN RATE OF ADJUSTMENT TO OPTIMAL CAPITAL STRUCTURE 20
3.1 Introduction 20
3.2 Sample Selection 21
3.3 Empirical Tests – Time-series Variation in Rate of Adjustment to Optimal Capital Structure – Full Sample 23
3.3.1 Book-based vs. Market-based Leverage 25
3.3.2 Firm-specific Dynamic Target Leverage 26
3.3.3 Contemporaneous Industry-mean Leverage 30
3.4 Empirical Tests – Time-series Variation in Rate of Adjustment to Optimal Capital Structure – Sample Split on Private Age History 33
3.5 Conclusions 35

CHAPTER 4 – TIME-SERIES VARIATION IN TAX-INDUCED FINANCING AND INVESTMENT DECISIONS 47
4.1 Introduction 47
4.2 Sample Selection and Descriptive Statistics 48
4.3 Regression Variables and Descriptive Statistics 49
4.3.1 Leverage 49
4.3.2 Marginal Tax Rates 51
4.3.3 Investment-related Tax Shields 54
4.3.4 Other Determinants of Capital Structure 58
4.3.5 Descriptive Statistics on the Regression Variables 65

4.4 Empirical Tests of the Time-series Variation in Tax-induced Financing and Investment Decisions 65
4.4.1 Cross-sectional Estimation 66
4.4.2 Fixed Effects Estimation 72
4.5 Sensitivity Tests 74
4.5.1 Selection Correction 74
4.5.2 Other Robustness Tests 77
4.6 Conclusions 85

CHAPTER 5 – THE INFLUENCE OF PRIVATE OPERATING HISTORY ON THE TIME-SERIES VARIATION IN TAX-INDUCED FINANCING AND INVESTMENT DECISIONS 100
5.1 Introduction 100
5.2 Empirical Tests of the Influence of Firms' Histories on Financing Decisions 100
5.2.1 Empirical Model Development 100
5.2.2 Lagged Dependent Variable Estimation 103
5.3 Empirical Tests of the Influence of Firms' Private Ages on Tax-induced Financing and Investment Decisions 106
5.4 Conclusions 109

CHAPTER 6 – CONCLUSIONS 115
6.1 Summary of Thesis Evidence 115
6.2 Thesis Contributions 117
6.3 Limitations 118
6.4 Potential Extensions 120

REFERENCES 123

List of Tables and Figures

CHAPTER 3

Table 3.1: Sample Selection Summary – Tests of Firms' Rate of Adjustment to Their Optimal Capital Structures 37

Table 3.2: Industry Distribution of Sample – Tests of Firms' Rate of Adjustment to Their Optimal Capital Structures 38

Table 3.3: Summary Statistics – Annual Cross-sectional Means of the Various Proxies for Optimal Capital Structure 39

Figure 3.1 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Nine-year Time-series Mean Debt Ratio) 40

Figure 3.2 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Three-year Historical Mean Debt Ratio) 40

Figure 3.3 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Three-year Lead Mean Debt Ratio) 41

Figure 3.4 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Contemporaneous Industry-Mean Debt Ratio) 41

Table 3.4: OLS Regression Results – Balanced Panel Tests of Firms' Reversion toward Various Proxies for Optimal Capital Structure 42

Figure 3.5 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Nine-year Time-series Mean Debt Ratio) - Sub-sample of Young Firms at IPO 43

Figure 3.6 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Nine-year Time-series Mean Debt Ratio) - Sub-sample of Old Firms at IPO 43

Figure 3.7 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Three-year Historical Debt Ratio) - Sub-sample of Young Firms at IPO 44

Figure 3.8 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Three-year Historical Mean Debt Ratio) - Sub-sample of Old Firms at IPO 44

Figure 3.9 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Three-year Lead Mean Debt Ratio) - Sub-sample of Young Firms at IPO 45

Figure 3.10 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Three-year Lead Mean Debt Ratio) - Sub-sample of Old Firms at IPO 45

Figure 3.11 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Contemporaneous Industry-mean Debt Ratio) - Sub-sample of Young Firms at IPO 46

Figure 3.12 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Contemporaneous Industry-mean Debt Ratio) - Sub-sample of Old Firms at IPO 46

CHAPTER 4

Table 4.1: Sample Selection Summary – Tests of Time-series Variation in Tax-induced Financing and Investment Decisions 87

Table 4.2: Industry Distribution of Sample – Tests of Time-series Variation in Tax-induced Financing and Investment Decisions 88

Table 4.3: Motivation for Regression Control Variables 89

Table 4.4: Summary Statistics 91

Table 4.5: Year-to-Year OLS Regression Results 94

Figure 4.1 - Time-series of Marginal Tax Rate Coefficients 95

Figure 4.2 - Time-series of Investment-related Tax Shield Coefficients 95

Table 4.6: Pooled Cross-sectional, Time-series Results – OLS, Tobit, and GMM Estimation 96

Table 4.7: Fixed Effects, Random Effects and Selection-corrected Regression Results 97

Table 4.8: Fixed Effects, Random Effects and Tobit Regression Results – Alternate Trichotomous Marginal Tax Rate Proxy 98

Table 4.9: Fixed Effects, Random Effects and Tobit Regression Results – Tax Status Estimated with Graham et al's (1998) Simulated Marginal Tax Rates 99

CHAPTER 5

Table 5.1: OLS and Fixed Effects Regression Results for Lagged Leverage Tests – Full Sample 112

Table 5.2: OLS and Fixed Effects Regression Results for Lagged Leverage Tests – Balanced Panel Partitioned by Private Age at IPO Date 113

Table 5.3: OLS and Fixed Effects Regression Results – Balanced Panel Partitioned by Private Age at IPO Date 114

Chapter 1

Introduction

1.1 Motivation and Summary of the Thesis

The tax-planning perspective adopted in Scholes and Wolfson (1992) emphasizes that managers should consider all costs and benefits of a proposed transaction, not just its tax aspects. This implies that a strategy that reduces the present value of the firm's tax payments would be rejected when the non-tax costs that would be incurred exceed the potential tax savings. For example, Scholes and Wolfson (1989a) suggest that managers responsible for arranging their firms' capital structures will increasingly encounter tension between taxes and re-financing costs. According to their argument, information and transaction costs that accumulate with age affect the time-series variation in firms' tax-induced financing and investment decisions.

Specifically, Scholes and Wolfson (1989a) predict that corporate capital structures gradually become more constrained over time by re-financing costs, which impede firms from adjusting their leverage when their marginal tax rates change. They also predict that firms will increasingly shift toward relying on non-debt tax shields, such as depreciation deductions and investment tax credits, as issuing and retiring securities becomes more difficult.

These predictions about the evolution in firms' debt and asset tax shields provide the motivation for the empirical tests in this thesis. The first thesis hypothesis, H_1, is that the relation between financial leverage and marginal tax rates will become less positive as firms age. The second thesis hypothesis, H_2, is that the relation between financial leverage and investment-related tax shields will become more negative as firms age.

Scholes and Wolfson (1989a) partially attribute the scarcity of evidence that taxes influence firms' financing decisions to research designs that neglect to specify the capital structure problem to include dynamic adjustment costs. The tests in this thesis are intended to address this concern by examining the time-series pattern in firms' re-financing costs and their

financing and investment decisions. This research on the time-series properties of firms' reactions to tax incentives contributes to the empirical capital structure literature, which is dominated by studies that observe either cross-sectional variation or time-series variation relating to changes in certain macroeconomic conditions.

The thesis estimates the time-series variation in firms' rate of adjustment to their optimal capital structures over the nine years following their initial public offerings. These tests are designed to measure the fraction of the distance between actual and target leverage the firm moved each year. These regressions provide evidence consistent with extant empirical and analytical research that generally implies that re-financing costs are increasing in firm age as their capital structures become less flexible and more complex. Further, these tests attempt to respond to Shevlin (1999a), which implores empirical tax researchers to more precisely model such non-tax costs.

This evidence that firms' reversion to their optimal capital structures gradually becomes more constrained over time is essential to justifying the subsequent tests of the thesis hypotheses. Scholes and Wolfson's (1989a) predictions about the evolution in firms' reliance on debt and investment-related tax shields are based on re-financing costs increasingly impeding leverage adjustments. Accordingly, the evidence on this primitive issue empirically validates their argument that it gradually becomes more difficult for firms to restore their optimal capital structures. The results from these tests would reinforce the credibility of evidence consistent with H_1 and H_2 by reducing the likelihood that other explanations are responsible for this empirical support.

Although the time-series evidence on firms' rate of adjustment to their optimal capital structures suggests that re-financing costs accumulate with age, these tests should be interpreted cautiously since there are indications that the proxies used for target leverage are coarse estimates. However, the direct tests of H_1 and H_2 avoid the complications involved with attempting to develop adequate proxies for firm-specific optimal capital structure.

In fact, it could be argued that these tests are more suitable for examining whether *dynamic* re-financing costs affect capital structure decisions by estimating the time-series variation in firms' reactions to the *static* incentives provided by debt and investment-related tax shields.

The moderating influence of re-financing costs can be inferred from the pattern in firms' responses to these tax incentives. For example, observing that firms' gradually shift toward relying more on investment tax shields and less on debt tax shields would imply that the capital structure adjustment process becomes more constrained over time.[1]

The thesis reports evidence consistent with Scholes and Wolfson's (1989a) predictions about the evolution in firms' financing and investment decisions in the years following their initial public offerings. The panel data tests ranging firms' first through ninth public years indicate that the positive relation between firms' leverage and their marginal tax rates subsides with age. In addition, firms are observed to gradually shift toward investment-related tax shields when re-financing costs begin to seriously impede capital structure changes. This strong evidence supporting H_1 and H_2 is robust to an extensive series of sensitivity tests.

The primary research design applied in this thesis measures a firm's age as the number of years that have elapsed since its IPO, although firms usually have a private operating history when they go public. This suggests that cross-sectional differences in firms' private ages, defined as the number of years between their incorporation and their IPOs, could affect the tests on the time-series pattern in firms' reliance on debt and investment tax shields.

This issue is evaluated by re-examining evidence provided earlier in the thesis after bisecting the sample according to firms' private ages. Scholes and Wolfson's (1989a) predict that re-financing costs that accumulate with age will induce the sample of older firms, which already have considerable histories at their IPO dates, to have capital structures that are relatively less responsive to their marginal tax rates. Further, they argue that the older firms will resort more to substituting investment-related tax shields for their debt tax shields as re-financing costs gradually constrain their capital structures. In fact, results from the split-sample tests suggest that the older firms are at a later stage in the predicted evolution in their tax shields. These tests provide evidence that the older firms have progressed farther than the younger firms in their transition toward relying more on investment tax shields and less on debt tax shields.

[1] However, explanations other than re-financing costs that are increasing in firm age could be responsible for any evidence consistent with Scholes and Wolfson's (1989a) predictions about the time-series variation in debt and investment-related tax shields. Several competing explanations have been identified and are

1.2 Thesis Organization

This thesis is organized into four main chapters. The next chapter positions this research relative to extant theory and evidence on the impact of taxes on corporate capital and asset structures and describes its main contributions to this literature. Chapter 2 also develops the thesis hypotheses, which are motivated by Scholes and Wolfson's (1989a) argument that dynamic re-financing costs affect the time-series variation in tax-induced financing and investment decisions. Accordingly, the literature review in this chapter emphasizes prior studies on firms' reliance on debt and investment tax shields and the time-series pattern in their re-financing costs.

Chapter 3 sets the stage for the empirical tests of H_1 and H_2 that follow by providing evidence on the time-series variation in firms' rate of adjustment to their optimal capital structures. This chapter develops target adjustment models that are estimated in separate regressions with various proxies for firm-specific optimal capital structure borrowed from previous empirical research on the leverage adjustment process. These tests are intended to measure the fraction of the distance between actual and target leverage the firm covered during each year in the nine years following their initial public offerings. The purpose of these tests is to evaluate the accuracy of Scholes and Wolfson's (1989a) prediction that firms' capital structures gradually become more constrained by re-financing costs that are increasing in firm age.

Chapter 4 reports the primary thesis evidence on the predictions about the evolution in firms' tax-induced financing and investment policies. The empirical tests in this chapter examine firms' adjustments to their capital and asset structures in response to re-financing costs that are (potentially) increasing in firm age. These fixed effects tests, which control for unobserved firm-specific effects to avoid omitted variable bias and to refine the estimation of within-firm dynamics, are ideal for observing the time-series variation in firms' reactions to tax incentives. This chapter concludes with sensitivity tests designed to determine whether the choice of panel data estimation technique, the choice and specification of the regression variables, survivorship bias, censorship bias, firms' dependence on external financing, the duration of the panel time-series, or outliers affect the results of the tests on H_1 and H_2.

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examined later in the thesis.

The empirical tests in Chapters 3 and 4 estimate a firm's age as the number of years that have passed since its initial public offering. However, the research design applied in those chapters deliberately ignores that firms ordinarily will have private operating experience before going public. The purpose of Chapter 5 is to examine whether cross-sectional variation in firms' private ages, measured as the number of years between their incorporation and their IPOs, matters to the evidence presented in the preceding chapters.

Finally, Chapter 6 concludes by reviewing the empirical evidence reported in the thesis and identifying potential extensions.

Chapter 2

Review of Extant Literature, Hypotheses Development, and Thesis Contributions

2.1 Introduction

In this chapter, extant research relating to this thesis is reviewed. This review includes the analytical and empirical literature on the influence of taxes and re-financing costs on firms' capital structures. This chapter also develops the thesis hypotheses that are motivated by Scholes and Wolfson's (1989a) argument that re-financing costs affect the time-series variation in tax-induced financing and investment decisions. Accordingly, this literature review emphasizes studies that consider the time-series pattern in firms' re-financing costs and their reliance on tax shields. In addition, this chapter describes the contribution of this thesis to existing evidence concerning corporate financing and investment policy.

Although subsequent chapters will review prior research that has implications for issues of particular importance to those chapters, this chapter is intended to provide a summary of the literature that relates to the general theme of the thesis. This chapter is divided into five sections, including this introduction.

Section 2.2 summarizes studies pertaining to the first thesis hypothesis, which predicts that the association between firms' financial leverage and their marginal tax rates will become less positive with age. This section begins by describing existing evidence on the influence of taxes on firms' financing decisions. This review indicates that this research stream has been dominated by cross-sectional studies that frequently ignore the potentially moderating influence of re-financing costs on capital structure. This section continues with a review of theory and evidence on re-financing costs that appears to generally support Scholes and Wolfson's (1989a) prediction that firms' capital structures gradually become more constrained with age.

Section 2.3 develops the motivation for the second thesis hypothesis, which predicts that firms will gradually shift toward investment-related tax shields when re-financing costs begin to obstruct capital structure changes. This review reveals that extant empirical evidence on

DeAngelo and Masulis's (1980) theory of tax shield substitution is also mainly cross-sectional.

The contribution of this thesis to the capital structure literature is specified in Section 2.4. Section 2.5 concludes with a summary of this chapter and a preview of the tests conducted in the following chapter on the time-series variation in firms' rate of adjustment to their optimal capital structures.

2.2 Time-series Variation in Tax-induced Financing Decisions

Since Modigliani and Miller's (1963) tax correction paper was published, it has been suggested that the deductibility of interest for corporate income tax purposes should encourage some firms to have a relatively high proportion of debt in their capital structures; i.e., leverage should be positively associated with firms' marginal tax rates. Although theoretical research, such as Miller (1977) and DeAngelo and Masulis (1980), has contributed testable hypotheses that specify relations among capital structure, tax rates, and non-debt tax shields, evidence supporting these predictions until recently has been elusive.

For example, cross-sectional studies by Taub (1975), Marsh (1982), Stickney and McGee (1982), Bradley et al (1984), Auerbach (1985), Long and Malitz (1985), Ang and Peterson (1986), Chaplinsky (1987), Titman and Wessels (1988), Fischer et al (1989), Chaplinsky and Niehaus (1990), Kale et al (1991), Barclay et al (1995), and Gupta and Newberry (1997) do not consistently find a positive relation between tax status and financial leverage. However, MacKie-Mason (1990), Givoly et al (1992), Graham (1996a) and Graham et al (1998) provide empirical evidence that taxes influence capital structure decisions.

The irrelevance propositions formally derived by Modigliani and Miller (1958, 1963) (hereafter MM) include the result that a firm's financing policy should not affect its market value. They demonstrate that capital structure does not matter for firms operating in an economy characterized by the following highly restrictive conditions: there are no corporate or personal income taxes, there are no informational asymmetries, investment policy is not influenced by financing or dividend decisions, and there are no contracting costs.

Of course, the assumptions underlying the MM model are so unrealistic that the irrelevance propositions could themselves be criticized for being irrelevant. Yet, as explained by Harris and Raviv (1991) and others, a subtle, but very salient contribution of the model is its guidance on where *not* to look for the actual determinants of capital structure. In fact, subsequent analytical research has examined the effect on leverage decisions of systematically relaxing the MM conditions.

However, extant research has largely ignored contracting costs other than those associated with re-organizing financially distressed firms.[2] In fact, Myers (1984) in his presidential address to the American Finance Association comments:

> Large adjustment costs could possibly explain the observed wide variation in actual debt ratios, since firms would be forced into long excursions away from their *initial* debt ratios... If adjustment costs are large, so that some firms take extended excursions away from their targets, then we ought to give less attention to refining our static trade-off stories and relatively more to understanding what the adjustment costs are, why they are so important and how rational managers would respond to them (page 578, italics added).
>
> I know of no study clearly demonstrating that a firm's tax status has predictable, material effects on its debt policy. I think the wait for such a study will be protracted (page 588).

Shevlin (1999a) suggests in his presentation to the American Accounting Association Doctoral Consortium that future tax research should improve the empirical specification of non-tax costs:

> I would like to see future research in the non-tax costs area are push harder to refine the tests so that we can better understand the role of non-tax costs and also begin to quantify their magnitude. I think it is well accepted that firms trade-off tax benefits with non-tax costs... I would like to see more effort expended in carefully laying out the likely non-tax costs in any setting and proxies for these non-tax costs factored into the research design (page 438).

Similarly, Slemrod and Shobe (1990) advocate exploiting the characteristics of panel data when examining the behavioral response to tax incentives:

> Panel data have a natural advantage over cross-sectional data for the analysis of dynamic phenomena. The estimated cross-sectional correlation between behavior and tax rate has usually been interpreted as being the long-run equilibrium response,

[2] Exceptions include Lev and Pekelman (1975), Jalilvand and Harris (1984), and Fischer et al (1989).

> but this may be incorrect if individuals are slow to respond to changes in the environment. Panel data allows the researchers to trace the immediate impact of changes in the environment on behavior and also the lagged effect. Past behavior may also have a direct effect on current behavior (page 449).

This thesis will attempt to partially reconcile these concerns by providing evidence that firms prevented from making tax-motivated re-capitalizations by adjustment costs increasingly resort to implementing other tax-shielding strategies. Notwithstanding Myers' (1984) prediction quoted above, recent studies by MacKie-Mason (1990), Givoly et al (1992), Graham (1996a), and Graham et al (1998) have found that taxes affect firms' financing decisions.[3] This research will contribute to this literature by developing an empirical design that represents a richer empirical characterization of the capital structure problem to isolate the influence of dynamic adjustment costs on financing and investment policies.

Re-financing costs are another type of contracting cost that do not exist in a MM world since firms inhabiting their stylized economy can readily adjust their capital structures.[4] In reality, these costs may impede firms from altering their capital structures to exploit changes in their tax status. Accordingly, tax-planning opportunities such as reducing leverage when marginal tax rates decline may not be pursued when the re-financing costs involved exceed the potential tax savings.

These re-financing costs may take several forms. For example, some public debt may only be redeemable at par, although its market value may be less than this amount. Similarly, as the debt remaining after any redemption occurs becomes less risky, a premium would be necessary to persuade current bondholders to tender their securities.[5] In addition, any gain on

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[3] Chapter 4 provides details on the research designs applied in studies that report evidence consistent with taxes affecting firms' capital structures.

[4] For expositional convenience, the term "re-financing costs" is used in this thesis to refer to a broad range of contracting costs other than the direct or indirect re-organization costs incurred by financially distressed firms.

[5] Traditionally, publicly traded bonds have been restructured through voluntary exchange offers that can involve severe holdout problems. Provided enough bonds are exchanged that the firm avoids bankruptcy, the bondholders that do not tender benefit at the expense of those that tender. Although there are ways to arrange the exchange offer to penalize holdouts, these typically only ensure that the bondholders that tendered are in a better position if the firm subsequently files for bankruptcy (Gilson, 1991).

debt re-purchase would be considered a taxable event to the issuer. Frequently, bond indentures restrict additional debt issues and penalize or prohibit early retirement.

For private debt, Peterson and Rajan (1994) find that developing close ties with an institutional lender increases the availability and slightly lowers the cost of financing. Firms may be hesitant to adjust their loans to avoid damaging this relationship, especially since their evidence indicates that borrowing from multiple lenders increases the price and rationing of credit.

Firms may also be deterred from altering their equity securities to restore their optimal capital structures.[6] Myers and Majluf (1984) model a pecking order of financing that is attributable to asymmetric information between managers and potential investors. Leverage adjustments are costly because of the tendency of investors to discount the value of new securities, particularly equity, as compensation for their informational disadvantage.[7] In addition, relatively high flotation costs induce firms to only occasionally issue equity (Smith, 1977). Finally, there are costs incurred when re-arranging capital structure distracts management from operating the business.

However, more relevant to this thesis is existing research on the time-series variation in re-financing costs, which suggests that these impediments accumulate with age as firms' capital structures become less flexible and more complex. Scholes and Wolfson (1989a) argue that the temporal structure of corporate investment and financing decisions may constrain the effect that firms' marginal tax rates have on their leverage choices. They suspect that capital structure is largely a legacy of decisions made over the course of a firm's existence. Relative to seasoned firms, younger companies may not be as encumbered by their histories such that their capital structure re-financing costs may be lower.

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[6] The very existence of firm-specific optimal capital structure continues to be the subject of much debate in the finance literature (e.g., Helwege and Liang, 1996; and Shyam-Sunder and Myers, 1999). The static trade-off theory implies that firms pursue a target capital structure that balances the tax subsidy provided by debt against the expected costs of financial distress. However, Myers and Majluf's (1984) pecking-order model of corporate financing argues that firms do not attempt to gradually adjust toward targeted leverage.

[7] Smith (1986) and Barclay et al (1995) provide evidence that the market reaction to securities offerings supports Myers and Majluf's (1984) pecking order theory.

In fact, Helwege and Liang (1996) provide descriptive statistics on the time-series pattern of seasoned equity, public bond, and private debt offerings by a sample of firms that underwent an IPO in 1983 that support this argument. They report that firms access the capital markets less frequently as they mature with issues primarily occurring in the first four years following IPO and declining steadily thereafter. For example, external financing is obtained in 47 percent of the firm-year observations in the 1984-1988 period and in only 28 percent of the observations in the 1989-1992 period. Helwege and Liang argue that the attractive capital market conditions for issuing securities that prevailed in the early 1990s suggests that the gradual reduction in issues with age cannot be explained as simply an artifact of the years examined.[8]

Krishnaswami et al (1999) document that firms gradually shift from private debt toward public debt as they mature, which may undermine their capital structure flexibility.[9] Most private lenders are institutions such as commercial banks and life insurance companies that specialize in conducting comprehensive credit evaluations before a debt issue and monitoring firm performance after a debt issue. These private lenders have been the subject of extensive analytical and empirical research concerning their financing flexibility. Nakamura (1989), Berlin and Mester (1992), Rajan (1992), and Gorton and Kahn (1993) contend that the properties of loan contracts that facilitate renegotiations and the selective relaxing of covenants imply that borrowing from private lenders is more conducive to recontracting. In Chemmanur and Fulghieri (1994), banks attempt to acquire a reputation for contractual flexibility to attract firms that have access to both public and private debt.

Preece and Mullineuaux (1996) present evidence on syndicates of private lenders that supports the hypothesis that the capacity to renegotiate private debt contracts relatively inexpensively complements monitoring as a source of value to borrowers. This evidence is corroborated by research that indicates that private loans are frequently renegotiated; e.g.,

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[8] Helwege and Liang (1996) do not control for the economic conditions occurring in the decade following the 1983 IPOs. As explained below, the research design used in this study at least partially stifles potential confounding arising from shifts in macroeconomic conditions.

[9] There is evidence that young firms rely almost exclusively on private debt. In Slovin et al (1994), only a single firm from their sample of 175 entered the public debt market between the time of their IPO and their first seasoned equity issue. Datta et al (1999) report that their sample firms began issuing public debt on average 6.5 years after going public.

Beneish and Press (1993), Carey et al (1993), and Kwan and Carleton (1995).[10] Accordingly, the tendency of firms to gradually substitute public debt for private debt with age suggests that this coincides with a reduction in their capital structure flexibility.

In fact, this transition toward relying less on private debt over time may be accompanied by the characteristics of public debt exacerbating the reduction in financing flexibility, further impeding firms' reaction to changing tax incentives.[11] The free-rider problem can prevent the renegotiation of public debt (Grossman and Hart, 1980; and Gertner and Scharfstein, 1991); i.e., each individual bondholder might behave atomistically by holding out assuming that others will accept the offer to tender their securities. Also, Datta et al (2000) and Guedes and Opler (1996) respectively report that initial and seasoned debt issues have a mean maturity of 12 years; the mean maturity of bank loans is only about five years (Lummer and McConnell, 1989; and Tufano, 1993).

In addition, legislative obstacles make the modification of public debt contracts more difficult than private debt contracts. For example, restructurings of public debt are governed by the Trust Indenture Act of 1939, which prohibits the alteration of substantive provisions of the bond contract without the consent of each bondholder. The trustees in public debt issues are provided with only limited discretion during renegotiations not involving bankruptcy since it is difficult to obtain approval to change the covenants from the widely dispersed bondholders.

Existing theoretical and empirical research suggests that the presence of public debt in firms' capital structures also affects renegotiations with private lenders. For example, Park (2000) argues that when a secured (typically private) lender is better informed than unsecured (typically public) lenders, restructuring debt involving both classes of creditors will be impeded by informational asymmetry. Gilson et al (1990) report that firms are less likely to

[10] The argument that institutional lenders that have obtained proprietary firm-specific information are in a better position to evaluate firm prospects and collateral (e.g., Berlin and Mester, 1992) is consistent with Fama (1985), who reports that banks often negotiate representation on the board of directors to monitor the impact of borrowers' decisions on default risks.

[11] Extant analytical and empirical research is not unanimous that the addition of public debt reduces capital structure flexibility; e.g., Datta et al (2000) provide evidence consistent with Diamond's (1991b) argument that increased financing flexibility is afforded to high growth firms by the introduction of public debt.

restructure privately as the fraction of public debt to total liabilities increases. Asquith et al (1994) observe that banks seldom offer concessions to firms with non-investment grade public debt outstanding.

Similarly, James (1996) finds that banks only provide concessions to firms attempting to recover from financial distress when public debtholders also agree to renegotiate their claims. Since renegotiations with private debtholders appear to be conditioned on renegotiations with public debtholders, difficulty adjusting leverage may be increasing in the amount of public debt, which in turn tends to increase with age (Helwege and Liang, 1996; and Krishnaswami et al, 1999).

Further, there is evidence that firms gradually reduce their financing with securities that have intrinsic repayment flexibility. For example, Mayers (1998) finds that firms' reliance on convertible debt is decreasing in firm age; this evidence is corroborated by Helwege and Liang's (1996) descriptive statistics.[12] Several studies (e.g., Gupta, 1969; Schmidt, 1976; Titman and Wessels, 1988) detect that small firms use relatively more short-term debt in their capital structures, which relates to age since median firm size is found to gradually increase over time in this thesis. Scholes and Wolfson (1992) maintain that, although more expensive than long-term debt, short-term debt provides more flexibility for firms to choose optimal capital structures. In fact, Newberry and Novack (1999) report that taxes affect debt maturity decisions.

The evolution in firms' external financing decisions with age suggests that their capital structures also gradually become more complex. For example, Helwege and Liang (1996) report that the quantity of debt issues increase at a decreasing rate over firms' first decade of public operation. This can be interpreted as implying that adjusting capital structure becomes more difficult since covenants frequently prohibit or penalize subsequent debt issues such that more contracts result in more financing restrictions. Also, Gilson et al (1990) and Asquith et al (1994) find that firms with more classes of debtholders are more apt to resolve financial distress with a costly Chapter 11 filing, evidence supporting Bolton and

[12] However, from a tax perspective, conversion usually occurs at the least opportune time: i.e., when earnings improve and, accordingly, marginal tax rates have increased (Scholes and Wolfson, 1989a).

Scharfstein's (1996) argument that increasing the number of creditors complicates debt re-negotiations.[13]

This literature review generally corroborates Scholes and Wolfson's (1989a) argument that firms' capital structures gradually become more constrained by re-financing costs with age. This implies that firms are increasingly impeded over time from returning to their optimal capital structures. Scholes and Wolfson contend that the empirical relation between tax status and leverage would be more prominent in younger firms, which motivates the first thesis hypothesis (stated in alternate form):

> H_1: The association between financial leverage and marginal tax rates will become less positive as firms age.

However, certain non-tax effects relating to the pattern of information transparency and flotation costs on issuing securities may obstruct the detection of this hypothesis. The predicted positive relation between debt policy and tax status for young firms might be moderated by firm age being a determinant of the availability and cost of borrowing. Their short financial histories and their tendency to operate in emerging industries imply that young firms endure greater asymmetric information problems relative to seasoned firms. Since less information is available to capital markets about their operations and investment opportunities, these firms may resort more to bank loans or other private debt placements that involve greater monitoring and more restrictive covenants to mitigate debt-related moral hazard problems.

Consistent with these information problems gradually subsiding, Krishnaswami et al (1999) report that firms substitute public debt for private debt as they age. Their evidence supports Diamond's (1991a) prediction that borrowers, which initially depend on the financial intermediation of private debt placement to alleviate moral hazard, eventually begin to rely more on public debt when they have acquired a reputation for repaying loans. Diamond's

[13] However, Helwege's (1999) evidence on junk bond defaults indicates that the number of bond classes outstanding and whether the debt is publicly traded do not seriously affect the bargaining process. In contrast to Gilson et al (1990), Chatterjee et al (1996) find that firms with the more bank debt in their capital structures tend to resort to a traditional Chapter 11 filing rather than a prepackaged bankruptcy or a private workout.

theory predicts that firms attempt to establish a credit history for servicing their loans, which lowers their cost of capital and reduces credit rationing.

Petersen and Rajan (1994) document that borrowing costs are higher and credit is less available for younger private firms.[14] Datta et al (1999) report that firm age is negatively related to borrowing costs for initial public debt offers, additional evidence supporting Diamond's (1989, 1991a) theory on reputation formation in debt markets. Ritter (1991) finds that stock price performance improves the longer the duration between incorporation (or re-incorporation) and initial public offering which would justify lenders' reliance on firm age when evaluating borrower prospects. Lang (1991) provides theory and evidence that the magnitude of stock price reactions to earnings announcements decline with age, which is interpreted as indicating that firm-specific information is gradually revealed. In fact, empiricists (e.g., Carter and Manaster, 1990; James and Weir, 1990; and Ritter, 1991) have often specified age as a proxy for the extent of *ex ante* uncertainty about firm value.

Younger, smaller firms do not only have to contend with more acute asymmetric information problems when obtaining financing. If there are economies of scale in the cost of issuing securities (Smith, 1977, 1986; and Blackwell and Kidwell, 1988), then larger firms may alter their capital structures more frequently since their transactions costs are lower. For example, Jalilvand and Harris (1984) detect that large firms adjust more quickly to target long-term debt, which they attribute to lower flotation costs and better access to capital markets.

Titman and Wessels (1988) find that small firms use relatively more short-term debt, with this financing practice interpreted as supporting the notion that transaction costs on equity and long-term debt are higher for smaller firms. Carey et al (1993) provide evidence that only public debt issues exceeding $100 million are cost-effective, although private debt placements are economical for smaller issues. This is consistent with Krishnaswami et al's (1999) evidence that firms with larger issue sizes exploit their proportionally lower flotation costs of public debt.

[14] There are other studies that find interest rates are decreasing in firm age (e.g., Dennis et al, 1988; Peterson and Rajan, 1995; and Berger and Udell, 1995) and credit availability is increasing in firm age (e.g., Peterson and Rajan, 1995; and Cole, 1998).

Issue size is positively related to firm size and descriptive statistics for the sample of firms examined for their first nine public years in this thesis indicate that firm size increases every year. This implies that certain re-financing costs; i.e., flotation and asymmetric information costs on issuing securities, may be monotonically *decreasing*, thereby reducing the power of the tests to identify the predicted negative trend in the tax status-leverage relation.[15]

2.3 Time-series Variation in Tax-induced Investment Decisions

Scholes and Wolfson (1989a) also argue that seasoned firms, deterred by information and transactions costs from issuing and retiring securities to alter their capital structures, will begin to rely more on other tax shields, such as changing their asset structures, when their tax status changes. The second component of this thesis will attempt to provide evidence that the interaction between tax-motivated investment and financing decisions depends on firm maturity.

