

# **THREE ESSAYS ON CAPITAL STRUCTURE**

by

**GUANGZHONG LI**

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_____	_____
Date	Prof. Armen Hovakimian
	Chair of Examining Committee

_____	_____
Date	Prof. Joseph Weintrop
	Executive Officer

Prof. Armen Hovakimian	_____
Prof. Linda Allen	_____
Prof. Jay Dahya	_____
Prof. Joseph Weintrop	_____
Supervisory Committee	

THE CITY UNIVERSITY OF NEW YORK

# **ABSTRACT**

## THREE ESSAYS ON CAPITAL STRUCTURE

by

GUANGZHONG LI

Adviser: **Professor Armen Hovakimian**

In the first chapter, various target proxies and modifications to the standard methodologies are examined to identify partial-adjustment and debt-equity choice models that have power to reject the target adjustment hypothesis. The resulting estimates of the speed of adjustment are in the range of five-thirteen percent per year.

The second chapter suggests that the estimates of the speed of adjustment to target leverage tend to be significant but low. I use both ex-ante and ex-post information to identify situations when the adjustment should be full. I find no evidence of full adjustment. The adjustments to target are rarely full, with many firms adjusting beyond or away from the target. The results imply that empirical analyses, such as partial adjustment regressions, that rely on the existence of a well-defined target debt ratio may be ill-suited for quantifying the importance of dynamic tradeoff behavior vis-a-vis alternative theories.

The third chapter shows that shorter debt maturity structure increase firm value for growth firms. The liquidity effect is another important determinant of valuation effects of shorter debt maturity structure. The analysis on corporate investment supports these findings.

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To the persons who love me and whom I love so much, I dedicate this dissertation.

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# CHAPTER 1

## In search of conclusive evidence: how to test for adjustment to target capital structure

### 1. Introduction

The concept of target leverage plays an important role in capital structure theory. According to the tradeoff theory of capital structure, firms choose their target debt ratios by trading off tax and other benefits of debt financing against financial distress and other costs of debt. Consistent with this theory, numerous studies find that observed leverage ratios relate to firm characteristics such as size, growth opportunities, collateral (liquidation) value of assets, and the marginal tax rate in a manner consistent with the predictions of the tradeoff theory (e.g., Titman and Wessels (1988), Rajan and Zingales (1995), Graham (1996a)).

Modern tests of the tradeoff theory also recognize that adjustment costs may prevent firms from continuously adjusting to their target capital structures.<sup>1</sup> As a result, firms may adjust to target debt ratios only occasionally, when the benefits of adjusting to the target exceed the adjustment costs. The tests of such dynamic target-adjustment behavior tend to fall into two categories.

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<sup>1</sup> See, for example, the dynamic capital structure models of Fischer, Heinkel, and Zechner (1989), Goldstein, Ju, and Leland (2001), and Strebulaev (2007).

One group of studies (e.g., Marsh (1982), Jalilvand and Harris (1984), Bayless and Chaplinsky (1991), MacKie-Mason (1990), Hovakimian, Opler, and Titman (2001)) uses qualitative dependent variable models to examine how firm characteristics affect the issuing (repurchasing) firm's debt vs. equity choice. Consistent with the tradeoff theory, these studies find that the probability of issuing debt rather than equity or repurchasing equity rather than debt declines with the extent to which the firm's debt ratio exceeds its target.

Another group of studies examines the average speed with which firms adjust toward their target debt ratios using partial adjustment regression models.<sup>2</sup> The estimated magnitude of the speed of adjustment (SOA) has important implications for the tradeoff theory. Specifically, if it takes an average firm five years to offset 35-40 percent of the initial deviation from the target, as in Kayhan and Titman (2007), then target leverage can be viewed as a secondary factor in corporate financing decisions, at best. If, in contrast, the average adjustment speed is on the order of 35 percent per year, as in Flannery and Rangan (2006), then target leverage is of central importance.

In two recent studies, Shyam-Sunder and Myers (SSM, 1999) and Chang and Dasgupta (CD, 2008), argue that partial adjustment regression tests are seriously flawed. Specifically, they show that such regressions generate significantly positive speeds of adjustment to target even on simulated datasets where, by design, firms do not exhibit target adjustment behavior.

In this paper, we apply the simulation methods of Shyam-Sunder and Myers (1999) and Chang and Dasgupta (2008) to assess various specifications of the two main empirical models used to test the target adjustment hypothesis – the partial adjustment model and the debt-equity choice model. Our goal is to identify specifications that lead to meaningful tests of the

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<sup>2</sup> See, e.g., Fama and French (2002), Flannery and Rangan (2006), Antoniou, Guney, and Paudyal (2007), Huang and Ritter (2007), Kayhan and Titman (2007), Lemmon, Roberts, and Zender (2007).

hypothesis and draw conclusions based on these tests.

The implementation of these target-adjustment tests is complicated because target leverage is not observable and a proxy for target has to be used. The most popular approach in recent years has been to estimate the target debt ratio as the fitted value from a regression of observed debt ratios on a set of firm characteristics believed to proxy for the factors identified by the tradeoff theory as important determinants of the target. This is done either explicitly, by estimating the target in the first stage and then using it as an independent variable in the second stage (see, e.g., Fama and French (2002)), or implicitly, by estimating a reduced form model in a single step (see, e.g., Flannery and Rangan (2006)).

This means that all empirical tests of adjustment to target are, in fact, tests of a joint hypothesis that (1) firms adjust to target capital structure and (2) the target proxy approximates the true target relatively well. Earlier studies frequently assumed that using a proxy for the target can only weaken the evidence of adjustment to target and interpreted evidence of significant adjustment effects as validation of the target proxies used. The results in Shyam-Sunder and Myers (1999) and Chang and Dasgupta (2009) imply that this is not necessarily the case.

To investigate the impact of the choice of a target proxy on empirical evidence of adjustment to target, we generate a number of different proxies for target leverage, relying on prior literature for guidance. Specifically, we differentiate between proxies that model the target as a function of time-varying firm characteristics and those that do not. Similarly, we differentiate between proxies that account for unexplained cross-firm time-invariant heterogeneity in leverage ratios and those that do not.<sup>3</sup> We also differentiate between proxies that

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<sup>3</sup> The primary approach used in the literature to account for unexplained cross-firm heterogeneity in leverage is to use regressions with fixed firm-specific effects. Flannery and Rangan (2006) argue that firm-level fixed effects regressions generate sharper target leverage estimates, which explains the higher estimates of adjustment speeds generated by these regressions.

utilize ex-post information (e.g., via full sample panel regressions) and those that rely on ex-ante information only (e.g., via rolling historical panel regressions). Both approaches have been used in prior literature.

We use these proxies to estimate both single- and two-step partial-adjustment models as well as debt-equity choice models. Our results show that the highest estimates of the speed of adjustment (0.328 – 0.357) are obtained with models of target leverage that utilize full-sample data with fixed firm effects, both single-step and two-step. The estimates of the speed of adjustment (SOA) generated by two-step models with target leverage estimated in the first step using historical data with fixed firm effects are about twice as low (0.147 – 0.169). The lowest SOAs (0.127 – 0.133) are obtained with OLS regressions without fixed effects, whether one-step or two-step.

When these models are estimated on data simulated using financing choices that follow the pecking order (in the spirit of Shyam-Sunder and Myers (1999)) or are determined by a flip of a coin (in the spirit of Chang and Dasgupta (2008)), the patterns remain the same. The highest estimates of the speed of adjustment (0.256 – 0.284) are generated by full-sample fixed effects models of target leverage (one-step and two-step). The results are statistically significant and suggest that the estimates of the speed of adjustment produced by these models are significantly biased in favor of the target adjustment hypothesis. The OLS and historical fixed effects models of the target generate much lower SOAs (0.074 – 0.094) in the simulated data, though these estimates are still statistically significant. Single-step fixed effects models estimated using methods that account for the bias in dynamic fixed effects panel models (e.g., system GMM of Blundell and Bond (1998)) generate estimates of the speed of adjustment that are similar to the OLS and historical fixed effects speeds of adjustment in both magnitude and significance.

Even more surprisingly, the estimation results for debt vs. equity issue and equity vs. debt repurchase choice models also show the presence of a significant bias. Marginal effects of the deviations from OLS targets on the probability of debt vs. equity issuance are of similar magnitude and significance in both real data (0.242 – 0.275) and flipped-coin data (0.242 – 0.260). The marginal effects obtained on the real data using fixed effects targets (0.384 – 0.891) somewhat exceed those obtained on the flipped-coin data (0.309 – 0.809), but the latter estimates are still significant, both statistically and economically. Similar results are obtained from the security repurchase models.

We next examine the sources of these biases. First, we note that although full sample fixed effects targets generate SOAs that are 2 – 2.5 times higher than the SOAs generated using historical fixed effects targets, this difference appears to be 100% due to the larger bias introduced by the full sample fixed effects targets. Further analysis suggests that the substantially higher bias with full sample fixed effects targets is due to the fact that these targets incorporate ex-post information about the debt ratios. Specifically, our experiments show that adding a single ex-post year (year 0) to the historical sample (years -1, -2, etc.) used to estimate the fixed effects targets, more than doubles the real-data SOAs from 0.147 – 0.169 to 0.357 – 0.393 and increases the  $R^2$  of the partial adjustment regression from about three percent to about ten percent. Similar results are observed in both simulated datasets. The fact that adding a single ex-post year to the fixed effects target regressions results in such dramatic increases in the estimated speeds of adjustment implies that these increases are due to a look-ahead bias.

Our results also show the presence of “mechanical” mean reversion suggested by Chang and Dasgupta (2008). Both in the simulated and the real data, there are no observations with debt ratios less than zero or greater than one. Thus, debt ratios of zero can either go up or remain

constant, but cannot go down. Similarly, debt ratios of one can either go down or remain constant, but cannot go up. The resulting “mechanical” mean-reversion makes the lagged debt ratio a strong predictor of both changes in leverage and of the debt vs. equity financing choice. Indeed, the effect of the lagged debt ratio is so strong that the impact of the deviation from the target on both the change in leverage and on the debt vs. equity choice remains statistically and economically significant when we replace the estimated targets with randomly generated target debt ratios. Similarly, a one-step model with only the lagged debt ratio on the right hand side generates economically and statistically significant estimates of the speed of adjustment. These results indicate that standard tests of target adjustment behavior can generate statistically and economically significant evidence of adjustment even when the target lacks any economic meaning.

For a more meaningful test of the hypothesis that firms adjust to our proxy for the target debt ratio, we enter the target and the lagged debt ratios separately into the second stage partial adjustment or debt vs. equity choice regressions and focus on the coefficient estimate on the target proxy. This approach substantially improves our ability to reject the target-adjustment hypothesis when it is incorrect. Needless to say, the coefficients on random targets are always insignificant. In partial-adjustment regressions, the coefficient estimates on historical fixed effects targets become economically trivial (1%-3%) in both the pecking order and the flipped coin datasets, although they remain statistically significant with all but one target proxy. The effects of target proxies on debt vs. equity repurchase regressions also become statistically insignificant. Only in the debt vs. equity issue choice regressions estimated on the flipped coin sample most of our target proxies remain statistically and economically significant.

A substantial number of observations in our simulated datasets are close to the boundary

values of 0 and 1. The mean-reversion bias could be especially severe for these observations. While the real data shows similar clustering of observations close to 0, the fraction of observations with very large debt ratios is substantially smaller than in the simulated datasets. The higher frequency of very high debt ratios in the simulated datasets could make these datasets more susceptible to the mean-reversion bias than the real data. Indeed, when we exclude observations with debt ratios above 0.8, the coefficient estimates on all proxies for the target that do not rely on future information are insignificant in debt vs. equity issue and repurchase choice regressions estimated on the flipped-coin data.

The magnitudes of the estimates suggest that a ten percentage point deviation from the target may lead to a change in the probability of debt vs. equity issuance of up to 7-8 percentage points and to a change in the probability of debt vs. equity repurchase of 1-2 percentage points, depending on the target proxy. The estimated speeds of adjustment are in the range of 7-9 percent per year. These estimates are at the low end of the range of estimates from earlier studies of target adjustment. In the context of dynamic tradeoff models of capital structure with fixed adjustment costs and infrequent rebalancing (e.g., Fischer, Heinkel, and Zechner (1989), Goldstein, Ju, and Leland (2001), and Strebulaev (2007)), these values imply that an average firm actively rebalances to its target capital structure only once in eleven to fourteen years.

These long adjustment times suggest that either adjusting to target capital structure is not a high priority goal for an average firm or the empirical models currently used in the literature are not well-suited to identify the ways in which firms facing various tradeoffs manage their debt ratios. While the focus of the current paper was on identifying methods that would allow us to reject the target adjustment hypothesis when firms do not adjust to the target, understanding the reasons behind the relatively low economic importance of target debt ratios in partial adjustment

and debt-equity choice models is a priority for future capital structure research.

The paper proceeds as follows. Section 1 describes the proxies for target leverage. Section 2 presents partial adjustment regression results for the real and for the simulated datasets. Section 3 presents the debt-equity choice results. Section 4 examines the sources of bias. Section 5 presents modified partial-adjustment and debt-equity choice tests of the tradeoff theory. Section 6 summarizes our conclusions.

## **2. Target Leverage**

### **2.1 Data**

Our initial sample is drawn from Annual Compustat files covering the 1970-2007 period.<sup>4</sup> To minimize the influence of outliers and misreported data in our analysis, we exclude firms with values of total assets or sales less than one million dollars and replace extreme observations of all ratio variables with missing values.<sup>5</sup> As in other studies of capital structure, we exclude financial firms (SIC codes 6000-6999) from the sample. For second stage regressions, we limit the sample to observations for which all target proxies are available and for which the change in the debt ratio can be calculated. The resulting sample consists of 105,995 firm-year observations. Table 1 presents the sample averages for firm characteristics important for our subsequent analysis.

### **2.2 Target leverage regressions**

As in many earlier studies of capital structure (e.g., Rajan and Zingales, 1995), we measure leverage as the book debt ratio, BDR, defined as the sum of long-term and short-term

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<sup>4</sup> Because we need to calculate changes in leverage and use lagged values of firm characteristics to estimate target debt ratios, observations used in partial adjustment regressions start in 1974.

<sup>5</sup> Extreme values are defined as those in one percent of both tails of the distribution. Variables that are censored at zero, e.g, R&D, are not trimmed on the censored side. Leverage is trimmed to exclude values in excess of one.

debt scaled by the book value of assets.<sup>6</sup> We model the target level of leverage as the predicted value from the following regression of observed leverage ratios.

$$BDR_{it+1} = \beta X_{it} + \varepsilon_{it+1}. \quad (1)$$

Following Hovakimian, Opler, and Titman (2001), the set of independent variables,  $X$ , used in our base specification includes firm size, asset tangibility, market-to-book, and research and development (R&D) expenses.<sup>7</sup> Because a large number of firms with no R&D do not report it, we replace missing values of R&D with zeroes. Since firms that do not report R&D may be different from those that do, we include an indicator variable set to one for firms with non-missing R&D. Bradley, Jarrell, and Kim (1984) document strong industry effects in the cross-section of corporate leverage ratios, suggesting that there could be otherwise unobservable determinants of capital structure that vary across industries, but are relatively constant across firms within the same industry. We, therefore, include industry median leverage ratio as an additional independent variable in regression (1).<sup>8</sup>

Our base specification of regression (1) does not include profitability and depreciation, considered earlier by Flannery and Rangan (2006), among others. Earlier literature consistently finds a negative relation between profitability and observed debt ratios. We find such a relation in our sample as well. The negative relation between profitability and leverage is usually attributed to preference for internal (over external) funds exhibited by firms that follow pecking

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<sup>6</sup> Leverage, BDR, is [short-term debt (Compustat data 34) + long-term debt (data 9)]/assets (data 6).

<sup>7</sup> These variables have also been considered by Titman and Wessels (1988), Rajan and Zingales (1995), and others. Size is the natural log of sales (data 12), adjusted for inflation. Tangibility is the property, plant, and equipment (data 8) scaled by total assets. R&D is the research and development expense (data 46) scaled by sales. Market-to-book is (total assets – book equity + market equity)/total assets. Market equity is price (data 199) times shares outstanding (data 25). Book equity is the book value of stockholders' equity, plus balance sheet deferred taxes and investment tax credit (if available), minus the book value of preferred stock. Depending on availability, we use the redemption (data 56), liquidation (data 10), or par value (data 130) to estimate the book value of preferred stock. Stockholders' equity is (data 216), if it is available. If not, we measure stockholders' equity as the book value of common equity (data 60) plus the par value of preferred stock, or the book value of assets minus total liabilities (data 181).

<sup>8</sup> We use forty-nine Fama-French industry definitions throughout this paper. These definitions are downloaded from Kenneth French's online data library.

order and have no leverage targets. The relation can also arise if firms have target debt ratios, but do not immediately offset the effects of random shocks to profitability on leverage because capital structure adjustments are costly. In estimating regression (1), our goal is not to test the extant theories of capital structure, but to obtain a proxy for target leverage calculated as the predicted value based on parameter estimates of regression (1). As neither of the above interpretations implies that more profitable firms should have lower *target* debt ratios, we do not include profitability in our base specification of the target leverage regression.

Similarly, although the primary motivation for inclusion of depreciation is its association with non-debt tax shields, the empirical relation between depreciation and leverage documented in earlier studies (and in this paper) tends to be positive, which is inconsistent with that interpretation. Nevertheless, to ensure that our findings are not driven by the omission of these variables from our base specification, we also estimate a specification with a set of independent variables extended to include profitability and depreciation.

Table 2 presents three sets of results for target leverage regression (1) estimated on our full sample. The first set of results is for our base specification estimated using OLS regression. Consistent with earlier studies and the theoretical predictions that firms with higher financial distress costs should target lower debt ratios, the debt ratios increase with tangibility and decline with market-to-book and R&D. Consistent with Bradley, Jarrell, and Kim (1984), the results show the presence of significantly positive industry effects.

The second set of results is for the base specification of the target debt ratio model estimated using fixed firm effects regressions. The inclusion of fixed effects is motivated by the finding of strong persistence in corporate debt ratios in Lemmon, Roberts, and Zender (2007). Most of the results here are qualitatively similar to the OLS results, except R&D switches the

sign and firm size gains (positive) significance. The latter result is traditionally viewed as reflecting the lower cash flow risk of larger firms.

The third set of results is for the specification with the extended set of independent variables estimated using fixed firm effects regression. The impact of profitability on leverage is significantly negative whereas the impact of depreciation is significantly positive. As we discussed earlier, these results are consistent with the earlier literature, but are inconsistent with the standard predictions based on tradeoff theory of capital structure. All other variables are significant, with signs consistent with the tradeoff theory.

### 3. Partial Adjustment Regressions

In this section, we examine how well the deviations from various proxies for the target debt ratio predict changes in the debt ratio using the following standard partial adjustment model.

$$BDR_{i,T+1} - BDR_{i,T} = \alpha + \lambda(BDR_{i,T+1}^* - BDR_{i,T}) + \varepsilon_{iT+1}, \quad (2)$$

where

$$BDR_{i,T+1}^* = \hat{\beta}X_{iT}. \quad (3)$$

Equation (3) states that  $BDR^*$  is the predicted value obtained from regression (1). In the absence of frictions, firms would fully adjust to their target each period. In the presence of adjustment costs, firms would not continuously fully adjust. In this framework,  $\lambda$  measures the average speed of adjustment to the target.

A number of studies (e.g., Flannery and Rangan (2006)) estimate the speed of adjustment in a single step by substituting (3) into (2) and rearranging to obtain the following reduced-form partial adjustment model.

$$BDR_{i,T+1} = \alpha + \lambda\hat{\beta}X_{iT} + (1 - \lambda)BDR_{i,T} + \varepsilon_{iT+1}. \quad (4)$$

In subsequent sections, we report estimates generated both by single-step models (4) and by two-

step models with regression (1) estimated in the first stage and regression (2) estimated in the second stage. Both approaches have been used in the literature. For our purposes, the two-step approach is a bit more flexible, allowing us to examine some target model specifications that cannot be implemented using one-step approach as we discuss below.

There are a number of econometric issues with both the two-stage model (1)-(2) and the one-step partial adjustment model (4). The basic issue is that the target debt ratio,  $BDR^*$ , is unobservable and the complete set of its determinants is not known and/or observable. This means that  $BDR^*$  is measured with error, leading to attenuation of the estimate of the speed of adjustment in the second-stage regression (2). In one-step model (4), the incompleteness of the set of determinants of the target,  $X$ , leads to an omitted variables problem, which can bias the estimate of  $1-\lambda$ . Introducing firm-level fixed effects into the target debt ratio model mitigates these issues by removing concerns about those omitted variables that vary across firms but are constant in time.

Using target debt ratios estimated based on full sample regressions such as those reported in Table 2 could be problematic. Specifically, such an approach uses information about future debt ratios to estimate the current target, which could lead to look-ahead bias. To avoid this, in both one-step and two-step approaches, we also use cross-sectional regressions that estimate the target based on current information only. A potential drawback of this cross-sectional approach is that it does not incorporate information about unexplained cross-firm heterogeneity in debt ratios. We address this drawback by adding a set of regressions where the target is estimated using historical panel data that excludes any future information. This last approach can be applied only to the two-step version of the partial adjustment model.

The analysis that follows allows us to empirically assess the magnitude of an upward bias

in the estimates of  $\lambda$  produced by various implementations of the partial adjustment models (1)-(2) and (4). Although the presence of a downward bias remains a real possibility, it is beyond the scope of this paper to assess the extent of such a bias.

Panels A, B, and C of Table 3 present the speeds of adjustment, t-statistics, and  $R^2$  based on second-stage regression (2) estimated with ten different proxies for the target. Panel D presents the estimates from five econometric implementations of the one-step partial-adjustment model (4). The first set of estimates in Table 3 is for the actual data, the second set is for the pecking order simulated data, and the third set is for the flipped-coin simulated data.

### **3.1 Estimates of the speed of adjustment: Actual data**

The estimates in Panels A, B, and C are in the spirit of Fama and MacBeth (1973). Specifically, the second stage regressions are estimated annually on cross-sectional data. The reported SOAs and  $R^2$  are the averages of these annual estimates. The reported t-statistics are based on the time-series standard errors of the average coefficients. This approach follows Fama and French (2002) who find that cross-sectional correlation in residuals leads to substantially inflated t-statistics in pooled cross-section time-series regressions. They also find that autocorrelation in annual slopes of second stage partial-adjustment regressions is not a serious problem.<sup>9</sup>

An additional benefit of this approach is that year-by-year variation in SOA estimates, which determines their standard errors, incorporates the estimation error due to the fact that target debt ratios used in Panels A and B are estimated by annual first stage regressions. We have experimented with alternative ways to calculate the t-statistics such as using robust standard errors with annual clustering as well as bootstrapped standard errors with annual clustering. The

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<sup>9</sup> Fama and French (2002) find that the autocorrelation in annual slopes is a serious problem for regressions of leverage levels but not leverage changes.

results were similar, however, Fama-MacBeth t-statistics tended to be more conservative.<sup>10</sup>

Panel A presents the results based on cross-sectional estimates of the target,  $BDR^*$ . Specifically,  $\hat{\beta}$  is obtained by cross-sectional regression (1) of debt ratios,  $BDR$ , observed in year  $T$  on firm characteristics,  $X$ , observed in year  $T-1$ . Proxies for target at time  $T+1$ ,  $BDR_{i,T+1}^*$ , are then generated by the predicted value  $\hat{\beta}X_{iT}$ , i.e., out of sample.<sup>11</sup> The base specification (A1) of the target generates an adjustment speed of 0.128. Adding profitability and depreciation to form the expanded specification (A2) results in a trivial change in the speed of adjustment (0.132).

In Panel B, we use historical panel data to obtain  $\hat{\beta}$ . Specifically,  $\hat{\beta}$  is obtained by panel regression (1) of debt ratios,  $BDR$ , observed in years 2 through  $T$  on firm characteristics,  $X$ , observed in years 1 through  $T-1$ . Proxies for target at time  $T+1$ ,  $BDR_{i,T+1}^*$ , are then generated by the predicted value  $\hat{\beta}X_{iT}$ , i.e., out of sample. The base specification (B1) of the target generates an adjustment speed of 0.127. Adding firm fixed effects to account for persistence in corporate debt ratios raises the adjustment speeds to 0.163 (B3). Expanding the set of independent variables to include profitability and depreciation (B4) does not substantially alter the estimated speed of adjustment (0.169).

A comparison of the results based on target specifications (A1) and (B1) implies that simply adding years of historical information to estimate target leverage ratios does not produce “better” estimates of the target in terms of the speed of adjustment or the fit of the second stage partial-adjustment regression. The benefits of historical data are apparent only when fixed effects

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<sup>10</sup> Petersen (2009) finds that clustered standard errors are downward biased when the number of annual clusters is small.

<sup>11</sup> The predicted value here is generated by applying the historical coefficient estimates to updated firm characteristics (as of time  $T$ ).

regressions are used to estimate the target. Indeed, a comparison of specifications (B1), (B2), and (B3) suggests that most of the effect of firm characteristics on target comes from cross-sectional variation and, thus, overlaps with the effects of firm-level indicators. The incremental impact of introducing fixed effects into the first stage target regression (switching from (B1) to (B3)) on the speed of adjustment in the second stage is higher than the incremental impact of adding time-varying firm characteristics to fixed effects only first stage model (switching from (B2) to (B3)). These results are consistent with the conclusions in Lemmon, Roberts, and Zender (2007) that target debt ratios are to a large extent time-invariant and vary primarily in the cross-section. We should note, however, that the fit ( $R^2$ ) of the second stage partial-adjustment model is lower with fixed-effects based proxies for target leverage.

In Panel C, instead of using historical debt ratios up to year  $T$ , the full sample is used to estimate  $\hat{\beta}$ . Specifically,  $\hat{\beta}$  is obtained by panel regression (1) of debt ratios,  $BDR$ , observed in years 2 through  $N$  on firm characteristics,  $X$ , observed in years 1 through  $N-1$ . Proxies for target at time  $T+1$ ,  $BDR_{i,T+1}^*$ , are then generated by the predicted value  $\hat{\beta}X_{iT}$ , i.e., in sample.

We find that, when the target proxy comes from a first stage pooled OLS regression (C1), the estimated adjustment speed (0.127) is comparable to the estimates based on target specifications (A1) and (B1). Thus, using a longer time-series and ex-post data does not lead to higher estimates of the speed of adjustment to the target. An introduction of fixed firm effects in the first stage regressions increases the estimated speed of adjustment in the second stage (C3) to 0.350 and increases the  $R^2$  of the partial adjustment regression to 15.6 percent. The full sample fixed effects target specifications with expanded set of independent variables (C4) and with no time-varying variables at all (C2) produce very similar SOAs and  $R^2$ . These results provide very strong support for conclusions in Lemmon, Roberts, and Zender (2007). As we show in the next

section, however, fixed effects estimators of target leverage severely bias the partial adjustment results in favor of the tradeoff theory when ex-post information is used to generate proxies for the target (specifications (C2), (C3), and (C4)).

In Panel D, the estimates of the speed of adjustment are obtained in a single step. Estimates using several econometric specifications are reported, with all, except (D3), based on the expanded set of factors as presented in Table 2. In specification (D1), partial adjustment model (4) is estimated using pooled time-series cross-section regressions with standard errors robust to heteroscedasticity and firm-level clustering. In specification (D2), partial adjustment model (4) is estimated using Fama-MacBeth approach. The estimates of SOA (0.139 for (D1) and 0.136 for (D2)) are very similar to the estimates from two stage OLS regression models without fixed effects (specifications (A1), (A2), (B1), and (C1)).

In specification (D3), partial adjustment model (4) is estimated with fixed firm effects and no time-varying firm characteristics. Specification (D4) adds firm characteristics to the fixed effects specification (D3). The estimates of SOA (0.330 and 0.352) are practically identical to the estimates from two stage models that utilize full panel fixed effects regressions in the first stage ((C2), (C3), and (C4)).

Fixed effects regressions with lagged dependent variable can lead to biased coefficient estimates, especially when estimated on short panel (Wooldridge (2002)). A number of approaches have been used to resolve this problem in fixed effects partial adjustment model (4). Faulkender, Flannery, Hankins, and Smith (2007) and Lemmon, Roberts, and Zender (2008) use system GMM approach of Blundell and Bond (1998), GMMBB, that is designed to address the bias in dynamic panel models with fixed effects. Huang and Ritter (2008) use long-differencing technique of Hahn, Hausman, and Kuersteiner (2005), which they argue is less biased than

GMM.

In specifications (D5) and (D6), we report respectively GMMBB and long-differencing estimates of the speed of adjustment.<sup>12</sup> Both the GMMBB (0.203) and long differencing (0.195) estimates of the speed of adjustment are lower than the estimates from full sample fixed effects models (C2-C4, D3, D4) but are higher than all the other estimates in Table 3.

### **3.2 Estimates of the speed of adjustment: Pecking order simulated data**

The results in the previous section of the paper show that the target adjustment model generates speeds of adjustment in the range of 12.7-35.7 percent depending on the target specification used. A legitimate question to ask is whether the adjustment speeds of the magnitudes observed in our sample could be generated spuriously even if firms did not target specific debt ratios. This issue is raised by Shyam-Sunder and Myers (1999) and Chang and Dasgupta (2008), who argue, based on simulation experiments, that partial adjustment regressions have no power to reject the target adjustment hypothesis. SSM, for example, show that they obtain a statistically and economically significant speed of adjustment (0.23) even when they estimate a partial adjustment regression model on data simulated according to pecking order behavior.

In this section, we conduct SSM-style simulation experiments and reestimate the discussed sixteen specifications of the partial adjustment model on data simulated on the basis of pecking order financing behavior. Specifically, for each sample firm, we start with its chronologically initial debt ratio and simulate a path of future debt ratios under the assumption that firms follow the pecking order of financing but that this does not affect the asset side of their balance sheets. In other words, we assume that financing deficit and surplus are exogenous with

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<sup>12</sup> The length of the time over which the change in leverage is calculated using this estimator is 8 years as in main results of Huang and Ritter (2007).

respect to the type (debt or equity) of the securities issued or repurchased and that the actual financing deficits observed in each year are fully financed with debt, whereas financing surpluses are fully used to retire a portion of outstanding debt. If the firm has no outstanding debt, the financing surplus is used to repurchase equity. If the debt ratio reaches one, the financing deficit is covered with equity issuance.

The second set of results reported in Table 3 represents the estimates obtained for this simulated dataset. The results show that all versions of the partial adjustment model generate statistically significantly positive estimates of the speed of adjustment, despite the fact that the data are generated using pecking order financing behavior. Thus, none of the partial adjustment specifications have power to reject the target adjustment hypothesis in statistical terms.

The bias is especially large for models utilizing full-sample fixed effects targets (both one-step and two-step). The estimates of the speed of adjustment generated by these models (specifications (C2), (C3), (C4), (D3), and (D4)) vary between 0.262 and 0.284. These estimates are statistically significant and economically large and, in fact, are close to Shyam-Sunder and Myers (1999) estimates.

In contrast, one-step and two-step models without fixed effects ((A1), (A2), (B1), (C1), (D1), and (D2)) and two-step models with fixed effects using only ex-ante information ((B2), (B3), and (B4)) produce much smaller estimates of the speed of adjustment (between 0.074 and 0.103), although even these are significantly different from zero. A similar estimate of SOA is produced by one-step system GMM model (0.082), whereas the long differencing estimator produces a somewhat larger speed of adjustment (0.108).

### **3.3 Estimates of the speed of adjustment: Flipped coin simulated data**

In this section, we conduct simulation experiments in the spirit of Chang and Dasgupta

(2008) and estimate different versions of the partial adjustment model on data simulated on the basis of random financing behavior. Specifically, for each sample firm, we start with its chronologically initial debt ratio and simulate a path of future debt ratios under the assumption that firms make random financing choices with debt or equity chosen with probability fifty percent each. In other words, we assume that the actual financing deficits observed in each year are financed with debt 50% of the time, and the financing surpluses are used to retire a portion of outstanding debt also 50% of the time. These simulations are subject to the constraint that the ratio of debt to assets cannot exceed one or drop below zero. For example, if the firm has no outstanding debt, financing surplus is used to repurchase equity.