DeAngelo and Masulis (1980) contribute the seminal model on tax shield substitution by examining whether leverage irrelevancy will persist under a more realistic specification of the personal and corporate provisions of the tax code. They show that the presence of corporate tax shield substitutes for debt such as accounting depreciation, depletion allowances, and investment tax credits imply a market equilibrium in which each firm has a unique interior optimum leverage decision. The existence of non-debt tax shields is sufficient to reverse Miller's (1977) irrelevancy theorem such that firm-specific optimal capital structure is restored. Their model predicts a tax substitution effect that amounts to an inverse cross-sectional relation between leverage and investment-related tax shields.

The second thesis hypothesis (stated in alternate form) is:

> H_2: The association between financial leverage and investment-related tax shields will become more negative as firms age.

Until recent studies by MacKie-Mason (1990), Dhaliwal et al (1992), and Trezevant (1992), evidence supporting DeAngelo and Masulis's (1980) tax shield substitution hypothesis has

[15] Firm age and firm size are entered as separate control variables in the regressions reported in Chapters 4

been scarce. However, in a departure from extant empirical research which specify cross-sectional tests of the tax shield substitution hypothesis, the time-series variation in investment behavior will be evaluated in this thesis.

Some examples of cross-sectional studies that have used investment-related tax shields when testing the tax substitution hypothesis include Bradley et al (1984), Kim and Sorensen (1986), Titman and Wessels (1988), Chaplinsky and Niehaus (1990), MacKie-Mason (1990), Kale et al (1991), Dhaliwal et al (1992), Barclay et al (1995), Graham (1996a), and Cloyd et al (1997). Trezevant (1992) and Trezevant (1994), which examine firms' reactions to tax law changes, provide the only existing time-series evidence on tax shield substitution.

2.4 Thesis Contributions

This section is intended to only describe the main contributions of this thesis to the capital structure literature, with details on the empirical tests provided in later chapters.

Hypotheses H_1 and H_2 will be tested in a longitudinal setting with firms observed for their first nine full years of public operation. This research relates to the efficient design of organizations by examining *dynamic* (i.e., potentially increasing) re-financing costs relative to the *static* subsidy available on debt and investment-related tax shields. Evidence supporting the two predictions would provide a partial explanation for the scarcity of research finding that taxes matter to corporate financing and investment decisions. In fact, this evidence would be consistent with Scholes and Wolfson's (1992) contention that the presence of market frictions implies that efficient tax planning may involve abandoning the goal of tax minimization. In this situation, firms behave rationally by balancing the tax incentives to adjust their capital and asset structures against the re-financing costs that would be incurred.

This thesis will make several contributions to the extant literature on the influence of debt and investment-related tax shields on firms' capital structures. First, the research design described in Chapters 3 to 5 represents a more complete specification of the capital structure

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and 5.

problem by isolating the effect of dynamic adjustment costs on firms' financing and investment decisions. The empirical testing of H_1 and H_2 in Chapters 4 and 5 is preceded by tests that are designed to evaluate whether Scholes and Wolfson's (1989a) prediction that firms are gradually impeded from reverting to their optimal capital structures by re-financing costs that are increasing in firm age is descriptively accurate.

Second, this thesis contributes to the empirical capital structure literature by examining the time-series properties of firms' responses to tax incentives. Extant research studies either cross-sectional variation, usually by pooling across firms and through time to compile a sufficiently large data set that is assumed to contain independent observations drawn from the population, or time-series variation arising from changes in macroeconomic conditions.[16]

For example, Schulman et al (1996) study the time-series pattern in capital structure decisions of firms operating in Canada and New Zealand in the period surrounding tax law changes in those countries. In addition to other studies that examine whether financing decisions are affected by tax regime shifts (e.g., Givoly et al, 1992; and Trezevant, 1992), capital structure research has considered a variety of other macroeconomic factors, including recent capital market performance (Marsh, 1982), the availability of competing securities (Taggart, 1985) and the stage of the business cycle (Viswanath, 1993).

Conversely, testing in this thesis is designed to suppress the influence of transitions in macroeconomic conditions on the time-series variation in firms' tax-induced financing and investment decisions. This is accomplished by aligning the data used in this thesis in event time starting with the first full year since the firm went public, rather than in calendar time. This should reduce confounding admitted by shifts in macroeconomic conditions that affect financing decisions.

Third, this thesis responds to Slemrod and Shobe's (1990) and Shevlin's (1999a) call for tax research that more precisely examines non-tax costs and the time-series behavior of firms' reactions to tax incentives, respectively. This is largely achieved by exploiting the features of panel data to control for unobserved firm-specific effects to avoid omitted variable bias and

[16] The fact that firms' time-series behavior has seldom been examined in empirical research follows from

to refine the observation of within-firm dynamics. Specific econometric improvements to the extant literature will be described in the following chapters.

2.5 Conclusions

This chapter reviews prior theoretical and empirical research that relates to the development of the thesis hypotheses, which are motivated by Scholes and Wolfson's (1989a) predictions about the time-series variation in firms' financing and investment decisions. Specifically, they predict that since re-capitalizations become more difficult as firms age, their capital structures gradually become less sensitive to changes in their tax rates. Scholes and Wolfson also predict that firms increasingly impeded from adjusting their leverage by re-financing costs will begin to shift more toward relying on investment-related tax shields. In addition, this chapter describes the contributions of this thesis to the extant literature on the impact of taxes on corporate capital and asset structures.

This thesis tests hypotheses H_1 and H_2 using panel data spanning firms' first through ninth years since their initial public offerings. The literature review presented in this chapter includes theory and evidence that generally implies that re-financing costs are increasing in firm age, which is consistent with Scholes and Wolfson's (1989a) argument. However, Chapter 3 provides formal tests that examine firms' rate of adjustment toward their optimal capital structures to determine whether their argument that financial policy gradually becomes more constrained with age is accurate.

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the theory of capital structure being more amenable to explaining cross-sectional differences in financing.

Chapter 3

Time-series Variation in Rate of Adjustment to Optimal Capital Structure

3.1 Introduction

The purpose of this chapter is to provide evidence on the time-series variation in firms' rate of adjustment to their optimal capital structures. These tests will complement the literature reviewed in Chapter 2 which generally, although not unanimously, implies that re-financing costs are increasing in firm age as capital structures become less flexible and more complex. Scholes and Wolfson's (1989a) predictions about the evolution in firms' reliance on debt and investment-related tax shields, which motivated thesis hypotheses H_1 and H_2, are predicated on financial policies gradually becoming more constrained.

This chapter examines this fundamental issue with tests that are designed to measure firms' rate of adjustment toward their target leverage. Evidence of a negative time-series pattern in the adjustment process can be considered a prerequisite to the testing of H_1 and H_2. This evidence would reduce the possibility that explanations other than re-financing costs that increase over time are responsible for the results reported in Chapters 4 and 5 on the time-series variation in tax-induced financing and investment decisions.

The rest of this chapter is organized as follows. Section 3.2 describes the data sample used in these tests. Section 3.3 provides time-series evidence from tests that are intended to measure the fraction of the distance between actual and target leverage the firm covered during each year in the nine years following their initial public offerings. These tests are complicated by the difficulty encountered with specifying adequate proxies for firm-specific optimal capital structure. This matter is addressed by estimating several different proxies: (i) three based on a time-series mean of the firm's actual debt ratios; and (ii) one based on a cross-section of all other firms operating in the same industry in the same calendar year.

Section 3.4 examines whether the results reported for the time-series variation in firms' reversion to their optimal capital structures are materially affected by the duration of their private operating histories. The tests conducted in Section 3.3 are repeated for sub-samples

of "young" and "old" firms at their IPO dates, defined as the number of years between their incorporation and when they went public. Section 3.5 concludes with a summary of the empirical evidence presented in this chapter.

3.2 Sample Selection

The sample used to observe the time-series properties of firms' rate of adjustment to their optimal capital structures includes only those that became public companies from 1977 to 1988, which is the maximal period for which nine consecutive years of the *Compustat* data used in this thesis is available.

A listing of the 3,458 SEC-registered initial public offerings conducted between January 1, 1977 and December 31, 1988 was obtained from Security Data Corporation. This listing was compared to the Annual Industrial and Research *Compustat* files to identify the IPOs for which any *Compustat* data was available. Table 3.1 reports that this procedure reduced the sample to 2,180 firms.

The deletion of firms from utilities (defined as SIC codes from 4911 to 4941), financial (SIC codes from 6022 to 6200), insurance (SIC codes from 6312 to 6400) and real estate (SIC codes from 6500 to 6799) industries eliminated an additional 249 firms.[17] These industries were excluded because their capital market behaviors are considered to be fundamentally different from that of other firms because of regulation and the financial nature of their operations (MacKie-Mason, 1990).

For example, firms in regulated industries might have incentives to have relatively high leverage. Their stable cash flows may increase debt capacity (e.g., reduced credit rationing by lenders since default risk is lower), while the asset substitution (Jensen and Meckling, 1976) and under-investment (Myers, 1977) problems are attenuated by management's discretion over investment policy being largely delegated to regulatory authorities.

[17] In addition to utilities, other industries were regulated at different points during the 1977 to 1998 period of this study. All results reported in this thesis were virtually identical when firms operating in the railroad (SIC 4011), trucking (SIC 4213), airline (SIC 4512), and telecommunications (SIC 4812 and 4813) industries were removed from the sample.

Similarly, it may be prudent to remove financial firms such as banks and insurance companies from the sample because their leverage decisions may be strongly influenced by explicit (or implicit) investor insurance schemes such as deposit insurance (Rajan and Zingales, 1995). In fact, their debt-like liabilities are not strictly comparable to the debt issued by non-financial firms. Further, regulations such as minimum capital requirements may directly affect their capital structures (Scholes et al, 1990).

This thesis follows firms from their first through their ninth year of operation as public companies, which results in the removal of firms that were de-listed from the *Compustat* database because of mergers, acquisitions, bankruptcy, or liquidation during this period. Requiring nine consecutive years of data, which further reduced the sample to 1,060 firms, may introduce survivorship bias. For example, the sample probably includes relatively few severely financially distressed firms, especially in the early years of the panel period.

Retaining firms with any observations; i.e., permitting firms to enter and leave the sample, would reduce potential survivorship bias at the cost of changing the composition of the sample over time. Evidence supporting the predicted negative time-series variation in firms' rate of adjustment to their target leverage could be an artifact of the change in sample, rather than a change in the behavior of individual firms.

In addition, allowing the sample composition to change over the nine years could admit the effects of events that caused the firm to be de-listed from *Compustat* (e.g., bankruptcy, merger, or acquisition). Restricting the sample to only survivors will ensure that firms that, for example, underwent mergers that might experience major discontinuous shifts in capital structure rather than the more routine (less distorted) evolution in financing decisions that is the subject of this thesis are excluded. The econometric implications of potential survivorship bias are extensively evaluated in Chapter 4.

Finally, firms were removed from the sample if any of the observations necessary to construct the regression variables were missing. This restriction lowered the sample to 599 firms (see Table 3.1 for a summary). As applying these screening criteria results in the attrition of 82.7 percent of the original sample, the final sample may differ systematically from the population of IPOs occurring between 1977 and 1988; e.g., these restrictions might

bias the sample toward larger firms in certain industries (see Table 3.2). However, inspection of the data suggests that the industry and calendar year clustering in the sample resembles the clustering in the population.

3.3 Empirical Tests – Time-series Variation in Rate of Adjustment to Optimal Capital Structure – Full Sample

The following equation specifies a simple model of leverage adjustment to evaluate the potential reversion to the firm-specific optimal capital structure:

$$L_{it} - L_{i,t-1} = (1 - \delta)(L^{*}_{it} - L_{i,t-1}) + \mu_{it} \qquad (3.1)$$

where L_{it} and $L_{i,t-1}$ represent the firm's leverage at the end of the current and prior years, respectively. L^{*}_{it} is the firm's target leverage at the end of the year absent any re-financing costs, which are instead examined with the coefficient, $1 - \delta$ ($\in(0,1)$). If re-financing costs do not affect capital structure; i.e., if $\delta = 0$, then firms are observed to be at their target leverage; i.e., $L_{it} = L^{*}_{it}$.

However, at the other extreme, when $\delta = 1$, equation (3.1) implies that firms are prevented from adjusting their capital structures; i.e., $L_{it} = L_{i,t-1}$. Estimates of δ between 0 and 1 would indicate the extent of path dependence in firms' financing behavior. The coefficient on ($1 - \delta$) measures the fraction of the distance between actual and target leverage the firm moved during the year. This empirical model attributes any delay in adjustment to firm-specific optimal capital structure to re-financing costs.

Empirical research using similar versions of this partial adjustment model includes Taggart (1977), Marsh (1982), Jalilvand and Harris (1984), Auerbach (1985), Gilson (1997) and Shyam-Sunder and Myers (1999). However, extant evidence of mean reversion in debt ratios or that firms gradually shift toward leverage targets is cross-sectional; e.g., Taggart (1977), Marsh (1982), Jalilvand and Harris (1984), Auerbach (1985), Fischer et al (1989), Opler and Titman (1994), Gilson (1997) and Shyam-Sunder and Myers (1999). The following tests examine the time-series variation in the speed of adjustment toward proxies for firm-specific

optimal capital structure by charting the pattern of the coefficients on $(1 - \delta)$ across the firms' first nine public years.

Two main methodologies have been applied in the empirical literature to estimate the adjustment model described in equation (3.1). The first involves specifying target leverage, L^*, as a linear function of the determinants of capital structure, excluding transactions costs, prescribed by theory. This approach enables the influence of transactions costs to be isolated in the estimated coefficient on lagged actual leverage, $L_{i,t-1}$ (e.g., Auerbach, 1985; and Gilson, 1997). For expositional simplicity, the results from such lagged dependent variable tests are reported in Chapter 5, which examines whether firms' financing decisions are affected by the duration of their private and public operating histories.

The second methodology relies on direct proxies for firm-specific optimal capital structure when estimating equation (3.1); e.g., Jalilvand and Harris (1984) and Shyam-Sunder and Myers (1999). The magnitude of reversion toward target leverage is estimated with the coefficient on $(1 - \delta)$. Firms' reactions to the assumed underlying drift from their optimal capital structures are identified through this reversion variable that measures the rate of adjustment. Scholes and Wolfson's (1989a) argue that re-financing costs that are increasing in firm maturity eventually prevent firms from altering their capital structures to exploit changes in their tax status. This implies that negative time-series variation will be observed in the rate of adjustment variable, $(1 - \delta)$, consistent with their prediction that firms are increasingly impeded by re-financing costs from restoring their optimal capital structures.

This research design is complicated by the difficulty involved with estimating adequate proxies for target leverage, L^*. To attenuate this problem, the time-series variation in firms' (potential) reversion toward their targets is examined using several different proxies for firm-specific optimal capital structure: (i) three based on a time-series mean of the firm's actual debt ratios; and (ii) one based on a cross-section of all other firms operating in the same four-digit SIC code in the same year. However, the difficulty with specifying sensible proxies for target leverage is a reminder that arguably the most justifiable methodology for examining whether *dynamic* re-financing costs affect capital structure decisions is to estimate the time-

series variation in firms' reactions to the *static* incentives provided by debt and investment-related tax shields. These tests of H_1 and H_2 are reported later in the thesis.

3.3.1 Book-based vs. Market-based Leverage

The target adjustment tests, which are reported in the next section, examine the timing and extent of (potential) reversion toward firm-specific optimal capital structure. In the presence of imperfect capital markets, management is responsible for developing an optimal dynamic adjustment policy that minimizes re-financing costs and the costs from continuing to deviate from their target leverage. Accordingly, this raises the issue of whether a market-based or a book-based debt ratio should be used in these tests which are intended to isolate deliberate financing choices.

Although the theory of capital structure suggests that debt ratios should be measured using market values, book values are justified by the reliance of many rating agencies and corporate treasurers on book leverage; e.g., bond covenant restrictions apply to book, not market, values. Also, using market equity in the denominator of the leverage ratio would impound price variances arising from extraneous economic conditions, a problem that might be exacerbated in this study of newly public companies as their share prices tend to be relatively volatile.

For example, Miller (1977) argues that book leverage is more suitable for examining firms' financing decisions than market leverage, which is very sensitive to the levels of share prices. Finally, Myers (1977) explains that there may be theoretical support for specifying book values, which correspond to the value of assets in place (tangible assets provide better loan collateral) and usually exclude the capitalized value of growth opportunities. Taggart (1977), Marsh (1982), Jalilvand and Harris (1984), Opler and Titman (1994), Gilson (1997) and Shyam-Sunder and Myers (1999) specify targets using book rather than market values.

The following target adjustment tests are conducted with leverage specified as the book value of short-term and long-term debt divided by the book value of the sum of short-term debt, long-term debt, and shareholders' equity. Unless otherwise stated, the results were

qualitatively similar when the tests were re-run with market-based leverage, defined as the book value of short-term and long-term debt divided by the sum of the book value of short-term and long-term debt and the market value of shareholders' equity.

3.3.2 Firm-specific Dynamic Target Leverage

In this section, target leverage is initially estimated as the time-series mean of the firm's debt ratio across the nine-year sample period, excluding the current year to reduce the mechanical relation with the dependent variable. For example, the target for a firm in its fourth public year is specified as the mean of its leverage for years one to three and five to nine. Taggart (1977), Marsh (1982), Jalilvand and Harris (1984), and Shyam-Sunder and Myers (1999) use similar proxies for firm-specific optimal capital structure.

The validity of this specification may depend on the optimum proxy remaining relatively constant to ensure that the speed of adjustment estimates, $(1 - \delta)$, are not spuriously related to changes in the target ratios.[18] In fact, volatility in the target proxies would not matter if these were measured without error, although this is almost certainly not the case. Stability in the yearly target proxies might appear to contradict Scholes and Wolfson's (1989a) prediction that re-financing costs that accumulate with age induce firms to gradually drift farther from their targets.[19] Their argument implies that the precision of the target proxy is reduced when the debt ratios of older firms are included. However, this should affect the yearly firm-

[18] Fortunately, the potential presence of a calendar year time trend in the data is at least partially suppressed by studying firms as they age; i.e., the data is aligned in event time starting with the first full year since the firm went public. For example, the sample of firms in their third public year is drawn from the IPOs conducted between 1977 and 1988. As explained in Chapter 2, this should reduce confounding admitted by shifts in macroeconomic conditions that might affect financing decisions. Jalilvand and Harris (1984) provide evidence that capital market conditions, such as management's expectations about interest rates and security prices, influence the speed of firms' adjustment to long-run leverage targets. Including calendar year time dummies in the target adjustment tests hardly affects the results reported in this chapter.

[19] There may be other reasons that the evolution in firms' capital structure decisions during their first nine public years might affect the stability of the time-series target proxy. For example, past security issues may induce temporary departures from the optimal capital structure arising from firms attempting to exploit short-term market conditions (Marsh, 1982). However, extending the duration over which the time-series mean for the target leverage proxy is determined to the entire sample period should reduce the impact of this.

specific targets similarly since all years, except the current year, are included in the time-series mean.

Descriptive statistics on the annual cross-sectional means reported in Table 3.3 provide no evidence that there is time-series variation in this target leverage proxy over firms' first nine years of public existence, which suggests that parameter instability is not a serious problem. Although aggregating over the entire sample period should also smooth temporal fluctuations and reduce the influence of outliers, this proxy remains an imperfect measure of the unobservable firm-specific optimal debt ratio.

In the following tests, the time-series mean of the firm's leverage for the nine-year panel, after removing the current year, is specified to represent the firm-specific target. To admit firm heterogeneity and to avoid serial correlation complications, equation (3.1) is estimated in separate cross-sectional regressions for each of the first nine post-IPO years using a random coefficients model based on Hildreth and Houck (1968) that attributes parameter heterogeneity to stochastic variation. This estimation technique, which has been modified to correct for heteroscedasticity, allows for cross-sectional variation in the nine rate of adjustment, $(1 - \delta)$, coefficients, each representing an average of individual firm coefficients for that observation. Results from OLS estimation are virtually identical.

The resulting series of year-to-year estimates (along with 90 percent confidence intervals) for this target leverage reversion proxy are reported in Figure 3.1. The Pearson correlation of these coefficients with firm age is –0.70, which is significant at the five-percent level in a one-tailed test assuming independence. Although this supports the predicted negative time-series variation in the reversion toward firm-specific optimal capital structure, this statistic does not consider the variable estimation error in the coefficients.

This matter is addressed by estimating a weighted least squares regression, with the weights determined by the standard errors of the coefficient estimates; i.e., the least weight is assigned to the observations that are measured with the most error. The weighted least squares regression of the reversion parameter estimates from the nine cross-sectional random

coefficients models on firm age also provides evidence at the five-percent level that a negative pattern exists (t-statistics are in parentheses):

$$(1 - \delta)\ coefficient_t = \underset{(7.669)}{0.5374} - \underset{(-2.712)}{0.0344}AGE + e_t$$

where: $(1 - \delta)$ = The difference between the target leverage proxy (the time-series mean of the firm's debt ratio over their first nine public years excluding the current year) and the one-year lag of the firm's debt ratio.

AGE = Firm age which is measured as the number of years elapsed since its IPO.

As explained above, any target proxy could be criticized for not properly estimating the firm-specific optimal capital structure. Accordingly, the concern that the evidence indicating that firms are gradually increasingly impeded from adjusting toward their leverage targets is spuriously induced by measurement error is considered by re-specifying the target proxy. Prior empirical research has estimated target leverage with three- and five-year historical moving averages (e.g., Jalilvand and Harris, 1984), which allows for more variability in the targets over time. Although this prevents examining firms' capital structures until their third full year of public operation, an alternate time-series proxy for the firm's target leverage is specified as its mean leverage over the three years preceding the current year.[20]

Equation (3.1) is re-estimated in cross-section for firms' third through ninth public years with their three-year historical mean leverage substituted as the target, L^*. The estimates for the target leverage reversion measure, $(1 - \delta)$, from the seven random coefficients models are graphed against firm age in Figure 3.2. The Pearson correlation with firm age is –0.90, which is significant at the one-percent level in a one-tailed test assuming independence. Again, as this correlation ignores the precision of the coefficient estimates, a weighted linear regression is run with the least weight allocated to the observations that are measured with the most error. The results from regressing the speed of adjustment, $(1 - \delta)$, estimates on firm

[20] This proxy, which is calculated using only three years of data, is probably more susceptible to confounding from capricious shifts in macroeconomic conditions than other prevailing proxies such as the mean leverage across the entire sample period. However, descriptive statistics indicate that there is no time-series variation in this target leverage proxy over firms' first third through ninth public years, which again provides some assurance that the reported regression results are not spuriously obtained because of parameter instability (see Table 3.3).

age provides evidence at the one-percent level of negative time-series variation in the reversion toward optimal capital structure (t-statistics are in parentheses):

$$(1 - \delta)\ coefficient_t = 0.4736 - 0.0436AGE + e_t$$
$$(7.885)\quad(-4.594)$$

where: $(1 - \delta)$ = The difference between the target leverage proxy (the time-series mean of the firm's debt ratio over the three preceding years) and the one-year lag of the firm's debt ratio.

AGE = Firm age which is measured as the number of years elapsed since its IPO.

Finally, re-estimating equation (3.1) by replacing the target proxy with the firm's mean leverage over the three years subsequent to the current year will provide additional evidence on whether firms become more constrained from moving toward their optimal capital structures with age. The nine cross-sectional estimates for the speed of adjustment measure, $(1 - \delta)$, when this proxy represents target leverage, L^*, are reported in Figure 3.3. These coefficients and firm age have a Pearson correlation of –0.82, which is significant at the one-percent level in a one-tailed test assuming independence. However, the results from measuring target leverage with the firm's mean leverage over the three years following the current year cannot be properly interpreted until the estimation error in the coefficients is considered.

This is again handled by estimating a weighted linear regression with the least weight assigned to the $(1 - \delta)$ coefficients that were estimated least precisely. The results from regressing the estimates from the nine random coefficients models on firm age also provide evidence at the one-percent level of a negative time-series pattern in the adjustment toward optimal capital structure (t-statistics are in parentheses):[21]

[21] However, significant evidence at the five-percent level of *positive* time-series variation in the rate of adjustment coefficients is observed in this test when market-based, rather than book-based, debt ratios are used to measure leverage in equation (3.1). The negative time-series variation reported for the tests that use the three other proxies for optimal capital structure (the nine-year time-series mean debt ratio, the three-year historical mean debt ratio, and the contemporaneous industry-mean debt ratio) are robust to re-specifying the debt ratios in equation (3.1) with market-based leverage. This market-based debt ratio is defined as the book value of short-term and long-term debt divided by the sum of the book value of short-term and long-term debt and the market value of shareholders' equity. The book-based debt ratio is identical except that the book value of shareholders' equity is used in the denominator.

$$(1 - \delta)\ coefficient_t = 0.4943 - 0.0135AGE + e_t$$
$$(23.539)\ (-3.493)$$

where: $(1 - \delta)$ = The difference between the target leverage proxy (the time-series mean of the firm's debt ratio over the three subsequent years) and the one-year lag of the firm's debt ratio.

AGE = Firm age which is measured as the number of years elapsed since its IPO.

3.3.3 Contemporaneous Industry-mean Leverage

The industry-mean (or median) leverage has been used to represent firm-specific target debt ratios (e.g., Lev, 1969; and Lev and Pekelman, 1975). Evans (1991) reports that restructuring consultants frequently rely on industry benchmarks when advising clients on choosing new capital structures. Several studies (e.g., Schwartz and Aronson, 1967; Bowen et al, 1982; Bradley et al, 1984; and Campbell, 1986) detect persistent industry clustering in debt ratios, which has been interpreted as evidence supporting the existence of optimal capital structures.

In the following tests, the contemporaneous industry-mean debt ratio of firms in the same four-digit SIC code, after removing the firm being studied, in the same calendar year is specified to proxy for the firm-specific target. The procedure of estimating equation (3.1) using separate cross-sectional random coefficients regressions for each of the first nine post-IPO years is repeated to chart the dynamic pattern in the (potential) reversion toward target leverage.

The resulting series of year-to-year estimates of the reversion parameter, $(1 - \delta)$, are presented in Figure 3.4. Each estimate is surrounded by a 90 percent confidence interval. The Pearson correlation of these coefficients with firm age is –0.54, which is significant at the ten-percent level in a one-tailed test assuming independence.

Since this statistic ignores the variable estimation error in the coefficients, a weighted least squares regression is performed with the weights again assigned according to the standard errors of the coefficient estimates; i.e., the least weight is placed on the observations

estimated with the least precision. This test, which involves regressing the reversion parameter estimates from the nine random coefficients models on firm age, also provides evidence at the ten-percent level that firms' adjustment to their leverage targets gradually becomes more impeded (t-statistics are in parentheses):

$$(1 - \delta)\ coefficient_t = \underset{(6.775)}{0.1662} - \underset{(-1.582)}{0.0069}AGE + e_t$$

where: $(1 - \delta)$ = The difference between the target leverage proxy (the industry-mean debt ratio in the same calendar year of firms in the same four-digit SIC code, after removing the firm being studied) and the one-year lag of the firm's debt ratio

AGE = Firm age which is measured as the number of years elapsed since its IPO.

However, the evidence that indicates that reversion toward the industry-mean target debt ratio becomes more difficult with age should be interpreted cautiously. For example, young firms may be radically different from mature firms, which typically dominate the industry means, on important characteristics, such as size and investment opportunity sets.[22]

It is important to stress that this evidence, which consistently implies that the capital structure adjustment process becomes more impeded as firms age, depends on the adequacy of the target leverage proxies.[23] However, the estimation of sensible proxies for target leverage is particularly difficult for this sample of maturing firms. For example, initially small and risky, these firms might systematically increase their debt ratios as they age, rather than

[22] Jain and Kini (1994) examine the change in operating performance of firms making the transition from private to public ownership through IPOs and find that in the post-issue period these firms have high growth rates in sales and capital expenditures relative to other firms in the same industry. It could be argued that firms should have been matched on other characteristics, such as size, growth options, and profitability, as well as their SIC codes when estimating this target leverage proxy. However, this would have seriously reduced the precision of the industry matching since only broad SIC codes (usually one-digit) were available when firms were matched on other characteristics in addition to their industry.

[23] The tests developed in this chapter follow extant research on whether adjustment costs affect financing decisions by estimating the magnitude of firms' reversion toward their target leverage. These regressions examine firms' reactions to the assumed underlying drift from their optimal capital structures through the reversion variable that measures the rate of adjustment. However, a more primitive issue is the extent that firms' drift from their optimal capital structures each year in the nine years following their initial public offerings. In fact, tests using the above proxies for target leverage provide consistent time-series evidence (not tabulated) that firms drift farther from their optimal capital structures with age.

revert to the specified optimum (Diamond, 1989, 1991a; and Shyam-Sunder and Myers, 1999). In fact, the panel data tests of the tax hypotheses, H_1 and H_2, reported later provide evidence that firms' leverage ratios increase with age consistent with Diamond's theory of reputation formation in debt markets.[24]

This issue is examined with pooled OLS tests that condense the time-series variation in the adjustment process into a single coefficient, firm age interacted with the reversion parameter, $(1 - \delta)$. As reported in Table 3.4, evidence at p-values under 0.01 supporting the predicted negative time-series variation in the reversion toward target leverage is found for each of the four target proxies.

In these pooled tests, positive regression intercept terms and low explanatory power would be symptoms of the sample being biased by firms operating below their optimal capital structures. This appears to be the situation when the three-year historical mean debt ratio and the contemporaneous industry-mean debt ratio are specified to estimate target leverage (columns 2 and 4 in Table 3.4, respectively). The constant is positive and significant and the adjusted-R^2 is low in each case. However, both the nine-year time-series mean debt ratio and the three-year lead mean debt ratio are apparently acceptable target proxies (columns 1 and 3 in Table 3.4, respectively); i.e., in both cases, the estimated intercept is near zero and the adjusted-R^2 is considerably higher.

The pooled tests also provide an opportunity to compare the reversion parameter, $(1 - \delta)$, estimates to extant research on target adjustment models. Table 3.4 reports the following parameter estimates for the tests that measure firms' reversion toward the following proxies for their optimal capital structures: 0.512 for the nine-year time-series mean debt ratio; 0.586 for the three-year historical mean debt ratio; 0.452 for the three-year lead mean debt ratio;

[24] Diamond (1989, 1991a) argues that incentive problems in debt markets are worse in firms' early years when they suffer from severe information deficiencies. However, young firms can moderate asset substitution and moral hazard problems by developing a reputation for repaying their debts, which in turn lowers their interest rates and increases credit availability. Diamond models the dynamics of borrowers' incentives with lenders learning over time from observing firms' credit histories. Firms are interested in acquiring a reputation for servicing their loans since this valuable asset lowers their cost of capital and reduces credit rationing. The panel data tests of the tax hypotheses, H_1 and H_2, include firm age as a control variable. These tests provide evidence consistent with Diamond's prediction that older firms experience less credit rationing by lenders; i.e., debt ratios are found to be increasing in firm age.

and 0.211 for the contemporaneous industry-mean debt ratio. In comparison, Jalilvand and Harris (1984, Table II) and Shyam-Sunder and Myers (1999, Table 2) report target adjustment parameter estimates of 0.383 and 0.410, respectively.

The reversion parameter estimates for the sample of firms examined in this thesis are generally slightly larger than the corresponding estimates provided by previous empirical studies. This was expected to occur for at least two reasons. First, Scholes and Wolfson (1989a) predict that young firms that have relatively more flexible and less complex capital structures can more expediently adjust to their target leverage.

Second, the sample firms are in their early public years, a period in which they have incentive to frequently change their capital structures. For example, Rajan and Zingales (1998) and Helwege and Liang (1996) provide evidence consistent with the argument that there exists a life cycle in the pattern of corporate financing, with firms relying more on external capital early in their histories to finance their lucrative initial investment projects. Their volatile investment demands may induce these younger firms to adjust their capital structures more often than the mature, stable firms that almost certainly dominate the samples in Jalilvand and Harris (1984) and Shyam-Sunder and Myers (1999).

3.4 Empirical Tests – Time-series Variation in Rate of Adjustment to Optimal Capital Structure – Sample Split on Private Age History

The research design in this thesis primarily measures a firm's age as the number of years that have elapsed since its IPO, although firms ordinarily have a private operating history when they go public. This suggests that cross-sectional differences in firms' private ages, defined as the number of years between their incorporation and their IPOs, could affect the above tests that measure the rate of adjustment toward target leverage.

This section reports results from specifications in which the panel is split into the top and bottom distributions of firms' median private histories; i.e., the sample is bisected into the youngest and oldest firms at their IPO dates. Similarly, the tests of hypotheses H_1 and H_2 in Chapter 5 examine the time-series variation in tax-induced financing and investment decisions for these sub-samples.