The third set of results reported in Table 3 is for this simulated sample. The reported results are the averages of one hundred simulations with the significance levels based on whether the 95% and 99% confidence intervals span zero. The results show that specifications utilizing full-sample fixed effects targets (both one-step and two-step) produce severely biased estimates of the speed of adjustment. Despite the fact that the data are generated using random financing rule, the estimates of the speed of adjustment generated by these models vary between 0.244 and 0.262 for full-sample fixed effects specifications ((C2), (C3), (C4), (D3), and (D4)). These estimates are statistically and economically significant.

As in the case of pecking order simulated data, the models without fixed effects ((A1), (A2), (B1), (C1), (D1), and (D2)) and those with fixed effects but using only ex-ante information ((B2), (B3), and (B4)) produce much smaller estimates of the speed of adjustment (between 0.060 and 0.086), although they are still significantly different from zero. Single-step fixed effects models estimated using methods designed to deal with the dynamic panel bias also produce relatively low estimates of the speed of adjustment. The system GMM estimate of the

speed of adjustment is 0.077, whereas the long differencing estimate is 0.093, both statistically significantly different from zero.

Overall, the comparison of the results in Panels A, B, C, and D suggests that the magnitude of the estimated speed of adjustment is primarily affected by the presence of fixed effects and whether it is implemented on the full sample or the historical panel only. When the full sample panel is utilized, the magnitude of the estimated speed of adjustment does not depend on whether the single-step or the two-step implementation of the partial adjustment model is used.

To summarize, the results in this section show that the estimates of the average speed of adjustment to target capital structure in our sample vary between 12.7 and 35.7 percent per year, depending on target specification. The estimates of SOA in the simulated pecking order and flipped coin samples vary between 7.4 and 28.4 percent per year, implying that the real sample SOAs are potentially severely biased. We examine sources of the bias and potential remedies in section 4 of the paper.

#### 4. Debt vs. Equity Choice Regressions

In this section, we examine how well the deviations from various proxies for the target debt ratio predict the choice between debt and equity for firms that issue or repurchase securities using the following models.

$$DI_{i,T+1}^* = \alpha + \beta(BDR_{i,T+1}^* - BDR_{i,T}) + \varepsilon_{iT+1}, \quad (5)$$

$$ER_{i,T+1}^* = \alpha + \beta(BDR_{i,T+1}^* - BDR_{i,T}) + \varepsilon_{iT+1}, \quad (6)$$

As in our partial adjustment regressions,  $BDR^*$  is the predicted value obtained from regression (1). In (5), the dependent variable,  $DI^*$ , is a latent continuous variable measuring the

propensity to issue debt rather than equity. Its observable counterpart is a binary variable set to one if the firm issues debt and set to zero if it issues equity.<sup>13</sup> In (6), the dependent variable,  $ER^*$ , is a latent continuous variable measuring the propensity to repurchase equity rather than retire debt. Its observable counterpart is a binary variable set to one if the firm repurchases equity and set to zero if it retires debt.<sup>14</sup> If target ratings are important, then we expect the estimates of the coefficients  $\beta$  to be positive and statistically and economically significant.

Table 4 reports the estimation results of regressions (5) and (6) using the same target proxies that were used in the partial-adjustment regressions presented in Table 3. As in the case of the two-step partial adjustment models from Table 3, we use Fama-MacBeth approach and present the time-series averages of the cross-sectional estimates and t-statistics calculated based on time-series standard errors. The reported values are the marginal effects evaluated at the points of sample means.

The first set of results is for the actual data. In both the issue and the repurchase choice regressions, the estimated marginal effects are statistically significant for all ten target proxies. In terms of magnitudes, the patterns for the issue choice model are similar to the patterns observed for the speeds of adjustment in Table 3. The marginal effects are the strongest (0.870-0.891) with full sample fixed effects targets (specifications (C2)-(C4)), they are less than half as strong (0.384-0.427) with historical fixed effects targets (specifications (B2)-(B4)), and they are the weakest (0.242-0.275) with OLS targets (specifications (A1), (A2), (B1), (B2)). For repurchase choice models, the strongest marginal effects (1.275-1.281) are again observed for full sample

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<sup>13</sup> Debt issues are identified as the cases when net debt issued, calculated as the change in the book value of debt (Item 9 + Item 34), exceeds five percent of total assets. Equity issues are identified as the cases when net equity issued, defined as the proceeds from sale of common and preferred stock (Compustat Annual Item 108) - amount of common and preferred stock repurchased (Item 115) - change in the value of preferred stock (Item 10), exceeds five percent of total assets. The procedure is identical to the one used in Hovakimian, Opler, and Titman (2001).

<sup>14</sup> Debt reductions are identified as the cases when net debt issued is less than negative five percent of total assets. Equity repurchases are identified as the cases when net equity issued is less than negative five percent of total assets.

fixed effects targets (specifications (C2)-(C4)), with all the remaining targets producing similar results (0.877-0.917).

The second set of results is for the flipped coin simulated sample. Here, our expectation is that the deviation from target leverage should not matter since the choice between debt and equity was determined by the flip of the coin. Yet, the marginal effects for both the issue and the repurchase models are statistically significant and economically large. The magnitude patterns in this simulated dataset are similar to the patterns for the real data, implying that the bias is the strongest with full sample fixed effects targets. These results suggest that the evidence of adjustment to target generated by debt-equity choice models may, to significant extent, be biased.

## **5. Sources of Bias**

The significance of traditional indicators of target adjustment behavior in simulated datasets generated with no target adjustment in mind is a cause for concern. In this section, we examine the potential sources of the bias with a goal of identifying empirical methodologies that would lead to meaningful tests of target adjustment behavior.

### **5.1 Does using ex-post information to estimate the target bias the speed of adjustment?**

The estimates of the speeds of adjustment and the marginal effects obtained from specifications (C2)-(C4) in Tables 3-4 are about twice as high as the estimates from specifications (B2)-(B4). This holds for the actual as well as both simulated datasets. The only difference between these panels is that target proxies in Panels B utilize only historical data, whereas target proxies in Panels C are generated using ex-post information. That is, in Panels C, the proxies for the target in year  $T+1$ ,  $BDR_{i,T+1}^*$ , are generated using not only historical data from

years  $I$  through  $T$  as in Panels D, but also future data from years  $T+1$  through  $N$ . The fact that both sets of estimates are based on target proxies generated using fixed effects regressions in the first stage suggests that the difference in the speeds of adjustment is not due to unobserved firm-specific time-invariant determinants of the target. It is likely that the use of ex-post information in Panels C leads to a look-ahead bias in the estimated speed of adjustment.

Even starker evidence of bias emerges when we reestimate the partial adjustment model (2) and the debt vs. equity choice models (5) and (6) with a modified proxy for the target. Specifically, instead of using historical debt ratios up to year  $T$  as in Panels B, debt ratios up to time  $T+1$  are used to estimate  $\hat{\beta}$ . That is,  $\hat{\beta}$  is obtained by panel regression (1) of debt ratios,  $BDR$ , observed in years 2 through  $T+1$  on firm characteristics,  $X$ , observed in years 1 through  $T$ . Proxies for target at time  $T+1$ ,  $BDR_{i,T+1}^*$ , are then generated by the predicted value  $\hat{\beta}X_{iT}$ , i.e., in sample.

Table 5 reports the estimated speeds of adjustment in Panel A and the marginal effects of the deviations from the targets on debt vs. equity issue and repurchase choice models in Panel B. Comparing the results in Panel A of Table 5 with those in Panel B of Table 3, we find that, with fixed effects target models, adding a single ex-post year to the sample used to estimate the target increases the adjustment speeds from 0.147-0.169 in Table 3 to 0.357-0.393 in Table 5 and increases the  $R^2$  of the partial adjustment regression from about three percent to about ten percent. Similarly large increases are observed for both simulated samples. The magnitudes of the speed of adjustment in Panel A of Table 5 are double and triple of the magnitudes in Panel B of Tables 3. In contrast, if we use an OLS regression to estimate the proxies for target, adding a single future year to the target estimation, generates adjustment speeds of 0.127 (real data), 0.077 (pecking order data), and 0.072 (flipped coin data) that are almost the exact matches of the

estimates reported in Table 3 (respectively, 0.127, 0.076, and 0.072).

The patterns for marginal effects of deviations from target leverage on debt-equity choice are similar. In Panel B of Table 5, the marginal effects (0.813-0.881 in the issue choice models and 1.222-1.250 in the repurchase choice models) are substantially larger than the effects in Panel B of Table 4 (0.384-0.427 in the issue choice models and 0.892-0.917 in the repurchase choice models).

The fact that adding a single ex-post year to the fixed effects target models results in such dramatic changes in the estimated speeds of adjustment and debt vs. equity choice marginal effects implies that ex-post data in the fixed effects target models lead to a large upward bias in the estimates of the speed of adjustment and in debt vs. equity choice effects.

## **5.2 Mean-reversion**

A number of earlier studies (e.g., Kayhan and Titman (2007) and Chang and Dasgupta (2008)) argue that the fact that debt ratios are limited between zero and one can lead to mechanical mean-reversion and can bias partial-adjustment coefficient estimates. In this section, we examine the impact of mechanical mean-reversion on the speed of adjustment estimated on real as well as simulated data.

We start by examining the distribution of the debt ratios in the real and the simulated samples in Table 6 where we sort our observations into ten bins of equal width based on the lagged value of the debt ratio so that bin 1 contains observations with lagged debt ratios less than 0.1, bin 2 contains observations with lagged debt ratios between 0.1 and 0.2, and so on. The results in Panel A show that distribution of the debt ratios in the real data is skewed towards the lower end. More than a quarter of the sample has debt ratios below 0.1. Less than 11% have debt ratios exceeding 0.5 and less than 1% have debt ratios exceeding 0.8. In the simulated datasets

the left skew is less apparent and the fractions of extremely high debt ratios are much higher (the frequency of debt ratios in excess of 0.8 is 3.4% in the flipped coin sample and 5.5% in the pecking order sample).

Neither the simulated nor the real datasets contain observations with debt ratios less than zero or greater than one. Because debt ratios of zero cannot decrease whereas debt ratios of one cannot increase, the observations at both extremes can introduce mechanical mean-reversion biasing the SOAs. We examine these effects in Panel B, which reports changes in debt ratios observed in each leverage bin in real data as well as in both simulated datasets. The results show a clear trend in real data where the changes in leverage become less positive and more negative as the lagged leverage increases. Similar patterns are observed in the pecking order and in the flipped coin data where these effects can only be mechanical in the sense that, by design, the financing rules used to create these datasets should not generate mean-reversion. These results illustrate that the effects of mechanical mean-reversion can be very pervasive.<sup>15</sup>

More generally, leverage ratios may exhibit mean-reverting behavior for various reasons. Shyam-Sunder and Myers (1999), for example, argue that positively serially correlated capital investment and cyclical cash flows can generate mean reversion towards average debt ratio. When testing the tradeoff theory, our interest usually is not simply in any kind of mean reversion, but a reversion to a target that is consistent with the predictions of the tradeoff theory. That is the reason for the inclusion in the partial-adjustment regressions (explicitly in regression (2) and implicitly in regression (4)) of the target debt ratio, modeled as a function of firm characteristics identified by the theory as important determinants of the target. Applications of

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<sup>15</sup> When the simulated data are generated without any restrictions on the values of the simulated debt ratios, i.e., without restricting them to be within the interval  $[0,1]$ , the mechanical mean-reversion disappears. In such a simulated dataset, the estimated speeds of adjustment are significant only for full sample fixed effects models (one-step or two-step). The mechanical mean-reversion reappears again, when the data generated by such unrestricted simulation is subjected to trimming at the top and bottom one percent.

the partial-adjustment model, however, tend to interpret any evidence of mean reversion as consistent with tradeoff theory.

To assess the extent to which the results may be driven by mean-reversion rather than adjustment to a target that is meaningful from the point of view of the tradeoff theory, we re-estimate our two-step partial adjustment regressions and debt vs. equity choice models using deviations from randomly generated targets. Specifically, these targets are generated as random draws from the normal distribution with the mean and the standard deviation matching those observed in the real data for specification (A1). In the context of the one-step partial adjustment model (4), a random target debt ratio would be absorbed by the intercept and the error term leaving only the lagged debt ratio on the right hand side of the regression.

The results in Table 7 show that the estimated SOAs are all statistically significant and are fairly large in magnitude. For example, in the real data, the speed of adjustment generated by the Fama-Macbeth implementation of the one-step model (4) is 0.111. This estimate can be interpreted as the speed of adjustment to a naïve target debt ratio set equal to the average debt ratio in the sample. This “speed of adjustment” is quite close to the speed of adjustment of 0.132 generated with all of our hypothesized determinants of the theoretical target on the right hand side (see Panel D of Table 3). Similarly, for the two-step model, the speed of adjustment to the random target is 0.092 in Table 7, which is uncomfortably close to the speed of adjustment for (A1) target (0.128) reported in Table 3. The SOAs observed in the simulated datasets are 0.060 for the pecking order data, and 0.061 for the flipped coin data. The marginal effects in the debt vs. equity choice regressions are also all statistically significant.

Thus, even with a proxy that has no relation to the true target debt ratio, the partial adjustment models (1)-(2) and (4) can produce evidence of statistically and economically

significant adjustment to target. Given that the target debt ratios in these regressions are random, the results are clearly driven by the significance of the lagged debt ratios and reflect mean reversion but not a reversion to a meaningful target. The implication of these results is that the significance of SOAs obtained from partial adjustment regressions (2) and (4) and of debt-equity choice effects obtained from models (5) and (6) cannot be interpreted as evidence of adjustment to a specific target proxy.

## 6. Testing the Trade-off Theory

In this section, we examine whether modifications to the standard partial-adjustment and debt-equity choice regression methods can improve their ability to reject the target adjustment hypothesis when no target adjustment is present.

### 6.1 The importance of the target

The analysis in the previous section suggests that meaningful tests of the tradeoff theory should examine adjustments to a specific proxy for the target debt ratio rather than mean-reversion in general. A simple way to achieve that is to include the target and the lagged debt ratios as separate regressors in the partial adjustment regression (2) and the debt-equity choice regressions (5) and (6) as follows.

$$BDR_{i,T+1} - BDR_{i,T} = \alpha + \lambda_1 BDR_{i,T+1}^* + (1 - \lambda_2) BDR_{i,T} + \varepsilon_{iT+1}, \quad (7)$$

$$DI_{i,T+1}^* = \alpha + \beta_1 BDR_{i,T+1}^* + \beta_2 BDR_{i,T} + \varepsilon_{iT+1}, \quad (8)$$

$$ER_{i,T+1}^* = \alpha + \beta_1 BDR_{i,T+1}^* + \beta_2 BDR_{i,T} + \varepsilon_{iT+1}, \quad (9)$$

Regression (7) provides two potential estimates of the speed of adjustment,  $\lambda_1$  and  $\lambda_2$ . As the results in the previous section imply, however, the statistical and economic significance of  $\lambda_2$  in

equation (7) may be due to mechanical mean-reversion. Therefore, we focus on the coefficient estimates for the target,  $BDR^*$ , to draw inferences about adjustment to target.<sup>16</sup>

The estimation results of these partial adjustment regressions are presented in Panels A and B of Table 8. The results show that, in the real data, the speeds of adjustment based on the coefficient estimates for the target (0.073-0.102) tend to be substantially lower than those based on the coefficient estimates for the lagged debt ratio (0.127-0.179) or those from the standard partial adjustment regressions reported in Table 3 (0.127-0.169).

In the simulated datasets, the targets with historical fixed effects and no time-varying factors (B2) generate particularly low speeds of adjustment. The SOAs in both the pecking order data (0.015) and the flipped coin data (-0.003) are statistically insignificant at five percent, allowing us to reject the target adjustment hypothesis in both simulated samples. The SOAs based on historical fixed effects specifications of the target with time-varying factors (B3 and B4) are statistically significant but economically small in both the pecking order and the flipped coin data. Although the SOAs of 1-4 percent generated by these targets do not allow us to reject the target adjustment hypothesis in statistical terms, such economically trivial speeds imply economical irrelevance of target adjustment as a determinant of corporate financing behavior.

In Panels C and D of Table 8, we report the marginal effects of the target and the lagged debt ratios on the likelihood of debt vs. equity issuance. In the flipped coin data, the marginal effects of the target range from statistically insignificant  $-0.056$  to significantly positive  $0.153$ . These numbers are substantially lower than the effects of the lagged debt ratios (0.241-0.335), the effects of the target in the real data (0.414-0.937), or the effects of deviations from target in the flipped coin data reported in Table 4 (0.242-0.324). Nevertheless, the statistical significance

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<sup>16</sup> This is identical to adding lagged leverage as a control variable for mechanical mean reversion into the standard second stage regression (2).

of four out of five target proxies suggests the presence of some residual bias.

In Panels E and F of Table 8, we report the marginal effects of the target and the lagged debt ratios on the likelihood of debt vs. equity repurchase. In the flipped coin data, the marginal effects of the lagged debt ratios are all significant, but the marginal effects of the target are never significantly positive.

To summarize, modified partial adjustment regressions with historical fixed effects targets reduce the upward bias in estimated SOAs to levels that make it possible to reject the target adjustment as economically consequential facet of corporate financing behavior even when the bias remains significant in statistical terms. In modified debt-equity repurchase choice models the bias is statistically indistinguishable from zero. However, modified debt-equity issue choice model with target and lagged leverage as independent variables is still unable to reject the target adjustment hypothesis.

## **6.2 The impact of extreme debt ratios**

The results in the previous section indicate that although entering the target and the lagged debt ratio independently into the second stage partial adjustment regression reduces the bias in the estimates of the SOA, most SOA estimates remain statistically significantly biased. Furthermore, for all target proxies, the marginal effects of the deviation from the target on debt-equity issue choice also remain significant.

As indicated in Table 6, a substantial number of observations in our simulated datasets are close to the boundary values of 0 and 1. The mean-reversion bias could be especially severe for these observations. While the real data shows similarly large clustering of observations close to 0, the fraction of observations with very large debt ratios is substantially smaller than in the simulated datasets. The higher frequency of very high debt ratios in the simulated datasets could

make these datasets more susceptible to the mean-reversion bias than the real data. We, therefore, reestimate the modified target adjustment regressions (7)-(9) after dropping the observations with debt ratios above 0.8 from the sample. According to Table 6, this eliminates 5.6% of the pecking order sample and 3.4% of the flipped coin sample, but only 0.8% of the real data sample.

Panels A and B of Table 9 present these partial adjustment results. The results show that target specification (B2) reliably rejects the target adjustment hypothesis in both the pecking order and the flipped coin simulated samples. In addition, specification (B3) is able to reject the target adjustment hypothesis in the flipped coin data, but in the pecking order data the estimated speed of adjustment is statistically significantly positive, although it is economically small (0.021). The speeds of adjustment generated by specification (B4) are statistically significantly positive but economically small in both datasets. The estimates of the speed of adjustment generated by models without fixed effects ((A1), (A2), and (B1)) remain statistically and economically significant.

Compared to Table 8, the ability of the debt-equity issue choice models to reject the target adjustment hypothesis improves dramatically in Table 9. As the results in Panels C and D show, the marginal effects of all proxies for the target,  $BDR^*$ , are significantly positive in the real data but are not significantly positive in the flipped coin data. Panels E and F report the results for the debt-equity repurchase choice model with all proxies for the target,  $BDR^*$ , insignificant in the flipped coin data. Thus, all six specifications of the target are able to reject the target adjustment hypothesis in the flipped coin dataset for both the issue choice and the repurchase choice models.

It is important to note that the estimates of the speed of adjustment generated by models

without fixed effects ((A1), (A2), and (B1)) in Table 9 show bias in favor the target-adjustment hypothesis, whereas the marginal effects of these proxies for the target in debt-equity choice models do not. This finding is consistent with Chang and Dasgupta's (2008) argument that part of the bias in the traditional estimates of the speed of adjustment is due to a correlation between the financing deficit and the traditional determinants of the target debt ratio. Debt-equity choice analysis is free of this bias because it is applied to samples where the financing deficit is at least five percent of assets and there is no difference in the average financing deficit across debt and equity subsamples in the flipped coin dataset.

To summarize, debt-equity choice regressions estimated on a sample that excludes very high (more than 0.8) debt ratios, have the power to reject the target adjustment hypothesis with any of the six historical proxies for target used in this study. The magnitudes of the estimates suggest that a ten percentage point deviation from the target may lead to a change in the probability of debt vs. equity issuance of 4-9 percentage points and to a change in the probability of debt vs. equity repurchase of 1-2 percentage points, depending on the target proxy. Among partial-adjustment models, regressions with historical fixed effects proxies for target leverage can reject the target-adjustment hypothesis when it is incorrect. The estimated speeds of adjustment generated by these models in the real data are in the range of 7-9 percent per year.

## **7. Conclusions**

Simulation experiments show that both partial adjustment and debt-equity choice models can generate spuriously significant estimates that are consistent with the hypothesis that firms have target debt ratios to which they periodically adjust. Regressions relying on full-sample fixed effects models of target leverage, in particular, produce results severely biased in favor of the

target adjustment hypothesis. This conclusion holds for both one-step and two-step implementations of the partial adjustment model.

We find that the bias in favor of the target adjustment hypothesis can be eliminated by a combination of methods. Most importantly, historical fixed effects proxies for the target should be used, which is easy to do with two-step models. In addition, estimating the partial adjustment and debt-equity choice regressions on a sample that excludes observations with extremely high debt ratios (above 0.8) further reduces the bias without substantially altering the real Compustat sample.

In the context of dynamic tradeoff models of capital structure with fixed adjustment costs and infrequent rebalancing, the magnitudes of the estimates suggest that it takes more than ten years for a firm to adjust to its target capital structure. These long adjustment times suggest that either adjusting to target capital structure is not a high priority goal for an average firm or that the empirical models currently used in the literature are not well-suited to identify the ways in which firms facing various tradeoffs manage their debt ratios. Understanding the reasons behind the relatively low economic importance of target debt ratios in partial adjustment and debt-equity choice models is a priority for future capital structure research.

Table 1: Sample Statistics

Size is the natural log of total assets, adjusted for inflation. Tangibility is the property, plant, and equipment scaled by total assets. Market-to-book is (total assets – book equity + market equity)/total assets. R&D intensity is the research and development expenses scaled by sales. Missing values of R&D are replaced with zeroes. R&D indicator is set to one for firms with non-missing R&D. Selling expense is selling, general, and administrative expense over sales. Profitability is operating income before depreciation scaled by lagged total assets. Depreciation is depreciation and amortization scaled by lagged assets. Book debt ratio is (short-term debt + long-term debt)/(total assets).

	Mean	Min	Max	Obs
Size	4.686	-0.619	12.044	132,665
Tangibility	0.335	0.000	1.000	132,665
Market-to-book	1.575	0.089	9.655	132,665
R&D	0.033	0.000	1.200	132,665
R&D indicator	0.607	0.000	1.000	132,665
Profitability	0.135	-0.681	0.645	132,665
Depreciation	0.050	0.000	0.213	132,665
Book debt ratio	0.253	0.000	0.999	132,665

Table 2: Target leverage regressions: Full sample estimates

The dependent variable in all regressions is the book debt ratio defined as (short-term debt + long-term debt)/(total assets). Size is the natural log of total assets, adjusted for inflation. Tangibility is the property, plant, and equipment scaled by total assets. Market-to-book is (total assets – book equity + market equity)/total assets. R&D intensity is the research and development expenses scaled by sales. Missing values of R&D are replaced with zeroes. R&D indicator is set to one for firms with non-missing R&D. Industry is defined based on Fama-French 49 industry classification. Profitability is operating income before depreciation scaled by lagged total assets. Depreciation is depreciation and amortization scaled by lagged assets. The t-statistics reflect robust standard errors adjusted for heteroskedasticity and firm-level clustering. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively.

	OLS: Factors		Fixed effects: Factors		Fixed effects: Factors, expanded set	
	Coeff.	t-stat.	Coeff.	t-stat.	Coeff.	t-stat.
Size	0.000	-0.6	0.014**	8.9	0.016**	10.5
Tangibility	0.143**	20.4	0.178**	17.1	0.155**	14.9
Market-to-book	-0.019**	-20.5	-0.011**	-14.6	-0.005**	-6.7
R&D intensity	-0.060**	-4.9	0.033*	2.3	-0.067**	-4.7
R&D indicator	-0.024**	-8.2	-0.009**	-3.2	-0.007*	-2.5
Industry median debt ratio	0.422**	25.9	0.375**	18.2	0.397**	19.6
Profitability					-0.160**	-25.8
Depreciation					0.286**	7.6
Book debt ratio (t-1)						
Fixed firm effects	No		Yes		Yes	
R2	0.169		0.668		0.674	
Observations	132,665		132,665		132,665	

Table 3: Target proxies and partial adjustment speed

The dependent variable in Panels A, B, and C is the change in the book debt ratio,  $BDR(t+1) - BDR(t)$ . BDR is defined as (short-term debt + long-term debt)/(total assets). The independent variable in Panels A, B, and C is the deviation from target debt ratio,  $BDR^*(t+1) - BDR(t)$ . The target debt ratios are the predicted debt ratios from first stage regressions of observed debt ratios on determinants of the target as in Table 2. For the real and the pecking order samples, the reported coefficients and  $R^2$  are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively. For the flipped coin sample, the reported parameter estimates are the averages obtained from 100 replications of the simulation and the significance levels are marked with \* and \*\* if, respectively, 95% and 99% confidence intervals do not span zero.

	Real data			Pecking order data			Flipped coin data	
	SOA	t-stat.	$R^2$	SOA	t-stat.	$R^2$	SOA	$R^2$
Panel A: Cross-sectional target								
(A1) OLS: Factors	0.128**	28.7	0.052	0.078**	24.3	0.030	0.073**	0.025
(A2) OLS: Factors, expanded set	0.132**	29.1	0.053	0.090**	25.4	0.037	0.086**	0.032
Panel B: Historical panel target								
(B1) OLS: Factors	0.127**	28.8	0.051	0.076**	23.0	0.028	0.072**	0.024
(B2) Firm fixed effects: No factors	0.147**	18.5	0.030	0.074**	9.9	0.010	0.060**	0.006
(B3) Firm fixed effects: Factors	0.163**	23.2	0.034	0.085**	14.0	0.012	0.068**	0.008
(B4) Firm fixed effects: Factors, exp. set	0.169**	23.8	0.035	0.094**	15.4	0.015	0.079**	0.010
Panel C: Full sample panel target								
(C1) OLS: Factors	0.127**	28.8	0.052	0.077**	24.0	0.029	0.073**	0.025
(C2) Firm fixed effects: No factors	0.338**	37.3	0.152	0.267**	28.1	0.117	0.244**	0.104
(C3) Firm fixed effects: Factors	0.350**	38.6	0.156	0.274**	29.8	0.122	0.251**	0.107
(C4) Firm fixed effects: Factors, exp. set	0.357**	38.2	0.158	0.284**	29.9	0.127	0.262**	0.114

Table 3 (cont'd): Target proxies and partial adjustment speed

The partial adjustment results in Panel D are from one step models with  $BDR(t+1)$  as the dependent variable and  $BDR(t)$  and the determinants of the target debt ratio (as in Table 2) on the right hand side.  $BDR$  is defined as  $(\text{short-term debt} + \text{long-term debt})/(\text{total assets})$ . For the real and the pecking order samples, the reported coefficients and  $R^2$  are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively. For the flipped coin sample, the reported parameter estimates are the averages obtained from 100 replications of the simulation and the significance levels are marked with \* and \*\* if, respectively, 95% and 99% confidence intervals do not span zero.

	Real data		Pecking order data		Flipped coin data
	SOA	t-stat.	SOA	t-stat.	SOA
Panel D: Single step model					
(D1) OLS	0.133**	52.0	0.093**	44.2	0.094**
(D2) Fama-MacBeth	0.132**	28.8	0.091**	25.0	0.084**
(D3) Firm fixed effects: No factors	0.328**	67.3	0.256**	55.9	0.241**
(D4) Firm fixed effects	0.344**	66.1	0.275**	58.5	0.255**
(D5) Firm fixed effects: System GMM	0.214**	21.2	0.082**	7.6	0.057**
(D6) Firm fixed effects: Long diff.	0.195**	50.2	0.108**	26.7	0.093**
Observations	105,995		98,369		100,410

Table 4: Target proxies and debt vs. equity issue and repurchase choices

The dependent variable in the issue choice regressions is a binary variable set to one when there is a debt issue and to zero when there is an equity issue. The dependent variable in the repurchase choice regressions is a binary variable set to one when there is an equity repurchase and to zero when there is a debt reduction. Observations with no issues (repurchases) are dropped from the issue (repurchase) regression. The independent variable is the deviation from target debt ratio,  $BDR^{*(t+1)} - BDR(t)$ . The target debt ratios are the predicted debt ratios from first stage regressions of observed debt ratios on determinants of the target as in Table 2. For the real sample, the reported coefficients are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively. For the flipped coin sample, the reported parameter estimates are the averages obtained from 100 replications of the simulation and the significance levels are marked with \* and \*\* if, respectively, 95% and 99% confidence intervals do not span zero.

	Issue			Repurchase		
	Real data		Flipped coin data	Real data		Flipped coin data
	Marginal eff.	t-stat.	Marginal eff.	Marginal eff.	t-stat.	Marginal eff.
Panel A: Cross-sectional target						
(A1) OLS: Factors	0.275**	8.4	0.258**	0.908**	12.5	0.709**
(A2) OLS: Factors, expanded set	0.242**	7.1	0.242**	0.880**	12.8	0.702**
Panel B: Historical panel target						
(B1) OLS: Factors	0.255**	7.7	0.260**	0.877**	13.5	0.706**
(B2) Firm fixed effects: No factors	0.384**	9.1	0.324**	0.892**	16.3	0.729**
(B3) Firm fixed effects: Factors	0.427**	9.4	0.316**	0.916**	16.0	0.726**
(B4) Firm fixed effects: Factors, exp. set	0.420**	8.7	0.309**	0.917**	16.3	0.734**
Panel C: Full sample panel target						
(C1) OLS: Factors	0.264**	7.9	0.259**	0.893**	13.1	0.734**
(C2) Firm fixed effects: No factors	0.870**	14.2	0.792**	1.275**	13.3	1.106**
(C3) Firm fixed effects: Factors	0.889**	13.9	0.809**	1.281**	13.5	1.118**
(C4) Firm fixed effects: Factors, exp. set	0.891**	13.3	0.807**	1.278**	13.7	1.118**
Observations	24,954		29,645	16,692		17,079

Table 5: Sources of bias: historical plus current year data

The dependent variable in Panel A is the change in the book debt ratio,  $BDR(t+1) - BDR(t)$ . BDR is defined as (short-term debt + long-term debt)/(total assets). In Panel B, the dependent variable in the issue choice regressions is a binary variable set to one when there is a debt issue and to zero when there is an equity issue. The dependent variable in the repurchase choice regressions is a binary variable set to one when there is an equity repurchase and to zero when there is a debt reduction. Observations with no issues (repurchases) are dropped from the issue (repurchase) regressions. The independent variable in all regressions is the deviation from target debt ratio,  $BDR^*(t+1) - BDR(t)$ . The target debt ratios are the predicted debt ratios from first stage regressions of observed debt ratios on determinants of the target as in Table 2. For the real and the pecking order samples, the reported coefficients and  $R^2$  are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively. For the flipped coin sample, the reported parameter estimates are the averages obtained from 100 replications of the simulation and the significance levels are marked with \* and \*\* if, respectively, 95% and 99% confidence intervals do not span zero.