Data on firms' private ages were collected from a variety of sources. Professor Jay Ritter of the University of Florida maintains an IPO database that provided the majority of the founding dates for sample firms having their initial public offerings between 1977 and 1984.[25] For the 1985 to 1989 IPOs, *Ward's Business Directory of U.S. Private and Public Companies* was the primary data source. In addition, incorporation dates were found in *Moody's International Manual*, *Moody's Industrial Manual*, *Moody's OTC Industrial Manual*, and *Directory of Corporate Affiliations*.

The Scholes and Wolfson (1989a) argument implies that the financing decisions of the older firms may be more constrained since they already have extensive histories by the time that they go public. However, the mean of their coefficient estimates for the optimal leverage reversion parameter, $(1 - \delta)$, over their first nine public years was generally only slightly larger than that of the sub-sample of young firms. This result (not tabulated) is found for all target leverage specifications except for the three-year historical time-series mean, for which there is statistically significant evidence that firms older at their IPO dates are slower to adjust to their optimal capital structures in the post-IPO period.

However, this thesis is more concerned with the evolution in firms' financing decisions in the nine years following their initial public offerings. The tests in Section 3.3 that examine the time-series variation in firms' rate of adjustment to their targets were re-run on both the young and old firm sub-samples. The following eight figures graph the $(1 - \delta)$ coefficients obtained from estimating equation (3.1) using year-to-year random coefficients models for each sub-sample for each of the four proxies for optimal capital structure.

In Figures 3.5 and 3.6, the nine-year time-series mean debt ratio proxies for target leverage. The Pearson correlation between firm age and the reversion parameter estimates, $(1 - \delta)$, is –0.50 and –0.76 for the young and old firm sub-samples, respectively. These statistics are respectively significant at the ten- and one-percent levels in one-tailed tests assuming

[25] Professor Ritter explains that the founding year is occasionally difficult to determine because of firms that started as partnerships and later incorporated; firms that arose from mergers or divestitures; or firms that were inactive for a period of years after their formal incorporation. Generally, the founding date listed on his database corresponds to the incorporation date of the major part of the business. The other publications; i.e., *Ward's Business Directory*, the various *Moody's* directories, and *Directory of Corporate Affiliations*, simply report firms' incorporation dates without commenting on whether there was any

independence. This statistical significance remains when weighted linear regressions are run to correct for the variable estimation error in the coefficients. This evidence suggests that both young and old firms at their IPO dates experience negative time-series variation in their reversion toward optimal capital structure in their first nine public years.

In fact, similar evidence is found when this estimation procedure is repeated with the other target leverage proxies substituted for L^* in equation (3.1). Specifically, the reversion parameter coefficients from the year-to-year cross-sectional random coefficients models were separately estimated for the sub-samples of young and old firms at their IPO dates. For the following target leverage proxies, these Pearson correlations between the $(1 - \delta)$ coefficients and firm age were observed (one-tailed significance levels after correcting for the coefficient estimation error are in parentheses):

(i) –0.85 (one-percent) and –0.70 (five-percent) for the young firms and old firms in Figures 3.7 and 3.8, respectively, when the three-year historical mean debt ratio represents target leverage;

(ii) –0.51 (ten-percent) and –0.62 (five-percent) for the young firms and old firms in Figures 3.9 and 3.10, respectively, when the three-year lead mean debt ratio represents target leverage; and

(iii) –0.44 (not significant) and –0.52 (ten-percent) for the young firms and old firms in Figures 3.11 and 3.12, respectively, when the contemporaneous industry-mean debt ratio represents target leverage.

3.5 Conclusions

In summary, the evidence presented in this chapter is consistent with Scholes and Wolfson's (1989a) contention that firms' capital structure decisions are hindered by re-financing costs that are increasing in firm age. The results reported in Section 3.3 using various proxies for firm-specific optimal capital structure support their prediction that the adjustment process gradually becomes more constrained. Further, the evidence provided in Section 3.4 suggests that the time-series variation in firms' reversion toward their target leverage is not affected by their private operating histories; i.e., a negative pattern was detected in both sub-samples of young and old firms at their IPO dates.

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confusion about the correct date.

Although these tests should be interpreted cautiously since there are indications that the proxies used for target leverage are coarse estimates, the observed negative time-series variation in the adjustment process would reinforce the credibility of evidence supporting H_1 and H_2. The results reported in this chapter would reduce the likelihood that explanations other than gradually increasing re-financing costs are responsible for evidence consistent with Scholes and Wolfson's (1989a) predictions about the evolution in firms' reliance on tax shields.

The empirical tests of H_1 and H_2 conducted in Chapters 4 and 5 examine the time-series variation in tax-induced financing and investment decisions. The research design in those chapters provide additional evidence on the *dynamic* pattern in re-financing costs by estimating firms' reactions to the *static* incentives available on debt and investment-related tax shields. This approach avoids the complications involved with attempting to specify proxies for optimal capital structure.

Table 3.1: Sample Selection Summary – Tests of Firms' Rate of Adjustment to Their Optimal Capital Structures

Calendar Year	1977	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	Total
Number of SEC registered IPOs	32	38	62	149	348	122	685	357	355	728	415	167	3,458
Number of firms not followed by *Compustat* since initial public offering	(11)	(20)	(22)	(67)	(125)	(40)	(246)	(129)	(131)	(290)	(150)	(47)	(1,278)
Number of firms from utilities, financial, insurance, and real estate industries	(2)	(3)	(4)	(11)	(26)	(7)	(51)	(24)	(29)	(53)	(27)	(12)	(249)
Number of firms that did not survive through their first nine years of public operation	(12)	(2)	(10)	(27)	(83)	(36)	(178)	(111)	(96)	(166)	(100)	(50)	(871)
Number of firms with missing *Compustat* observations	(7)	(7)	(10)	(20)	(44)	(21)	(90)	(41)	(53)	(95)	(47)	(26)	(461)
Number of firms in the sample	0	6	16	24	70	18	120	52	46	124	91	32	599

Table 3.2: Industry Distribution of Sample – Tests of Firms' Rate of Adjustment to Their Optimal Capital Structures

2-Digit SIC Code	Number	Industry Description	Percentage
13	13	Oil and gas extraction	2.2%
20	14	Food	2.3%
27	10	Printing and publishing	1.7%
28	37	Chemicals	6.2%
35	62	Industrial and commercial machinery	10.4%
36	61	Electronic and electrical equipment	10.2%
38	44	Measuring, analyzing, and controlling equipment	7.3%
39	10	Miscellaneous manufacturing industries	1.7%
48	13	Communications	2.2%
50	14	Wholesale trade – durable goods	2.3%
56	13	Apparel and accessory stores	2.2%
58	10	Restaurants	1.7%
59	17	Miscellaneous retail	2.8%
73	53	Business services	8.8%
80	10	Health services	1.7%
87	13	Engineering, accounting, research, management, and related services	2.2%
Sub-total	394		65.8%
	205	Industries with fewer than 10 firms	34.2%
Total	599		100%

Table 3.3: Summary Statistics – Annual Cross-sectional Means of the Various Proxies for Firm-specific Optimal Capital Structure

Post-IPO Year	Firm-specific Optimal Capital Structure Proxy: Nine-year Time-series Mean Debt Ratio	Three-year Historical Mean Debt Ratio	Three-year Lead Mean Debt Ratio	Contemporaneous Industry-mean Debt Ratio
	1	2	3	4
Year 1	0.2841		0.2492	0.2449
Year 2	0.2816		0.2668	0.2491
Year 3	0.2802	0.2449	0.2733	0.2592
Year 4	0.2796	0.2693	0.2748	0.2621
Year 5	0.2803	0.2844	0.2723	0.2533
Year 6	0.2788	0.2891	0.2634	0.2437
Year 7	0.2802	0.2879	0.2578	0.2397
Year 8	0.2787	0.2871	0.2508	0.2289
Year 9	0.2773	0.2915	0.2470	0.2199
Number of annual observations	599	599	280	514

Notes:

This table presents the annual sample means for the four proxies for firm-specific optimal capital structure used in the target adjustment tests. The firm-specific optimal capital structure proxies are defined as follows. The nine-year time-series mean debt ratio includes all of the firm's first nine years of public operation except for the current year. The three-year historical mean debt ratio is the firm's time-series mean debt ratio for the three years preceding the current year. The three-year lead mean debt ratio is the firm's time-series mean debt ratio for the three years succeeding the current year. The contemporaneous industry-mean debt ratio is the industry-mean debt ratio for the same calendar year of firms in the same four-digit SIC code, after removing the firm under study. The firm-specific optimal capital structure proxies are specified for firms' first through ninth years of public operation, except for the three-year historical mean debt ratio which is specified for firms' third through ninth public years. For each proxy, the debt ratio is defined as the book value of short-term and long-term debt divided by the sum of the book value of short-term debt, long-term debt, and shareholders' equity.

Figure 3.1 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Nine-year Time-series Mean Debt Ratio)

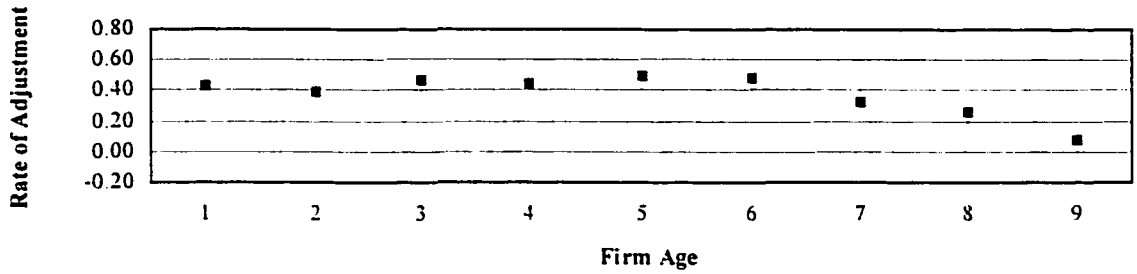

Figure 3.1 – This figure presents the cross-sectional coefficient estimates for firms' first through ninth years of public operation for the rate of adjustment variable, $(1 - \delta)$, in the target adjustment model specified in equation (3.1). The firm's nine-year time-series mean debt ratio, which is defined in Table 3.3, represents target leverage in this specification. The rate of adjustment coefficient measures the fraction of the distance between actual and target leverage that the firm moved during the year. The squares in this figure indicate the slope coefficients from the nine cross-sectional random coefficients models. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Figure 3.2 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Three-year Historical Mean Debt Ratio)

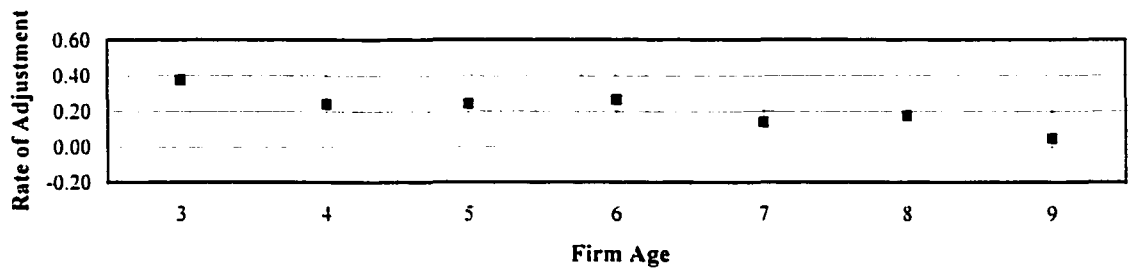

Figure 3.2 - This figure presents the cross-sectional coefficient estimates for firms' third through ninth years of public operation for the rate of adjustment variable, $(1 - \delta)$, in the target adjustment model specified in equation (3.1). The firm's three-year historical mean debt ratio, which is defined in Table 3.3, represents target leverage in this specification. The rate of adjustment coefficient measures the fraction of the distance between actual and target leverage that the firm moved during the year. The squares in this figure indicate the slope coefficients from the seven cross-sectional random coefficients models. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Figure 3.3 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Three-year Lead Mean Debt Ratio)

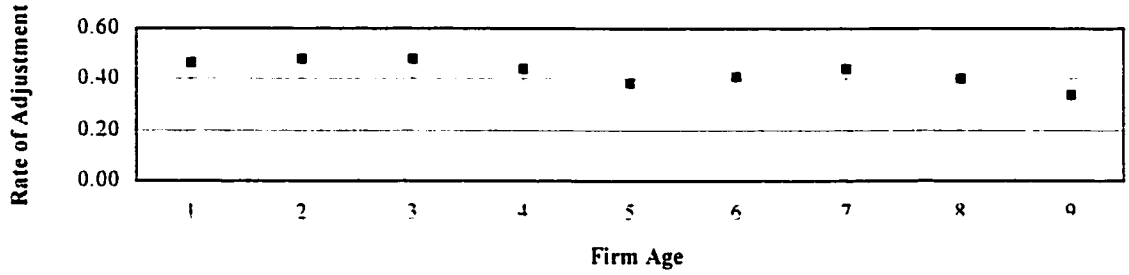

Figure 3.3 - This figure presents the cross-sectional coefficient estimates for firms' first through ninth years of public operation for the rate of adjustment variable, $(1 - \delta)$, in the target adjustment model specified in equation (3.1). The firm's three-year lead mean debt ratio, which is defined in Table 3.3, represents target leverage in this specification. The rate of adjustment coefficient measures the fraction of the distance between actual and target leverage that the firm moved during the year. The squares in this figure indicate the slope coefficients from the nine cross-sectional random coefficients models. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Figure 3.4 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Contemporaneous Industry-Mean Debt Ratio)

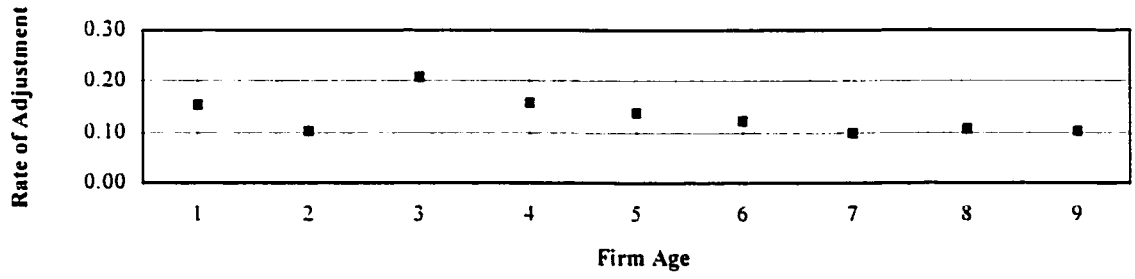

Figure 3.4 - This figure presents the cross-sectional coefficient estimates for firms' first through ninth years of public operation for the rate of adjustment variable, $(1 - \delta)$, in the target adjustment model specified in equation (3.1). The contemporaneous industry-mean debt ratio, which is defined in Table 3.3, represents target leverage in this specification. The rate of adjustment coefficient measures the fraction of the distance between actual and target leverage that the firm moved during the year. The squares in this figure indicate the slope coefficients from the nine cross-sectional random coefficients models. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Table 3.4: OLS Regression Results – Balanced Panel Tests of Firms' Reversion toward Various Proxies for Optimal Capital Structure

$$\Delta LEVERAGE_{it} = \alpha + \beta_1 REVERSION_{it} + \beta_2 AGE_{it} + \beta_3 AGE*REVERSION_{it} + \varepsilon_{it}$$

		Firm-specific Optimal Capital Structure Proxy			
Variable	Prediction	Nine-year Time-series Mean Debt Ratio 1	Three-year Historical Mean Debt Ratio 2	Three-year Lead Mean Debt Ratio 3	Contemporaneous Industry-mean Debt Ratio 4
Intercept	?	0.004	0.020***	0.002	0.020***
Reversion parameter	+	0.512***	0.586***	0.452***	0.211***
Firm Age	+	0.001	-0.002**	0.000	-0.001*
Age* Reversion parameter	-	-0.030***	-0.059***	-0.013***	-0.011***
Adjusted R^2		0.137	0.040	0.253	0.055
F-statistic		285.46°	176.98°	286.01°	90.92°
Number of observations		5,391	4,192	2,520	4,626

Notes:

This table presents regression results for changes leverage models using ordinary least squares to examine the reversion toward various specifications of firms' optimal capital structures. The reversion parameter is the difference between the proxy for firm-specific optimal capital structure and the one-year lag of the firm's debt ratio (see also equation (3.1)). The firm-specific optimal capital structure proxies are defined as follows. The nine-year time-series mean debt ratio includes all of the firm's first nine years of public operation except for the current year. The three-year historical mean debt ratio is the firm's time-series mean debt ratio for the three years preceding the current year. The three-year lead mean debt ratio is the firm's time-series mean debt ratio for the three years succeeding the current year. The contemporaneous industry-mean debt ratio is the industry-mean debt ratio for the same calendar year of firms in the same four-digit SIC code, after removing the firm under study. All regressions include firms' first through ninth years of public operation, except for the regression with the three-year historical mean debt ratio specified as the firm-specific optimal capital structure proxy (column 2), which includes only firms' third through ninth public years. For each firm-specific optimal capital structure proxy and the dependent variable, the debt ratio is defined as the book value of short-term and long-term debt divided by the sum of the book value of short-term debt, long-term debt, and shareholders' equity. Firm age is the number of years that have elapsed since the firm's initial public offering. Regression equation *F*-tests significant at less than 0.001 are identified by a ° superscript. In this table, the subscripts i and t identify firms and time, respectively. The superscript asterisks indicate explanatory variable coefficient significance at *p*-values less than 0.10 (*), 0.05 (**), and 0.01 (***) in one-tailed tests where directional predictions are made and two-tailed tests otherwise.

Figure 3.5 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Nine-year Time-series Mean Debt Ratio) - Sub-sample of Young Firms at IPO

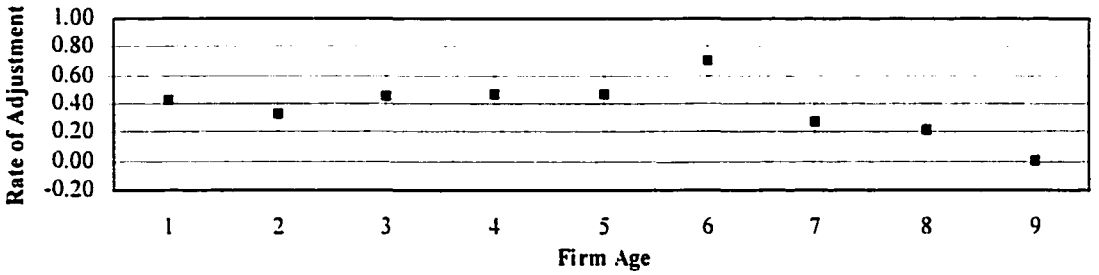

Figure 3.5 – This figure presents the cross-sectional coefficient estimates for firms' first through ninth years of public operation for the rate of adjustment variable, $(1 - \delta)$, in the target adjustment model specified in equation (3.1). This sub-sample includes only younger firms that are defined as those for which the duration of their private history, which is measured as the number of years that have elapsed between their incorporation and their initial public offering, is below the median for the full sample. The firm's nine-year time-series mean debt ratio, which is defined in Table 3.3, represents target leverage in this specification. The rate of adjustment coefficient measures the fraction of the distance between actual and target leverage that the firm moved during the year. The squares in this figure indicate the slope coefficients from the nine cross-sectional random coefficients models. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Figure 3.6 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Nine-year Time-series Mean Debt Ratio) - Sub-sample of Old Firms at IPO

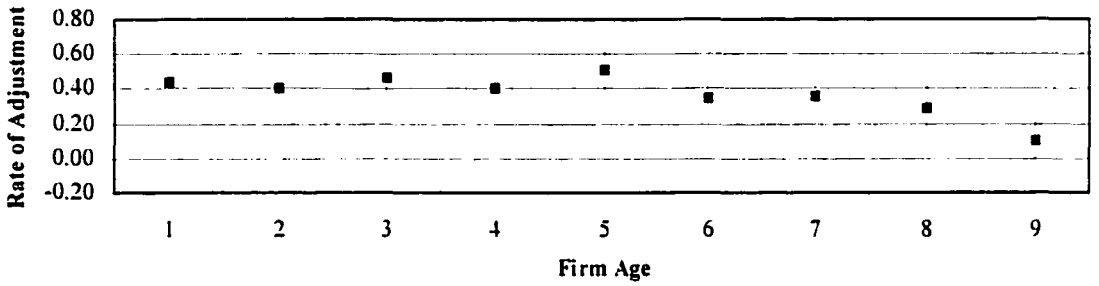

Figure 3.6 – This figure presents the cross-sectional coefficient estimates for firms' first through ninth years of public operation for the rate of adjustment variable, $(1 - \delta)$, in the target adjustment model specified in equation (3.1). This sub-sample includes only older firms that are defined as those for which the duration of their private history, which is measured as the number of years that have elapsed between their incorporation and their initial public offering, is above the median for the full sample. The firm's nine-year time-series mean debt ratio, which is defined in Table 1, represents target leverage in this specification. The rate of adjustment coefficient measures the fraction of the distance between actual and target leverage that the firm moved during the year. The squares in this figure indicate the slope coefficients from the nine cross-sectional random coefficients models. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Figure 3.7 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Three-year Historical Mean Debt Ratio) - Sub-sample of Young Firms at IPO

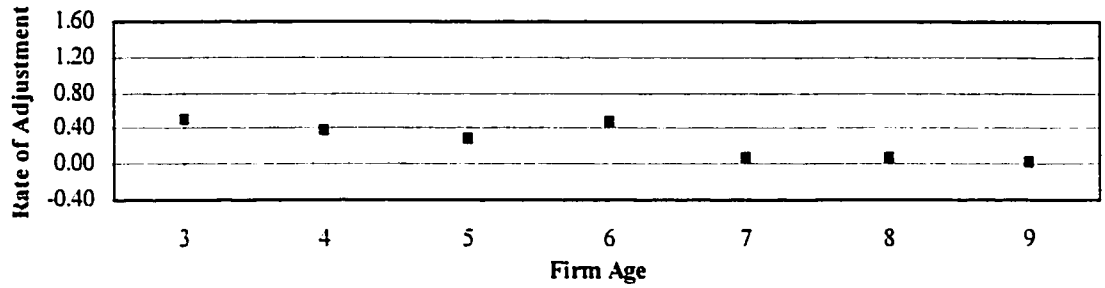

Figure 3.7 - This figure presents the cross-sectional coefficient estimates for firms' third through ninth years of public operation for the rate of adjustment variable, $(1 - \delta)$, in the target adjustment model specified in equation (3.1). This sub-sample includes only younger firms that are defined as those for which the duration of their private history, which is measured as the number of years that have elapsed between their incorporation and their initial public offering, is below the median for the full sample. The firm's three-year historical mean debt ratio, which is defined in Table 3.3, represents target leverage in this specification. The rate of adjustment coefficient measures the fraction of the distance between actual and target leverage that the firm moved during the year. The squares in this figure indicate the slope coefficients from the seven cross-sectional random coefficients models. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Figure 3.8 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Three-year Historical Mean Debt Ratio) - Sub-sample of Old Firms at IPO

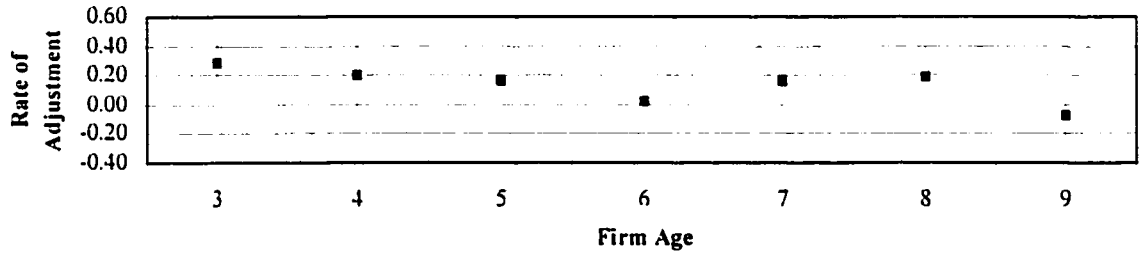

Figure 3.8 - This figure presents the cross-sectional coefficient estimates for firms' third through ninth years of public operation for the rate of adjustment variable, $(1 - \delta)$, in the target adjustment model specified in equation (3.1). This sub-sample includes only older firms that are defined as those for which the duration of their private history, which is measured as the number of years that have elapsed between their incorporation and their initial public offering, is above the median for the full sample. The firm's three-year historical mean debt ratio, which is defined in Table 3.3, represents target leverage in this specification. The rate of adjustment coefficient measures the fraction of the distance between actual and target leverage that the firm moved during the year. The squares in this figure indicate the slope coefficients from the seven cross-sectional random coefficients models. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Figure 3.9 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Three-year Lead Mean Debt Ratio) - Sub-sample of Young Firms at IPO

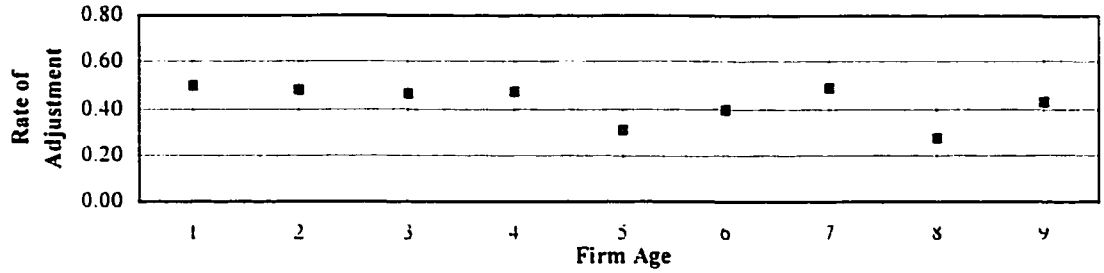

Figure 3.9 - This figure presents the cross-sectional coefficient estimates for firms' first through ninth years of public operation for the rate of adjustment variable, $(1 - \delta)$, in the target adjustment model specified in equation (3.1). This sub-sample includes only younger firms that are defined as those for which the duration of their private history, which is measured as the number of years that have elapsed between their incorporation and their initial public offering, is below the median for the full sample. The firm's three-year lead mean debt ratio, which is defined in Table 3.3, represents target leverage in this specification. The rate of adjustment coefficient measures the fraction of the distance between actual and target leverage that the firm moved during the year. The squares in this figure indicate the slope coefficients from the nine cross-sectional random coefficients models. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Figure 3.10 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Firm's Three-year Lead Mean Debt Ratio) - Sub-sample of Old Firms at IPO

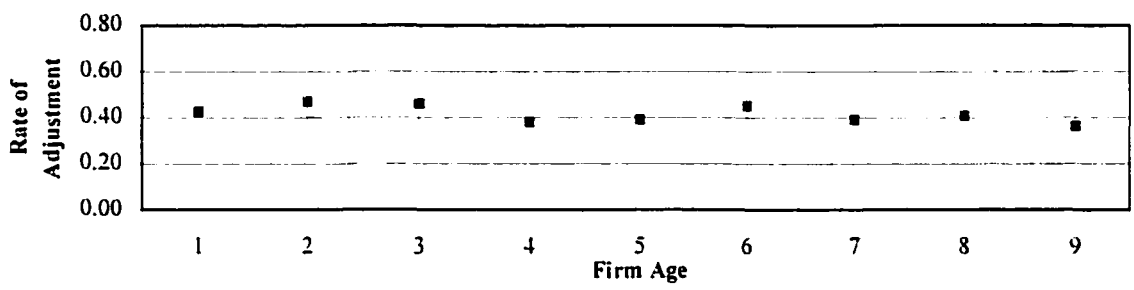

Figure 3.10 - This figure presents the cross-sectional coefficient estimates for firms' first through ninth years of public operation for the rate of adjustment variable, $(1 - \delta)$, in the target adjustment model specified in equation (3.1). This sub-sample includes only older firms that are defined as those for which the duration of their private history, which is measured as the number of years that have elapsed between their incorporation and their initial public offering, is above the median for the full sample. The firm's three-year lead mean debt ratio, which is defined in Table 3.3, represents target leverage in this specification. The rate of adjustment coefficient measures the fraction of the distance between actual and target leverage that the firm moved during the year. The squares in this figure indicate the slope coefficients from the nine cross-sectional random coefficients models. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Figure 3.11 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Contemporaneous Industry-mean Debt Ratio) - Sub-sample of Young Firms at IPO

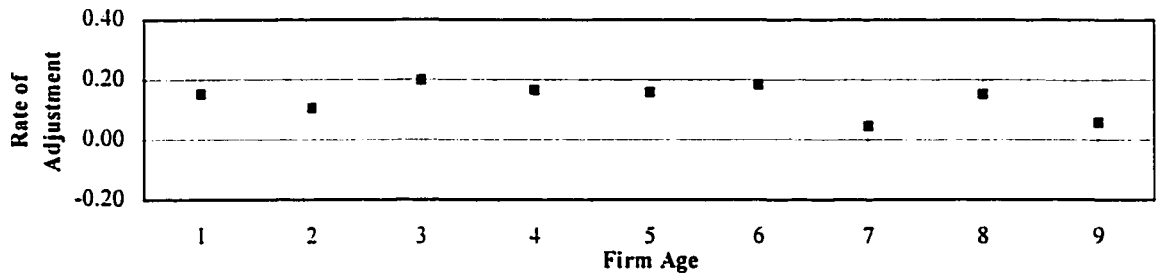

Figure 3.11 - This figure presents the cross-sectional coefficient estimates for firms' first through ninth years of public operation for the rate of adjustment variable, $(1 - \delta)$, in the target adjustment model specified in equation (3.1). This sub-sample includes only younger firms that are defined as those for which the duration of their private history, which is measured as the number of years that have elapsed between their incorporation and their initial public offering, is below the median for the full sample. The contemporaneous industry-mean debt ratio, which is defined in Table 3.3, represents target leverage in this specification. The rate of adjustment coefficient measures the fraction of the distance between actual and target leverage that the firm moved during the year. The squares in this figure indicate the slope coefficients from the nine cross-sectional random coefficients models. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Figure 3.12 - Time-series of Rate of Adjustment to Target Leverage (Defined as the Contemporaneous Industry-mean Debt Ratio) - Sub-sample of Old Firms at IPO

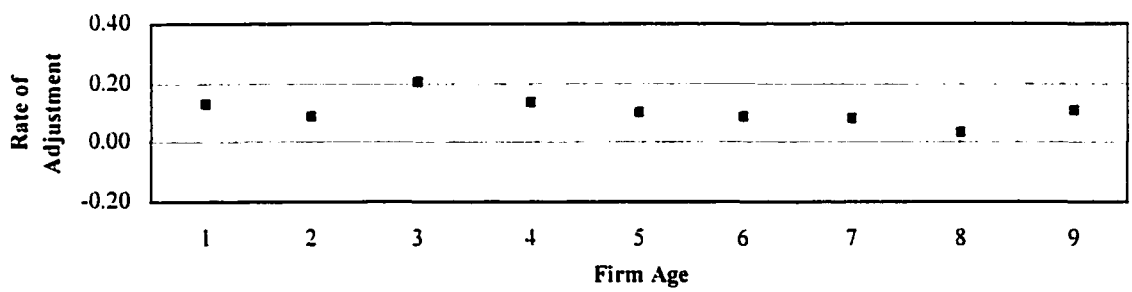

Figure 3.12 - This figure presents the cross-sectional coefficient estimates for firms' first through ninth years of public operation for the rate of adjustment variable, $(1 - \delta)$, in the target adjustment model specified in equation (3.1). This sub-sample includes only older firms that are defined as those for which the duration of their private history, which is measured as the number of years that have elapsed between their incorporation and their initial public offering, is above the median for the full sample. The contemporaneous industry-mean debt ratio, which is defined in Table 3.3, represents target leverage in this specification. The rate of adjustment coefficient measures the fraction of the distance between actual and target leverage that the firm moved during the year. The squares in this figure indicate the slope coefficients from the nine cross-sectional random coefficients models. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Chapter 4

Time-series Variation in Tax-induced Financing and Investment Decisions

4.1 Introduction

The purpose of this chapter is to provide evidence on the time-series variation in firms' tax-induced financing and investment decisions. Extant theoretical and empirical research reviewed in Chapter 2 generally supports Scholes and Wolfson's (1989a) prediction that firms' capital structures gradually become more constrained by re-financing costs that accumulate with age. In fact, Chapter 3 reports evidence from target adjustment models that firms are increasingly impeded in the nine years following their initial public offerings from restoring their optimal capital structures. The preceding chapters essentially set the stage for the empirical tests of the thesis hypotheses, H_1 and H_2, which are presented in this chapter.

Scholes and Wolfson (1989a) argue that the scarcity of evidence that taxes affect corporate financing decisions is partially attributable to research designs that neglect to specify the capital structure problem to include dynamic adjustment costs. The empirical tests in this chapter attempt to resolve this issue by examining whether the evolution in firms' financing and investment decisions is consistent with H_1 and H_2. The first thesis hypothesis predicts that the relation between financial leverage and marginal tax rates will become less positive as firms age.

There is considerable cross-sectional theory (e.g., DeAngelo and Masulis, 1980; and Dammon and Senbet, 1988) and evidence (e.g., MacKie-Mason, 1990; and Dhaliwal et al, 1992) on tax shield substitution. However, Scholes and Wolfson (1989a) suspect that the influence of taxes on investment and financing decisions also depends on firm maturity. Specifically, they argue that firms will shift more toward investment-related tax shields when re-financing costs begin to prevent changes to their capital structures. This chapter also provides evidence on the second thesis hypothesis, which predicts that the relation between financial leverage and investment-related tax shields will become more negative as firms age.

This chapter continues with a description in Section 4.2 of the data sample used to study the hypotheses. Section 4.3 surveys the analytical and empirical literature on the potential

determinants of capital structure to motivate the choice and specification of the regression variables. Section 4.4 develops the methodology applied to research the time-series variation in firms' tax-induced financing and investment decisions and reports the evidence on H_1 and H_2. Section 4.5 provides sensitivity tests. Finally, Section 4.6 concludes with a summary of the evidence presented in this chapter and a preview of the tests conducted in the following chapter on whether the duration of firms' private operating histories affect these results.

4.2 Sample Selection and Descriptive Statistics

The sample used to evaluate the time-series properties of firms' tax-induced financing and investment decisions is identical to that used in Chapter 3 with one important exception. The specification of additional regressions variables in Section 4.3 further reduces the sample to 189 firms, which are examined for their first nine years of public existence. Table 4.1 summarizes the data screening imposed to obtain the sample used in this chapter from the 3,458 SEC-registered initial public offerings occurring between January 1, 1977 and December 31, 1988. The sample selection procedure applied is similar to those used in prior capital structure studies: e.g., Mackie-Mason (1990), Rajan and Zingales (1995), Graham (1996a), and Graham et al (1998).

As explained in Section 3.2, the research design in this thesis restricts the sample to firms that survive through their ninth public year to ensure that the empirical evidence reported is not spuriously induced by changes in the composition of the sample over time. In addition, retaining firms with any observations; i.e., permitting firms to enter and leave the sample during the nine-year panel, could introduce confounding from events that caused these firms to be removed from *Compustat*. This thesis is more concerned with the typical evolution in capital structures than the frequently major discontinuous shifts experienced by firms in the period surrounding their merger, acquisition, bankruptcy, or liquidation.

However, requiring nine consecutive years of data contributes to the serious attrition in the sample, which represents only 5.5 percent of the original population of IPOs.[26] The econometric

[26] Although the 189 firms are only a small fraction of the initial population of IPOs, liberal screening criteria were applied whenever possible to preserve the sample. For example, it could be argued that the sample should

implications of potential survivorship bias are extensively considered later in this chapter. Finally, inspection of the data suggests that the calendar year (Table 4.1) and industry (Table 4.2) representation in the sample is comparable to that observed in the population.

4.3 Regression Variables and Descriptive Statistics

This section describes the dependent and explanatory variables that are used in the empirical tests of hypotheses H_1 and H_2. The specification of the three variables of primary interest in this thesis, leverage, marginal tax rates, and investment-related tax shields, is emphasized. The control variables representing the other potential determinants of capital structure prescribed by theory are then developed and specified. This section concludes with descriptive statistics on the regression variables.

4.3.1 Leverage

Empirical studies of capital structure have usually used the book value of debt and the market value of equity as the ingredients in a market-based leverage ratio. Bowman (1980) and Mulford (1985) provide evidence that the market value of debt is adequately measured by its book value.[27] Although certainly more theoretically defensible, market-based leverage measures are more affected by share price fluctuations than book-based measures and may not always accurately reflect firms' financing decisions (Miller, 1977). This problem may be exacerbated in this study of newly public firms as their share prices tend to be more volatile, which would hardly matter provided that firms have sufficient flexibility to adjust their outstanding securities to restore their optimal capital structures.

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exclude any firms that changed their fiscal year-ends during their first nine public years to ensure that the tests are synchronized at one-year intervals. However, as this screen would have further reduced the sample to 178 firms, instead the empirical tests of H_1 and H_2 are re-run later to determine whether the reported results are robust to this sub-sample.

[27] The reliance on debt reported in the financial statements implicitly assumes that the firm's debt is trading at par, which Ritter and Warr (1999) argue may be unjustified when prevailing interest rates have changed substantially since the debt was issued. For example, some firms in the present sample are observed in a time-series that includes the early 1980s, a period when high inflation was experienced. Ritter and Warr use the Warga Fixed Income Data Base to estimate firms' market long-term debt and find that their empirical results are not qualitatively affected by whether market or book debt is specified in their tests. Generally, market value of a firm's equity is considerably more variable than its debt.

However, market frictions may impede this adjustment (see also Scholes and Wolfson, 1988), which implies that measuring leverage at specific intervals in the nine-year panel may be problematic when attempting to detect deliberate changes. This concern is addressed later by re-estimating the regressions with other market and book leverage measures substituted as the dependent variable to determine whether the results are robust.

Debt is defined as total long-term, convertible, and short-term debt scaled by the market value of the firm in the primary thesis tests. Constantinides and Grundy (1989) and Stein (1992) argue that convertible debt provides cost-effective financing when informational asymmetries are relatively severe such as for the young, small firms that dominate this sample. In fact, Helwege and Liang (1996), Mayers (1998), and Krishnaswami et al (1999) report supporting evidence that firms tend to issue convertible bonds in their early public years. Essig (1991) finds that across industries small firms have the highest proportion of convertible securities. Short-term debt, which is easier to adjust in response to changes in marginal tax rates according to Scholes and Wolfson (1992), is also included in the primary leverage specification. Gupta (1969), Schmidt (1976), and Titman and Wessels (1988) provide evidence that small firms rely more on short-term debt.

The predictions are tested on debt levels. MacKie-Mason (1990) and Shevlin (1999a) argue that examining incremental capital structure decisions is preferable to studying absolute debt ratios, which represent the accumulation of historical decisions that may obscure the effect that taxes have on leverage.[28] Firms' financial policies are set based on their expectations about their future tax status. However, in the presence of imperfect capital markets, older firms might be constrained from adjusting toward their target leverage when they drift into the wrong tax

[28] This is certainly not the only complication caused by using leverage levels instead of changes. For example, Myers and Majluf's (1984) pecking-order theory of corporate financing argues that actual capital structure may deviate from the optimal ratio predicted by the static trade-off theory, which argues that firms balance the tax subsidy provided by debt against the expected costs of financial distress. Myers (1984) explains that random events (e.g., unexpected earnings) that move firms away from their optimal capital structure suggest that there will be some cross-sectional dispersion of observed debt ratios across a sample of firms having the same target leverage. This is less of an issue in this study, which examines the time-series variation in financing behavior, than in strictly cross-sectional studies. In addition, according to the Scholes and Wolfson's (1989a) argument tested in this thesis, younger firms should not have drifted that far from their optimal capital structures. Statistical problems inherent in the levels tests such as standard errors being biased by serial correlation in the residuals, correlated omitted variables, and censoring of debt ratios at values of zero are addressed below.

clientele. This implies that cross-sectional tests using proxies for the firms' marginal tax rates to explain debt levels will suffer from low explanatory power.

This matter is directly evaluated in this study since firms are observed as they age starting with their first post-IPO year, which ensures that they initially have no public financial histories.[29] More importantly, firms may have both contemporaneous and lagged responses to changing tax shield incentives – the differenced data would only identify the contemporaneous responses. Testing in levels is more consistent with the central tenet of the hypotheses that re-financing costs persuade firms to *gradually* adjust their capital and asset structures.

4.3.2 Marginal Tax Rates

A generally accepted measure of the marginal tax rate (MTR), which is defined as the present value of current and expected future taxes paid on an additional dollar of income earned today, has not emerged from the empirical literature. In fact, each of the studies (MacKie-Mason, 1990; Givoly et al, 1992; Graham, 1996a; and Graham et al, 1998) that detect the predicted positive relation between leverage and tax status specifies a different MTR proxy.

MacKie-Mason (1990) estimates MTR using the amount of tax loss carryforwards (TLCF). He supports this specification with Auerbach and Poterba (1986), who find that firms carrying forward losses have a high probability of having zero tax rates in the following year. Accordingly, TLCF significantly influence the marginal tax rate on interest as each dollar of TLCF will probably crowd out a dollar of interest deductions. By using the amount of TLCF available, rather than merely whether the firms in his sample have any TLCF, MacKie-Mason manages to increase the power of his tests by capturing firms' proximity to tax exhaustion.

Givoly et al (1992) proxy a firm's marginal tax rate with the average of its tax rate over the most recent ten-year period. This measure is estimated by dividing the sum of the amount of taxes paid and the discounted deferred taxes accumulated in this period by the pre-tax income earned in these ten years.

[29] These firms have private operating histories at their IPO dates, an issue that is examined in Chapter 5.

Graham (1996a) extends Shevlin (1990) to measure MTR through simulated tax rates that account for the carryback and carryforward provisions for net operating losses, the investment tax credit, and the alternative minimum tax. This procedure involves forecasting 18 years of taxable income and accruing taxes payable over a 22-year period (three years historical, one year contemporaneous, and 18 years forecasted) as follows. The forecasted taxable income is generated assuming that taxable income follows a random walk with a drift. The drift term is sampled from a normal distribution with mean and standard deviation equal to the mean and standard deviation of the first differences in the firm's taxable income series.

The present value of the total tax liability is then calculated for each of the 22 years (year t - 3 to year t +18), with Moody's corporate bond yield representing the economy-wide discount rate. Next, the present value of the total tax payable is recalculated after adding one dollar to current taxable income. The marginal tax rate is the difference between these two total tax liabilities. Graham (1996a) repeats this procedure 50 times for each firm-year with the average across the simulations being reported as the expected marginal tax rate.

Graham (1996b) evaluates how well ten of the most widely used MTR proxies predict perfect foresight marginal tax rates. He finds that his simulation algorithm produces the most accurate MTR measure currently available. He also concludes that proxies similar to the ones used by MacKie-Mason (1990) and Givoly et al (1992) are mediocre proxies for MTR.

Graham et al (1998) provide evidence that existing corporate tax status proxies are endogenous to financing decisions, which induces a spurious negative correlation when leverage is regressed on contemporaneous MTR. As interest payments are tax deductible, a firm that finances its operations with debt reduces its taxable income, which in turn may lower its expected marginal tax rate. Unless this endogeneity is remedied, tests of the influence of tax status on capital structure decisions may be biased against detecting the predicted positive relation.

Graham et al (1998) modify Graham's (1996a) simulation algorithm to obtain marginal tax rates (derived from taxable income before financing) that are not endogenously affected by capital structure decisions. They then rely on their before-financing simulated marginal tax rates to provide the first evidence of a positive relation between debt *levels* and tax status. Extant

research that documents a positive relation between debt policy and tax rates examine *incremental* financing decisions (MacKie-Mason, 1990; Givoly et al, 1992; and Graham, 1996a), which avoids the potential confounding induced by the endogeneity problem.

However, using Graham et al's (1998) before-financing marginal tax rates in this thesis would prevent examining firms' capital structure decisions until their fourth year of public operation since their simulation algorithm requires at least three years of historical data. Instead, a version of a trichotomous variable originally advocated by Shevlin (1990) is specified to measure MTR as:

(i) the top statutory corporate tax rate if taxable income is positive and TLCF is non-positive;

(ii) one-half the top statutory rate if either taxable income is positive or TLCF is non-positive; and

(iii) zero if taxable income is non-positive and TLCF is positive.

The top statutory corporate tax rate is 48 percent before 1979, 46 percent from 1979 to 1986, 39.5 percent in 1987, 34 percent from 1988 to 1992, and 35 percent starting in 1993. Actually, the tax rates for 1987 vary according to the month of the firms' fiscal year-ends, but using the average 1987 rate should be acceptable. Deferred tax expense grossed up by the top statutory tax rate is subtracted from the sum of income before extraordinary items, incomes tax expense, minority interest, and extraordinary items and discontinued operations (grossed up by one minus the top statutory tax rate) to arrive at taxable income (Shevlin, 1990).

This trichotomous variable was the second most accurate proxy of the perfect foresight marginal tax rate in Graham's (1996b) evaluation of extant MTR proxies. The trichotomous measure is lagged one year since, as Scholes et al (1990) explain, the tax status explanatory variable should be the MTR prevailing at the time that corporate decisions are made. This corresponds to a one-year lag relative to the leverage dependent variable, which reduces the potential for endogeneity to obscure the influence of taxes on financing and investment.

The issue of whether lagging is sufficient to suppress potential endogeneity bias is examined in Section 4.4 with cross-sectional tests that regress the trichotomous MTR proxy on leverage. A

spurious negative relation would indicate the presence of endogeneity, while evidence supporting the predicted positive relation would imply that lagging is adequate in this empirical setting. Also, the sensitivity of the results to the selection of the trichotomous variable to represent the marginal tax rate is considered in Section 4.5.

4.3.3 Investment-related Tax Shields

Scholes and Wolfson (1989a) predict that maturing firms gradually prevented from changing their capital structures by re-financing costs will increasingly resort to other tax-sheltering activities, such as adjusting their asset structures. This motivated the second thesis hypothesis that the inverse cross-sectional relation between leverage and investment-related tax shields modeled by DeAngelo and Masulis (1980) will become more negative as firms age. Accordingly, the empirical tests in this chapter will attempt to provide additional evidence that investment decisions are not independent of financing decisions. However, in a departure from extant empirical research which specify cross-sectional tests of DeAngelo and Masulis's substitution hypothesis, these tests will evaluate the time-series variation in investment-related tax shields.[30]

Until recent studies by MacKie-Mason (1990), Dhaliwal et al (1992), and Trezevant (1992), empirical support for the tax substitution effect has been scarce. For example, Bradley et al (1984) detect a significant positive association between debt and investment-related tax shields that they interpret as being evidence contradicting the tax shield substitution hypothesis. However, it appears that the predicted negative relation may have been obscured in early research by two important influences that were not properly controlled: the debt securability effect and the tax exhaustion effect.

DeAngelo and Masulis (1980) rely on accounting depreciation, depletion allowances, and investment tax credits to represent tax shield substitutes for debt in their model.[31] However,

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[30] Some examples of cross-sectional studies that have used investment-related tax shields when examining the tax substitution hypothesis include Bradley et al (1984), Kim and Sorensen (1986), Titman and Wessels (1988), Chaplinsky and Niehaus (1990), MacKie-Mason (1990), Kale et al (1991), Dhaliwal et al (1992), Trezevant (1992), Barclay et al (1995), Graham (1996a), and Cloyd et al (1997).

[31] DeAngelo and Masulis (1980) model interest as the tax shield that is used to obtain a value-maximizing

these tax deductions and credits are generated through the acquisition of capital assets that are frequently provided as loan collateral. Scott (1976) and others suggest that firms with substantial collateral may borrow at lower rates and reduce credit rationing by supplying security for these loans. This implies that this debt securability effect may interfere with the observation of the tax shield substitution effect.[32]

MacKie-Mason (1990) studies the incremental financing choices of firms and finds that the substitution effect applies to those with a substantial probability of tax shield loss. A firm in this position is described as being tax exhausted; i.e., its tax shields exceed its taxable income such that a portion of its available deductions and credits are carried forward as net operating losses or as a tax credit carryforward. The present value of tax shields is reduced when they can only be used in subsequent years, if ever. Therefore, the sensitivity of the predicted negative relation between debt and investment-related tax shields will depend on firms' proximity to tax exhaustion. For example, firms that are not nearing tax exhaustion would not be induced to substitute since they have sufficient operating earnings to cover all of their tax shields.

The data sample used in this thesis is partitioned according to the firms' probabilities of losing the deductibility of tax shields to facilitate the isolation of the tax substitution effect. This is accomplished by examining whether the extent to which firms substitute debt tax shields for non-debt tax shields depends upon their marginal tax rates. This research design integrates both tax-exhausted and non-tax-exhausted firms in a single regression equation that slightly modifies the specifications in Dhaliwal et al (1992) and Trezevant (1992). The following section of the main empirical model controls for both the debt securability effect and the tax exhaustion hypothesis (full equations for the cross-sectional and panel data tests are presented later):

$$\text{Leverage} = \alpha_0 + \alpha_1 * D + \beta_1 * IRTS + \beta_2 * D * IRTS \qquad (4.1)$$

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equilibrium of tax shields under the assumption that the non-tax costs of adjusting debt are less than the non-tax costs of adjusting other tax-deductible activities; i.e., debt is adjusted in response to changes to investment-related tax shields. However, evidence supporting the second hypothesis in this thesis would contradict DeAngelo and Masulis's exogenous specification of the investment decision since this would be consistent with firms altering investment-related tax shields when re-financing costs impede capital structure changes. It certainly could be argued that the choice of tax shields – whether debt or non-debt - is jointly determined. The research design in this thesis does not address this potential simultaneity.

[32] Studies detecting evidence consistent with the debt securability effect include Bradley et al (1984), Titman and Wessels (1988), MacKie-Mason (1990), Dhaliwal et al (1992), Trezevant (1992), and Graham (1996a).

where: D = 1 for firms with the highest probability of losing the immediate deductibility of debt tax shields unless investment-related tax shields are adjusted; i.e., tax-exhausted firms;

D = 0 otherwise; and

IRTS = investment-related tax shield measured as the sum of depreciation (including depletion) and grossed-up investment tax credits, scaled by firm market value (the sum of the book value of debt and the market value of equity).[33]

β_1 is predicted to be positive representing the debt securability effect, which is assumed to be constant across firms irrespective of their tax status. This control of the debt securability effect enables the tax shield substitution effect to be isolated in β_2, which is predicted to be negative following DeAngelo and Masulis (1980). Dhaliwal et al (1992) explain that it is important to empirically validate the assumption that the debt securability effect is common to all firms. For the regressions reported in Section 4.4, tests indicate that there is no statistical difference in β_1 for firms partitioned according to their tax status.

In this thesis, tax-exhausted firms are specified as those that have been assigned a zero marginal tax rate according to the trichotomous variable conditions. These firms have non-positive taxable income in the year and report a tax loss carryforward (TLCF) balance. The sensitivity of the empirical results to this approach to identifying tax-exhausted firms is examined in Section 4.5.

DeAngelo and Masulis (1980) advise researchers attempting to observe the substitution effect to scale all specified tax shields by before-tax earnings.[34] Dhaliwal et al (1992) implement this requirement by deflating both their interest dependent variable and their investment-related tax shield by pre-tax operating earnings in their primary regressions. However, in a sensitivity test, they find that their results are robust to a specification that replaces their dependent variable with

[33] Investment tax credits are grossed-up by the statutory corporate tax rate since these reduce the tax liability dollar for dollar. Bradley et al (1984) use unadjusted investment tax credits, while Dhaliwal et al (1992) and Trezevant (1992) properly gross-up these amounts.

[34] Another assumption in DeAngelo and Masulis (1980) is that firms are subject to a constant tax rate on positive taxable income. Dhaliwal et al (1992) argue that the condition that no progressivity exists in the corporate tax rate is likely satisfied for samples selected from firms that have survived on *Compustat* for several years. These firms should be sufficiently large that any positive income would be taxed at the highest statutory rate.

leverage, which provides support for using debt ratios as the dependent variable in this study. In addition, the empirical tests in this thesis include an operating earnings control variable.

The regression equations presented below include two control variables intended to remove any remaining potentially confounding influences on the tests of the tax-shield substitution hypothesis. First, the contemporaneous industry mean of the dependent variable, after removing the firm being examined, is specified to control for industry-specific effects. According to the model developed by Dammon and Senbet (1988), the tax-shield substitution effect may not persist when firms use different production technologies. This issue is mitigated assuming that the production decision is correlated with the financing decision (as Dammon and Senbet suggest) and firm-specific production technologies are adequately measured by the industry control variable.

Second, the ratio of property, plant, and equipment to total book assets is specified to control for cross-sectional differences in asset structures. However, Harris and Raviv (1991) notice that this may be considered another proxy for debt securability.[35] Dhaliwal et al (1992) explain that this variable may absorb lingering variation relating to the debt securability effect that is not captured by specifying ITCs and depreciation expense as the proxy for investment-related tax shields since:

(i) land is frequently provided as collateral although this asset does not generate any investment-related tax shields; and

(ii) assets such as buildings provide less investment-related tax shielding in their early years than less durable assets such as equipment and vehicles, although assets with longer lives generally provide more debt security.

4.3.4 Other Determinants of Capital Structure

Many theories have been proposed to explain the cross-sectional variation in leverage. Empirical researchers attempting to test these theories have had to estimate regression equations with proxies for unobservable theoretical characteristics. This testing appears to have been

[35] The inclusion of both proxies for committed investments (property, plant and equipment) and debt securability (the scaled sum of depreciation and grossed-up investment tax credits) is essential to avoid coefficient estimates being biased because of the omitted correlated variables problem. In addition, the coefficients on the committed investments variable were not statistically different across the tax rate partitions.

complicated by the difficulty of specifying proxies that sensibly correspond to the occasionally abstract attributes described by theory.

For instance, this thesis attempts to examine whether re-financing costs impede tax-efficient capital structure adjustments. As re-financing costs are obviously unobservable, the tendency of leverage decisions to become insensitive to tax status as firms age would be *interpreted* as evidence supporting the first hypothesis that capital structure is largely an artifact of a firm's history of financing and investment policies.

Notwithstanding these and other econometric problems relating to testing capital structure theories (see Titman and Wessels (1988) for an excellent discussion), the control variables summarized in Table 4.3 are included in the equations to reduce the possibility that the tax variables represent other explanatory effects. The following sections provide details on the extant research motivating the choice and empirical specification of these control variables, and the predicted signs on their coefficients.

Financial Distress Costs

Financial distress costs - whether the direct costs of bankruptcy or indirect costs such as strained relations with suppliers and customers - reduce shareholder wealth. This implies that firms become increasingly reluctant to issue debt since the interest and principal obligations increase the probability of distress.

Graham et al (1998) explain that the *ex post* occurrence of financial distress may obscure the empirical relation between leverage and tax status.[36] For example, a firm experiencing financial distress will likely have suffered a decline in the value of its equity such that a high debt ratio is reported, while its marginal tax rate probably would have been reduced by losses that precipitated

[36] MacKie-Mason (1990) and Graham et al (1998) rely on a measure of *ex ante* expected costs of financial distress that captures both the probability of enduring distress and asset intangibility (to proxy for the portion of firm value that is expected to be lost in liquidation) to control for this potential determinant of capital structure. However, this proxy is not available in this empirical setting since firms initially do not have sufficient operating histories to estimate their earnings variability, the conventional measure of the probability of encountering financial distress. As explained in the next section, the fraction of assets represented by property, plant, and equipment, which measures asset tangibility, is entered as a separate control variable for other reasons.

the distress. This negative relation between leverage and tax rates for distressed firms might confound the observation of the positive influence of taxes on the capital structure decisions of other firms.

In addition to a dummy variable to indicate if the book value of common equity is negative, *ex post* financial condition is controlled with an unlevered version of Altman's (1968) z-score, with lower-scoring firms predicted to have less financial security:

$$\text{ZPROB} = \text{totals assets}/(1.2w_1 + 1.4\,w_2 + 3.3w_3 + w_4) \qquad (4.2)$$

where: w_1 = working capital;
w_2 = retained earnings;
w_3 = earnings before interest and taxes; and
w_4 = sales.

This proxy measures the portion of Altman's z-score not affected by capital structure decisions by excluding $0.6w_5$ (w_5 = market value of equity divided by the book value of total debt) from the denominator to avoid spurious correlation with the dependent variable.

Asset Structure

Many theories suggest that capital structure is affected by the nature of a firm's assets. Myers and Majluf (1984) develop a model in which management has superior information concerning both the firm's assets-in-place and its investment opportunities. As management is assumed to be acting in the interests of existing shareholders, they will exploit any opportunity to issue securities at a higher price than what they are worth. However, potential investors, who surmise that management has better information about the firm's prospects, are protected. They react to the announcement of the issue of risky securities by adjusting their valuation of the firm to reflect the new information; i.e., existing shareholders ultimately bear the cost of the information asymmetry. Therefore, firms with assets that can be provided as collateral may be expected to issue more debt to benefit from this opportunity to reduce information costs.[37]

[37] There are some difficulties with applying Myers and Majluf (1984) in this empirical setting. First, their model relates more to the security issue decision than to continuing capital structure choices. For instance, the leverage specification in this thesis includes short-term debt, which, as Myers and Majluf explain, may reduce or even eliminate the information problems in their model. Second, their results rely on the assumption that management's objective is to maximize the wealth of current shareholders. Dybvig and Zender (1991) demonstrate that these results no longer obtain if shareholders could select a compensation policy for management such that *ex ante* firm value is maximized. Myers and Majluf's specification of management's objectives is particularly troubling considering that Jung et al (1996) provide strong empirical evidence that

Similarly, the contributions of Jensen and Meckling (1976) and Myers (1977) suggest that a positive relation should be observed between collateralizable assets and leverage. They contend that shareholders of leveraged firms have an incentive to invest sub-optimally to expropriate wealth from the firm's bondholders. Understandably, the bondholders will anticipate that the shareholders will exploit this opportunity when it arises. The provision of assets to secure debt may enable the firm to reduce these agency costs by committing the firm to use the loan proceeds for specified projects.

Assets-in-place should also retain more value in liquidation. Harris and Raviv (1990) argue that firms with low liquidation costs should have high leverage because for these firms the probability is relatively high that liquidation is more efficient than continuing current operations.

The predicted positive influence on leverage of the capacity to provide collateral will be controlled with property, plant, and equipment scaled by lagged total assets. The market-to-book value of equity is included to proxy for growth options (i.e., relative access to positive net present value projects) that are hypothesized to be negatively associated with leverage.

Convertible Securities

Nance et al (1993) suggest that financing with convertible debt and convertible preferred shares may be alternative strategies for reducing agency costs as these securities enable their holders to participate in risky projects. This influence on capital structure is controlled with the ratio of the sum of the book values of convertible debt and convertible preferred shares to the lagged market value of the firm. This proxy is predicted to be positively correlated with debt usage.

Free Cash Flow and Operating Earnings

Jensen (1986) identifies another agency problem that may encourage firms to have more debt in their capital structures. He argues that managers sometimes pursue their own objectives, such as firm expansion when good investment projects are not available, that undermine value maximization. Leverage limits management's discretion by reducing their control over the firm's

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agency cost models better explain capital structure decisions than the other prevailing theories. Of course, many agency models assume that management pursues its own objectives, rather than firm value maximization.

cash flows since a portion of these amounts must be spent on servicing these loans and by subjecting management to monitoring by creditors who are eager to protect their investment.

Firms with "free" cash flows may prefer debt financing as the interest and repayment commitment reduces the amount of discretionary cash available to managers. In addition, this increases management's fractional ownership of the residual claim on the firm. Jensen (1986) predicts that the market for corporate control will induce firms to increase leverage such that they are compelled to distribute cash generated by improved operating performance to investors. Ross (1977) models capital structure as a signal of private inside information with investors perceiving higher leverage as an indication of higher quality since mimicking is prevented by lower types enduring higher marginal expected bankruptcy costs. This theory predicts a positive correlation between firm profitability and leverage.

Operating earnings before depreciation, interest, and taxes deflated by lagged firm market value is specified to control for these potential determinants of capital structure.[38] Further, it is essential to include an operating earnings proxy to coarsely comply with the condition in DeAngelo and Masulis' (1980) third hypothesis, which predicts tax shield substitution, that earnings be held constant.

Firm Size

Extant empirical research has frequently found that capital structure is related to firm size. Ang et al (1982) and Warner (1977) provide evidence that direct bankruptcy costs are proportionally larger in small firms. Further, smaller firms appear to be more susceptible to bankruptcy since they tend to be less diversified. Graham (1996a) documents a positive association between firm size and marginal tax rates which implies that large firms should be highly leveraged to obtain the more generous tax subsidy available on interest deductions. Firm size may also be a proxy for the degree of asymmetric information since large firms are more closely followed by analysts and have more stringent reporting obligations.

[38] The empirical results reported later in this chapter were robust to re-specifying this variable with a version of Auerbach's (1985) cashflow deficit measure. This cashflow deficit measure was calculated as: (capital expenditures + average dividends - (cashflow + capital expenditures x (total debt/net assets))) / net sales; average dividends were determined over the lesser of three years and the years elapsed since initial public offering.

These arguments suggest that leverage is an increasing function of firm size. It may be particularly important to control for size effects in this thesis since it is likely that firm size will be increasing considerably as firms age.[39] Net sales scaled by firm market value is used to control for the predicted positive influence of firm size on debt policy.[40]

Firm Age

Asset substitution and moral hazard problems are moderated in Diamond (1989, 1991a) by a firm relying on its reputation for investing in projects that ensure debt repayment. The firm's reputation improves the longer its history of not defaulting, which reduces its borrowing costs and credit rationing. Diamond's theory of reputation formation in debt markets predicts that older firms will have lower default rates and lower interest costs, which implies that leverage is increasing in firm age.[41]

Industry Membership

Titman (1984) provides a model in which customers, suppliers, and workers of firms that produce unique or specialized products are subjected to relatively high costs in the event of firm liquidation. Their customers may encounter difficulty locating alternative servicing for the firms' products, while suppliers and workers may have acquired firm-specific capital. This implies that firms in such industries would be induced to maintain relatively low debt levels to decrease the probability of bankruptcy.

In addition to models that borrow features from the industrial organization literature to develop capital structure theories concerning product market strategy (e.g., Brander and Lewis, 1986) and

[39] Sample firms in this study are required to operate as independent companies for at least nine full years following their initial public offerings. These survivors are almost certainly the more successful firms of the IPO pool such that their annual growth rates may be relatively high. For example, Jain and Kini (1994) report that firms in their early post-IPO years have high growth rates in sales and capital expenditures relative to other firms in the same industry.

[40] Alternatively, the natural logarithm of sales could have been specified since size is expected to provide diminishing returns on each of the arguments described. However, the regression results reported in Section 4.4 were almost identical when this proxy was used to control for firm size.

[41] It is difficult to unambiguously predict that leverage and firm age will be positively correlated since, as Peterson and Rajan (1994) explain, a firm's debt is jointly determined by its demand for credit and the supply of credit from lenders. The tests in this thesis cannot distinguish whether creditors ration older firms or that these firms simply have less demand for loans.

characteristics of products or inputs (e.g., Titman, 1984), there are other potential explanations for observed inter-industry variations in leverage. For example, industry-specific influences such as the extent of regulation (Stulz, 1990) and asymmetric information (Myers and Majluf, 1984) and the tendency of firms to generate "free" cash flows (Jensen, 1989) may affect financing decisions. Empirically, Lev (1969), Bowen et al (1982), Marsh (1982), Bradley et al (1984), Long and Malitz (1985), Titman and Wessels (1988), and Garvey and Hanka (1999) find industry differences in leverage.

The primary industry control in this study is the contemporaneous industry mean of the dependent variable, where industry mean represents the same-year mean among firms, other than the firm being examined, in the narrowest SIC code that includes at least five firms.[42] This design should provide more precise and efficient estimation of industry-specific effects relative to the common practice in the empirical capital structure literature of specifying industry dummies.[43]

Contemporaneous Macroeconomic Effects

Table 4.1 reports on the erratic pattern in the number of IPOs occurring in the 1977 to 1988 period of this thesis that suggests that macroeconomic conditions affect a very important capital structure decision: i.e., the decision to become a public company. Obviously, continuing capital structure choices may also be influenced by prevailing conditions.

For example, the Tax Reform Act of 1986 (TRA 86) may have induced firms to increase leverage by altering corporate and personal tax incentives. Although the top statutory corporate tax rate was reduced to 34 percent by 1988, this rate now exceeded the top personal rate of 28 percent. In addition, the capital gains rate for individuals was increased considerably and the generous depreciation deductions afforded firms were largely repealed. These developments might have

[42] Frequently, *Compustat* data was available for fewer than eight firms in a four-digit SIC code such that industry medians could not be reliably used as the measure of central tendency. The firm under study is excluded from the calculation of the industry-mean leverage to ensure that correlation with the dependent variable is not mechanically induced.

[43] For example, specifying two-digit SIC code dummies in this study would probably adequately partition industry effects, but this would also consume 38 degrees of freedom in estimating each year-to-year regression, which is costly since the sample is comprised of 189 firms. Alternatively, specifying one-digit SIC code dummies would sacrifice precision in favor of preserving power.

contributed to the tendency of firms to aggressively replace equity with debt during the period surrounding TRA 86 (see Scholes and Wolfson (1989b) for aggregate statistics).