Panel A: Partial adjustment models	Real data			Pecking order data			Flipped coin data	
	SOA	t-stat.	$R^2$	SOA	t-stat.	$R^2$	SOA	$R^2$
(1) OLS: Factors	0.127**	29.0	0.051	0.077**	23.3	0.029	0.072**	0.034
(2) Firm fixed effects: No factors	0.357**	9.4	0.097	0.264**	6.2	0.050	0.163**	0.040
(3) Firm fixed effects: Factors	0.379**	10.0	0.105	0.286**	6.7	0.059	0.182**	0.047
(4) Firm fixed effects: Factors, exp. set	0.393**	10.2	0.108	0.305**	7.1	0.066	0.195**	0.052
Observations	105,995			98,369			100,410	

Panel B: Debt-equity choice models	Real data		Issue	Flipped coin data		Repurchase	
	Marginal eff.	t-stat.	t-stat.	Marginal eff.	Marginal eff.	t-stat.	Marginal eff.
(1) OLS: Factors	0.256**	7.7	7.7	0.260**	0.880**	13.5	0.708**
(2) Firm fixed effects: No factors	0.813**	11.2	11.2	0.668**	1.222**	15.0	0.942**
(3) Firm fixed effects: Factors	0.872**	11.6	11.6	0.677**	1.232**	16.5	0.946**
(4) Firm fixed effects: Factors, exp. set	0.881**	11.1	11.1	0.677**	1.250**	16.4	0.956**
Observations	24,954			17,079		16,692	

Table 6: The Distribution of Leverage Ratios and Changes in Leverage

Book debt ratio, BDR, is (short-term debt + long-term debt)/(total assets). Observations are sorted into ten bins of equal width based on the value of the debt ratio, BDR(t), so that bin 1 contains observations with lagged debt ratios less than 0.1, bin 2 contains observations with lagged debt ratios between 0.1 and 0.2, and so on.

	Book debt ratio bins									
	1	2	3	4	5	6	7	8	9	10
Fraction										
Real data	0.257	0.164	0.189	0.171	0.115	0.057	0.027	0.013	0.006	0.002
Pecking order data	0.179	0.131	0.159	0.154	0.127	0.100	0.062	0.033	0.019	0.037
Flipped coin data	0.279	0.186	0.172	0.136	0.091	0.053	0.031	0.018	0.012	0.022
BDR(t+1)-BDR(t)										
Real data	0.027	0.014	0.001	-0.008	-0.016	-0.021	-0.034	-0.060	-0.113	-0.168
Pecking order data	0.027	0.013	0.004	0	-0.003	-0.002	0.001	-0.004	-0.015	-0.080
Flipped coin data	0.018	0.006	0	-0.004	-0.006	-0.005	-0.003	-0.009	-0.028	-0.091

Table 7: Random targets

The SOA is the speed of adjustment from a regression of the change in the book debt ratio,  $BDR(t+1) - BDR(t)$  on the deviation from target debt ratio,  $BDR^*(t+1) - BDR(t)$ . The target debt ratios is a random variable drawn from a normal distribution with a mean and a standard deviation equal to the mean and the standard deviation of A(1) specification of the target. For the debt-equity choice regressions, the reported coefficient is the marginal effect of the deviation from the target  $BDR^*(t+1) - BDR(t)$ , on the probability of debt vs. equity issuance or equity vs. debt repurchase. For the real and the pecking order samples, the reported coefficients are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively. For the flipped coin sample, the reported parameter estimates are the averages obtained from 100 replications of the simulation and the significance levels are marked with \* and \*\* if, respectively, 95% and 99% confidence intervals do not span zero.

	Coeff.	t-stat.
1 step SOA: Real data	0.111**	27.5
1 step SOA: Pecking order data	0.067**	19.5
1 step SOA: Flipped coin data	0.066**	
SOA: Real data	0.092**	26.3
SOA: Pecking order data	0.060**	18.8
SOA: Flipped coin data	0.061**	
Debt-equity issue choice: Real data	0.074*	2.0
Debt-equity issue choice: Flipped coin data	0.230**	
Debt-equity repurchase choice: Real data	0.720**	13.1
Debt-equity repurchase choice: Flipped coin data	0.658**	

Table 8: Adjustment to target vs. mean reversion: Target impact

The dependent variable in Panels A and B is the change in the book debt ratio,  $BDR(t+1) - BDR(t)$ . BDR is defined as (short-term debt + long-term debt)/(total assets). The independent variables in Panels A and B are the target debt ratio,  $BDR^*(t+1)$ , and the lagged debt ratio,  $BDR(t)$ . The target debt ratios are the predicted debt ratios from first stage regressions of observed debt ratios on determinants of the target as in Table 2. For the real and the pecking order samples, the reported coefficients are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively. For the flipped coin sample, the reported parameter estimates are the averages obtained from 100 replications of the simulation and the significance levels are marked with \* and \*\* if, respectively, 95% and 99% confidence intervals do not span zero.

	Real data				Pecking order data				Flipped coin data	
	BDR*	t-stat.	BDR	t-stat.	BDR*	t-stat.	BDR	t-stat.	BDR*	BDR
Panel A: Cross-sectional target										
(1) OLS: Factors	0.095**	12.1	-0.128**	-28.6	0.097**	11.5	-0.078**	-24.1	0.087**	-0.073**
(2) OLS: Factors, expanded set	0.102**	12.2	-0.132**	-28.7	0.132**	10.9	-0.090**	-25.1	0.151**	-0.086**
Panel B: Historical panel target										
(1) OLS: Factors	0.090**	11.7	-0.127**	-28.6	0.084**	9.6	-0.076**	-22.9	0.069**	-0.072**
(2) Firm fixed effects: No factors	0.073**	10.6	-0.163**	-22.8	0.015	1.9	-0.077**	-10.2	-0.003	-0.064**
(3) Firm fixed effects: Factors	0.087**	14.3	-0.174**	-27.1	0.027**	4.7	-0.086**	-15.0	0.007**	-0.071**
(4) Firm fixed effects: Factors, exp. set	0.093**	15.3	-0.179**	-27.8	0.038**	6.6	-0.094**	-16.3	0.020**	-0.080**
Observations	105,995				98,369				100,410	

Table 8 (cont'd): Adjustment to target vs. mean reversion: Target impact

The dependent variable in the issue choice regressions is a binary variable set to one when there is a debt issue and to zero when there is an equity issue. The dependent variable in the repurchase choice regressions is a binary variable set to one when there is an equity repurchase and to zero when there is a debt reduction. Observations with no issues (repurchases) are dropped from the issue (repurchase) regression. The independent variables are the target debt ratio,  $BDR^*(t+1)$ , and the lagged debt ratio,  $BDR(t)$ . The target debt ratios are the predicted debt ratios from first stage regressions of observed debt ratios on determinants of the target as in Table 2. For the real sample, the reported coefficients are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively. For the flipped coin sample, the reported parameter estimates are the averages obtained from 100 replications of the simulation and the significance levels are marked with \* and \*\* if, respectively, 95% and 99% confidence intervals do not span zero.

	Real data				Flipped coin data	
	BDR*	t-stat.	BDR	t-stat.	BDR*	BDR
Debt-equity issue choice						
Panel C: Cross-sectional target						
(1) OLS: Factors	0.937**	6.2	-0.264**	-7.9	0.109*	-0.258**
(2) OLS: Factors, expanded set	0.707**	5.4	-0.234**	-6.7	-0.056	-0.241**
Panel D: Historical panel target						
(1) OLS: Factors	0.832**	6.1	-0.242**	-7.2	0.153**	-0.261**
(2) Firm fixed effects: No factors	0.414**	6.7	-0.388**	-8.8	0.129**	-0.335**
(3) Firm fixed effects: Factors	0.458**	7.5	-0.424**	-9.1	0.113**	-0.325**
(4) Firm fixed effects: Factors, exp. set	0.448**	7.3	-0.418**	-8.5	0.094**	-0.313**

Table 8 (cont'd): Adjustment to target vs. mean reversion: Target impact

The dependent variable in the issue choice regressions is a binary variable set to one when there is a debt issue and to zero when there is an equity issue. The dependent variable in the repurchase choice regressions is a binary variable set to one when there is an equity repurchase and to zero when there is a debt reduction. Observations with no issues (repurchases) are dropped from the issue (repurchase) regression. The independent variables are the target debt ratio,  $BDR^*(t+1)$ , and the lagged debt ratio,  $BDR(t)$ . The target debt ratios are the predicted debt ratios from first stage regressions of observed debt ratios on determinants of the target as in Table 2. For the real sample, the reported coefficients are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively. For the flipped coin sample, the reported parameter estimates are the averages obtained from 100 replications of the simulation and the significance levels are marked with \* and \*\* if, respectively, 95% and 99% confidence intervals do not span zero.

	Real data				Flipped coin data	
	BDR*	t-stat.	BDR	t-stat.	BDR*	BDR
Debt-equity repurchase choice						
Panel E: Cross-sectional target						
(1) OLS: Factors	0.194**	4.1	-0.897**	-11.6	0.023	-0.703**
(2) OLS: Factors, expanded set	-0.091	-1.8	-0.852**	-11.3	-0.162**	-0.683**
Panel F: Historical panel target						
(1) OLS: Factors	0.030	0.7	-0.869**	-12.1	-0.016	-0.701**
(2) Firm fixed effects: No factors	0.204**	4.6	-1.016**	-13.0	0.000	-0.702**
(3) Firm fixed effects: Factors	0.208**	4.2	-1.021**	-13.0	0.002	-0.703**
(4) Firm fixed effects: Factors, exp. set	0.180**	3.6	-1.004**	-13.0	-0.016	-0.691**

Table 9: Adjustment to target vs. mean reversion: Excluding very high debt ratios (>0.8)

The dependent variable in Panels A and B is the change in the book debt ratio,  $BDR(t+1) - BDR(t)$ . BDR is defined as (short-term debt + long-term debt)/(total assets). The independent variables in Panels A and B are the target debt ratio,  $BDR^*(t+1)$ , and the lagged debt ratio,  $BDR(t)$ . The target debt ratios are the predicted debt ratios from first stage regressions of observed debt ratios on determinants of the target as in Table 2. For the real and the pecking order samples, the reported coefficients are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively. For the flipped coin sample, the reported parameter estimates are the averages obtained from 100 replications of the simulation and the significance levels are marked with \* and \*\* if, respectively, 95% and 99% confidence intervals do not span zero.

	Real data				Pecking order data				Flipped coin data	
	BDR*	t-stat.	BDR	t-stat.	BDR*	t-stat.	BDR	t-stat.	BDR*	BDR
Panel A: Cross-sectional target										
(1) OLS: Factors	0.085**	11.1	0.119**	28.0	0.094**	10.8	0.060**	18.4	0.077**	0.053**
(2) OLS: Factors, expanded set	0.097**	11.8	0.124**	28.4	0.151**	10.7	0.071**	22.1	0.154**	0.066**
Panel B: Historical panel target										
(1) OLS: Factors	0.081**	10.5	0.118**	28.0	0.079**	8.7	0.058**	17.0	0.054**	0.051**
(2) Firm fixed effects: No factors	0.068**	10.1	0.152**	22.2	0.011	1.5	0.056**	-8.4	0.013**	0.036**
(3) Firm fixed effects: Factors	0.082**	13.6	0.163**	26.2	0.021**	3.9	0.064**	11.9	-0.002	0.044**
(4) Firm fixed effects: Factors, exp. set	0.088**	14.5	0.168**	26.7	0.033**	6.0	0.073**	13.5	0.014**	0.056**
Observations	105,209				92,923					

Table 9 (cont'd): Adjustment to target vs. mean reversion: Excluding very high debt ratios

The dependent variable in the issue choice regressions is a binary variable set to one when there is a debt issue and to zero when there is an equity issue. The dependent variable in the repurchase choice regressions is a binary variable set to one when there is an equity repurchase and to zero when there is a debt reduction. Observations with no issues (repurchases) are dropped from the issue (repurchase) regression. The independent variables are the target debt ratio,  $BDR^*(t+1)$ , and the lagged debt ratio,  $BDR(t)$ . The target debt ratios are the predicted debt ratios from first stage regressions of observed debt ratios on determinants of the target as in Table 2. For the real sample, the reported coefficients are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively. For the flipped coin sample, the reported parameter estimates are the averages obtained from 100 replications of the simulation and the significance levels are marked with \* and \*\* if, respectively, 95% and 99% confidence intervals do not span zero.

	Real data				Flipped coin data	
	BDR*	t-stat.	BDR	t-stat.	BDR*	BDR
Debt-equity issue choice						
Panel C: Cross-sectional target						
(1) OLS: Factors	0.953**	6.2	-0.278**	-7.6	0.017	-0.056**
(2) OLS: Factors, expanded set	0.721**	5.5	-0.245**	-6.5	-0.039**	-0.049**
Panel D: Historical panel target						
(1) OLS: Factors	0.845**	6.1	-0.253**	-6.9	0.034	-0.058**
(2) Firm fixed effects: No factors	0.419**	6.6	-0.390**	-8.7	0.036	-0.081**
(3) Firm fixed effects: Factors	0.465**	7.3	-0.428**	-8.9	0.031	-0.077**
(4) Firm fixed effects: Factors, exp. set	0.456**	7.1	-0.423**	-8.2	0.023	-0.071**

Table 9 (cont'd): Adjustment to target vs. mean reversion: Excluding very high debt ratios

The dependent variable in the issue choice regressions is a binary variable set to one when there is a debt issue and to zero when there is an equity issue. The dependent variable in the repurchase choice regressions is a binary variable set to one when there is an equity repurchase and to zero when there is a debt reduction. Observations with no issues (repurchases) are dropped from the issue (repurchase) regression. The independent variables are the target debt ratio,  $BDR^*(t+1)$ , and the lagged debt ratio,  $BDR(t)$ . The target debt ratios are the predicted debt ratios from first stage regressions of observed debt ratios on determinants of the target as in Table 2. For the real sample, the reported coefficients are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively. For the flipped coin sample, the reported parameter estimates are the averages obtained from 100 replications of the simulation and the significance levels are marked with \* and \*\* if, respectively, 95% and 99% confidence intervals do not span zero.

	Real data				Flipped coin data	
	BDR*	t-stat.	BDR	t-stat.	BDR*	BDR
Debt-equity repurchase choice						
Panel E: Cross-sectional target						
(1) OLS: Factors	0.208**	3.9	-0.960**	-11.6	0.037	-0.626**
(2) OLS: Factors, expanded set	-0.070	-1.3	-0.915**	-11.2	-0.085	-0.614**
Panel F: Historical panel target						
(1) OLS: Factors	0.044	1.0	-0.931**	-12.0	-0.022	-0.622**
(2) Firm fixed effects: No factors	0.198**	4.3	-1.073**	-13.0	-0.024	-0.608**
(3) Firm fixed effects: Factors	0.202**	4.0	-1.078**	-13.0	-0.016	-0.613**
(4) Firm fixed effects: Factors, exp. set	0.176**	3.4	-1.062**	-13.0	-0.026	-0.607**

## CHAPTER 2

# Do firms have unique target debt ratios to which they adjust?

### 1. Introduction

The speed with which firms adjust toward their target debt ratios has recently become a topic of intense debate.<sup>17</sup> This debate represents a shift from the earlier literature that primarily focused on the existence of leverage targets to a new emphasis on quantifying how important these targets are. Specifically, if it takes an average firm five years to offset 35-40 percent of the initial deviation from the target, as in Kayhan and Titman (2007), then target leverage can be viewed as a secondary factor in corporate financing decisions, at best. If, in contrast, the average adjustment speed is on the order of 35 percent per year, as in Flannery and Rangan (2006), then target leverage is of central importance. Because of these far reaching conclusions, the speed of adjustment is considered to be the most important issue in contemporary capital structure research (Huang and Ritter (2007)).