Further, it is important to mention that potentially significant non-tax influences on corporate financing behavior happened around the same time, including the proliferation of new securities such as strip financing. As such, IPO-year dummies are included in an effort to improve the overall specification of the regression equations, rather than to speculate on the conditions that they may capture.[44]

The predicted signs on the coefficients are summarized in the second column of Table 4.5.

4.3.5 Descriptive Statistics on the Regression Variables

The descriptive statistics reported in Table 4.4 indicate that there is substantial variation across all variables. Although there is evidence of slight collinearity across the tax variables in both the year-to-year and the pooled regressions, the generally low cross-correlations provide support that other determinants of the financing decision do not explain the observed tax effects. However, to address potential multicollinearity concerns, explanatory variables with absolute cross-correlations with the tax proxies exceeding 0.15 were removed from the equations one at a time. The regression results presented below were not qualitatively affected by any of these re-specifications.

[44] Notice that the potential influence of macroeconomic conditions is partially controlled in this thesis by the manner in which the data set is aligned. Firms are observed as they age starting with the year that they went public, rather than across calendar years. This provides some dispersion of the fixed time effects across the IPO years. Still, this only imperfectly suppresses the impact of prevailing conditions since the sample firms are not equally distributed across the IPO years; e.g., only three firms in the sample underwent an IPO in 1978, while 47 firms had their IPO in 1983. For instance, incentives arising from the Tax Reform Act of 1986 for firms to increase their leverage are not completely controlled with this design. However, any persisting influence of TRA 86 would likely oppose the detection of evidence supporting Scholes and Wolfson's (1989a) prediction that firms' capital structures become less responsive with age to changes in their tax status. As most firms in the sample (Table 4.1) are followed in years surrounding the enactment of TRA 86, the larger available debt tax shield – after considering both personal and corporate taxes (Miller, 1977) – post-1986 would moderate the impact of re-financing costs that Scholes and Wolfson argue gradually rise with firm age. Potential implications of TRA 86 on the tests of the time-series variation in investment-related tax shields are examined later in this chapter.

4.4 Empirical Tests of the Time-series Variation in Tax-induced Financing and Investment Decisions

This section continues to develop the research design applied in this thesis to examine the time-series variation in firms' reliance on debt and investment-related tax shields. The empirical tests of hypotheses H_1 and H_2 presented in this section are intended to provide evidence on Scholes and Wolfson's (1989a) predictions concerning the influence of re-financing costs on the evolution in tax-induced financing and investment decisions.

Chapter 3 reports strong, robust evidence that firms' capital structures gradually become more constrained with age. Although the time-series pattern in firms' reversion to their leverage targets supports Scholes and Wolfson's (1989a) prediction that re-financing costs accumulate over time, these tests were complicated by difficulty specifying adequate proxies for firm-specific optimal capital structure.

The empirical tests in this section avoid these complications by evaluating firms' adjustments to their capital and asset structures in response to re-financing costs that are (potentially) increasing in firm age. This research implicitly relates to the efficient design of organizations since the influence of dynamic re-financing costs can be deduced from the time-series variation in firms' reactions to the static subsidies available on debt and investment tax shields.

The empirical testing of H_1 and H_2 begins with year-to-year and pooled OLS estimation to provide evidence that is comparable to the existing cross-sectional literature on capital structure. However, the panel data tests, which control for unobserved firm-specific effects to avoid omitted variable bias and to refine the observation of within-firm dynamics, that follow represent the primary evidence on the thesis hypotheses.

4.4.1 Cross-sectional Estimation

The cross-sectional predictions about firms' debt and investment-related tax shields and those contributed by other theories of capital structure are examined in the following equation:

$$L_{it} = \alpha + \beta X_{it} + \varepsilon_{it} \qquad (4.3)$$

where L_{it} is the debt ratio of firm i at time t, α is the intercept that is common to all firms, and X_{it} is a vector of explanatory variables. The regression variables, which are described in detail in Section 4.3, are:

$LEVERAGE_{it}$	=	Leverage is the book value of total long-term debt, convertible debt, and short-term debt deflated by one-year lagged firm market value (the sum of market value of equity and book value of total debt).
D_{it}	=	Tax-exhausted firm dummy variable that indicates firms that have been assigned a zero marginal tax rate according to the trichotomous variable conditions (see below).
$MTR_{i,t-1}$	=	The marginal tax rate is a trichotomous variable which is equal to: (i) the top statutory corporate tax rate if taxable income is positive and the tax loss carryforwards (TLCF) balance is non-positive; (ii) one-half the top statutory rate if either taxable income is positive or TLCF is non-positive; and (iii) zero if taxable income is non-positive and TLCF is positive.
$IRTS_{it}$	=	The investment-related tax shield variable is the product of the debt securability proxy (see below) and the tax-exhausted firm indicator variable.
$SECURITY_{it}$	=	The debt securability proxy is the sum of financial statement depreciation and investment tax credits (missing observations are replaced by zeros) grossed-up by the statutory corporate tax rate. This amount is scaled by firm market value, which is defined as the sum of the book value of total debt and the market value of equity.
$Z\text{-}SCORE_{it}$	=	Altman's z-score is book total assets divided by the sum of 1.2 times working capital, 1.4 times retained earnings, 3.3 times earnings before interest and taxes, and net sales.
$INDUSTRY\text{-}MEAN_{it}$	=	Industry-mean leverage is the contemporaneous industry mean of the dependent variable, where industry-mean represents the mean among other firms in the narrowest SIC code that includes at least five firms other than the firm being examined.
$GROWTH_{it}$	=	Growth options is the market value of common equity divided by the book value of common equity.
$CONVERTIBLES_{it}$	=	Convertible securities is the sum of the book values of convertible debt and convertible preferred shares; this amount is deflated by one-year lagged firm market value.
$EARNINGS_{it}$	=	Operating earnings is earnings before depreciation, interest, and taxes deflated by one-year lagged firm market value.
$SIZE_{it}$	=	Firm size is net sales deflated by one-year lagged firm market value.
$NEG.EQUITY_{it}$	=	The negative book equity dummy indicates if the book value of common equity is negative.
$ASSETS_{it}$	=	Asset structure is total property, plant and equipment scaled by one-year lagged total assets.
AGE_{it}	=	Firm age is the number of years that have elapsed since the firm's initial public offering.

Coefficients for the explanatory variables are estimated using separate linear regressions for firms' first through ninth years following their initial public offerings. The complete specification of the cross-sectional equation is also reproduced in the top panel of Table 4.5.

These tests are initially estimated using OLS under the assumption that the error term in equation (4.3) is independent of X_{it} to generate results that are directly comparable to extant cross-sectional research. The significance of the results for the year-to-year regressions reported in Table 4.5 are calculated with standard errors obtained using White's (1980) heteroscedasticity-consistent covariance matrix. These results appear to support H_1 since the magnitude and significance of the MTR coefficients gradually subside; i.e., the influence that marginal tax rates have on financing decisions diminishes as firms age. Similarly, the year-to-year estimates appear to indicate that the time-series variation in tax-induced investment decisions is consistent with H_2 that tax shield substitution will increase over time.

Figures 4.1 and 4.2 illustrate the declining patterns that emerge when the MTR and the investment-related tax shield coefficients (and their 90 percent confidence intervals) are graphed against firm maturity. The Pearson correlation with firm age is –0.82 for the estimated MTR coefficients, which is significant at the one-percent level in a one-tailed test assuming independence. However, the Pearson correlation with firm age is –0.41 for the investment-related tax shield coefficients, which is not significant at conventional levels.[45] Although there is evidence to support the prediction about the time-series properties of tax-motivated financing decisions, these tests do not consider the variable estimation error in the coefficients.

This matter is handled by estimating a weighted least squares regression, with the weights determined by the standard errors of the coefficient estimates; i.e., the least weight is assigned to the observations that are measured with the least precision. The weighted least squares regression of the MTR estimates from the nine cross-sectional OLS models on firm age also

[45] The Pearson correlation coefficient measures the closeness of a linear relationship between two variables, while the Spearman correlation coefficient is a measure of the correlation of the ranks of the variables. The Spearman correlation between firm age and the marginal tax rate coefficients estimated in the nine OLS regressions is –0.72, which is significant at the one-percent level in a one-tailed test. The Spearman correlation between firm age and the investment-related tax shield coefficient estimates is –0.32, which is not statistically significant.

provides evidence at the one-percent level that a negative pattern exists (t-statistics are in parentheses):

$$MTR\ coefficient_{t-1} = 0.1575 - 0.0474AGE + e_t \quad (4.4)$$
$$(2.025)\ (-3.275)$$

where: MTR = The one-year lagged marginal tax rate is a trichotomous variable which is equal to: (i) the top statutory corporate tax rate if taxable income is positive and the tax loss carryforwards (TLCF) balance is non-positive; (ii) one-half the top statutory rate if either taxable income is positive or TLCF is non-positive; and (iii) zero if taxable income is non-positive and TLCF is positive.

AGE = Firm age is the number of years elapsed since its IPO.

However, the weighted least squares regression of the investment-related tax shield estimates from the nine cross-sectional models provide no evidence of the negative time-series variation predicted in H_2 (t-statistics are in parentheses):

$$IRTS\ coefficient_t = 0.3986 - 0.1676AGE + e_t \quad (4.5)$$
$$(0.400)\ (-1.029)$$

where: $IRTS$ = The investment-related tax shield variable is the product of the debt securability proxy and the tax-exhausted firm indicator variable. The debt securability proxy is the sum of financial statement depreciation and investment tax credits (missing observations are replaced by zeros) grossed-up by the statutory corporate tax rate. This amount is scaled by firm market value, which is defined as the sum of the book value of total debt and the market value of equity. The tax-exhausted firm dummy variable indicates firms that have been assigned a zero marginal tax rate according to the trichotomous variable conditions.

AGE = Firm age is the number of years elapsed since its IPO.

Similar evidence for both H_1 (significant at the one-percent level) and H_2 (not significant) is found when these weighted least squares regressions are repeated for the marginal tax rate and investment-related tax shield coefficients obtained with other estimation techniques as follows. First, the coefficients were re-estimated to introduce firm heterogeneity using a random coefficients model based on Hildreth and Houck (1968) that attributes parameter heterogeneity to stochastic variation. This estimation, which has been modified to correct for heteroscedasticity,

permits cross-sectional variation in the coefficients, each representing an average of individual firm coefficients for that observation. Second, as 213 of the 1,701 firm-year observations (12.5 percent) in the nine-year panel report no debt in their capital structures, the coefficients were re-estimated with nine cross-sectional tobit regressions to address the dependent variable being left-censored at values of zero.

Besides these year-to-year regressions, equation (4.3) is estimated using ordinary least squares for the observations pooled across the nine-year panel after including the following additional explanatory variables. In this regression, the time-series variation in tax-induced financing and investment decisions is examined by interacting firm age with the proxies for debt and non-debt tax shields:

$AGE*MTR_{i,t-1}$ = This interaction is the product of the one-year lagged marginal tax rate and firm age. The marginal tax rate is a trichotomous variable which is equal to: (i) the top statutory corporate tax rate if taxable income is positive and the tax loss carryforwards (TLCF) balance is non-positive; (ii) one-half the top statutory rate if either taxable income is positive or TLCF is non-positive; and (iii) zero if taxable income is non-positive and TLCF is positive. Firm age is the number of years elapsed since its IPO.

$AGE*IRTS_{it}$ = This interaction is the product of the investment-related tax shield variable and firm age. The investment-related tax shield is the product of the debt securability proxy and the tax-exhausted firm indicator variable. The debt securability proxy is the sum of financial statement depreciation and investment tax credits (missing observations are replaced by zeros) grossed-up by the statutory corporate tax rate. This amount is scaled by firm market value, which is defined as the sum of the book value of total debt and the market value of equity. The tax-exhausted firm dummy variable indicates firms that have been assigned a zero marginal tax rate according to the trichotomous variable conditions. Firm age is the number of years elapsed since its IPO.

The complete specification of the pooled equation is also presented in the top panel of Table 4.6. The stability of the debt and investment-related tax shields in the nine years following firms' initial public offerings is examined in this pooled cross-sectional, time-series OLS regression. However, another purpose of this test is to provide further evidence that is comparable to prior empirical research on the theory of capital structure.

The analysis in this chapter emphasizes the results from tests on the balanced panel, which is compiled by discarding the entire time-series of firms if any missing observations occur in the nine years. However, results are also provided throughout for the unbalanced sample, which is

compiled by discarding only the firm-year that has missing observations. Although the larger unbalanced panel increases statistical efficiency and may be less susceptible to survivorship bias, there is always the concern that evidence from tests using this sample could be spuriously induced by changes in its composition over time.

Table 4.6 reports balanced panel evidence in column (1) consistent with both the first and second thesis hypotheses for the pooled OLS regression, which ranges the firms' first nine years of public operation. The prediction in H_1 that the influence that tax status has on financing decisions becomes less positive as firms age is supported since:

(i) the coefficient on the trichotomous marginal tax rate proxy is positive and statistically significant (p-value under 0.05); and

(ii) the coefficient on the age-MTR interaction is negative and statistically significant (p-value under 0.01).

As explained in Section 4.3.2, the positive relation between firms' leverage and tax status in these cross-sectional tests implies that lagging the marginal tax rate proxy was sufficient to avoid the endogeneity bias that can arise in debt levels tests (see Scholes et al (1990) and Graham et al (1998).

The pooled tests also provide empirical evidence for the second hypothesis since the predicted negative time-series variation in tax-induced investment behavior, which is measured by the age-investment tax shield interaction, is observed (p-value under 0.10). However, it would be premature to interpret the results on H_1 and H_2 and the other potential determinants of capital structure until the following econometric issues are addressed.

For 213 of the 1,701 firm-year observations (12.5 percent), firms report having no debt in their capital structures. The matter of the dependent variable being left-censored at values of zero is considered by re-estimating equation (4.3) with a pooled time-series, cross-sectional tobit regression. Although statistical significance is probably exaggerated by the serial correlation of the error terms, the balanced tobit results reported in column (3) of Table 4.6 support H_1 and H_2 at p-values under 0.001 and 0.10, respectively.

The pooled equation (4.3) is also re-estimated using Generalized Method of Moments (see Hansen, 1982), which produces standard errors that are not affected by serial correlation of successive observations and cross-sectional heteroscedasticity. The OLS assumption of residual independence is plausible cross-sectionally since the regressions include industry-mean leverage and calendar-year variables, which should ensure that influences on a firm's capital structure experienced by other firms are largely confined to these variables.

However, the OLS regression on the levels panel data that probably has time-series autocorrelation and cross-sectional heteroscedasticity will generate unbiased coefficients, but standard errors that are biased downward, thereby artificially increasing statistical significance. The GMM estimator relies on Newey-West's (1987) correction for heteroscedasticity and first-order autocorrelation.

The GMM results on the balanced panel are presented in column (5) of Table 4.6. Although H_1 remains supported with the estimated coefficient negative and statistically significant at the one-percent level, the coefficient on the H_2 interaction is not significant. Finally, Table 4.6 reports OLS, tobit, and GMM results for the unbalanced sample in columns (2), (4), and (6), respectively, that provide consistent evidence at p-values under 0.01 for both H_1 and H_2.

Most pooled regression results for the control variables are consistently statistically significant in the predicted directions according to Table 4.6. An important exception is the estimate for the investment-related tax shield, which is not negatively correlated with leverage in these tests, an issue that is re-examined in Chapter 5.

The only other results inconsistent with theory were the coefficients for proxies for Altman's (1968) z-score and firm size, which were both estimated to be almost zero indicating that each contributed virtually no explanation.[46] The minuscule z-score coefficient is not entirely surprising since Begley et al (1996) find that this bankruptcy predictor performs poorly on out of

[46] The possibility that the precision of the other estimates was adversely affected by the inclusion of superfluous explanatory variables was examined by removing one at a time the variables that were not significant in the pooled regressions reported in Table 4.6; this did not qualitatively matter to these results.

sample data, especially for data from the 1980s which is used extensively in this thesis (see Table 4.1).

Generally, the evidence reported in Table 4.6 is similar to that provided by existing cross-sectional empirical capital structure research; e.g., Titman and Wessels (1988), Rajan and Zingales (1995), Graham (1996a), and Graham et al (1998). However, there remains concern that these results may be artifacts of an omitted variable bias since, for example, the industry-mean debt ratio variable might be a proxy for unobservable firm-specific effects. The fixed effects tests developed in the next section enables the issue of whether the cross-sectional evidence is attributable to mainly between-firm effects or within-firm dynamics to be examined.

4.4.2 Fixed Effects Estimation

Suppose that the unobservable error term in equation (4.3), ε_{it}, is more precisely described as:

$$\varepsilon_{it} = \mu_i + e_{it} \tag{4.6}$$

where μ_i is a firm-specific component and the residual term, e_{it}, is a measurement error or other form of stochastic shock. This would imply that the treatment of the firm-specific effect will depend on the assumptions made about the relationships amongst μ_i, e_{it}, and X_{it}. The simplest assumption is that μ_i, e_{it}, and X_{it} are mutually orthogonal which would enable equation (4.3) to be estimated as a random effects model containing firm-specific heteroscedasticity.

However, it is more plausible to assume that μ_i absorbs differences in, for example, the quality of management, which would suggest that firms that survive are probably higher quality. This would imply that a positive correlation exists between μ_i and X_{it}, although μ_i could remain orthogonal to e_{it}. The concern that firm-specific correlated omitted variables are present in the levels data can be remedied by specifying an intercept for each firm, α_i, to eliminate the time-invariant μ_i such that consistent coefficient estimates are obtained:[47]

[47] This model remains restrictive in its assumptions about the nature of the firm-specific effect. For example, a less constrained representation of ε_{it} in equation (4.6) is:

$$\varepsilon_{it} = \phi_{it} + \mu_i + e_{it}$$

$$L_{it} = \alpha_i + \beta X_{it} + e_{it} \tag{4.7}$$

The time-series pattern observed in the debt and investment-related tax shields for this fixed effects model, which preserves the time-series variation in leverage while accounting for individual firm heterogeneity, are similar to the cross-sectional evidence provided earlier. As reported in column (1) of Table 4.7, both H_1 and H_2 are strongly supported (at p-values under 0.01).

An F-test indicates that the null hypothesis that the constant terms (the fixed firm effects) are all equal is strongly rejected. In the presence of the firm effects, another F-test supports including time-specific parameters that correspond to the reporting year of each observation. These time effects absorb the unique, constant effect each calendar year has on capital structure decisions. Accordingly, the balanced panel results presented in column (1) of Table 4.7 are for a two-way fixed effects model with correction for unspecified heteroscedasticity.

Although a Hausman (1978) test indicates that the fixed effects model is the proper design choice, external validity is impaired by the coefficient estimates only applying to the firms in the sample.[48] This is potentially important since the sample contains at most 5.5 percent of the population of IPOs conducted between 1977 and 1988 (see Table 4.1). Although the selection procedure was certainly not random; e.g., firms were only included if they were immediately followed by *Compustat*, this study is interested in providing evidence to justify inferences about the entire population.

However, as reported in column (3) of Table 4.7, similar balanced panel results consistent with H_1 and H_2 are obtained when equation (4.7) is re-estimated with a random effects model, which assumes that $\{\alpha_i\}$ are drawn from an unknown population. Notwithstanding that the random

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where ϕ_{it} is the portion of the firm-specific effect that varies over time. As exogenous measures of ϕ_{it} are not available, separating this potential effect is beyond the scope of this thesis. However, a fixed time component, γ_t, is included later such that the specification becomes a two-way fixed effects model.

[48] Hausman (1978) relies on the fact that the random effects model is only a valid alternative to the fixed effects model when the firm-specific and time-specific effects are uncorrelated with the explanatory variables. His test statistic exploits the notion that in the absence of correlation both models are consistent, but the fixed effects model is inefficient. In the presence of correlation, the fixed effects model is consistent, but the random effects model is inconsistent since it does not allow for the variable coefficients, $\{\alpha_i\}$.

effects model may suffer from omitted variables bias, this is an important statistical matter since Hsiao (1986) and Griliches and Hausman (1986) explain that observing consistent estimates across alternative panel data estimation techniques indicates the absence of serious errors in variables problems. Also, this fixed and random effects evidence for both H_1 and H_2 is robust to the unbalanced sample according to columns (2) and (4), respectively.

The coefficients on many control variables tend to approach zero or are estimated very imprecisely in the fixed effects regression. However, the theories motivating the inclusion of these potential determinants of capital structure are more amenable to explaining differences in financing across firms. The introduction of fixed effects that remove the cross-sectional variation in the data ensures that these hypotheses are no longer testable. For these cross-sectional predictions, only the results that are reported in Table 4.6 are valid.

4.5 Sensitivity Tests

This section examines the sensitivity of the results reported in Section 4.4, beginning with tests intended to determine whether this evidence was spuriously induced by survivorship bias in the balanced panel data. This section also considers whether the empirical evidence on the time-series variation in firms' reliance on debt and investment-related tax shields persists for an extensive series of re-specifications and other robustness tests.

4.5.1 Selection Correction

The survivorship requirement needed to compile the balanced panel data used in this thesis almost certainly results in the removal of some severely financially distressed firms that became bankrupt, were liquidated, were acquired, or merged with another firm during the course of their first nine years of public existence. These troubled firms probably would be more concerned with financing matters other than tax management relative to the firms retained in the sample, which could bias the tests toward rejecting the null hypothesis that the marginal tax rate-leverage relation is stable across the years following IPO.

For example, the sample probably excludes, at least until the later years of the time-series, distressed firms that are acquisition targets. Zweibel (1996) argues that managers may increase leverage when experiencing threats from the market for corporate control as a defensive maneuver to signal its commitment to value-increasing restructuring.[49]

For investment policy, fixed production capacity can be difficult to reverse when firms become financially distressed (Jensen, 1993). The fixed and random effects models were re-estimated for the unbalanced panel; i.e., firms were permitted to enter and leave the sample during the nine-year period. As reported in columns (2) and (4) in Table 4.7, the results for H_1 and H_2 were robust to this sample, which provides initial evidence that relying on balanced panel data in this setting does not seriously undermine external validity.

The primary research design for the tests in this thesis involves using a balanced panel to ensure that the empirical results are not an artifact of the changing composition of the sample over time. However, considerable attrition occurred when the panel was compiled by discarding observations that do not appear in all years (see Table 4.1). This becomes an important matter since the empirical tests might generate biased estimates from balanced panels if the attrition is related to any endogenous variable. Selection bias arises when other than a random process determines how sampling from the underlying population occurs. This selection process may distort the representation of the true population, which in turn may distort inferences based on the observed data using standard statistical methods.

The panel data examined in this thesis was obtained by conditioning on survival, which suggests that the financial distress proxies (i.e., Altman's (1968) z-score and the negative book equity indicator) are adequate predictors of attrition. In fact, descriptive statistics (not tabulated) indicate that these proxies vary in the expected manner across the balanced and unbalanced panels and within these samples over time. For example, the mean of the negative book equity indicator for the unbalanced panel exceeds that of the balanced panel in all nine years and, for both samples, the respective means for this variable gradually rise with experience.

[49] Empirical support for this signalling mechanism is provided by Safieddine and Titman (1999) who find that large and significant improvements in operating and stock performance follow leverage increases.

This evidence implies that systematic attrition contributed to the composition of the balanced panel since selection cannot be dismissed as a random occurrence, either across firms or over time. In fact, if the selection rule were truly random, then consistent estimates could be obtained with the unbalanced panel, which would also increase efficiency since the incomplete information from the firms that did not survive for nine years would not have to be discarded.

Although testing the hypotheses on an unbalanced panel above coarsely attenuated this issue, the Heckman (1976, 1979) two-step estimation procedure is applied to generate consistent estimates in the presence of attrition. The first step estimates the parameters of a sample composition equation with the dependent variable having the value of one if the observation is retained in the sample (survival) and zero otherwise (attrition). The independent variables are the financial distress proxies, Altman's (1968) z-score and the negative book equity indicator, which are intended to explain attrition. This equation is estimated for the full sample (9,322 firm-year observations) with maximum likelihood probit with the estimates used to determine the conditional expectation of the error term of the regression model given the inclusion of an observation in the sample.

The second step involves estimating through OLS the parameters of the regression model specified in the top panel of Table 4.6 with this conditional expectation entered as an additional explanatory variable, λ_{it} (the inverse Mills ratio). This ensures that consistent estimates are obtained for the (apparently) non-randomly selected balanced panel containing 1,701 firm-year observations.

Essentially, this procedure eliminates the bias that could arise from omitted variables when the sample is not random. The fixed effects regression that models within-firm variation in financing decisions removes any time-invariant sample selection bias; i.e., the correction term for selectivity is absorbed in the firm-specific effect. However, the descriptive statistics for the financial distress proxies imply that there are time-series changes within firms in the probability of surviving through the nine years.[50]

[50] Applying selection correction in the cross-sectional tests eliminates the potential bias from relying on a non-random balanced panel, but ignores the firm-specific effects. Hausman and Wise (1979) extend the Heckman (1976, 1979) two-step estimation procedure to a fixed effects setting. However, programming sample selection in a fixed effects model is complicated by the manner in which the selection mechanism enters the regression in

The results for the balanced panel corrected for potential sample selection bias using the standard Heckman (1976, 1979) methodology that are reported in column (5) of Table 4.7 provide evidence consistent with H_1 and H_2. Virtually identical evidence supporting H_1 and H_2 is obtained with a tobit model with sample selection. Censorship bias remains a concern since 213 of the 1,701 firm-year observations (12.5 percent) have no debt in their capital structures.

4.5.2 Other Robustness Tests

Leverage Specification

It is prudent to examine whether the fixed effects results, which provide evidence that the time-series variation in firms' financing and investment decisions is consistent with H_1 and H_2, persist for other dependent variable definitions. The numerator of the dependent variable was re-specified from the sum of convertible, long-term and short-term debt to: (i) the sum of long-term and short-term debt; and (ii) long-term only. These leverage specifications produced evidence virtually identical to the results reported in Table 4.7.

In addition, the results were not sensitive to scaling the dependent variable with the lagged book value of equity or total assets, rather than lagged firm market value. Scaling debt by book equity or total assets is justified by the practice of many rating agencies and corporate treasurers to rely on book leverage; e.g., bond covenants apply to book, not market, values. Also, using market equity in the denominator of the debt ratio is liable to impound price variances arising from extraneous economic conditions, which implies that book equity may provide better isolation of deliberate financing decisions.

Censorship Bias

Although 12.5 percent of the 1,701 firm-year observations have no debt in their capital structures, the results reported in Table 4.7 were obtained from regressions that ignore the potential censorship bias that accompanies treating leverage as a continuous variable. As estimation

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a panel. Dr. William H. Greene, who developed the *LIMDEP* econometric software that has been used for most of the panel data tests in this thesis, plans to improve his software to accommodate selection correction in a fixed effects model. Dr. Greene has generously offered to provide this programming, which he expects to finish by the fall of 2000.

becomes complicated when the fixed effects and random effects models are extended to censored panel data (Maddala, 1987), robustness is instead evaluated by simply discarding observations with zero values for the dependent variable. In addition, to address the unbalancing of the panel that occurs when some firm-year observations are discarded, the tests were re-run on only firms that reported positive leverage in each of their first nine public years. Both samples provide evidence consistent with H_1 and H_2 in both random effects and fixed effects regressions.

Time-series Variation in Firms' Dependence on External Financing

Rajan and Zingales (1998) and Helwege and Liang (1996) report descriptive statistics consistent with the notion that there is a life cycle in the pattern of corporate financing, with firms more dependent on external capital early in their existence to finance their lucrative initial investment projects. This implies that the empirical evidence in this thesis might be spuriously induced by firms' demand for external financing subsiding with age, rather than re-financing costs impeding the satiation of this demand.

For example, a simple explanation for the tendency of firms to gradually reduce adjustments to their capital structures is that they are approaching equilibrium; i.e., as investment needs stabilize, so should their capital structures.[51] Scaling the dependent variable (and most of the independent variables) in the primary tests by lagged firm market value, which includes their growth options, mitigates this possibility. Accordingly, this provides some inherent control that the results for the tax variables that are reported in Tables 4.6 and 4.7 are not attributable to firms' investment opportunity sets changing over time.[52]

However, this matter was further examined by partitioning the sample according to whether a firm was above or below the median value of a proxy for dependence on external finance for the pooled sample.[53] This proxy, which is defined as capital expenditures minus cashflow from

[51] However, the evidence reported in Chapter 3 suggests that firms gradually become more constrained by re-financing costs from adjusting toward their optimal capital structures.

[52] Although one-year lagging is necessary to address potential endogeneity between the leverage dependent variable and several explanatory variables in this study, the delay certainly undermines controlling for recent changes in firms' growth options.

[53] This proxy, which is computed by aggregating over firms' first nine years of public operation, should smooth temporal fluctuations and reduce the influence of outliers.

operations divided by capital expenditures (Rajan and Zingales, 1998), is intended to measure the amount of desired investment that cannot be financed through internal resources. H_1 and H_2 were supported in both sub-samples.[54] Also, the original sample was restricted to firms for which the proxy for dependence on external finance was within the 10th and 90th percentiles; results were virtually identical with this data set.

Specification of Marginal Tax Rate

Several complications surface from specifying the trichotomous variable to represent the firms' marginal tax rates. Kinney and Swanson (1993) investigate the accuracy of *Compustat* tax data. They conclude that the tax loss carryforward (TLCF) data field has both the highest error of omission rate of all tax data items at 8.3 percent and the highest error in the reported amount rate at 3.4 percent.[55]

As the trichotomous MTR variable relies partially on the TLCF amounts compiled by *Compustat*, the relative inaccuracy of the TLCF data field affects the validity of this proxy. In the primary thesis tests, firms with missing TLCF observations are discarded, while observations with an error in the reported amount of TLCF are retained since they cannot be identified without examining the financial statements of the sample firms.

An alternative approach is to replace missing TLCF observations with zeros under the assumption that firms that do not report these amounts do not have any tax loss carryforwards (e.g., Graham, 1996b; and Klassen, 1997). Table 4.8 provides evidence that both H_1 and H_2 remain supported at *p*-values under 0.01 for both the fixed and random effects models for both the balanced and unbalanced panels with this re-specification.

[54] Both H_1 and H_2 remain supported when a version of Auerbach's (1985) cashflow deficit measure is substituted as the proxy for dependence on external financing. This proxy is calculated as: (capital expenditures + average dividends - (cashflow + capital expenditures x (total debt/net assets))) / net sales. Average dividends are determined over the lesser of three years and the years elapsed since initial public offering.

[55] An error of omission is defined as an instance in which *Compustat* incorrectly indicates that the data item is not disclosed in the financial statements. An error in the reported amount occurs when *Compustat* reports an amount that differs from the financial statements.

In addition, replacing missing TLCF observations with zeros in the trichotomous MTR variable increases the balanced panel from 189 to 335 firms, which provides additional evidence that the results presented in Section 4.4 are not attributable to survivorship bias. The potential impact of sample attrition on the coefficients is directly evaluated in column (5) in Table 4.8 that reports tobit estimation results with selection-correction. This cross-sectional evidence, which also addresses censoring in the dependent variable, supports the thesis hypotheses at *p*-values under 0.01.

Although the trichotomous variable performs well in Graham's (1996b) and Plesko's (1999) evaluations of conventional tax rate proxies, there appear to be serious limitations with using this measure in this empirical setting.[56] Shevlin (1990) explains that the trichotomous MTR proxy might be particularly imprecise when a firm faces a high probability of future tax losses.

For example, consider a firm that has positive taxable income in the current year and no TLCF balance such that a high MTR is assigned according to the trichotomous criteria. In fact, this firm's current MTR may actually be low if it has a high probability of future tax losses, thereby contradicting the trichotomous MTR assumptions. Shevlin (1990) comments that this concern is more important for firms operating in volatile industries (sample industry distribution is reported in Table 4.2) and *young* firms with erratic earnings.

Further, the tendency of firms to practice earnings management surrounding their IPO dates (Friedlan, 1994; and Teoh et al, 1998) may exacerbate the misclassification of MTRs using the trichotomous conditions. Firms may inflate earnings (thereby generating taxable income) and report no TLCFs (since they have not had much of an opportunity to establish an earnings history) such that they are assigned a high MTR. This might turn out to be very inappropriate considering the ensuing earnings volatility and reversal of earnings management.

[56] Although Shevlin (1999b) criticizes his methodology, Plesko (1999) finds that the trichotomous variable usually performs better than both Graham's (1996a) before-financing MTR and Graham et al's (1998) after-financing MTR in estimating the firm-specific MTR derived from corporate tax return data. In his evaluation of ten proxies, Graham (1996b) provides evidence that the accuracy of the trichotomous variable is second to Graham's (1996a) simulated rates in predicting the perfect foresight marginal tax rate. Equation (4.3) was re-estimated in first-differences with the one-year lagged Graham's (1996a) simulated rates representing firms' marginal tax rates. The evidence consistent with H_1 and H_2 reported in Section 4.4 is robust to this changes specification.

These problems should not as adversely affect Graham et al's (1998) after-financing simulated marginal tax rates, which have been specifically designed to examine debt levels. Replacing the trichotomous variable with the simulated tax rates would prevent observing firms until their fourth public year since Graham et al's (1998) algorithm requires at least three years of historical data.

However, this research design may be justified since the financing decisions made in the three years immediately following an IPO may relate more to the transition to becoming a public company than to continuing capital structure choices.[57] For example, Pagano et al (1998) provide evidence that equilibrating movements in leverage related to the initial public offering decision persist for three years.

The negative time-series variation in tax-induced financing and investment decisions predicted in H_1 and H_2 is also detected when Graham et al's (1998) simulated rates are substituted as the marginal tax rate proxy. The investment-related tax shield is re-specified in these tests to apply to tax-exhausted firms that are identified as those that have a one-year lagged simulated marginal tax rate below the 25th percentile of the distribution of all observations for firms of the same age. Lagging is appropriate since Graham et al's (1998) algorithm corrects the endogeneity relating to financing decisions, but remains affected by contemporaneous investment decisions.

Table 4.9 reports results across estimation techniques for tests with Graham et al's (1998) simulated rates replacing the trichotomous marginal tax rate, which increases the yearly sample from 189 to 438 firms. In columns (1) and (2), the two-way fixed effects models for the balanced and unbalanced panels, respectively, provide evidence consistent with H_1 and H_2. Although a Hausman (1978) test again implies that this is the proper design, external validity is sacrificed by the coefficient estimates from the fixed effects models only applying to the firms in these samples.

However, as reported in columns (3) and (4) in Table 4.9, similar results supporting H_1 and H_2 are found for both the balanced and unbalanced panels when the equation is re-estimated with a

[57] Jain and Kini (1994) provide evidence that a significant decline in firms' operating performance occurs in the three years following IPO, which implies that pre-IPO earnings reports cannot be reliably used as the seed data for the Graham (1996a) and Graham et al (1998) simulated marginal tax rate algorithms.

random effects model. This continues to be an important issue since this sample still only contains 12.7 percent of the population of IPOs conducted between 1977 to 1988.

Finally, the cross-sectional evidence for the balanced and unbalanced samples appearing in columns (5) and (6) in Table 4.9 is consistent with the thesis predictions. The tobit estimation on the balanced panel also includes correction for sample selection.

Specification of Investment-related Tax Shield

In the tests presented in Section 4.4, the firm's investment-related tax shield was measured as the sum of its depreciation (including depletion) and grossed-up investment tax credits, scaled by firm market value (the sum of the book value of debt and the market value of equity). However, there are reasons to suspect substantial discrepancies between the depreciation and ITC amounts reported in the financial statements and those reported on tax returns. Generally, accelerated depreciation is claimed for tax purposes and straight-line depreciation is expensed in the financial statements. Investment tax credits can be claimed immediately for tax purposes, but might be deferred and amortized over the useful life of the asset for accounting purposes.

Accordingly, the investment-related tax shield was re-measured as the sum of the depreciation expense and ITCs reported in the firms' financial statements, plus or minus the grossed-up increase or decrease in deferred taxes.[58] This specification assumes that all book-tax timing differences between financial accounting income and taxable income arise from differences in the calculation of depreciation and investment tax credits. Although this amounts to replacing one noisy proxy of the investment-related tax shield with another, the empirical results with this re-specification are qualitatively similar to those reported in Section 4.4.

The results are also robust to re-specifying the investment-related tax shield proxy to include rental expense, which could be a substitute for depreciation expense for firms that prefer to lease rather than buy capital assets (Smith and Wakeham, 1985). Finally, the results were virtually identical when the investment-related tax shield was re-specified to exclude ITCs. The simulations performed by Graham and Smith (1999) indicate that ITCs hardly affect the

[58] The change in deferred taxes was collected from the Statement of Changes in Financial Position (replaced by the change in the balance sheet amounts when the SCFP observations are missing).

convexity of the effective corporate tax function, which they interpret as evidence contradicting DeAngelo and Masulis's (1980) argument that the lower present value of unused ITCs should induce convexity.[59]

MacKie-Mason (1990) argues that only firms that are tax-exhausted will substitute tax shields as predicted by DeAngelo and Masulis (1980). In the tests reported in Section 4.4, tax-exhausted firms are identified as those that have been assigned a zero marginal tax rate according to the trichotomous variable conditions; i.e., these firms have non-positive taxable income in the year and a tax loss carryforward (TLCF) balance. However, representing tax status with the discrete trichotomous variable largely prevents examining the sensitivity of the empirical results to the identification of tax-exhausted firms.

Although both Dhaliwal et al (1992) and Trezevant (1992) classify tax-exhausted firms as those in the lower 25 percent of the tax status mass, there is no substantive extant empirical evidence or theory on the proper assignment of firms in the sample according to their probabilities of losing the deductibility of tax shields. Any delineation could be considered arbitrary which implies that the tests should be re-run on several *ad hoc* cut-offs to protect against the possibility that the results depend on the partitioning procedure.

This issue was evaluated by replacing the trichotomous variable with Graham et al's (1998) before-financing simulated marginal tax rates, although this prevents observing firms until their fourth public year. Negative and statistically significant coefficients supporting H_2 were estimated when the investment tax shield was separately re-specified to indicate firms in the bottom 10 percent, 25 percent, and 50 percent of the simulated MTR_{t-1} distribution for firms of

[59] In fact, there are other reasons to exclude ITCs from the investment-related tax shield proxy. First, the Tax Reform Act of 1986 (TRA 86) suspended the accumulation of investment tax credits, although firms were permitted to carry forward any unused amounts for up to 15 years. As most of the firms studied in this thesis are examined in years surrounding this change, it is important to evaluate whether this structural shift is responsible for the evidence on H_2. Second, Scholes et al (1990) explain that firms significantly reduce their tax burdens by investing in tax shelters such as ITCs. Accordingly, tax credit carryforwards may be considered a mixed signal that may indicate a high level of taxable income that the company is attempting to shelter rather than a lack of taxable income. MacKie-Mason (1990) argues that the tax shield substitution modeled by DeAngelo and Masulis (1980) will only be observed in firms experiencing tax exhaustion; i.e., those with low taxable incomes.

the same age.[60] This suggests that the evidence on the time-series variation in tax-induced investment decisions reported in Section 4.4 is robust to alternate procedures for identifying tax-exhausted firms.

Duration of Panel Time-Series

The regression results presented to this point in the thesis involve observing firms until their ninth year of public existence. This research design trades off compiling a sufficiently large balanced panel against having a sufficiently long time-series to test H_1 and H_2. For every year added to the panel, the sample is reduced not only by some firms not surviving another year, but also by entire IPO years being lost. For example, examining firms through their tenth, rather than their ninth, post-issue year would result in the 1988 IPOs being excluded from the sample (see Table 4.1).

It could be argued that following firms for nine years in the reported tests is arbitrary, which suggests that the time-series should be extended to determine whether the results are robust. In unbalanced panel tests, evidence (not tabulated) consistent with H_1 and H_2 is found when firms are examined from their first through their 12th public year and from their first through their 15th public year. The balanced panel could not be sensibly extended to 12 or 15 years because these samples were decimated by excluding IPOs conducted in the later years of the 1977 to 1988 period and, to a lesser extent, attrition from firms that subsequently became bankrupt, were liquidated, were acquired, or merged.

Outlying and Influential Observations

Although the fixed effects should absorb the impact of otherwise influential observations or outliers, the possibility that these dominate the data should be examined. According to the summary statistics reported in Table 4.4, the distribution of the leverage dependent variable has a mean of 0.195 and a standard deviation of 0.212. The primary equation developed in Section 4.4 was re-estimated after removing any firms that had a debt ratio during any of their first nine

[60] Dhaliwal et al (1992) also examine the sensitivity of their evidence by selecting the sample of tax-exhausted firms in separate tests using the bottom 10 percent, 25 percent, and 50 percent of the tax rate distribution.

public years that was not within three standard deviations of the sample mean.[61] Evidence from both the fixed and random effects models is consistent with H_1 and H_2 at p-values under 0.01 when these ten firms are removed from the balanced panel, which suggests that the observed time-series variation in debt and investment tax shields represents a pervasive economic phenomenon.

Other Tests

Further, a series of re-specifications indicate that the results for H_1 and H_2 are not time-period or industry-specific. This evidence on the time-series variation in tax-motivated financing and investment decisions also remains for various diminishing non-linear transformations of firm age and after removing firms that changed their fiscal year-ends during the nine-year panel (to ensure that the tests are synchronized at one-year intervals).[62] Finally, the results are robust to entering an additional control variable to measure the time since firms' last major capital structure change.[63]

4.6 Conclusions

In summary, the evidence presented in this chapter is consistent with Scholes and Wolfson's (1989a) predictions about the evolution in firms' financing and investment decisions in the years following their initial public offerings. Specifically, panel data tests spanning firms' first through ninth public years indicate that the positive relation between firms' leverage and their marginal tax rates subsides with age. In addition, firms are observed to gradually shift toward investment-related tax shields when re-financing costs begin to impede capital structure changes.

[61] The choice of three standard deviations is arbitrary, however, virtually identical evidence is found when influential observations are identified using 2.5 standard deviations from mean leverage.

[62] These non-linear transformations of firm age include the natural logarithm, the log of one plus age, second-order logs, square roots, and reciprocals to provide the data flexibility to choose the shapes of the curves for the marginal effects.

[63] The last major change in capital structure is identified as the last year in which the firm's leverage changed by more than 10 percent. As expected, the time since last major re-capitalization is very highly correlated with firm age such that this variable is omitted from the main tests reported in this thesis.

In fact, the reported sensitivity tests suggest that the choice and specification of the regression variables, survivorship bias, censorship bias, the time-series variation in firms' dependence on external financing, the duration of the panel time-series, or outliers are not responsible for this evidence on H_1 and H_2.

The research design applied in this chapter measures a firm's age as the number of years that have elapsed since its IPO, although firms usually have a private operating history when they go public. This implies that cross-sectional differences in firms' private ages, defined as the number of years between their incorporation and their IPOs, could affect the tests on the time-series pattern in firms' reliance on debt and investment tax shields. In Chapter 5, tests are conducted to determine whether the duration of firms' private histories matter to the evidence on H_1 and H_2 reported in this chapter.

Table 4.1: Sample Selection Summary – Tests of Time-series Variation in Tax-induced Financing and Investment Decisions

Calendar Year	1977	1978	1979	1980	1981	1982	1983	1984	1985	1986	1987	1988	Total
Number of SEC registered IPOs	32	38	62	149	348	122	685	357	355	728	415	167	3,458
Number of firms not followed by *Compustat* since initial public offering	(11)	(20)	(22)	(67)	(125)	(40)	(246)	(129)	(131)	(290)	(150)	(47)	(1,278)
Number of firms from utilities, financial, insurance, and real estate industries	(2)	(3)	(4)	(11)	(26)	(7)	(51)	(24)	(29)	(53)	(27)	(12)	(249)
Number of firms that did not survive through their first nine years of public operation	(12)	(2)	(10)	(27)	(83)	(36)	(178)	(111)	(96)	(166)	(100)	(50)	(871)
Number of firms with missing *Compustat* observations	(7)	(10)	(22)	(35)	(95)	(35)	(163)	(80)	(81)	(181)	(112)	(50)	(871)
Number of firms in the sample	0	3	4	9	19	4	47	13	18	38	26	8	189

Table 4.2: Industry Distribution of Sample – Tests of Time-series Variation in Tax-induced Financing and Investment Decisions

2-Digit SIC Code	Number	Industry Description	Percentage
20	6	Food	3.2%
27	7	Printing and publishing	3.7%
28	8	Chemicals	4.2%
35	29	Industrial and commercial machinery	15.3%
36	24	Electronic and electrical equipment	12.7%
38	8	Measuring, analyzing, and controlling equipment	4.2%
39	5	Miscellaneous manufacturing industries	2.6%
42	6	Freight transportation and warehousing	3.2%
50	7	Wholesale trade – durable goods	3.7%
56	8	Apparel and accessory stores	4.2%
58	5	Restaurants	2.6%
59	5	Miscellaneous retail	2.6%
73	16	Business services	8.5%
87	6	Engineering, accounting, research, management, and related services	3.2%
Sub-total	140		74.1%
	49	Industries with fewer than 5 firms	25.9%
Total	189		100%

Table 4.3: Motivation for Regression Control Variables

Explanatory Variable	Theory	Empirical Evidence	Motivation
Altman' s z-score Negative book equity indicator	Static trade-off theory that firms balance the tax subsidy provided by debt against the costs of financial distress (described in Myers, 1984)	Altman (1968), MacKie-Mason (1990), and Graham (1996a); Graham et al (1998)	The direct and indirect costs of financial distress induce firms to lower leverage to reduce the probability of distress; these proxies for the occurrence of financial distress are specified to avoid obscuring the empirical relation between capital structure and tax status.
Asset structure Growth options	Myers and Majluf (1984), Jensen and Meckling (1976), Myers (1977), and Harris and Raviv (1990)	MacKie-Mason (1990), and Givoly et al (1992)	Assets can be provided as collateral to alleviate asymmetric information and asset substitution problems, which increases debt capacity; assets-in-place should retain more value in liquidation.
Convertible securities	Myers and Majluf (1984), Constantinides and Grundy (1989)	Stein (1992), Nance et al (1993), Helwege and Liang (1996)	Convertible debt and convertible preferred shares might be alternative strategies to reduce agency costs as these securities enable their holders to participate in risky projects.
Operating earnings	Ross (1977), Jensen (1986)	MacKie-Mason (1990), Graham (1996a), and Graham et al (1998)	Leverage limits management's discretion by reducing their control over the firm's cash flows since a portion of these amounts must be spent on servicing these loans and by subjecting management to monitoring by creditors who are eager to protect their investment. Firms with "free" cash flows may prefer debt financing as the interest and repayment commitment reduces the amount of discretionary cash available to managers. Ross (1977) models capital structure as a signal of private inside information with investors perceiving higher leverage as an indication of higher quality - this theory predicts a positive correlation between firm profitability and leverage.
Firm size		Warner (1977) and Ang et al (1982)	Warner (1977) and Ang et al (1982) provide evidence that direct bankruptcy costs are proportionally larger in small firms. Further, smaller firms appear to be more susceptible to bankruptcy since they tend to be less diversified. Firm size may also be a proxy for the degree of asymmetric information since large firms are more closely followed by analysts and have more stringent reporting obligations.

Table 4.3: Motivation for Regression Control Variables (Continued)

Variable	Theory	Empirical Evidence	Motivation
Firm age	Diamond (1989, 1991a)	Peterson and Rajan (1994) and Cloyd et al (1996)	The asset substitution problem identified by Jensen and Meckling (1976) is moderated in Diamond (1989) by a firm relying on its reputation for investing in projects that ensure debt repayment. The firm's reputation improves the longer its history of not defaulting, which reduces its borrowing cost. Diamond's model predicts that older firms will have lower default rates and lower interest costs, which implies that leverage is increasing in firm age. In addition, firm age is specified as a separate explanatory variable to ensure that the coefficient estimates for the interactions used to test H_1 and H_2 are interpretable – firm age is an ingredient in both interactions used in the regressions (see Tables 5 to 7).
Industry membership	Titman (1984), Myers and Majluf (1984), Jensen (1989), and Stulz (1990)	Lev (1969), Bowen et al (1982), Marsh (1982), Bradley et al (1984), Long and Malitz (1985), Titman and Wessels (1988), and Garvey and Hanka (1999)	Titman (1984) provides a model in which customers, suppliers, and workers of firms that produce unique or specialized products are subjected to relatively high costs in the event of firm liquidation. Their customers may encounter difficulty locating alternative servicing for the firms' products, while suppliers and workers may have acquired firm-specific capital. This implies that firms in such industries would be induced to maintain relatively low debt levels to decrease the probability of bankruptcy.
Contemporaneous macroeconomic effects		Scholes and Wolfson (1989b), Givoly et al (1992), Trezevant (1992), Schulman et al (1996), Marsh (1982), Viswanath (1993), and Taggart (1985)	Capital structure decisions may be affected by prevailing economic conditions including tax regime shifts, recent capital market performance, the stage of the business cycle, and the availability of competing securities. Although testing in this study is designed to suppress the influence of transitions in macroeconomic conditions on the time-series pattern of firms' tax-induced financing and investment decisions, calendar-year dummy variables are specified to capture lingering macroeconomic effects.

Note:

This table describes the motivation for the control variables that are included in the regression equations presented in Tables 4.5 to 4.9. The empirical specifications of these explanatory variables are provided in Table 4.4.

Table 4.4: Summary Statistics

Panel A – Continuous variables

Variable	N	Mean	Std Dev	Median	Min	Max
Leverage	1,701	0.195	0.212	0.120	0	0.967
Marginal tax rate	1,701	0.302	0.143	0.340	0	0.480
Investment tax shield	1,701	0.009	0.035	0	0	0.484
Debt securability	1,701	0.053	0.100	0.037	0.002	3.626
Altman's z-score	1,701	-1.153	82.79	0.404	-3263	614
Industry-mean leverage	1,701	0.255	0.117	0.243	0.017	0.764
Growth options	1,701	2.517	4.021	1.932	-76	69
Convertible securities	1,701	0.022	0.111	0	0	3.572
Operating earnings	1,701	0.121	0.147	0.124	-2.908	0.787
Firm size	1,701	1.424	1.550	1.042	0.007	21
Asset structure	1,701	0.525	0.369	0.434	0.027	3.645
Firm age	1,701	5.000	2.583	5.000	1.000	9.000
Age*Marginal tax rate	1,701	1.411	0.949	1.360	0	3.680
Age*Investment tax shield	1,701	0.057	0.251	0	0	4.353

Panel B – Discrete variable

Variable	N	Percent
Negative book equity indicator	19	1.11%

Panel C – Cross-correlations with one-year lagged marginal tax rate in the year-to-year regressions

	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9
Correlation (MTR, Leverage)	-0.039	-0.017	-0.112	-0.169	-0.140	-0.185	-0.269	-0.331	-0.396
Correlation (MTR, Investment tax shield)	-0.374	-0.510	-0.493	-0.406	-0.176	-0.259	-0.408	-0.395	-0.431
Correlation (MTR, Debt securability)	-0.022	-0.135	-0.016	-0.272	-0.261	-0.191	-0.243	-0.295	-0.268
Correlation (MTR, Altman's z-score)	0.115	0.173	0.229	-0.110	0.026	-0.166	0.040	0.057	-0.008
Correlation (MTR, Industry-mean leverage)	0.034	0.008	0.023	0.051	0.029	0.009	-0.043	-0.008	-0.003
Correlation (MTR, Growth options)	-0.105	0.028	-0.051	-0.094	0.049	0.104	0.028	0.060	0.020
Correlation (MTR, Convertible securities)	-0.058	-0.006	0.109	-0.090	-0.064	-0.179	-0.127	-0.199	-0.187
Correlation (MTR, Operating earnings)	0.369	0.556	0.384	0.236	0.070	0.158	0.281	0.211	0.216
Correlation (MTR, Firm size)	0.180	0.112	0.010	-0.102	-0.121	-0.065	-0.076	-0.184	-0.134
Correlation (MTR, Negative book equity)	0.008	-0.119	-0.165	-0.217	0.027	-0.012	-0.014	-0.084	-0.235
Correlation (MTR, Asset structure)	0.034	-0.034	-0.101	-0.145	-0.061	-0.020	-0.154	-0.159	-0.118

Panel D – Cross-correlations with investment-related tax shield in the year-to-year regressions

	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9
Correlation (IRTS, Leverage)	0.180	0.103	0.337	0.181	0.213	0.098	0.061	0.270	0.353
Correlation (IRTS, Marginal tax rate)	-0.374	-0.510	-0.493	-0.406	-0.176	-0.259	-0.408	-0.395	-0.431
Correlation (IRTS, Debt securability)	0.279	0.403	0.020	0.276	0.135	0.582	0.635	0.342	0.695
Correlation (IRTS, Altman's z-score)	0.045	-0.006	-0.246	0.051	-0.047	0.117	-0.015	-0.040	0.065
Correlation (IRTS, Industry-mean leverage)	0.124	0.076	0.089	0.076	0.027	0.087	0.110	0.071	0.032
Correlation (IRTS, Growth options)	-0.116	-0.002	-0.069	-0.034	-0.060	-0.169	-0.062	-0.078	0.008
Correlation (IRTS, Convertible securities)	-0.029	0.011	-0.009	0.035	0.089	-0.002	-0.032	0.252	0.291
Correlation (IRTS, Operating earnings)	-0.265	-0.424	-0.468	-0.547	-0.299	-0.621	-0.260	-0.328	-0.212
Correlation (IRTS, Firm size)	-0.121	0.067	-0.064	-0.071	-0.001	0.012	0.533	0.123	0.226
Correlation (IRTS, Negative book equity)	-0.018	0.005	0.193	0.374	-0.022	0.058	-0.025	0.304	0.191
Correlation (IRTS, Asset structure)	0.166	0.244	0.301	0.322	0.101	0.244	0.283	0.292	0.305

Table 4.4: Summary Statistics (Continued)

Panel E -- Correlations

Variable	Leverage	Marginal Tax Rate	IRTS	Debt Security	Altman's z-score	Industry-Mean Debt	Growth Options	Convert Securities	Operating Earnings	Firm Size	Neg Equity	Asset Structure	Firm Age	Age* MTR	Age* IRTS
Leverage		-0.158	0.177	0.353	0.358	0.247	-0.378	0.349	-0.017	0.178	0.134	0.258	0.060	-0.104	0.178
		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.495)	(0.001)	(0.001)	(0.001)	(0.013)	(0.001)	(0.001)
Marginal Tax Rate	-0.204		-0.427	-0.225	-0.114	0.007	0.157	-0.174	0.205	-0.041	-0.099	-0.084	-0.347	0.323	-0.428
	(0.001)		(0.001)	(0.001)	(0.001)	(0.763)	(0.001)	(0.001)	(0.001)	(0.095)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Investment Tax Shield	0.200	-0.361		0.143	0.083	-0.023	-0.092	0.144	-0.440	-0.110	0.128	0.106	0.065	-0.329	0.999
	(0.001)	(0.001)		(0.001)	(0.001)	(0.346)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.007)	(0.001)	(0.001)
Debt Security	0.130	-0.115	0.235		0.149	0.190	-0.375	-0.006	0.360	0.450	0.015	0.647	0.161	-0.034	0.139
	(0.001)	(0.001)	(0.001)		(0.001)	(0.001)	(0.001)	(0.807)	(0.001)	(0.001)	(0.548)	(0.001)	(0.001)	(0.166)	(0.001)
Altman's z-score	0.015	0.022	-0.005	0.009		-0.030	-0.216	0.214	-0.228	-0.280	0.010	0.033	0.050	-0.080	0.086
	(0.537)	(0.361)	(0.849)	(0.699)		(0.209)	(0.001)	(0.001)	(0.001)	(0.001)	(0.681)	(0.179)	(0.038)	(0.001)	(0.001)
Industry-mean debt	0.247	0.028	0.056	0.109	0.031		-0.161	0.016	0.168	0.134	0.043	0.311	-0.079	-0.040	-0.026
	(0.001)	(0.241)	(0.020)	(0.001)	(0.196)		(0.001)	(0.508)	(0.001)	(0.001)	(0.076)	(0.001)	(0.001)	(0.099)	(0.283)
Growth Options	-0.186	0.013	-0.046	-0.074	-0.033	-0.053		0.002	0.017	-0.343	-0.182	-0.113	-0.148	-0.031	-0.090
	(0.001)	(0.603)	(0.057)	(0.002)	(0.175)	(0.029)		(0.937)	(0.496)	(0.001)	(0.001)	(0.001)	(0.001)	(0.200)	(0.001)
Convertible Securities	0.222	-0.106	0.157	0.024	0.004	0.021	0.008		-0.0141	-0.127	0.080	-0.077	0.078	-0.100	0.146
	(0.001)	(0.001)	(0.001)	(0.319)	(0.870)	(0.382)	(0.753)		(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Operating Earnings	-0.080	0.253	-0.327	0.034	0.019	0.086	-0.010	-0.444		0.496	-0.051	0.168	0.001	0.161	-0.439
	(0.001)	(0.001)	(0.001)	(0.162)	(0.445)	(0.001)	(0.700)	(0.001)		(0.001)	(0.036)	(0.001)	(0.977)	(0.001)	(0.001)
Firm Size	0.070	-0.076	0.137	0.191	0.013	0.070	-0.109	0.066	0.227		-0.054	0.068	0.125	0.073	-0.108
	(0.004)	(0.001)	(0.001)	(0.001)	(0.585)	(0.004)	(0.001)	(0.006)	(0.001)		(0.025)	(0.005)	(0.001)	(0.003)	(0.001)
Negative book equity	0.180	-0.109	0.134	0.012	0.003	0.030	-0.333	0.209	-0.194	-0.019		0.051	0.026	-0.069	0.127
	(0.001)	(0.001)	(0.001)	(0.613)	(0.913)	(0.219)	(0.001)	(0.001)	(0.001)	(0.430)		(0.034)	(0.284)	(0.005)	(0.001)
Asset Structure	0.194	-0.120	0.268	0.275	0.027	0.351	-0.033	-0.051	0.032	-0.027	0.061		0.118	0.032	0.103
	(0.001)	(0.001)	(0.001)	(0.001)	(0.270)	(0.001)	(0.168)	(0.036)	(0.189)	(0.272)	(0.012)		(0.001)	(0.184)	(0.001)
Firm Age	0.093	-0.269	0.112	0.059	-0.016	-0.079	-0.061	0.058	-0.025	0.094	0.026	0.136		0.638	0.078
	(0.001)	(0.001)	(0.001)	(0.015)	(0.505)	(0.001)	(0.012)	(0.017)	(0.304)	(0.001)	(0.284)	(0.001)		(0.001)	(0.001)
Age*MTR	-0.113	0.467	-0.214	-0.049	0.005	-0.044	-0.031	-0.068	0.153	-0.007	-0.069	0.009	0.641		-0.325
	(0.001)	(0.001)	(0.001)	(0.045)	(0.832)	(0.071)	(0.206)	(0.005)	(0.001)	(0.770)	(0.004)	(0.707)	(0.001)		(0.001)
Age*IRTS	0.189	-0.310	0.947	0.234	-0.008	0.033	-0.040	0.195	-0.277	0.159	0.129	0.254	0.185	-0.161	
	(0.001)	(0.001)	(0.001)	(0.001)	(0.728)	(0.180)	(0.100)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	

Table 4.4: Summary Statistics (Continued)

Notes:

This table presents summary statistics for the 1,701 firm-year observations over the period 1977 to 1988 used in the hypotheses tests. Panel A presents the distributional statistics for the continuous variables and Panel B presents the distribution for the discrete variable. Panel C reports cross-correlations between the one-year lagged marginal tax rate variable and the remaining explanatory variables for the year-to-year regressions; Panel D presents cross-correlations between the investment-tax shield variable and the remaining explanatory variables. Panel E provides correlations for the explanatory variables for the pooled regression. with Pearson correlations presented below the diagonal and Spearman correlations presented above the diagonal; related probability values are presented in parentheses.

Continuous variables are as follows (all dollar-denominated variables are stated in millions). Leverage is the book value of the sum of short-term, long-term, and convertible debt deflated by one-year lagged firm market value (the sum of market value of equity and book value of total debt). The marginal tax rate is a trichotomous variable which is equal to: (i) the top statutory corporate tax rate if taxable income is positive and the tax loss carryforwards (TLCF) balance is non-positive; (ii) one-half the top statutory rate if either taxable income is positive or TLCF is non-positive; and (iii) zero if taxable income is non-positive and TLCF is positive. A tax-exhausted firm dummy variable indicates those firms that have been assigned a zero marginal tax rate according to the above trichotomous variable conditions. The debt securability proxy is the sum of financial statement depreciation and investment tax credits (missing observations are replaced by zeros) grossed-up by the corporate statutory tax rate. This amount is scaled by firm market value, which is defined as the sum of the book value of total debt and the market value of equity. The investment-related tax shield variable is the product of the debt securability proxy and the tax-exhausted firm indicator variable. Altman's z-score is book total assets divided by the sum of 1.2 times working capital, 1.4 times retained earnings, 3.3 times earnings before interest and taxes, and net sales. Industry-mean leverage is the contemporaneous industry mean of the dependent variable, where industry-mean represents the mean among other firms in the narrowest SIC code that includes at least five firms other than the firm being examined. Growth options is the market value of common stock divided by the book value of common equity. Convertible securities is the sum of the book values of convertible debt and convertible preferred shares; this amount is deflated by one-year lagged firm market value. Operating earnings is earnings before depreciation, interest, and taxes deflated by one-year lagged firm market value. Firm size is net sales deflated by one-year lagged firm market value. Asset structure is total property, plant and equipment scaled by one-year lagged total assets. Firm age is the number of years that have elapsed since the firm's initial public offering. Age*MTR is the product of firm age and the marginal tax rate. Age*Investment-related tax shield is the product of firm age and the investment-related tax shield variable.

Indicator variables are as follows. The year dummy variables indicate the calendar year in which the firm underwent its initial public offering. The negative book equity dummy indicates if the book value of common equity is negative.

Table 4.5: Year-to-Year OLS Regression Results

$$LEVERAGE_{it} = \alpha + \gamma_t + \alpha_1 D_{it} + \beta_1 MTR_{i,t-1} + \beta_2 IRTS_{it} + \beta_3 SECURITY_{it} + \beta_4 Z\text{-}SCORE_{it} + \beta_5 INDUSTRY\text{-}MEAN_{it} + \beta_6 GROWTH_{it} + \beta_7 CONVERTIBLES_{it} + \beta_8 EARNINGS_{it} + \beta_9 SIZE_{it} + \beta_{10} NEG.EQUITY_{it} + \beta_{11} ASSETS_{it} + \varepsilon_{it}$$

		Year-to-Year Ordinary Least Squares (OLS) Estimation								
Variable	Prediction	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 9
Intercept	?	0.125**	0.006	-0.069	0.071	0.161**	0.171**	0.151**	0.085	0.073
Tax-exhausted indicator	?	-0.088**	0.118**	0.077	0.108*	0.121*	0.143**	0.307***	0.109*	0.139***
Marginal tax rate	+	-0.067	0.168*	0.139	-0.042	-0.010	-0.050	-0.178*	-0.328**	-0.319***
Investment-related tax shield	-	1.393*	-1.434**	2.108**	-1.400*	0.163	-1.787**	-1.654***	-0.466	-0.446
Debt security	+	2.172***	1.860***	0.039	0.399	0.587**	0.790*	0.817**	1.588***	1.075***
Altman's z-Score	-	-0.000	-0.000	0.015**	0.030*	-0.006	0.001	0.000	0.000	0.021
Industry-mean leverage	+	0.085	0.029	-0.031	0.276**	0.294**	0.322**	0.532***	0.593***	0.776***
Growth options	-	-0.018***	-0.013***	-0.005**	-0.014***	-0.042***	-0.040***	-0.035***	-0.003	-0.001
Convertible securities	+	0.781***	0.679***	1.276***	1.740***	1.323***	0.569***	1.093***	0.665***	-0.223**
Operating earnings	+	-0.165*	-0.147	0.061	-0.059	0.174	-0.308*	-0.043	0.094	-0.270**
Firm size	+	0.011	0.006	0.017	-0.003	-0.008	-0.005	-0.014	-0.016	0.008
Negative book equity indicator	+	-0.039	-0.308**	0.525***	0.283***	0.099	-0.319**	-0.172	0.228*	0.191
Asset structure	+	-0.012	0.032	0.156***	0.095**	-0.004	-0.034	-0.082*	-0.107**	-0.029
Adjusted R^2		0.279	0.226	0.337	0.381	0.393	0.222	0.325	0.252	0.366
F-statistic		4.63°	3.74°	5.79°	6.81°	7.09°	3.67°	5.52°	4.17°	6.41°
Number of observations		189	189	189	189	189	189	189	189	189

Notes:

This table presents regression results for the levels leverage models using year-to-year ordinary least squares estimation. There are a total of 1,701 observations for the firms' first through ninth years of public operation. The dependent variable and the explanatory variables are defined in Table 4.4. All regressions include unreported calendar year dummy variables. Regression equation *F*-statistics significant at less than 0.001 are identified by a ° superscript. In this table, the subscripts i and t identify firms and time, respectively. The superscripts indicate explanatory variable coefficient significance at *p*-values less than 0.10 (*), 0.05 (**), and 0.01 (***) in one-tailed tests where directional predictions are made and two-tailed tests otherwise.