The standard interpretation for why the estimates of the speed of adjustment are relatively low is that, due to adjustment costs, firms adjust to target relatively infrequently. This interpretation follows from the dynamic capital structure models of Fischer, Heinkel, and Zechner (1989), Goldstein, Ju, and Leland (2001), and Strebulaev (2007), among others. In these

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<sup>17</sup> See, e.g., Fama and French (2002), Flannery and Rangan (2006), Antoniou, Guney, and Paudyal (2007), Huang and Ritter (2007), Kayhan and Titman (2007), Lemmon, Roberts, and Zender (2007).

models, firms have well-defined “target” levels of debt to which they adjust, albeit only occasionally, when the benefits (e.g., additional tax shields) of adjusting to the target capital structure exceed the adjustment costs.

In this paper, we examine the speeds of adjustment to target capital structure observed at points in time when the benefits of adjustment to target are likely to exceed its costs. At these times, we expect the likelihood of full adjustment to be high and, thus, the estimated speed of adjustment to be close to one.

We follow two strategies to identify such points. First, we rely on ex-ante firm characteristics for identification. The specific characteristic we use is the amount of long-term debt expected to mature over the course of the year. This amount is predetermined by decisions made prior to the current year and has to be financed one way or another. As a result, the financing costs are likely to be substantially sunk, giving the firm an opportunity to adjust to the target at a relatively low marginal cost. Thus, if target leverage were important, we would expect these firms to refinance the maturing debt in a way that would allow them to reach their targets.<sup>18</sup>

Our second approach is to use corporate actions to identify ex-post the cases where the adjustment should be full. Specifically, we focus on years with significant corporate financing activity. The motivation for this empirical strategy is that, whereas existence of fixed transactions costs may deter firms from adjusting to target capital structure, for firms that initiate corporate financing transactions, the incremental cost of doing so in a manner that allows them to adjust to the target should be low. This approach is similar to Faulkender, Flannery, Hankins, and Smith (2007). However, we distinguish between different corporate financing transactions and identify years when firms initiate more than one type of a transaction, such as when firms

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<sup>18</sup> This type of behavior is modeled in Ju, Parrino, Poteshman, and Weisbach (2005).

issue both debt and equity or issue debt and repurchase equity. The argument for full adjustment is especially powerful in these cases as there would be little or no incremental costs associated with such an adjustment. Lack of full adjustment in these cases would be inconsistent with the notion that firms have target debt ratios to which they adjust when the benefits of adjustment exceed its costs.<sup>19</sup>

The most popular approach in recent years has been to estimate the target debt ratio as the fitted value from a regression of observed debt ratios on a set of firm characteristics believed to proxy for the factors identified by the tradeoff theory as important determinants of the target.<sup>20</sup> This approach allows the target debt ratio to vary not only cross-sectionally but also in the time series as the firm's characteristics change over time. To account for persistence in leverage documented by Lemmon, Roberts, and Zender (2007), we use regressions with fixed firm-specific effects.<sup>21</sup> In light of the findings of Shyam-Sunder and Myers (1999) and Chang and Dasgupta (2008) that partial adjustment regressions can produce spuriously significant estimates of the speed of adjustment, we follow recommendations in Hovakimian and Li (2009) to minimize the impact of this bias.

Our results show that the average speed of adjustment in our sample is around 9-11 percent. In the context of dynamic tradeoff models with fixed adjustment costs, our results imply that an average firm in our sample fully adjusts only once every 9-11 years. We then examine whether the speeds of adjustment are much higher at points in time when the incremental adjustment costs are low. We find that firms, especially those above their leverage targets, adjust

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<sup>19</sup> As Leary and Roberts (2005) have shown, full adjustment occurs only if the adjustment costs are fixed. Variable adjustment costs lead to small adjustments and disappearance of a well-defined target to which the adjustment is made.

<sup>20</sup> This is done either explicitly by estimating the target in the first stage and then using it as an independent variable in the second stage (see, e.g., Fama and French (2002)), or implicitly by estimating a reduced form model in a single step (see, e.g., Flannery and Rangan (2006)).

<sup>21</sup> Flannery and Rangan (2006) argue that firm-level fixed effects regressions generate sharper target leverage estimates, which explains the higher estimates of adjustment speeds generated by these regressions.

faster in years when they have large amounts of maturing long-term debt that needs to be refinanced. However, even in these cases, the average adjustment speed does not exceed twenty one percent.

When we classify the sample firm-years based on financing activity, the highest speed of adjustment to market leverage target (12.1%) is obtained in years when firms issue both debt and equity. The highest speed of adjustment to book leverage target (16.2%) is obtained in years when firms repurchase both debt and equity. Yet, even these speeds of adjustment are well below one.

These results are somewhat contradictory. The economic and statistical significance of the speed of adjustment in the overall sample and in various subsamples is consistent with some sort of adjustment to target. Yet, the partial-adjustment model fails to generate high estimates of adjustment speed in cases when the likelihood of full adjustment should be high.

It is possible that our target proxy is simply so noisy that the estimates of the speed of adjustment remain low even when firms fully adjust to the true target. Although this explanation is impossible to refute, we address the poor proxy issue in our robustness analyses, where we use alternative estimators of the speed of adjustment, including some that prior research has shown to be substantially biased upward. These robustness checks do not alter our conclusions.

We also pursue alternative testing strategies that are less sensitive to errors in measuring the target. For example, whereas measurement error in target debt ratio may lead to low estimates of the speed of adjustment, its impact on the direction of the adjustment is likely to be less important. Our results show that a significant fraction of debt issues and reductions (30-40 percent) and an even larger fraction of equity issues and repurchases (more than 60 percent) are associated with adjustments away from the target. These results imply that deviations from

target-adjustment behavior are quite common.

In addition, we find that the proportion of firms with post-transaction debt ratios within the 95% confidence interval around the target leverage is very small, ranging from 1-2 percent for equity issues accompanied by debt reductions to about 11-12 percent for equity issues. These results suggest that corporate financing transactions produce debt ratios that are not statistically close to our estimated targets. Furthermore, our results show that instead of adjusting closer to the target, many above-target firms simply become below-target as a result of corporate financing transactions and vice versa.

Finally, we examine the quartiles of deviations from target leverage before and after the corporate financing transactions. With the exception of equity issues, the results for median values of pre-transaction deviations from the target are mostly consistent with the traditional argument that overlevered firms tend to undertake transactions that reduce leverage whereas underlevered firms tend to undertake leverage increasing transactions. However, we find that the interquartile range of deviations from target becomes somewhat wider after the transactions. This is inconsistent with the hypothesis that firms move towards their target debt ratios when undertaking corporate financing transactions, as the interquartile range of deviations from target leverage should become smaller following such transactions.

The findings that a large fraction of firms adjust beyond the target when undertaking corporate financing transactions and thus exhibit adjustment speeds in excess of one and that a substantial proportion of firms exhibit negative speeds of adjustment, questions the standard interpretation of partial-adjustment coefficients as economically meaningful measures of the importance of target debt ratios. An average speed of adjustment of 0.25 may be observed, for example, in a sample where 50% of firms have a “speed of adjustment” of  $-0.75$  and 50% have a

“speed of adjustment” of +1.25. It is difficult to argue that the target debt ratio is more important for firms in such a sample than in a sample where 57.5% of firms have a “speed of adjustment” of  $-0.75$  and 42.5% have a “speed of adjustment” of +1.25, with an average “speed of adjustment” of 0.1.

Our results imply that a substantial fraction of firms in our sample behave in ways that are not consistent with the behavior implied by the standard dynamic tradeoff theories. The behavior that underlies and justifies the use of partial adjustment models arises in theoretical models of dynamic capital structure choice under fixed adjustment costs. However, this is not the only possible dynamic tradeoff behavior. As Leary and Roberts (2005) show, for example, in the presence of a significant proportional component to adjustment costs, there may not be a unique “target” level of leverage to which firms adjust when they hit the boundaries of the target range of debt ratios. Analyses of such alternative dynamic tradeoff strategies may be a fruitful direction for future capital structure research.

The paper proceeds as follows. Section I describes the data. Section II examines the determinants of target leverage. Section III presents partial adjustment regression results. Section IV presents the adjustment speeds for subsamples with low adjustment costs. Section V presents alternative evidence on adjustment to target. Section VI summarizes our conclusions.

## **2. Data**

Our initial sample is drawn from Annual Compustat files covering the 1970-2007 period.<sup>22</sup> To minimize the influence of outliers and misreported data in our analysis, we exclude firms with values of total assets or sales less than one million dollars and replace extreme

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<sup>22</sup> Because we need to calculate changes in leverage as well as use lagged values of some variables, observations used in partial adjustment regressions start in 1974.

observations of all ratio variables with missing values.<sup>23</sup> As in other studies of capital structure, we exclude financial firms (SIC codes 6000-6999) from the sample. The resulting base sample consists of 138,631 firm-year observations.<sup>24</sup>

## 2.1 Target leverage

We use both book and market value based measures of leverage in our analyses. Following Hovakimian, Opler, and Titman (2001), Faulkender and Petersen (2006), Flannery and Rangan (2006), Lemmon, Roberts, and Zender (2008), and others, the debt ratio, DR, is defined as the sum of long-term and short-term debt scaled by the book (for book leverage) or the market (for market leverage) value of assets.<sup>25</sup>

We model the target debt ratio as the predicted value from the following regression of observed debt ratios.

$$(1) \quad DR_{it+1} = \alpha_i + \beta X_{it} + \varepsilon_{it+1}.$$

The set of independent variables,  $X$ , used in our specification of target debt ratio model (1) includes firm size, asset tangibility, market-to-book, research and development (R&D) expenses, and industry median leverage ratio, considered earlier by Flannery and Rangan (2006), among others.<sup>26</sup>

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<sup>23</sup> Extreme values are defined as those in one percent of both tails of the distribution. Variables that take on only positive (negative) values are trimmed only on the left (right) tail of the distribution. Leverage ratios are trimmed at the value of one.

<sup>24</sup> The number of usable observations varies by the analysis performed due to availability of required data.

<sup>25</sup> Book debt ratio is [short-term debt (Compustat data 34) + long-term debt (data 9)]/assets (data 6). Market debt ratio is [short-term debt + long-term debt]/[assets – book equity + market equity]. Market equity is price (data 199) times shares outstanding (data 25). Book equity is the book value of stockholders' equity, plus balance sheet deferred taxes and investment tax credit (if available), minus the book value of preferred stock. Depending on availability, we use the redemption (data 56), liquidation (data 10), or par value (data 130) to estimate the book value of preferred stock. Stockholders' equity is (data 216), if it is available. If not, we measure stockholders' equity as the book value of common equity (data 60) plus the par value of preferred stock, or the book value of assets minus total liabilities (data 181).

<sup>26</sup> These variables have also been considered by Titman and Wessels (1988), Rajan and Zingales (1995), and others. Size is the natural log of sales (data 12), adjusted for inflation. Tangibility is the property, plant, and equipment (data 8) scaled by total assets. R&D is the research and development expense (data 46) scaled by sales. Selling expense is

Larger firms are expected to have higher target debt ratios as they are less risky and therefore have more debt capacity. Firms with high asset tangibility are expected to target higher leverage ratios as tangible assets can serve as collateral reducing the costs of financial distress. Firms with high market-to-book ratios and high R&D intensity (good investment opportunities) are expected to target low leverage ratios as high leverage may lead to problems associated with debt overhang (Myers (1977)). Because a large number of firms with no R&D do not report it, we replace missing values of R&D with zeroes. Since firms that do not report R&D may be different from those that do, we include an indicator variable set to one for firms with non-missing R&D.

Holding leverage constant, the probability of default should decline with profitability, allowing more profitable firms to target higher debt ratios. Bradley, Jarrell, and Kim (1984) document strong industry effects in the cross-section of corporate leverage ratios, suggesting that there could be otherwise unobservable determinants of capital structure, such as business risk, that vary across industries, but are relatively constant across firms within the same industry. Motivated by the findings in Lemmon, Roberts, and Zender (2007) that debt ratios are very persistent, we include firm indicator variables (fixed firm effects,  $\alpha_i$ ) in regression (1). Table 1 presents the sample averages for these firm characteristics.

Table 2 presents the results for book and market specifications of target leverage regression (1) estimated on our full sample with fixed firm effects. The reported t-statistics reflect robust standard errors adjusted for heteroskedasticity and firm-level clustering. Both book and market debt ratios increase with firm size and asset tangibility, consistent with the view that such firms are generally less risky and, thus, can afford to have more debt in their capital

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selling, general, and administrative expense (data 189) over sales. Market-to-book is (total assets – book equity + market equity)/total assets. We use forty-nine Fama-French industry definitions throughout this paper. These definitions are downloaded from Kenneth French’s online data library.

structure. The debt ratios tend to be lower for firms with higher values of market-to-book, consistent with the traditional argument that such firms should protect their valuable growth opportunities by choosing less levered capital structures. The impact of industry leverage is positive and statistically significant. Firms that report R&D tend to choose significantly lower book debt ratios likely to protect the intangible value created by R&D. Inconsistent with this argument, however, the book debt ratios tend to increase with R&D. The effects of both R&D variables in the market debt ratio regression are insignificant.

### 3. Partial adjustment regressions

Prior literature relies on the following standard partial adjustment model to examine how well deviations from target debt ratios predict changes in leverage.

$$(2) \quad DR_{i,T+1} - DR_{i,T} = \alpha + \lambda(DR_{i,T+1}^* - DR_{i,T}) + \varepsilon_{iT+1}, \text{ where}$$

$$(3) \quad DR_{i,T+1}^* = \hat{\beta}X_{iT}.$$

Equation (3) states that  $DR^*$  is the predicted value obtained from regression (1). In the absence of frictions, firms would fully adjust to their target each period. In the presence of adjustment costs, firms would not continuously fully adjust. In this framework,  $\lambda$  measures the average speed of adjustment to the target.

Two approaches have been used in the literature to estimate the partial adjustment model (2)-(3). Some studies (e.g., Flannery and Rangan (2006)) estimate the speed of adjustment in a single step by substituting (3) into (2) and rearranging to obtain the following reduced-form partial adjustment model.

$$(4) \quad DR_{i,T+1} = \alpha + \lambda\beta \times X_{iT} + (1 - \lambda) \times DR_{i,T} + \varepsilon_{iT+1}.$$

Other studies (Fama and French (2002), Kayhan and Titman (2007)) estimate the model

in two steps, with target leverage regression (1) estimated in the first step, followed by the partial adjustment regression (2) in the second step. In this paper, we use the two-step model. The primary reason for this is the flexibility afforded by this choice. Specifically, the goal of the current paper is to examine the speeds of adjustment in subsamples where it should be very high if firms behave in ways consistent with the standard dynamic tradeoff models of capital structure. The two-step approach allows us to use the same proxy of the target from the first stage to examine how the speeds of adjustment vary across different subsamples.

The estimates of the speed of adjustment generated by both one-step and two-step versions of the partial adjustment model (2)-(3) have been shown by Shyam-Sunder and Myers (1999) and Chang and Dasgupta (2008) to be biased in favor of the target adjustment hypothesis. Hovakimian and Li (2009) examine various specifications of the partial adjustment model and find that it is possible to minimize and even eliminate the bias by (1) using rolling historical fixed effects regressions to predict the target debt ratio and (2) using  $\lambda^*$  from the following modified specification of the partial adjustment regression as an estimate of the speed of adjustment.

$$(5) \quad DR_{i,T+1} - DR_{i,T} = \alpha + \lambda^* \times DR_{i,T+1}^* - \lambda \times DR_{i,T} + \varepsilon_{iT+1}.$$

Note that equation (2) is simply a restricted version of equation (5) with the coefficients  $\lambda$  and  $\lambda^*$  restricted to be equal in magnitude. Also note that two-step estimation of model (5) produces exactly the same estimate of  $\lambda$  as the one-step model (4) if the first stage target debt ratio regression is a full sample regression of  $DR_T$  on  $X_{iT}$ .<sup>27</sup> However, one-step model (4) does not produce an estimate of  $\lambda^*$  and relies on  $\lambda$  as an estimate of the speed of adjustment.

There are various reasons why the magnitudes of the estimates of  $\lambda$  and  $\lambda^*$  may diverge.

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<sup>27</sup> As we discuss below, our first stage regressions are not estimated on the full sample to avoid introducing a look-ahead bias in our results.

One important reason is that firms with extremely low or high debt ratios experience what Chang and Dasgupta (2008) call mechanical mean-reversion: As the debt ratios are constrained between zero and one, low debt ratios tend to increase whereas high debt ratios tend to decrease. This effect biases the estimates of  $\lambda$  upward, making it easier to reject the null hypothesis that there is no adjustment to target. In addition, the estimates of  $\lambda$  and  $\lambda^*$  may also diverge because  $DR^*$  in regression (5) is only an estimate obtained from regression (1) and as such measures the target debt ratio with error. This could bias  $\lambda^*$  downward, making it harder to reject the null hypothesis that there is no adjustment to target. Thus,  $\lambda^*$  could be thought of as a more conservative estimate of the speed of adjustment. In the tables that follow, we report the estimates of both  $\lambda$  and  $\lambda^*$  generated by the two-step partial adjustment model (1), (5). In addition, we conduct analysis of the sensitivity of our conclusions to alternative specifications of the partial adjustment model.

It is important to note that we estimate the first stage regression (1) with firm fixed effects. A number of earlier studies, e.g., Flannery and Rangan (2006), argue that fixed firm effects produce sharper estimates of the target. Hovakimian and Li (2009) show, however, that full sample fixed effects models (both one-step and two-step) lead to substantial look-ahead bias in the estimates of the speed of adjustment. Their simulations suggest that using only historical data (up to year  $T$ ) to estimate the fixed effects target for year  $T+1$  resolves this problem. We follow this approach in our paper.

### **3.1 Full sample results**

Table 3 presents the results of the partial-adjustment regression (5) estimated on our full sample. The proxies for the target book and market debt ratios are generated based on regressions similar to those reported in Table 2, but using rolling subsamples of historical data to

obtain  $\hat{\beta}$ . Specifically,  $\hat{\beta}$  is obtained by panel regression (1) of debt ratios,  $DR$ , observed in years 2 through  $T$  on firm characteristics,  $X$ , observed in years 1 through  $T-1$ . The regressions are estimated with fixed firm effects. Proxies for target at time  $T+1$ ,  $DR_{i,T+1}^*$ , are then generated by the predicted value  $\hat{\beta}X_{iT}$ , i.e., out of sample.

The estimates in Table 3 are in the spirit of Fama and MacBeth (1973). Specifically, we estimate the second stage regression (5) annually on cross-sectional data. Table 3 reports the averages of the annual estimates of the speeds of adjustment and the  $R^2$ . The reported t-statistics are based on the time-series standard errors of the average coefficients. This approach follows Fama and French (2002) who report that cross-sectional correlation in residuals leads to substantially inflated t-statistics in pooled cross-section time-series partial adjustment regressions. They find, however, that autocorrelation in annual slopes of second stage partial-adjustment regressions is not a serious problem.<sup>28</sup>

An additional benefit of this approach is that year-by-year variation in  $\lambda^*$ , which determines its standard error, incorporates the estimation error due to the fact that target debt ratios,  $DR^*$ , used in Table 3 are estimated by annual first stage regressions. Alternative t-statistics, based on robust standard errors with annual clustering or bootstrapped standard errors with annual clustering, are similar but tend to be somewhat larger.<sup>29</sup>

The estimates of the adjustment speed,  $\lambda^*$ , in Table 3 are statistically significantly different from zero, with the market leverage speed of adjustment at 0.089 and the book leverage speed of adjustment at 0.113. These estimates are close in magnitude to the estimates in Fama and French (2002) and Kayhan and Titman (2007) and are at the low end of the range of

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<sup>28</sup> Fama and French (2002) find that the autocorrelation in annual slopes is a serious problem for regressions of leverage levels but not leverage changes.

<sup>29</sup> Petersen (2009) finds that clustered standard errors are downward biased when the number of annual clusters is small.

estimates provided by prior studies of the speed of adjustment to target capital structure. They are also similar to the estimates of the speed of adjustment reported in Hovakimian and Li (2008) based on specifications of the partial adjustment model that are free of the bias in favor of the target adjustment hypothesis. Shyam-Sunder and Myers (1999) and Chang and Dasgupta (2008) find such bias in various specifications of the partial adjustment model.

#### **4. Do firms fully adjust when the adjustment costs are low?**

The notion of the speed of adjustment to target relies on the existence of a unique well-defined target debt ratio from which firms may deviate but to which they eventually adjust. The theoretical motivation for the empirical analysis of corporate capital structure decisions in partial adjustment framework comes from dynamic tradeoff models such as those of Fischer, Heinkel, and Zechner (1989), Goldstein, Ju, and Leland (2001), and Strebulaev (2007).

Figure 1 illustrates typical leverage dynamics implied by these models. Firms allow their debt ratios to drift passively away from the target,  $DR^*$ , and adjust fully to the target once the benefits of adjustment exceed fixed adjustment costs. The latter happens when the leverage ratio reaches the upper,  $\overline{DR}$ , or the lower,  $\underline{DR}$ , boundary of the target leverage range.

As Figure 1 illustrates, our finding that the average speed of adjustment in a broad sample of Compustat firms is around 9-11 percent per year implies that such full adjustments take place once every 9-11 years, on average. Our goal now is to identify the years when these adjustments are likely to take place and examine whether the estimated speeds of adjustment are indeed close to unity in these years.

#### 4.1 Speed of adjustment by current maturity long-term debt

In this section, we use current maturity long-term debt to identify the years when the benefits of adjustment are likely to exceed its costs. The terms of the maturing long-term debt have been set years ago. At the time of maturity, the amount due has to be financed, one way or another. In this sense, maturing long-term debt is an exogenous variable that allows us to identify cases when the adjustment costs are largely sunk, allowing the firm to adjust to the target at a relatively low incremental cost. The hypothesis is that if target leverage is important, then the firm should use this opportunity and choose a financing mix that allows it to adjust to the target fully.

The estimated speeds of adjustment for subsamples based on the amount of maturing long-term debt are presented in Table 4. Panel A of Table 4 presents the adjustment speeds for three sub-samples sorted by the beginning-of-the-year ratio of maturing long-term debt to total assets. Firms in the bottom and the top ten percent of values of maturing debt are in, respectively, the Low and the High maturing debt subsamples, with the rest of the firms in the Medium group.<sup>30</sup>

For both market and book debt ratios, the results support the hypothesis that firms whose adjustment costs are more likely to be sunk adjust more quickly. The speeds of adjustment are the highest for firms in the highest maturing debt group. Importantly, however, the estimated speeds never come close to full adjustment, neither for market (0.157) nor for book (0.110) debt ratios.

Incremental adjustment costs of firms with maturing debt could depend on whether the firm is above or below its target leverage. Overlevered firms could adjust to target by simply refinancing a portion of their maturing debt with equity (internal or external). The incremental

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<sup>30</sup> Observations with no maturing long-term debt are excluded from this table.

cost of reducing debt instead of rolling it over could be particularly low if the firm paid off the excess debt with internal funds. In contrast, underlevered firms would have to raise more new debt than the amount due, and, possibly, use the surplus of funds to repurchase equity.

The results in Panel B, which reports the speeds of adjustment for overlevered firms, confirm this conjecture. The results show that, for both book and market leverage, the adjustment speed increases with maturing debt. These results imply that firms time net reductions in debt to periods of high maturing debt, consistent with the argument that the incremental adjustment costs are lower at such times. The highest estimated speeds of adjustment (0.210 for market leverage and 0.201 for book leverage), observed for overlevered firms in the top ten percent of distribution of maturing debt, are about twice as high as the adjustment speeds in the overall sample (Table 3). Still, even these higher speeds are nowhere close to full adjustment.

Panel C reports the speeds of adjustment for underlevered firms. Overall, the speeds of adjustment in Panel C are lower than the speeds observed in Panel B for overlevered firms. Furthermore, the estimated speed of adjustment declines with the amount of maturing debt, implying that the incremental costs of adjusting to target capital structure are higher for underlevered firms with maturing debt.

To summarize, while we observe significantly higher speeds of adjustment when firms, especially overlevered ones, have large amounts of maturing debt, the adjustment speeds are still very low. It is possible that our target proxy is simply so noisy that the estimates of the speed of adjustment remain low even when firms fully adjust to the true target. Although this explanation is impossible to refute, we should note that this proxy reflects the current state of the literature on capital structure and that proxies of this type have supplied much of the evidence on the existence of target leverage in this literature. In addition, to reduce the likelihood of our results

being driven by our methodological choices, we examine the robustness of our conclusions to a variety of alternative methods of estimating the speed of adjustment.

First, we note that coefficients  $\lambda$  provide alternative, and higher, estimates of the speed of adjustment. Consistent with our results for  $\lambda^*$ , the highest  $\lambda$  estimates in Table 4 (0.322 for market leverage and 0.302 for book leverage) are observed for overlevered firms with high amounts of maturing long-term debt. Although the higher values of  $\lambda$  have been shown to be upward biased (Hovakimian and Li, 2009), even these estimates are nowhere close to full adjustment.

Second, we generate more traditional (but shown to be upward biased) estimates of the speed of adjustment based on equation (2) rather than equation (5). We find these estimates to be slightly lower than the reported coefficients  $\lambda$  and, thus, to lead to the same conclusion that the speeds of adjustment are well below full. Third, we experiment with alternative specifications of the target model (1). For example, we drop R&D variables from the target regression (1) since they have insignificant or counterintuitive coefficients in Table 2. We also add other potential determinants, such as profitability and depreciation, that have been used in some prior studies to generate proxies for target capital structure. In all cases, the estimated speeds of adjustment remain close to the numbers reported in Table 3 and, thus, do not alter our conclusions.

#### **4.2 Speed of adjustment by financing activity**

In this section, we pursue our second strategy to identify the years where the adjustment should be full. Specifically, we examine the speeds of adjustment to target debt ratio observed in years when firms undertake corporate financing transactions. Although fixed transactions costs may deter firms from adjusting to target capital structure, firms that undertake financing transactions, e.g., to finance corporate investment, have already chosen to incur the associated

transaction costs. As a result, the marginal cost of doing so in a manner that would allow them to adjust to the target should be low.

Since the cost structure is likely to vary across different corporate financing transactions, we differentiate between issues and repurchases of equity and debt.<sup>31</sup> We further differentiate the cases when there are issues and repurchases of alternative securities in the same year. The argument for full adjustment is especially powerful in the cases when firm undertake more than one transaction. Hovakimian, Hovakimian, and Tehranian (2004), for example, argue that firms that issue both debt and equity (dual issuers) are likely to fully adjust to the target. For firms that undertake both debt and equity transactions in the same year, the marginal cost of doing so in a way that the resulting capital structure is close to the target should be especially low.

Table 5 presents the adjustment speeds estimated for our sample observations classified into nine groups based on the nature of corporate financing activity. The variation in the speed of adjustment suggests that incremental costs of adjustment to target vary across different transactions, which justifies our identification strategy of considering these transactions separately. The first row of results is for years with no financing activity. The estimated speeds of adjustment are negative and economically trivial at -0.005 for market and -0.027 for book leverage.

The highest estimated speed of adjustment to market leverage target (0.121) is obtained in the case of dual debt and equity issues, consistent with the argument that the incremental costs for resetting the firm's capital structure to the target should be lower for such firms. The highest

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<sup>31</sup> Net equity issued is defined as equity issued (data 108) minus equity repurchased (data 115). Net debt issued is defined as long-term debt issuance (data 111) minus long-term debt reduction (data 114). For observations with format code 1, long-term debt reduction data item in Compustat includes reclassification of long-term debt that becomes due within one year as short-term debt. We restore the consistency of the definition of long-term debt reduction by adding debt due in one year (data 44) to long-term debt reduction (data 114) when format code is 1. Observations with missing values of external financing data are dropped from the sample.

speed of adjustment to book target (0.162) is observed in the case of debt reductions accompanied by equity repurchases, confirming the intuition that dual transactions should be more likely to be associated with target adjustment behavior. The estimated speeds of adjustment are much lower in the cases of other transactions.

As in the previous subsection, we perform sensitivity analysis of our conclusions using estimates of the speed of adjustment based on coefficients  $\lambda$ . For both market and book debt ratios, the highest estimates of the speed of adjustment (respectively, 0.317 and 0.319) are observed in the case of dual debt and equity issues. The results are similar when we use equation (2) rather than equation (5) to estimate the speeds of adjustment or when we use alternative sets of determinants of the target described in the previous section. In all cases, the coefficient estimates are well below full adjustment.

Our results, thus far, are somewhat contradictory. On one hand, we find that the average speed of adjustment in our sample is around 9-11 percent and substantially higher in certain subsamples. On the other hand, we find no evidence of full adjustment when we examine corporate financing transactions that should be used to effect these adjustments. Indeed, the adjustment speeds observed when firms engage in significant corporate financing activity are fairly low. An average firm rarely fully adjusts to the target when it issues or repurchases debt or equity. This is inconsistent with the behavior implied by the standard dynamic tradeoff theories as represented in Figure 1.

## **5. Alternative tests of adjustment to target**

In this section, we depart from the partial-adjustment regression framework we have used earlier in the paper and pursue a less parametric approach. Our goal is to use analyses that are

more robust to misspecification of the target adjustment model and mismeasurement of the target debt ratios.

### **5.1 The direction of adjustment**

Whereas measurement error in target debt ratio may lead to low estimates of the speed of adjustment, its impact on the direction of the adjustment is likely to be less important. Therefore, in this section, we examine the fractions of firms that adjust in the direction of the target.<sup>32</sup> This analysis also allows us to assess whether the low speeds of adjustment arise because few firms adjust towards the target or because firms do not adjust fully.

Table 6 summarizes the results separately for each of the nine corporate financing activity groups. The fraction of firms adjusting in the direction of the target varies between 36 and 68 percent for market debt ratios and between 36 and 63 percent for book debt ratios. The highest fractions of adjustments towards the target (68 percent for market and 63 percent for book leverage) are observed for firms that issue debt while also repurchasing equity. Debt transactions, in general, tend to be more likely to be associated with adjustments in the direction of the target. The smallest fractions of adjustments towards the target are observed for pure equity transactions. When not accompanied by debt transactions, equity issues and repurchases are associated with adjustments in the direction of the target only 36-39 percent of the time. Overall, the fraction of firms that seem to deviate from the target-adjustment policy when issuing or repurchasing debt or equity is substantial. More than sixty percent of pure equity and more than thirty five percent of pure debt transactions are associated with adjustments away from the target, i.e., exhibit negative speeds of adjustment.

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<sup>32</sup> The adjustment is considered to be towards the target for underlevered firms that experience increases in their leverage ratios and overlevered firms that experience declines in their leverage ratios.

## 5.2 How close to the target do firms adjust?

Because target leverage is only an estimate, it is measured with error. To gauge whether corporate financing transactions produce debt ratios that are statistically close to our estimated targets, we use the target leverage regression results to generate ninety-five percent confidence intervals around the targets.

Table 7 reports the percentages of sample firms with their debt ratios below, within, and above the target leverage confidence interval. Panel A contains the results for market leverage. Panel B contains the results for book leverage. The first three columns of the results in each panel are for the post-transaction debt ratios,  $DR_{T+1}$ , and the corresponding targets,  $DR^*_{T+1}$ . To help more clearly identify the impact of each corporate financing transaction, the last three columns in each panel report the distribution of pre-transaction debt ratios,  $DR_T$ , around the same target leverage,  $DR^*_{T+1}$ . The table contains a number of interesting results.

First, the proportion of firms with post-transaction debt ratios within the target leverage confidence interval is relatively small, ranging from 1-2 percent for equity issues accompanied by debt reductions to about 11-12 percent for equity issues. Furthermore, for six (eight) corporate financing groups, the proportion of firms within the market (book) leverage confidence interval significantly declines as a result of the transaction, with the changes for the remaining three (one) corporate financing groups statistically insignificant. Thus, it does not appear that these transactions bring many firms closer to the target.