Figure 4.1 - Time-series of Marginal Tax Rate Coefficients

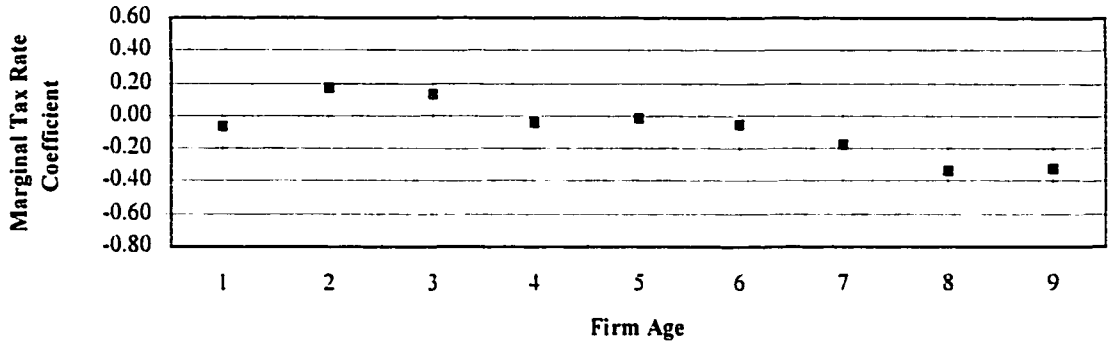

Figure 4.1 – This figure presents the cross-sectional coefficient estimates for firms' first through ninth years of public operation for the one-year lagged marginal tax rate variable specified in equation (4.3). Squares indicate the slope coefficients from the nine cross-sectional OLS regression models reported in Table 4.5. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Figure 4.2 - Time-series of Investment-related Tax Shield Coefficients

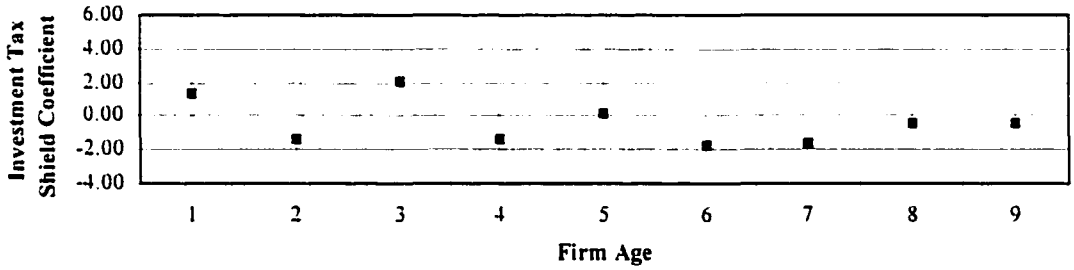

Figure 4.2 – This figure presents the cross-sectional coefficient estimates for firms' first through ninth years of public operation for the contemporaneous investment-related tax shield variable specified in equation (4.3). Squares indicate the slope coefficients from the nine cross-sectional OLS regression models reported in Table 4.5. Each coefficient estimate is surrounded by a 90 percent confidence interval that is represented by a vertical line.

Table 4.6: Pooled Cross-sectional, Time-series Results – OLS, Tobit, and GMM Estimation

$$LEVERAGE_{it} = \alpha + \gamma_t + \alpha_1 D_{it} + \beta_1 MTR_{i,t-1} + \beta_2 IRTS_{it} + \beta_3 SECURITY_{it} + \beta_4 Z\text{-}SCORE_{it} + \beta_5 INDUSTRY\text{-}MEAN_{it} + \beta_6 GROWTH_{it} + \beta_7 CONVERTIBLES_{it} + \beta_8 EARNINGS_{it} + \beta_9 SIZE_{it} + \beta_{10} NEG.EQUITY_{it} + \beta_{11} ASSETS_{it} + \beta_{12} AGE_{it} + \beta_{13} AGE_{it} * MTR_{i,t-1} + \beta_{14} AGE_{it} * IRTS_{it} + \varepsilon_{it}$$

		OLS		Tobit		GMM	
Variable	Prediction	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced
		1	2	3	4	5	6
Intercept	?	-0.038	-0.097***	-0.042	-0.152***	-0.038	-0.103***
Tax-exhausted indicator	?	0.124***	0.072***	0.139***	0.079***	0.122***	0.072***
Marginal tax rate	+	0.149**	0.134***	0.134**	0.147***	0.149**	0.133***
Investment-related tax shield	-	0.284	0.898***	0.272	0.933***	0.284	0.890***
Debt security	+	0.105**	0.051	0.121**	0.050	0.105*	0.050
Altman's z-score	-	0.000	0.000	0.000	0.000	0.000***	0.000
Industry-mean leverage	+	0.379***	0.669***	0.366***	0.750***	0.379***	0.695***
Growth options	-	-0.008***	0.000	-0.009***	0.000	-0.008***	0.000
Convertible securities	+	0.447***	0.371***	0.476***	0.410***	0.447***	0.370***
Operating earnings	+	0.149***	0.097***	0.192***	0.116***	0.149**	0.096***
Firm size	+	-0.000	0.010***	-0.003	0.011***	-0.000	0.009***
Negative book equity indicator	+	0.101**	0.171***	0.115**	0.180***	0.101*	0.170***
Asset structure	+	0.037***	0.010**	0.062***	0.017***	0.037**	0.010
Firm age	+	0.022***	0.021***	0.022***	0.022***	0.022***	0.021***
Age*MTR (H_1)	-	-0.059***	-0.051***	-0.063***	-0.054***	-0.059***	-0.051***
Age*IRTS (H_2)	-	-0.077*	-0.156***	-0.090*	-0.168***	-0.077	-0.154***
Adjusted R^2 (Log likelihood)		0.219	0.268	(-31.39)	(-479.26)	0.219	0.268
Number of observations		1,701	5,613	1,701	5,613	1,701	5,613

Notes:

This table presents regression results for the levels leverage models for both balanced and unbalanced panels using pooled cross-sectional, time-series ordinary least squares (OLS), tobit, and generalized method of moments (GMM) estimation for the firms' first through ninth years of public operation. The balanced sample discards the entire time-series of firms if any missing observations are encountered in the nine years; the unbalanced panel discards only the firm-year when missing observations are encountered. The dependent variable and the explanatory variables are defined in Table 4.4. All regressions include unreported calendar year dummy variables. Unreported tests indicate that all regression equations are significant at less than 0.001. In this table, the subscripts i and t identify firms and time, respectively. The superscript asterisks indicate explanatory variable coefficient significance at *p*-values less than 0.10 (*), 0.05 (**), and 0.01 (***) in one-tailed tests where directional predictions are made and two-tailed tests otherwise.

Table 4.7: Fixed Effects, Random Effects and Selection-Corrected Regression Results

$$LEVERAGE_{it} = \alpha_i + \gamma_t + \alpha_1 D_{it} + \beta_1 MTR_{i,t-1} + \beta_2 IRTS_{it} + \beta_3 SECURITY_{it} + \beta_4 Z\text{-}SCORE_{it} + \beta_5 INDUSTRY\text{-}MEAN_{it} + \beta_6 GROWTH_{it} + \beta_7 CONVERTIBLES_{it} + \beta_8 EARNINGS_{it} + \beta_9 SIZE_{it} + \beta_{10} NEG.EQUITY_{it} + \beta_{11} ASSETS_{it} + \beta_{12} AGE_{it} + \beta_{13} AGE_{it} * MTR_{i,t-1} + \beta_{14} AGE_{it} * IRTS_{it} + \varepsilon_{it}$$

Variable	Prediction	Fixed Effects		Random Effects		Selection-corrected OLS
		Balanced	Unbalanced	Balanced	Unbalanced	
		1	2	3	4	5
Intercept	?	0.136***	0.062***	0.080***	-0.057***	0.392
Tax-exhausted indicator	?	0.090***	0.061***	0.093***	0.070***	0.118***
Marginal tax rate	+	0.016	0.016	0.026	0.121***	0.129**
Investment-related tax shield	-	0.580*	0.813***	0.657**	0.904***	0.380
Debt security	+	0.042	0.089***	0.048*	0.056*	0.102**
Altman's z-score	-	0.001	0.000	0.000	0.000	-0.001
Industry-mean leverage	+	0.061	0.350***	0.167***	0.728***	0.373***
Growth options	-	-0.003***	0.000	-0.003***	0.000	-0.008***
Convertible securities	+	0.232***	0.286***	0.255***	0.376***	0.431***
Operating earnings	+	-0.018	-0.051***	-0.004	-0.094***	0.162***
Firm size	+	-0.003	-0.001	-0.002	0.011***	-0.002
Negative book equity indicator	+	0.121***	-0.120***	0.118***	0.167***	0.372
Asset structure	+	0.033*	0.004	0.055***	0.006*	0.042***
Firm age	+	0.012***	0.015***	0.014***	0.020***	0.022***
λ (inverse Mills ratio)	?					-0.289
Age*MTR (H_1)	-	-0.032***	-0.025***	-0.033***	-0.049***	-0.057***
Age*IRTS (H_2)	-	-0.125***	-0.139***	-0.137***	-0.155***	-0.087*
Adjusted R^2		0.681	0.683	0.219	.205	0.212
F-statistic		17.33°	15.17°			29.56°
χ^2-statistic				2,047°	2,638°	
Number of observations		1,701	5,414	1,701	5,414	1,701

Notes:

This table presents regression results for the levels leverage models for both balanced and unbalanced panels using fixed effects and random effects estimation for the firms' first through ninth years of public operation. Also, the results for the Heckman (1976, 1979) two-stage estimation procedure are reported in the selection-corrected column. The balanced sample discards the entire time-series of firms if any missing observations are encountered in the nine years; the unbalanced panel discards only the firm-year when missing observations are encountered. The dependent variable and the explanatory variables are defined in Table 4.4. All regressions include unreported calendar year dummy variables. Regression equation

F-tests for the fixed effects models and Lagrange multiplier (χ^2) tests for the random effects models significant at less than 0.001 are identified by a ° superscript. In this table, the subscripts i and t identify firms and time, respectively. The superscript asterisks indicate explanatory variable coefficient significance at *p*-values less than 0.10 (*), 0.05 (**), and 0.01 (***) in one-tailed tests where directional predictions are made and two-tailed tests otherwise.

Table 4.8: Fixed Effects, Random Effects and Tobit Regression Results – Alternate Trichotomous Marginal Tax Rate Proxy

$$LEVERAGE_{it} = \alpha_i + \gamma_t + \alpha_1 D_{it} + \beta_1 MTR_{i,t-1} + \beta_2 IRTS_{it} + \beta_3 SECURITY_{it} + \beta_4 Z\text{-}SCORE_{it} + \beta_5 INDUSTRY\text{-}MEAN_{it} + \beta_6 GROWTH_{it} + \beta_7 CONVERTIBLES_{it} + \beta_8 EARNINGS_{it} + \beta_9 SIZE_{it} + \beta_{10} NEG.EQUITY_{it} + \beta_{11} ASSETS_{it} + \beta_{12} AGE_{it} + \beta_{13} AGE_{it} * MTR_{i,t-1} + \beta_{14} AGE_{it} * IRTS_{it} + \varepsilon_{it}$$

		Fixed Effects		Random Effects		Tobit	
Variable	Prediction	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced
		1	2	3	4	5	6
Intercept	?	0.137***	0.059***	0.074***	-0.040***	-0.383***	-0.092***
Tax-exhausted indicator	?	0.035***	0.054***	0.035***	0.068***	0.036***	0.073***
Marginal tax rate	+	0.024	0.028	0.039	0.061**	0.105***	0.059*
Investment-related tax shield	-	0.347***	0.496***	0.561***	0.841***	0.642***	0.906***
Debt security	+	0.064**	0.083***	0.070**	0.042	0.085***	0.033
Altman's z-score	-	0.001**	0.000	0.001*	0.000	0.000***	0.000
Industry-mean leverage	+	0.158***	0.358***	0.0302***	0.752***	0.422***	0.829***
Growth options	-	0.002	0.000	0.001	-0.000	0.001**	-0.000
Convertible securities	+	0.207***	0.311***	0.0223***	0.405***	0.165***	0.449***
Operating earnings	+	-0.133***	-0.070***	-0.111***	0.100***	0.052***	0.124***
Firm size	+	0.001	0.000	0.002	0.011***	0.003***	0.012***
Negative book equity indicator	+	0.095***	0.143	0.107***	0.179***	-0.020	0.186***
Asset structure	+	-0.008*	0.002	0.004	0.012***	0.003*	0.018***
Firm age	+	0.009***	0.014***	0.013***	0.016***	0.012***	0.016***
Age*MTR (H_1)	-	-0.017***	-0.020***	-0.020***	-0.038***	-0.029***	-0.040***
Age*IRTS (H_2)	-	-0.055***	-0.088***	-0.088***	-0.144***	-0.111***	-0.158***
Adjusted R^2 (Log likelihood)		0.681	0.698	0.223	.252	(-5.869)	(-683)
Number of observations		3.015	6.824	3.015	6.824	3.015	6.824

Notes:

This table presents regression results for the levels leverage models for both balanced and unbalanced panels using fixed effects, random effects, and tobit estimation for the firms' first through ninth years of public operation. The results for the Heckman (1976, 1979) two-stage selection-correction procedure extended to tobit estimation are reported in column (5). The balanced sample discards the entire time-series of firms if any missing observations are encountered in the nine years; the unbalanced panel discards only the firm-year when missing observations are encountered. The dependent variable and the explanatory variables are identical to those defined in Table 4.4 with the following exceptions. The marginal tax rate is re-specified to replace any missing tax loss carryforward (TLCF) data with zeros. This re-specification affects the following four explanatory variables: the marginal tax rate, the investment-related tax shield, the age*marginal tax rate interaction, and the age*investment-related tax shield interaction. All regressions include unreported calendar year dummy variables. Unreported tests indicate that all regression equations are significant at less than 0.001. In this table, the subscripts i and t identify firms and time, respectively. The superscript asterisks indicate explanatory variable coefficient significance at *p*-values less than 0.10 (*), 0.05 (**), and 0.01 (***) in one-tailed tests where directional predictions are made and two-tailed tests otherwise.

Table 4.9: Fixed Effects, Random Effects and Tobit Regression Results – Tax Status Estimated with Graham et al's (1998) Simulated Marginal Tax Rates

$$LEVERAGE_{it} = \alpha_i + \gamma_t + \alpha_1 D_{it} + \beta_1 MTR_{it} + \beta_2 IRTS_{it} + \beta_3 SECURITY_{it} + \beta_4 Z\text{-}SCORE_{it} + \beta_5 INDUSTRY\text{-}MEAN_{it} + \beta_6 GROWTH_{it} + \beta_7 CONVERTIBLES_{it} + \beta_8 EARNINGS_{it} + \beta_9 SIZE_{it} + \beta_{10} NEG.EQUITY_{it} + \beta_{11} ASSETS_{it} + \beta_{12} AGE_{it} + \beta_{13} AGE_{it} * MTR_{it} + \beta_{14} AGE_{it} * IRTS_{it} + \varepsilon_{it}$$

		Fixed Effects		Random Effects		Tobit	
Variable	Prediction	Balanced	Unbalanced	Balanced	Unbalanced	Balanced	Unbalanced
		1	2	3	4	5	6
Intercept	?	0.181***	0.123***	0.101***	-0.007	-0.375***	-0.069*
Tax-exhausted indicator	?	0.033***	0.019***	0.039***	0.004	0.088***	0.008
Marginal tax rate	+	-0.045	-0.037	0.200	0.202**	0.336***	0.228**
Investment-related tax shield	-	0.382*	0.0655	0.0273	0.026	-0.046	0.027
Debt security	+	0.643***	0.285***	0.657***	0.342***	0.967***	0.325***
Altman's z-score	-	0.000	0.000	0.000	0.000	0.001	0.000
Industry-mean leverage	+	0.133***	0.392***	0.288***	0.732***	0.641***	0.795***
Growth options	-	-0.000**	-0.000	-0.000**	-0.000	0.000	-0.000
Convertible securities	+	0.171***	0.218***	0.200***	0.352***	0.283***	0.0396***
Operating earnings	+	-0.089***	-0.067***	-0.076***	-0.037*	0.074***	-0.036
Firm size	+	-0.013***	-0.005***	-0.009***	0.007***	0.004**	0.009***
Negative book equity indicator	+	0.114***	0.146***	0.119***	0.232***	-0.081***	0.240***
Asset structure	+	0.142	0.050***	0.029**	0.032***	0.005	0.053***
Firm age	+	0.005	0.008***	0.006*	0.010**	0.010*	0.008*
Age*MTR (H_1)	-	-0.018*	-0.020**	-0.020**	-0.035**	-0.032*	-0.032**
Age*IRTS (H_2)	-	-0.085***	-0.039**	-0.081***	-0.048*	-0.094**	-0.051*
Adjusted R^2 (Log likelihood)		0.787	0.733	0.284	0.263	(-4,187)	(-644)
Number of observations		2,628	4,394	3,015	4,394	2,628	4,394

Notes:

This table presents regression results for the levels leverage models for both balanced and unbalanced panels using fixed effects, random effects, and tobit estimation for the firms' fourth through ninth years of public operation. The results for the Heckman (1976, 1979) two-stage selection-correction procedure extended to tobit estimation are reported in column (5). The balanced sample discards the entire time-series of firms if any missing observations are encountered in the nine years; the unbalanced panel discards only the firm-year when missing observations are encountered. The dependent variable and the explanatory variables are identical to those defined in Table 4.4 with the following exceptions. The marginal tax rate is Graham et al's (1998) simulated marginal tax rate. A tax-exhausted firm dummy variable indicates firms that have a one-year lagged simulated marginal tax rate below the 25th percentile of the distribution of all observations for firms of the same age. The debt securability proxy remains the sum of financial statement depreciation and investment tax credits (missing observations are replaced by zeros) grossed-up by the corporate statutory tax rate. This amount is scaled by firm market value, which is defined as the sum of the book value of total debt and the market value of equity. The investment-related tax shield variable is the product of the debt securability proxy and the tax-exhausted firm indicator variable. All regressions include unreported calendar year dummy variables. Unreported tests indicate that all regression equations are significant at less than 0.001. In this table, the subscripts i and t identify firms and time, respectively. The superscript asterisks indicate explanatory variable coefficient significance at *p*-values less than 0.10 (*), 0.05 (**), and 0.01 (***) in one-tailed tests where directional predictions are made and two-tailed tests otherwise.

Chapter 5

The Influence of Private Operating History on the Time-series Variation in Tax-induced Financing and Investment Decisions

5.1 Introduction

This chapter examines whether the duration of firms' private operating histories affects the time-series variation in their tax-induced financing and investment decisions. Chapter 4 provides strong, robust evidence consistent with Scholes and Wolfson's (1989a) prediction that firms gradually shift from debt to investment-related tax shields in the years following their initial public offerings. These tests on the evolution in firms' financing and investment policies involve observing firms for their first nine public years, but deliberately ignore that these firms also have private operating histories. The purpose of this chapter is to report evidence on the influence of private age, which is defined as the number of years elapsed between incorporation and IPO, on the time-series pattern in firms' reliance on debt and investment tax shields.

This chapter begins with tests in Section 5.2 intended to provide additional evidence on whether firms' capital structures gradually become more constrained by re-financing costs that accumulate with age. Chapter 3 reports evidence from target adjustment models that firms are increasingly impeded over time from restoring their optimal capital structures. This section complements those tests by specifying lagged leverage in regressions designed to directly examine the influence of firms' histories – both private and public – on their financing decisions.

This chapter continues with tests in Section 5.3 that re-examine the evidence reported in Chapter 4 by separating the sample into "young" and "old" firms according to their private ages at their IPO dates. Finally, Section 5.4 concludes with a summary of the results presented in this chapter.

5.2 Empirical Tests of the Influence of Firms' Histories on Financing Decisions

5.2.1 Empirical Model Development

Initially in this section, lagged dependent variable estimation is used in cross-sectional tests to investigate whether re-financing costs inhibit the process of altering capital structure. Later,

additional tests are conducted to determine whether re-financing costs are increasing in firm age consistent with Scholes and Wolfson's (1989a) prediction that capital structures eventually become more complex and less flexible. Lagged leverage is specified to represent re-financing costs in these tests, which evaluate whether firms' private and public histories affect their capital structures.

In Chapter 3, equation (3.1) describes a simple model of leverage that is repeated here for expositional convenience:

$$L_{it} - L_{i,t-1} = (1 - \delta)(L^{*}_{it} - L_{i,t-1}) + \mu_{it} \qquad (5.1)$$

where L_{it} and $L_{i,t-1}$ are the firm's leverage at the end and the beginning of the year, respectively. L^{*}_{it} is the firm's target leverage at the end of the year absent any re-financing costs, which are instead isolated in the coefficient, $1 - \delta$ ($\in(0,1)$).

If re-financing costs do not affect capital structure; i.e., if $\delta = 0$, then firms always remain at their target leverage according to equation (5.1); i.e., $L_{it} = L^{*}_{it}$. However, at the other extreme, when $\delta = 1$, this equation indicates that firms are prevented from restoring their optimal capital structures; i.e., $L_{it} = L_{i,t-1}$. Estimates of δ between 0 and 1 imply the extent of path dependence in firms' financing.

The tests reported in Chapter 3 estimate the coefficient on $(1 - \delta)$ in equation (5.1) to measure the fraction of the distance between actual and target leverage the firm covered during the year. This empirical model attributes any delay in adjustment to re-financing costs. The tests in that chapter examine the time-series variation in the speed of adjustment toward various proxies for firm-specific optimal capital structure by graphing the pattern of the coefficients on $(1 - \delta)$ across the firms' first nine public years.

The evidence presented in Chapter 3 supports Scholes and Wolfson's (1989a) argument that firms' capital structure decisions are hindered by re-financing costs that are increasing in firm age. However, additional tests in Section 3.4 suggest that the time-series variation that is observed in firms' reversion toward their target leverage is not affected by the duration of their

private operating histories. Specifically, a negative pattern is found in both sub-samples of young and old firms at their IPO dates.

The target reversion models developed in Chapter 3 are one of two main research designs that have been applied in the empirical literature to estimate the partial adjustment model in equation (5.1). The second involves specifying target leverage, L^*, as a linear function of the determinants of capital structure, excluding re-financing costs, prescribed by theory. This design enables the influence of these costs to be estimated in the coefficient on lagged actual leverage, $L_{i,t-1}$ (e.g., Auerbach, 1985; and Gilson, 1997). Fortunately, this approach avoids the difficulties encountered in Chapter 3 with identifying adequate proxies for firm-specific optimal capital structure.

Returning to equation (5.1), assume that the target L^* is represented by $\beta X_{it} + \mu_i + e_{it}$ (the potential determinants of capital structure other than re-financing costs) and through substitution obtain:

$$L_{it} = (1 - \delta)\beta X_{it} + \delta\, L_{i,t-1} + (1 - \delta)\mu_i + e_{it} \qquad (5.2)$$

where μ_i is a time-invariant firm-specific effect and the residual term, e_{it}, is a measurement error or other form of stochastic shock. If re-financing costs matter, then observed leverage, L, will deviate from optimal leverage.

This empirical model with adjustment costs is intended to separate the effects on observed L of adjustment costs and of changes in optimal L^*. Notice that since the rate of response, $(1 - \delta)$, is always less than 1 if there are transactions costs in equation (5.2), models that ignore these costs underestimate the true impact of X on target leverage. The observed leverage L minimizes the cost of drifting from the target.

The coefficient on the lagged dependent variable in equation (5.2), δ ($\in(0,1)$), represents the rate of adjustment of actual to target leverage by capturing the potential reversion to the firm-specific optimal capital structure. If re-financing costs do not affect capital structure; i.e., if $\delta = 0$, then firms operate at their target leverage; i.e., $L_{it} = L^*_{it}$. Conversely, when $\delta = 1$, re-financing costs dominate such that firms inherit their capital structures; i.e., $L_{it} = L_{i,t-1}$.

Accordingly, when capital structure adjustment occurs instantaneously, equation (5.2) would reduce to the fixed-effects model developed in Chapter 4 to test the tax hypotheses, H_1 and H_2. However, the purpose of estimating equation (5.2) in this chapter is to implement the intuition underlying Scholes and Wolfson (1989a) by measuring the influence of private and public history – through the lagged leverage term – on financing decisions.

5.2.2 Lagged Dependent Variable Estimation

This section initially provides tests of the empirical model specified in equation (5.2) using the full sample of firms that went public between 1977 and 1988 (Tables 4.1 to 4.3 report descriptive statistics). Then, the regressions are re-run after bisecting the sample according to the duration of firms' private operating histories, which is measured as the number of years that have passed between their incorporation and IPO.

The mechanics of estimating this fixed-effects model with a lagged dependent variable is explained through a series of regressions, starting with OLS estimation. Leverage is defined in these tests as the book value of short-term and long-term debt divided by the book value of the sum of short-term debt, long-term debt, and shareholders' equity to be consistent with the specification for the target adjustment models reported in Chapter 3.[64] Unless otherwise stated, the results were qualitatively similar with tests that replace this book-based measure with market-based leverage. Lagged leverage is the prior year's debt ratio; all other explanatory variables and the screening imposed to compile the data sample are described in Chapter 4.

The coefficient on the lagged dependent variable for the OLS results reported in column (1) in Table 5.1 is massive since this explanatory variable constitutes an excellent proxy for the omitted fixed effects. An earlier *F*-test indicated that these unobservable firm-specific effects significantly contributed to the explanation of observed capital structure.

[64] The book debt ratio is unbounded since shareholders' equity can be negative. All results from tests using book leverage in this chapter remain when the dependent variable is winsorized at values of 0 and 1. This amounts to setting all debt ratios below and above this range to 0 and 1, respectively, which should also reduce the potential influence of outliers and *Compustat* coding errors.

Next, the fixed-effects model with a lagged dependent variable specified in equation (5.2) is estimated with the results reported in column (2) in Table 5.1.[65] This evidence implies that, *within firms*, the lagged dependent variable affects capital structure decisions; i.e., that there is a gradual adjustment process.[66]

These results provide evidence that both firm heterogeneity and path dependence matter to corporate financing decisions. Specifically, respective F-tests support the addition of firm-specific fixed effects to the OLS model that contains lagged leverage as an explanatory variable and the addition of the lagged dependent variable to the fixed effects model. The fixed effect μ_i in equation (5.2) represents unobserved heterogeneity; i.e., firms choose capital structures that persist over time. Adjustment costs induce path dependence with firms' current leverage affecting their future leverage since it is costly to change L when optimal L^* evolves over time because of changes in the determinants of capital structure, X.

[65] The fixed effects are slightly inconsistently estimated with the least-squares dummy-variables (LSDV) estimator since the error term is correlated with the lagged dependent variable such that a negative bias is mechanically induced on this coefficient (Nickell, 1981). This bias occurs because the difference of the lagged dependent variable from its mean and the difference of the residual from its mean are correlated by construction. This correlation results in a negative bias when the time-series is small, which is the typical situation with panel data including here. However, the correlation vanishes when the regression is first-differenced to eliminate the fixed effect and the difference in the lagged dependent variable is instrumented with a longer lag, which enables consistent coefficient estimates to be obtained. Hsaio (1986, page 75) explains that applying twice-lagged leverage, $L_{i,t-2}$, would be a suitable instrument to generate consistent estimates of δ. The results from these tests are qualitatively similar to the levels tests presented above. In addition, there remains the possibility that the significance of the lagged dependent variable is spuriously induced by serial correlation in the residuals. The first-differences tests were run using Hatanaka's (1974) efficient estimator for this dynamic adjustment model in the presence of autocorrelated errors (see Greene (1997), pages 605-606).

[66] The estimated coefficient of 0.321 on lagged leverage from the fixed effects regression reported in column (2) in Table 5.1 implies a target adjustment parameter, $1 - \delta$, of 0.679. The estimate of this parameter, which measures the rate at which firms adjust to their optimal debt ratios, is considerably higher than that observed in other empirical research estimating this partial adjustment model. For example, Auerbach (1985) reports an annual adjustment speed of 0.274 when leverage is defined to include all debt. There are at least two reasons to expect larger estimates of $1 - \delta$ in the thesis sample. First, these younger firms may have relatively more flexible and less complex capital structures that facilitate financing adjustments (Scholes and Wolfson, 1989a). Second, these firms are at such an early stage in their history that they frequently change their capital structures. Rajan and Zingales (1998) and Helwege and Liang (1996) provide descriptive statistics that support the perception that there is a life cycle in the pattern of corporate financing, with firms tending to rely more on external capital in their early years to finance their initial investment projects. As the investment needs of these younger firms have yet to stabilize, they are more apt to alter their capital structures relative to the older firms that probably dominate the sample in Auerbach (1985).

However, this thesis is more interested in examining the time-series variation in the (potential) influence of firms' histories on their capital structures. This issue is considered in the fixed-effects regression by including the interaction of lagged leverage and firm age as an additional explanatory variable. The results from this specification, which are reported in column (3) in Table 5.1, provide no evidence that re-financing costs increase over time for the full sample of firms.[67]

The research design applied in the primary tests of the tax shield predictions, H_1 and H_2, in Chapter 4 measures a firm's age as the number of years that have elapsed since its IPO, which might admit confounding since firms also have a private operating history at their offering date. In fact, a potential explanation for the absence of time-series evidence indicating that it becomes increasingly difficult for public firms to change their capital structures is cross-sectional differences in their private ages.

For example, the histories of firms that existed for many years before their IPO might already severely impede leverage adjustments.[68] Also, firms that are older at their IPO dates might benefit from being relatively larger and having less acute information problems when they go public. In fact, some extant theory and empirical research that was reviewed in Chapter 2 suggests that re-financing costs may subside over firms' early years of public operation.

This section continues by reporting results from specifications in which the panel is split into firms that are above and below the median private history for the full sample, which is ten years; i.e., the sample is partitioned into the youngest and oldest firms at their IPO dates. The Scholes

[67] This matter was examined further by estimating year-to-year random coefficient models (accounts for firm heterogeneity and avoids serial correlation issues) with lagged leverage specified as an explanatory variable to absorb the extent of reversion to the firm-specific optimal capital structure. A statistically significant positive time-series pattern emerges for lagged leverage, which appears to support the argument that adjustment costs are increasing in firm age. However, these tests, which were performed using a weighted regression for the nine year-to-year δ estimates, were sensitive to the choice of leverage definition. A positive pattern was detected when leverage was defined as the ratio of debt to book firm value; no time-series evidence was found when debt was deflated by market firm value.

[68] However, the evidence from the target adjustment models reported in Chapter 3 implies that this is not the case. These tests indicate that the capital structure adjustment process in both young and old firms at their IPO dates gradually becomes more constrained in the nine years following their initial public offerings.

and Wolfson (1989a) argument implies that the younger firms will be more apt to experience increasing re-financing costs in their first nine public years.

Consistent with this prediction, the fixed-effects results reported in column (4) in Table 5.2 provide evidence that the capital structure decisions of the younger subset of firms become more affected by their histories over time; i.e., the coefficient on the firm age-lagged leverage interaction is positive and statistically significant. Further, the lagged leverage explanatory variable in this regression provides strong evidence that these firms are constrained by their histories.

However, the coefficient on lagged leverage for the subset of younger firms is statistically and economically smaller than the corresponding coefficient for the subset of older firms that is reported in column (2) in Table 5.2. In fact, the time-series variation in the impact of the past on current capital structure as measured by the firm age-lagged leverage interaction is negative for the sample of older firms.

In summary, for the sample of younger firms at their IPO dates, history matters less to their financing decisions, but gradually becomes more important. For the sample of older firms, the opposite situation is observed: in absolute terms, they are affected more by their histories, but this subsides over their first nine public years. Accordingly, this split-sample evidence suggests that the evolution in firms' reliance on debt and investment tax shields in their early public years may depend on their private ages, an issue that is examined in the next section.

5.3 Empirical Tests of the Influence of Firms' Private Ages on Tax-induced Financing and Investment Decisions

Chapter 4 reports strong, robust evidence consistent with the thesis predictions about the time-series variation in tax-induced financing and investment decisions. These tests indicate that firms gradually shift from debt to investment-related tax shields in their early public years, which Scholes and Wolfson (1989a) argue represents the rational reaction to re-financing costs that accumulate with experience. In fact, Chapter 3 relies on various proxies for target leverage to

provide corroborating evidence that restoring their optimal capital structures becomes more difficult for firms over time.

The research design adopted in the preceding chapters measures a firm's age as the number of years that have elapsed since its IPO. However, the regression results presented in Section 5.2 suggest that firms' private ages, which is the number of years that have passed between their incorporation and IPO, affect their capital structures in the first nine public years. For example, these tests find that firms that are older when they go public are relatively more constrained by their histories, as estimated through the lagged leverage explanatory variable, over these nine years.

This section re-examines the evidence reported in Chapter 4 on the thesis hypotheses, H_1 and H_2, after bisecting the sample into the youngest and oldest firms at their initial public offerings. The Scholes and Wolfson (1989a) argument implies that the financing decisions of the older firms will be relatively less responsive to their tax status since they already have extensive histories by the time that they have their IPOs. The median private age of this sample is 18 years compared to four years for the sample of younger firms.

As predicted, the pooled OLS results presented in column (1) of Table 5.3 provide evidence that the capital structure decisions of these older firms are not affected by their marginal tax rates; i.e., the estimated coefficient of 0.058 on this variable is not statistically significant. These firms are also observed to have shifted toward relying on the investment-related tax shield according to the negative and significant (p-value under 0.01) coefficient on this variable. This cross-sectional evidence is consistent with the prediction that firms begin to resort to other tax sheltering strategies when re-financing costs begin to hinder capital structure changes.

Further, the results from the two-way fixed effects tests that are suited to examining the within-firm dynamics predicted in the thesis hypotheses are reported in column (2) in Table 5.3. This evidence indicates that these older firms continue to gradually reduce their reliance on debt tax shields during their first nine public years; i.e., the coefficient on the firm age-marginal tax rate interaction is negative and strongly significant, which is consistent with H_1.

However, there is no empirical support for H_2, which suggests that the evolution in tax shield choice is relatively advanced for these older firms. It appears that the transition in firms' tax-induced financing and investment policies predicted by Scholes and Wolfson (1989a) is largely complete for this sample. For example, the cross-sectional results in column (1) in Table 5.3 provide evidence that the older firms rely only on investment-related tax shields; their marginal tax rates do not affect their capital structures. Accordingly, the stability in the time-series pattern of investment tax shield substitution, which is observed through the coefficient on firm age-investment-related tax shield interaction in column (2) in Table 5.3, is expected.

Almost completely opposite tax-motivated financing and investment behavior is found for the younger firms, which for the purposes of these tests are defined as those in the lower half of the private age distribution as measured from their incorporation dates. Again consistent with the prediction that younger firms have more flexibility to adjust their capital structures in reaction to changing tax incentives, a positive and statistically significant coefficient is estimated for the marginal tax rate variable in this sample (see column (3) in Table 5.3).

Also, the coefficient on the investment-related tax shield proxy reported in column (3) in Table 5.3 is positive and significant, which contradicts the negative relation modeled in DeAngelo and Masulis (1980). This cross-sectional evidence supports the argument that these younger firms have capital structures that are not seriously affected by accumulating re-financing costs such that there is less urgency for them to substitute tax shields.

The fixed effects results for the younger firms, which are reported in column (4) of Table 5.3, provide evidence consistent with both H_1 and H_2; i.e., these younger firms experience negative time-series variation in debt and non-debt tax shields.[69] Apparently, these firms have not progressed as far as the older firms in their transition from relying on debt tax shields toward investment-related tax shields.

[69] The results described for the tests that include the duration of firms' private operating histories are not sensitive to replacing firm age with its natural logarithm (some firms were founded more than 100 years before going public) or to using the unbalanced panel (firms can enter and leave the sample anytime during the nine-year public period).

Finally, it could be argued that the higher R^2 in Table 5.3 observed for the sample of older firms in both the pooled OLS and fixed effects tests implies that these firms are less deterred by re-financing costs from responding to the explanatory variables. According to equation (5.2), the coefficients on the potential determinants of target leverage excluding transactions costs, L^*, are multiplicative in $(1 - \delta)$, the capital structure adjustment parameter. The estimated coefficients on the other explanatory variables by construction approach 0 when the coefficient on lagged leverage approaches 1.

For example, excluding lagged leverage from the regressions reported in Table 5.3 constrains $(1 - \delta)$ to equal 1 to enable examination of whether the other explanatory variables independently affect firms' capital structures. These proxies for the determinants of firms' financial policies measure their incentives to restore their optimal capital structures. The consistent evidence that these variables provide more explanation of financing decisions in the sample of older firms should be interpreted cautiously for several reasons:

(i) Much of the explanation is contributed in this sample by strongly significant coefficients that are in the opposite direction from that predicted by theory; e.g., Altman's z-score and operating earnings in column (1) in Table 5.3.

(ii) The negative and strongly significant coefficient on the investment-related tax shield, although consistent with the substitution predicted in DeAngelo and Masulis (1980), may also proxy for re-financing costs. Scholes and Wolfson (1989a) argue that firms increasingly shift toward investment-related tax shields when constrained from adjusting their capital structures.

(iii) Some explanatory variables are more apt to affect older firms that have more mature capital structures; e.g., the proxies for convertible securities and industry-mean leverage.

Aside from the above comments, this thesis does not examine this intractable issue by, for example, attempting to untangle the various cross-sectional and time-series influences on the samples of younger and older firms.

5.4 Conclusions

This chapter complements the results reported in Chapter 3 by providing additional evidence on whether corporate capital structures gradually become more constrained by re-financing costs that are increasing in firm age. These tests specify lagged leverage in regressions that are

intended to evaluate the impact of firms' private and public histories on their financing decisions. According to the results for the full sample of firms reported in Table 5.1, the positive and significant coefficient on lagged leverage implies that re-financing costs impede firms from changing their capital structures. However, there is no evidence that this influence varies over time.

The research design applied for the tests reported in Table 5.1 and in the preceding chapters involves observing firms for their first nine public years without considering that firms also have private operating histories that may affect the time-series pattern in their financing and investment decisions. This issue is addressed by re-examining evidence provided earlier in the thesis after bisecting the sample according to firms' private ages, which is measured as the number of years that have passed between their incorporation and IPO.

Table 5.2 presents results from tests that are identical to those reported in Table 5.1 except that the sample is split into the younger and older firms at their IPO dates. The coefficients on the lagged leverage explanatory variables in the respective samples suggest that the capital structures of the younger firms are less rigid compared to those of the older firms.

However, this thesis is more concerned with the evidence that the time-series variation in the path dependence in financing decisions also differs across the younger and older firms. For the sample of younger firms, Table 5.2 provides evidence that adjusting leverage gradually becomes more difficult over their first nine years of public operation. In contrast, the capital structures of the older firms become less constrained over this period.

The split-sample evidence in Table 5.2 on the capital structure adjustment process implies that firms' private ages may affect the evolution in their tax-induced financing and investment choices. Chapter 4 presents strong, robust evidence supporting the thesis hypotheses, H_1 and H_2. The panel data tests ranging firms' first through ninth public years in that chapter indicate that the positive relation between firms' leverage and their marginal tax rates subsides with age. In addition, firms are observed to gradually shift toward relying on investment-related tax shields.

The tests described in Section 5.3 re-examine the evidence on H_1 and H_2 reported in Chapter 4 by again dividing the sample into younger and older firms at their initial public offerings. The Scholes and Wolfson (1989a) argument that re-financing costs accumulate with age implies that the financing decisions of the older firms, which already have extensive histories at their IPO dates, will be relatively less sensitive to their marginal tax rates. They also predict that the older firms will replace this debt tax shield with investment-related tax shields when re-financing costs prevent changes to their capital structures.

Table 5.3 provides cross-sectional evidence consistent with these predictions. For the sample of older firms, only the investment-related tax shield matters to their financing decisions; their marginal tax rates do not affect their capital structures. This table also reports on within-firm dynamics which suggest that the evolution in firms' tax-induced financing and investment policies predicted by Scholes and Wolfson (1989a) is largely complete for this sample. This evidence includes that these older firms continue to gradually reduce their reliance on debt tax shields during their first nine public years, which is consistent with H_1. However, the stable time-series variation in their investment tax shield substitution suggests that the older firms have finished the shifting predicted in H_2.

For the sample of younger firms, the evidence on their debt and investment tax shields indicates that these firms are at an earlier stage in the evolution argued by Scholes and Wolfson (1989a). The cross-sectional results in Table 5.3 include that firms' marginal tax rates affect their financing decisions. Also, unlike in the sample of older firms, there is no empirical support for DeAngelo and Masulis's (1980) theory of investment-related tax shield substitution. This cross-sectional evidence is consistent with the argument that these younger firms have capital structures that are not seriously constrained by re-financing costs. Since these firms have more flexibility to adjust their leverage, there is less incentive for them to substitute tax shields.

Finally, the fixed effects regression results reported in Table 5.3 provide additional evidence that the younger firms have not progressed as far as the older firms in their transition toward relying on investment-related tax shields. This test detects support for both H_1 and H_2; i.e., these younger firms experience negative time-series variation in their tax-induced financing and investment policies.

Table 5.1: OLS and Fixed Effects Regression Results for Lagged Leverage Tests – Full Sample

Pooled OLS regression

$$LEVERAGE_{it} = \alpha + \gamma_t + \alpha_1 D_{it} + \beta_1 MTR_{i,t-1} + \beta_2 IRTS_{it} + \beta_3 SECURITY_{it} + \beta_4 Z\text{-}SCORE_{it} + \beta_5 INDUSTRY\text{-}MEAN_{it} + \beta_6 GROWTH_{it} + \beta_7 CONVERT_{it} + \beta_8 EARNINGS_{it} + \beta_9 SIZE_{it} + \beta_{10} NEG.EQUITY_{it} + \beta_{11} ASSETS_{it} + \beta_{12} AGE_{it} + \beta_{13} LAGGED\text{-}LEVERAGE_{i,t-1} + \varepsilon_{it}$$

Fixed effects regression

$$LEVERAGE_{it} = \alpha_i + \gamma_t + \alpha_1 D_{it} + \beta_1 MTR_{i,t-1} + \beta_2 IRTS_{it} + \beta_3 SECURITY_{it} + \beta_4 Z\text{-}SCORE_{it} + \beta_5 INDUSTRY\text{-}MEAN_{it} + \beta_6 GROWTH_{it} + \beta_7 CONVERT_{it} + \beta_8 EARNINGS_{it} + \beta_9 SIZE_{it} + \beta_{10} NEG.EQUITY_{it} + \beta_{11} ASSETS_{it} + \beta_{12} AGE_{it} + \beta_{13} LAGGED\text{-}LEVERAGE_{i,t-1} + \beta_{14} AGE_{it} * LAGGED\text{-}LEVERAGE_{i,t-1} + \varepsilon_{it}$$

Variable	Prediction	Ordinary Least Squares	Fixed Effects	
			Without Age Interaction	With Age Interaction
		1	2	3
Intercept	?	0.004	0.150***	0.151***
Tax-exhausted indicator	?	0.097***	0.087***	0.087***
Marginal tax rate	+	0.096***	0.038	0.038
Investment-related tax shield	-	-0.002	-0.064	-0.064
Debt security	+	0.053	0.052*	0.052*
Altman's z-score	-	-0.000***	-0.000	-0.000
Industry-mean leverage	+	0.075**	-0.044	-0.044
Growth options	-	-0.001	0.003***	0.003***
Convertible securities	+	0.189**	0.130***	0.129***
Operating earnings	+	0.119*	0.105***	0.105***
Firm size	+	-0.001	-0.001	-0.001
Negative book equity indicator	+	0.385**	0.590***	0.590***
Asset structure	+	0.017*	-0.003	-0.003
Firm age	+	-0.003*	-0.002	-0.002
Lagged leverage	+	0.682***	0.321***	0.320***
Age*Lagged leverage	+			0.001
Adjusted R^2		0.621	0.700	0.700
F-statistic		88.04°	18.97°	18.88°
Number of observations		1,701	1,701	1,701

Notes:

This table presents regression results for the levels leverage models for the balanced panel using ordinary least squares and two-way fixed effects estimation for the firms' first through ninth years of public operation. The balanced sample discards the entire time-series of firms if any missing observations are encountered in the nine years. The dependent variable and the explanatory variables are defined in Table 4.4 except that leverage is the book value of the sum of short-term and long-term debt deflated by the sum of short-term debt, long-term debt, and shareholders' equity. All regressions include unreported calendar year dummy variables. Regression equation *F*-tests for the models that are significant at less than 0.001 are identified by a ° superscript. In this table, the subscripts i and t identify firms and time, respectively. The superscript asterisks indicate explanatory variable coefficient significance at *p*-values less than 0.10 (*), 0.05 (**), and 0.01 (***) in one-tailed tests where directional predictions are made and two-tailed tests otherwise.

Table 5.2: OLS and Fixed Effects Regression Results for Lagged Leverage Tests – Balanced Panel Partitioned by Private Age at IPO Date

Pooled OLS regression

$$LEVERAGE_{it} = \alpha + \gamma_t + \alpha_1 D_{it} + \beta_1 MTR_{i,t-1} + \beta_2 IRTS_{it} + \beta_3 SECURITY_{it} + \beta_4 Z\text{-}SCORE_{it} + \beta_5 INDUSTRY\text{-}MEAN_{it} + \beta_6 GROWTH_{it} + \beta_7 CONVERT_{it} + \beta_8 EARNINGS_{it} + \beta_9 SIZE_{it} + \beta_{10} NEG.EQUITY_{it} + \beta_{11} ASSETS_{it} + \beta_{12} AGE_{it} + \beta_{13} LAGGED\text{-}LEVERAGE_{i,t-1} + \beta_{14} AGE_{it} * LAGGED\text{-}LEVERAGE_{i,t-1} + \varepsilon_{it}$$

Fixed effects regression

$$LEVERAGE_{it} = \alpha_i + \gamma_t + \alpha_1 D_{it} + \beta_1 MTR_{i,t-1} + \beta_2 IRTS_{it} + \beta_3 SECURITY_{it} + \beta_4 Z\text{-}SCORE_{it} + \beta_5 INDUSTRY\text{-}MEAN_{it} + \beta_6 GROWTH_{it} + \beta_7 CONVERT_{it} + \beta_8 EARNINGS_{it} + \beta_9 SIZE_{it} + \beta_{10} NEG.EQUITY_{it} + \beta_{11} ASSETS_{it} + \beta_{12} AGE_{it} + \beta_{13} LAGGED\text{-}LEVERAGE_{i,t-1} + \beta_{14} AGE_{it} * LAGGED\text{-}LEVERAGE_{i,t-1} + \varepsilon_{it}$$

		Older Firms		Younger Firms	
Variable	Prediction	Pooled OLS	Fixed Effects	Pooled OLS	Fixed Effects
		1	2	3	4
Intercept	?	-0.029	0.035	0.040*	0.178***
Tax-exhausted indicator	?	0.113***	0.115***	0.066***	0.050**
Marginal tax rate	+	0.109***	0.100**	0.084*	-0.002
Investment-related tax shield	-	-0.725***	-0.634***	0.284	0.177
Debt security	+	0.180	0.603***	0.052	0.035
Altman's z-score	-	0.034***	0.036***	-0.000***	-0.000
Industry-mean leverage	+	0.076**	-0.027	0.119**	-0.009
Growth options	-	-0.008***	-0.007***	0.004*	0.007***
Convertible securities	+	0.430***	0.565***	0.087	0.012
Operating earnings	+	-0.059	0.034	0.139**	0.074*
Firm size	+	0.006	-0.008	-0.001	0.001
Negative book equity indicator	+	-0.263	-0.010	0.631***	0.756***
Asset structure	+	0.008	-0.006	0.020	0.001
Firm age	+	-0.001	0.004*	-0.010***	-0.006*
Lagged leverage	+	0.854***	0.673***	0.420***	0.136***
Age*Lagged leverage	+	-0.008	-0.022***	0.031**	0.016**
Adjusted R^2		0.756	0.783	0.589	0.687
F-statistic		82.75°	25.13°	38.86°	15.52°
Number of observations		873	873	828	828

Notes:

This table presents regression results for the levels leverage models for older and younger firms using ordinary least squares and two-way fixed effects estimation for the firms' first through ninth years of public operation. The balanced sample discards the entire time-series of firms if any missing observations are encountered in the nine years. Older firms are defined as those for which the duration of their private history, which is measured as the number of years that have elapsed between their incorporation and their initial public offering, is above the median for the sample. Younger firms are defined as those with private histories that are shorter than the median for the sample. The dependent variable and the explanatory variables are defined in Table 4.4 except that leverage is the book value of the sum of short-term and long-term debt deflated by the sum of short-term debt, long-term debt, and shareholders' equity. All regressions include unreported calendar year dummy variables. Regression equation *F*-tests significant at less than 0.001 are identified by a ° superscript. In this table, the subscripts i and t identify firms and time, respectively. The superscript asterisks indicate explanatory variable coefficient significance at *p*-values less than 0.10 (*), 0.05 (**), and 0.01 (***) in one-tailed tests where directional predictions are made and two-tailed tests otherwise.

Table 5.3: OLS and Fixed Effects Regression Results – Balanced Panel Partitioned by Private Age at IPO Date

Pooled OLS regression

$$LEVERAGE_{it} = \alpha + \alpha_1 D_{it} + \beta_1 MTR_{i,t-1} + \beta_2 IRTS_{it} + \beta_3 SECURITY_{it} + \beta_4 Z\text{-}SCORE_{it} + \beta_5 INDUSTRY\text{-}MEAN_{it} + \beta_6 GROWTH_{it} + \beta_7 CONVERT_{it} + \beta_8 EARNINGS_{it} + \beta_9 SIZE_{it} + \beta_{10} NEG.EQUITY_{it} + \beta_{11} ASSETS_{it} + \beta_{12} AGE_{it} + \beta_{13} AGE_{it} * MTR_{i,t-1} + \beta_{14} AGE_{it} * IRTS_{it} + \varepsilon_{it}$$

Fixed effects regression

$$LEVERAGE_{it} = \alpha_i + \gamma_t + \alpha_1 D_{it} + \beta_1 MTR_{i,t-1} + \beta_2 IRTS_{it} + \beta_3 SECURITY_{it} + \beta_4 Z\text{-}SCORE_{it} + \beta_5 INDUSTRY\text{-}MEAN_{it} + \beta_6 GROWTH_{it} + \beta_7 CONVERT_{it} + \beta_8 EARNINGS_{it} + \beta_9 SIZE_{it} + \beta_{10} NEG.EQUITY_{it} + \beta_{11} ASSETS_{it} + \beta_{12} AGE_{it} + \beta_{13} AGE_{it} * MTR_{i,t-1} + \beta_{14} AGE_{it} * IRTS_{it} + \varepsilon_{it}$$

		Older Firms		Younger Firms	
Variable	Prediction	Pooled OLS	Fixed Effects	Pooled OLS	Fixed Effects
		1	2	3	4
Intercept	?	0.026	0.197***	-0.041	0.091**
Tax-exhausted indicator	?	0.174***	0.129***	0.090***	0.064***
Marginal tax rate	+	0.058	0.072	0.189**	0.057
Investment-related tax shield	-	-3.362***	-2.215***	1.276**	1.167***
Debt security	+	0.977***	1.628***	0.072*	0.011
Altman's z-score	-	0.039***	0.019***	0.000	0.000
Industry-mean leverage	+	0.428***	0.044	0.338***	0.075
Growth options	-	-0.011***	-0.007***	-0.004***	-0.006
Convertible securities	+	1.187***	0.779***	0.326***	0.212***
Operating earnings	+	-0.355***	-0.405***	0.244***	0.072**
Firm size	+	0.020***	-0.019**	-0.009**	-0.006
Negative book equity indicator	+	0.004	-0.008	0.205***	0.148***
Asset structure	+	-0.004	-0.066*	0.033**	0.036*
Firm age	+	0.016**	0.013***	0.022***	0.011***
Age*MTR (H_1)	-	-0.045**	-0.040***	-0.066***	-0.035***
Age*IRTS (H_2)	-	0.172	0.101	-0.142**	-0.157***
Adjusted R^2		0.366	0.743	0.227	0.676
F-statistic		34.57°	20.36°	17.21°	14.80°
Number of observations		873	873	828	828

Notes:

This table presents regression results for the levels leverage models for older and younger firms using ordinary least squares and two-way fixed effects estimation for the firms' first through ninth years of public operation. The balanced sample discards the entire time-series of firms if any missing observations are encountered in the nine years. Older firms are defined as those for which the duration of their private history, which is measured as the number of years that have elapsed between their incorporation and their initial public offering, is above the median for the sample. Younger firms are defined as those with private histories that are shorter than the median for the sample. The dependent variable and the explanatory variables are defined in Table 4.4. Coefficients on the unreported calendar year dummy variables are not reported. Regression equation *F*-tests significant at less than 0.001 are identified by a ° superscript. In this table, the subscripts i and t identify firms and time, respectively. The superscript asterisks indicate explanatory variable coefficient significance at *p*-values less than 0.10 (*), 0.05 (**), and 0.01 (***) in one-tailed tests where directional predictions are made and two-tailed tests otherwise.

Chapter 6

Conclusions

6.1 Summary of Thesis Evidence

Scholes and Wolfson (1989a) argue that firms' capital structures gradually become more constrained by re-financing costs that accumulate with age. This implies that firms are increasingly impeded over time from restoring their optimal capital structures, which may partially explain the scarcity of evidence that taxes affect firms' financing decisions. In fact, Chapter 2 reviews extant empirical and analytical research that generally suggests that re-financing costs are increasing in firm age as their capital structures become less flexible and more complex.

Chapter 3 attempts to empirically validate this theory and evidence by examining the time-series variation in firms' rate of adjustment to their optimal capital structures. These tests are intended to measure the fraction of the distance between actual and target leverage the firm covered during each year in the nine years following their initial public offerings. Although complicated by difficulty specifying adequate proxies for firm-specific optimal capital structure, these tests provide consistent time-series evidence that the adjustment process gradually becomes more constrained.

The negative time-series pattern observed in firms' reversion to their optimal capital structures can be considered a prerequisite to testing Scholes and Wolfson's (1989a) predictions about the evolution in firms' reliance on debt and investment-related tax shields. Their contention that dynamic (i.e., increasing) re-financing costs eventually restrict firms' capital structures motivates the first thesis hypothesis that the relation between financial leverage and marginal tax rates will become less positive as firms age. Further, their prediction that firms will resort to relying on other tax shelters when re-financing costs impede changes to their capital structures motivates the second thesis hypothesis that the relation between financial leverage and investment-related tax shields will become more negative as firms age.

Chapter 4 provides strong, robust evidence consistent with the thesis hypotheses on the time-series variation in firms' tax-induced financing and investment decisions. The research design applied in that chapter measures a firm's age as the number of years that have elapsed since its IPO, although firms typically have a private operating history when they go public. This implies that cross-sectional differences in firms' private ages, defined as the number of years between their incorporation and their IPOs, could affect the tests of the hypotheses, H_1 and H_2.

Chapter 5 examines the influence of firms' private ages on the time-series pattern in their reliance on debt and investment tax shields. This chapter begins with tests designed to provide additional evidence on whether firms' capital structures gradually become more constrained by re-financing costs that accumulate with age. Chapter 3 reports evidence from target adjustment models that firms are increasingly inhibited over time from restoring their optimal capital structures. This negative time-series variation in firms' reversion to their target leverage is detected in both samples of younger and older firms at their IPO dates. Chapter 5 complements those tests by specifying lagged leverage in regressions intended to directly evaluate whether the duration of firms' histories – both private and public – affect their financing decisions.

The evidence provided earlier in the thesis is re-examined in Chapter 5 after bisecting the sample according to firms' private ages. The split-sample tests indicate that only for the sample of younger firms does adjusting leverage gradually becomes more difficult over their first nine public years. In contrast, for both samples of younger and older firms at their IPO dates, the target adjustment models estimated in Chapter 3 suggest that firms' capital structures become more constrained over time.

Additional tests in Chapter 5 re-examine the evidence on H_1 and H_2 reported in Chapter 4 by again dividing the sample into younger and older firms at their initial public offerings. The results from these regressions imply that the younger firms are at an earlier stage in the evolution predicted by Scholes and Wolfson (1989a). For example, the cross-sectional evidence in Chapter 5 includes that their marginal tax rates, but not their investment tax shields, affect the financing decisions of the younger firms. For the sample of older firms,

the opposite situation is observed; i.e., their investment tax shields, but not their marginal tax rates, matter to their capital structures.

Further, the results from the fixed effects tests in Chapter 5 provide additional evidence that the younger firms have not progressed as far as the older firms in their transition toward relying on investment-related tax shields. These regressions find empirical support for both H_1 and H_2; i.e., these younger firms experience negative time-series variation in their tax-induced financing and investment decisions.

In comparison, only evidence consistent with H_1 is found for the older firms in the fixed effects models; i.e., these firms continue to reduce their reliance on debt tax shields. However, the absence of evidence for H_2 suggests that their shifting toward investment tax shields is largely complete; e.g., the cross-sectional tests detect that this sample of older firms already strongly rely on their investment-related tax shields.

In summary, for the full sample of firms, Chapters 3 and 4 report evidence consistent with Scholes and Wolfson's (1989a) predictions concerning the time-series properties of firms' re-financing costs and their tax-induced financing and investment policies. However, the tests in these chapters, which represent the main evidence in this thesis, deliberately ignore that firms have private operating histories when they have their initial public offerings. Chapter 5 investigates the impact on this evidence of firms' private ages by re-estimating the regressions after the splitting the sample into younger and older firms at their IPO dates. From the perspective of the evolution in their tax shields, the results from these tests suggest that the younger firms are at an earlier stage in their development toward relying more on investment tax shields and less on debt tax shields.

6.2 Thesis Contributions

This thesis makes several contributions to the empirical literature on corporate tax shields. First, the research design applied in the thesis represents a more complete specification of the capital structure problem by isolating the influence of changing adjustment costs on firms' financing and investment decisions. Hypotheses H_1 and H_2 were tested in a longitudinal

setting with firms observed for their first nine public years. This research concerns the efficient design of organizations by examining dynamic re-financing costs relative to the static subsidy available on debt and investment-related tax shields. The evidence reported in the thesis supports the two predictions, which provides a partial explanation for the paucity of studies finding that taxes affect firms' financing and investment policies.

Second, this thesis contributes to the empirical capital structure literature by evaluating the time-series properties of firms' responses to tax incentives. Extant research examines either cross-sectional variation or time-series variation arising from changes in certain macroeconomic conditions.

Third, this thesis answers Slemrod and Shobe's (1990) and Shevlin's (1999a) call for research that more precisely examines non-tax costs and the time-series pattern in firms' reactions to tax incentives, respectively. This was largely accomplished by exploiting the features of panel data to control for unobserved firm-specific effects to avoid omitted variable bias and to refine the observation of within-firm dynamics.

6.3 Limitations

The purpose of the empirical tests in this thesis is to provide evidence to justify inferences about the population of initial public offerings occurring between 1977 and 1988. However, the coefficient estimates from the fixed effects tests only pertain to the firms in the sample, which represents only 5.5 percent of these IPOs according to Table 4.1. Additional procedures were performed to examine whether the evidence warrants generalizing to the population, including:

(i) Estimating with a random effects model that assumes that the firm-specific influences are sampled from an unknown population.

(ii) Re-estimating both the fixed and random effects models on the unbalanced panel, which contains 18 percent of the population of initial public offerings.

(iii) Implementing Heckman's (1976, 1979) two-step estimation procedure that is designed to generate consistent estimates in the presence of attrition; i.e., this eliminates the potential selection bias when the sample is not random.

(iv) Re-estimating both the fixed and random effects models on both the balanced and unbalanced panels after replacing the marginal tax rate (MTR) variable with proxies that reduce sample attrition. Specifically, in separate regressions, measuring firms' tax status with an alternate trichotomous proxy and Graham et al's (1998) before-financing simulated marginal tax rates enlarges the sample to 9.7 percent and 12.7 percent of the original population of initial public offerings, respectively.

These tests together provide consistent evidence supporting the thesis hypotheses on the time-series variation in firms' tax-induced financing and investment policies. Nevertheless, with the sample still only containing a small portion of the population in each of these tests, there remains the possibility that survivorship bias affects the results reported in the thesis.

Another limitation of this research is that the empirical tests only estimate firms' substitution to investment-related tax shields. In their seminal text on managerial tax planning, Scholes and Wolfson (1992) stress that firms consider the tax and non-tax implications of their entire portfolio of available strategies. This implies that firms prevented from adjusting their capital structures by re-financing costs may begin to shift toward relying more on a variety of non-debt tax shields other than the depreciation, depletion allowances, and investment tax credits examined in this thesis. For example, Trezevant (1994) explores firms' substitution of pension contributions, advertising expenditures, and the provision for bad debts in reaction to legislation enacted in 1981 that made investment-related tax shields more attractive.[70] In fact, Scholes and Wolfson (1989a) criticize extant empirical capital structure studies for not factoring other non-debt tax shields into their research designs.

Further, investment-related tax shields might be an imperfect candidate for substitution when firms' financing decisions become more constrained over time. Unlike Canadian firms that may store unused depreciation deductions, American firms must either fully deduct or forfeit the permissible amount of depreciation (IRC Regulation 1.167(a)-10). This not only provides justification for DeAngelo and Masulis's (1980) modeling of leverage as being adjusted in response to investment-related tax shields, but also intimates that this thesis ignores the costs of adjusting firms' asset structures by focusing only on re-financing costs.

[70] The Economic Recovery Tax Act (ERTA) of 1981 introduced accelerated cost recovery system (ACRS) depreciation and more generous investment tax credit provisions, which altered the incentives provided through investment-related tax shields.

6.4 Potential Extensions

The thesis research could be extended by testing the predictions in other empirical settings such as firms emerging from bankruptcy protection. These newly re-organized firms may resemble recently public firms since they also may not have had the opportunity to accumulate the re-financing costs that impede tax-efficient leverage adjustments. For example, Gilson (1997) provides evidence that, relative to out of court debt restructuring, the lower transactions costs incurred with Chapter 11 protection gives financially distressed firms more flexibility to choose optimal capital structures.

In fact, this may be a cleaner setting than the recently public firms since bankrupt firms can essentially select a brand new capital structure before leaving Chapter 11 (Alderson and Betker, 1995).[71] In contrast, evidence in this thesis suggests that post-IPO firms have capital structures that are partially inherited from their pre-IPO history of financing and investment decisions. Initial inquires suggest that a ten-year panel of about 150 firms that emerged from bankruptcy protection can be compiled from various sources.

In addition to estimating the time-series variation in tax-induced financing and investment policies for firms having left bankruptcy protection, firms could be observed following other major re-capitalizations such as seasoned equity offerings. It could be argued that firms delay adjusting their capital structures until the benefits obtained exceed the re-financing costs that would be incurred. This implies that firms' financing decisions would become increasingly insensitive to their marginal tax rates as their re-financing costs accumulate in the intervening period between major re-capitalizations.

Another research project could involve investigating whether young firms react more quickly to changes to tax laws affecting their financing incentives. This follows from the evidence reported in this thesis that supports Scholes and Wolfson's (1989a) argument that these firms have more flexible capital structures. For instance, studies that examine the impact of tax regime shifts on financing decisions such as Givoly et al (1992) and Schulman et al (1996) could be re-evaluated for evidence that younger firms are more responsive to these changes.

[71] However, examining companies that recently left bankruptcy protection may present other difficulties. For example, there is evidence that firms released from Chapter 11 frequently return to bankruptcy –

The research design in this thesis specifies depreciation, depletion, and investment tax credits as the non-debt tax shield that firms shift toward when re-financing costs eventually prevent changes to their leverage. This choice was partly motivated by the extensive extant theory (e.g., DeAngelo and Masulis, 1980; and Dammon and Senbet, 1988) and cross-sectional evidence (e.g., Bradley et al, 1984; and Dhaliwal et al, 1992) on investment-related tax shield substitution. However, it is certainly plausible that firms would decide to adjust other non-debt tax shields when their capital structures are seriously constrained. For example, Shih (1995) reports that investment-related tax shields represent only a small fraction of the substitutes available to firms according to the U.S. tax return data published in *Statistics of Income: Corporate tax returns.*

This thesis presents indirect evidence that re-financing costs induce firms to gradually drift farther from their optimal capital structures over time. Alternatively, direct proxies for adjustment costs could be specified to measure capital structure flexibility and complexity using data collected from firms' 10K reports. For example, Gilson (1997) develops several proxies including:

(i) the extent to which firms finance with securities that have intrinsic repayment flexibility such as convertible debt;

(ii) the extent to which firms finance with securities that permit them to opportunistically redefine the event of default by, for example, exercising the option to extend debt maturity or the option to accrue, rather than pay, interest;

(iii) the quantity of debt contracts; e.g., covenants often prohibit subsequent debt issues which implies that more contracts result in more capital structure restrictions: and

(iv) the proportion of public debt in firms' capital structures relative to private debt since holdout problems are thought to be more severe when creditors are widely dispersed.

In addition, having a history of defaulting on loans would almost certainly reduce capital structure flexibility by subjecting violators to additional constraints. Sweeney (1994) reports that specific costs frequently incurred by defaulting firms include restrictions on additional borrowing, increased collateralization, and the imposition of additional covenants. Another potential proxy for re-financing costs is the fraction of leverage in short-term debt, which

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Altman (1993) and Hotchkiss (1995) report high recidivism rates.

Scholes and Wolfson (1989a, 1992) argue, although more expensive than long-term debt, provides more flexibility.

Finally, the theories of Miller (1977) and DeAngelo and Masulis (1980) argue that some portion of the corporate tax advantage of debt is recovered at the personal level. Graham et al (1998) modify their before-financing simulated MTR to incorporate the personal tax penalty on debt and find that personal taxes affect capital structure. The research design described in this thesis could be extended by similarly conditioning tax status on investors' personal tax rates.

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