Second, the results show that corporate financing transactions induce significant redistribution of firms across the regions outside of the target leverage confidence interval. Specifically, leverage reducing transactions, such as equity issues and debt reductions, result in a significant reduction of the proportion of firms above the target leverage confidence interval and

a significant increase in the proportion of firms below the target leverage confidence interval. Leverage increasing transactions, such as debt issues, result in a significant reduction of the proportion of firms below the target leverage confidence interval and a significant increase in the proportion of firms above the target confidence interval. This redistribution tends to be especially significant in the cases of debt issues and debt reductions. These results imply that, instead of adjusting closer to the target as a result of corporate financing transactions, many above-target firms simply become below-target and vice versa. Using partial-adjustment terminology, such firms adjust beyond the target when undertaking corporate financing transactions and thus exhibit adjustment speeds in excess of one.

This result, along with our earlier finding that the proportion of issue and repurchase transactions with negative speeds of adjustment is also substantial, questions the standard interpretation of partial-adjustment coefficients as meaningful estimates of the speed of adjustment. For example, whereas the economic and the statistical significance of the partial-adjustment coefficient of 0.11 in our sample suggests that a significant fraction of our sample firms tend to maintain their market debt ratios around certain target levels, literal interpretation of this coefficient estimate as implying that it takes nine years for an average firm to adjust to the target is a stretch. A speed of adjustment of 0.11 could be obtained, for example, if half the time our sample firms exhibited a speed of adjustment of 1.5 and the other half of the time the speed of adjustment was  $-1.28$ , with no firm ever adjusting to the target.

For an alternative assessment of how corporate financing transactions affect the deviations from target leverage, Table 8 reports the 25<sup>th</sup>, 50<sup>th</sup>, and 75<sup>th</sup> percentiles of the deviations from target leverage observed pre-transaction ( $DR^*_{T+1}-DR_T$ ) and post-transaction ( $DR^*_{T+1}-DR_{T+1}$ ) for each of nine corporate financing groups considered earlier. Panel A reports

the results for market leverage whereas Panel B reports the results for book leverage. Several observations can be made based on the results in Table 8.

First, the results for median values of pre-transaction deviations from the target are mostly consistent with the traditional finding in the literature that overlevered firms tend to undertake transactions that reduce leverage whereas underlevered firms tend to undertake leverage increasing transactions. The only exceptions to this “rule” are that equity issuers and firms that retire debt and repurchase equity in the same year are underlevered before the transaction and become even more underlevered after the transaction.

However, if firms indeed adjust to their targets as a result of corporate financing transactions, then the range of deviations from target leverage should become smaller following such transactions. We find no evidence of tightening of the interquartile range of deviations from target leverage. Quite to the contrary, the range is somewhat larger after the transaction for all nine corporate financing groups. Thus, the results in Table 8 are not consistent with the hypothesis that firms adjust towards target capital structure when they issue or repurchase securities.

### **5.3 Discussion**

The results of the alternative tests of adjustment to target reported in Tables 7-8 are consistent with our earlier partial-adjustment results that firms in our sample do not adjust to the target even when the marginal costs of doing so should be low. These results also suggest that only very large errors may be able to reconcile these results with the hypothesis that these firms adjust to the target. Furthermore, for the results in Tables 6 and 7 to be explained by a poor proxy problem, the proxy we use would have to be not only poorly measured but also upward biased for firms that undertake leverage decreasing transactions and downward biased for firms

undertaking leverage increasing transactions. Overall, it appears unlikely that measurement errors implicit in our proxies for target leverage are responsible for the low speeds of adjustment.

If the target estimates are not grossly mismeasured and biased, then the results in Tables 6-8 imply that a substantial fraction of firms in our sample behave in ways that are not consistent with the behavior implied by the standard dynamic tradeoff theories as represented in Figure 1. With about forty percent of major financing transactions associated with adjustments away from the target (i.e., negative speed of adjustment) and many firms adjusting beyond the target (i.e., having a speed of adjustment in excess of one), Figure 1 is unlikely to reflect the financing behavior of the majority of our sample firms.

Two conclusions are possible based on the presented evidence. First, tradeoff considerations may play only a minor role in corporate financing policies. Second, if firms follow a dynamic tradeoff strategy, it may not be the kind shown in Figure 1. There are alternative views of dynamic tradeoff behavior that could reconcile the qualitative importance of deviation from target leverage and the inability of partial-adjustment regressions to generate full adjustment speeds at points when, according to standard dynamic tradeoff models, the likelihood of full adjustment should be high.

Leary and Roberts (2005) show, for example, that in the presence of variable adjustment costs, there may not be a single “target” level of leverage to which firms adjust when they hit the boundaries of the target range of debt ratios. Figure 2 illustrates the typical leverage dynamics with proportional costs of adjustment, whereas Figure 3 illustrates the dynamics when adjustment costs have both fixed and weakly convex components. Neither of these dynamics is likely to generate high speeds of adjustment to a “target” level, which the standard estimation procedures will place somewhere in the middle of the target range.

Recent survey evidence also suggests that tradeoff factors may lead to a target range of debt ratios with no single target level. In their survey 392 corporate CFOs, Graham and Harvey (2001) report that most financial executives focus on ranges of target debt ratios rather than specific targets. Analyses of alternative dynamic tradeoff strategies may be a fruitful direction for future capital structure research.

## **6. Conclusions**

This paper examines the speeds of adjustment to target leverage when the benefits of adjustment to target are likely to exceed its costs and the likelihood of full adjustment is high. Specifically, large amounts of maturing long-term debt that has to be refinanced one way or another provide the firm with an opportunity to adjust to the target at a relatively low marginal cost. Similarly, whereas existence of fixed transactions costs may deter firms from adjusting to target capital structure, for firms that initiate corporate financing transactions, the marginal cost of doing so in a manner that allows them to adjust to the target should be low.

We find that even in these cases firms do not fully adjust, although the estimated speeds of adjustment are substantially higher. Furthermore, a substantial fraction of major financing transactions are associated with adjustments away from the target or adjustments beyond the target. These findings imply that firms behave in ways that are not consistent with the behavior implied by the standard dynamic tradeoff theories.

Our findings challenge the notion that firms have well-defined target debt ratios to which they fully adjust when the adjustment benefits exceed adjustment costs. The findings also challenge the use of partial adjustment regression coefficients to assess the importance of dynamic tradeoff behavior. Finally, our findings suggest that further examination of alternative

dynamic tradeoff strategies may be a fruitful direction for future capital structure research.

Table 1: Sample Statistics

BDR is the book debt ratio defined as  $(\text{short-term debt} + \text{long-term debt})/\text{assets}$ . MDR is the market debt ratio defined as  $(\text{short-term debt} + \text{long-term debt})/(\text{assets} - \text{book equity} + \text{market equity})$ . Market equity is price times shares outstanding. Size is the natural log of sales, adjusted for inflation. Tangibility is the property, plant, and equipment scaled by total assets. Market-to-book is  $(\text{total assets} - \text{book equity} + \text{market equity})/\text{total assets}$ . R&D intensity is the research and development expenses scaled by sales. Missing values of R&D are replaced with zeroes. R&D indicator is set to one for firms with non-missing R&D.

	Mean	Min	Max	Obs
BDR	0.250	0.000	1.000	138,631
MDR	0.216	0.000	0.953	138,631
Size	4.593	-0.668	12.044	138,631
Tangibility	0.332	0.000	1.000	138,631
Market-to-book	1.640	0.089	9.655	138,631
R&D	0.035	0.000	1.201	138,631
R&D indicator	0.605	0.000	1.000	138,631

Table 2: Target leverage regressions

MDR is market debt ratio defined as (short-term debt + long-term debt)/(assets – book equity + market equity). BDR is book debt ratio defined as (short-term debt + long-term debt)/asset. Market equity is price times shares outstanding. Size is the natural log of sales, adjusted for inflation. Tangibility is the property, plant, and equipment scaled by total assets. Market-to-book is (total assets – book equity + market equity)/total assets. R&D intensity is the research and development expenses scaled by sales. Missing values of R&D are replaced with zeroes. R&D indicator is set to one for firms with non-missing R&D. Industry DR is the median industry debt ratio (market or book) based on Fama and French 49 industry definitions. Values significantly different from zero at the 5% and the 1% level are marked \* and \*\* respectively. The reported t-statistics and significance levels reflect standard errors adjusted for heteroskedasticity and firm-level clustering.

	MDR		BDR	
	Coeff.	t-stat.	Coeff.	t-stat.
Size	0.020**	16.1	0.014**	9.6
Market-to-book	-0.023**	40.4	-0.010**	15.6
Tangibility	0.177**	20.6	0.185**	18.7
R&D intensity	0.006	0.7	0.028*	2.3
R&D indicator	0.003	1.0	-0.009**	3.0
Industry DR	0.395**	30.8	0.366**	18.1
R <sup>2</sup>	0.10		0.04	
Observations	138,631		138,631	

Table 3: Second stage partial adjustment regressions

MDR is market debt ratio defined as (short-term debt + long-term debt)/(assets – book equity + market equity). BDR is book debt ratio defined as (short-term debt + long-term debt)/asset. Market equity is price times shares outstanding. DR stands for MDR in market debt ratio regression and for BDR in book debt ratio regression. DR\* is the target debt ratio defined as the predicted debt ratios from rolling first stage historical panel regressions of observed debt ratios on determinants of the target similar to those reported in Table 2. The reported coefficients and R<sup>2</sup> are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively.

	MDR(t+1) - MDR(t)		BDR(t+1) - BDR(t)	
	Coeff.	t-stat.	Coeff.	t-stat.
Constant	0.023**	7.7	0.027**	15.1
DR*(t+1)	0.113**	16.0	0.089**	13.5
DR(t)	-0.204**	20.2	-0.183**	26.8
R <sup>2</sup>	0.07		0.06	
Observations	120,980		120,980	

Table 4: Partial adjustment speed by maturing long-term debt

The table reports the estimates from second stage partial adjustment regressions. MDR is market debt ratio defined as (short-term debt + long-term debt)/(assets – book equity + market equity). BDR is book debt ratio defined as (short-term debt + long-term debt)/asset. Market equity is price times shares outstanding. DR stands for MDR in market debt ratio regression and for BDR in book debt ratio regression. DR\* is the target debt ratio defined as the predicted debt ratios from rolling first stage historical panel regressions of observed debt ratios on determinants of the target similar to those reported in Table 2.  $\lambda^*$  is the coefficient estimate for DR\*(t+1).  $\lambda$  is the coefficient estimate for DR(t). Maturing long-term debt is long-term debt due in one year scaled by total assets. Low maturing debt subsample includes observations with values of maturing long-term debt in the bottom 10 percent of the distribution. High maturing debt subsample includes observations with values of maturing long-term debt in the top 10 percent of the distribution. Medium maturing debt subsample includes observations with values of maturing long-term debt in the middle 80 percent of the distribution. The reported coefficients and R<sup>2</sup> are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively.

	MDR(t+1) - MDR(t)				BDR(t+1) - BDR(t)			
	$\lambda^*$	t-stat.	$\lambda$	t-stat.	$\lambda^*$	t-stat.	$\lambda$	t-stat.
Panel A: Full sample								
Maturing debt: Low	0.108**	7.1	-0.165**	-10.1	0.091**	3.6	-0.169**	-6.6
Maturing debt: Medium	0.093**	11.5	-0.187**	-19.0	0.070**	11.1	-0.161**	-24.9
Maturing debt: High	0.157**	9.4	-0.275**	-14.1	0.110**	5.6	-0.214**	-9.5
Panel B: Firm-years with leverage surplus: DR*(t+1)>DR(t)								
Maturing debt: Low	0.054	0.9	-0.122*	-2.3	0.012	0.3	-0.091*	-2.1
Maturing debt: Medium	0.078**	5.7	-0.178**	-12.9	0.054**	4.4	-0.144**	-12.7
Maturing debt: High	0.210**	5.5	-0.322**	-8.8	0.201**	8.0	-0.302**	-11.1
Panel C: Firm-years with leverage deficit: DR*(t+1)<DR(t)								
Maturing debt: Low	0.157**	5.4	-0.203**	-6.8	0.176**	6.6	-0.261**	-9.4
Maturing debt: Medium	0.106**	8.5	-0.193**	-13.0	0.095**	10.2	-0.189**	-18.3
Maturing debt: High	0.063	2.0	-0.184**	-5.6	-0.024	-0.5	-0.075	-1.6

Table 5: Partial adjustment speed by financing status

The table reports the estimates from second stage partial adjustment regressions. MDR is market debt ratio defined as (short-term debt + long-term debt)/(assets – book equity + market equity). BDR is book debt ratio defined as (short-term debt + long-term debt)/asset. Market equity is price times shares outstanding. DR stands for MDR in market debt ratio regression and for BDR in book debt ratio regression. DR\* is the target debt ratio defined as the predicted debt ratios from rolling first stage historical panel regressions of observed debt ratios on determinants of the target similar to those reported in Table 2.  $\lambda^*$  is the coefficient estimate for DR\*(t+1).  $\lambda$  is the coefficient estimate for DR(t). Security issues (repurchases) are identified as firm-years with net debt or equity issued (repurchased) in excess of five percent of lagged total assets. Net equity issued is equity issued minus equity repurchased, scaled by lagged total assets. Net debt issued is long-term debt issuance minus long-term debt reduction, all scaled by lagged total assets. The reported coefficients and R<sup>2</sup> are the averages obtained from annual cross-sectional regressions. The reported t-statistics are based on the time-series standard errors of the average coefficients. Coefficient estimates significantly different from zero at 5% and 1% level are marked \* and \*\*, respectively.

	MDR(t+1) - MDR(t)				BDR(t+1) - BDR(t)				Observations
	$\lambda^*$	t-stat.	$\lambda$	t-stat.	$\lambda^*$	t-stat.	$\lambda$	t-stat.	
No transaction	-0.005	-0.7	-0.016	-1.4	-0.027**	-4.5	0.021**	3.9	55,688
Equity issue	-0.015	-0.7	-0.113**	-4.3	0.027	0.6	-0.126*	-2.7	4,351
Debt reduction & Equity issue	0.026	1.0	-0.296**	-12.6	0.045	1.6	-0.160**	-5.5	2,810
Debt reduction	0.043**	4.1	-0.092**	-6.2	0.011	1.0	0.029*	-.4	14,800
Debt issue	0.070**	5.1	-0.191**	-13.8	0.046**	4.7	-0.213**	-21.6	24,664
Debt issue & Equity repurchase	0.087	1.7	-0.131**	-3.1	0.070	1.6	-0.177**	-5.4	1,211
Equity repurchase	0.044	1.3	-0.059	-1.7	0.014	0.4	-0.029	-0.8	2,824
Debt reduction & Equity repurchase	0.064	1.4	-0.151**	-3.1	0.162*	2.4	-0.127*	-2.3	388
Debt issue & Equity issue	0.121**	3.8	-0.317**	-11.4	0.044	1.3	-0.319**	-8.8	3,592

Table 6: Fractions adjusting towards the target

The table reports the percentages of firms that adjust to target. MDR is market debt ratio defined as  $(\text{short-term debt} + \text{long-term debt}) / (\text{assets} - \text{book equity} + \text{market equity})$ . BDR is book debt ratio defined as  $(\text{short-term debt} + \text{long-term debt}) / \text{asset}$ . Market equity is price times shares outstanding. Adjusting towards the target is defined as when the change in the debt ratio has the same sign as the deviation from the target debt ratio. The target debt ratio (MDR\* or BDR\*) is defined as the predicted debt ratios from rolling first stage historical panel regressions of observed debt ratios on determinants of the target similar to those reported in Table 2. Security issues (repurchases) are identified as firm-years with net debt or equity issued (repurchased) in excess of five percent of lagged total assets. Net equity issued is equity issued minus equity repurchased, scaled by lagged total assets. Net debt issued is long-term debt issuance minus long-term debt reduction, all scaled by lagged total assets.

	Adjust to MDR* <sub>T+1</sub>	Adjust to BDR* <sub>T+1</sub>	Observations
No transaction	41.93%	40.54%	55,688
Equity issue	38.82%	39.23%	4,351
Debt reduction & Equity issue	52.70%	61.46%	2,810
Debt reduction	56.72%	57.80%	14,800
Debt issue	62.32%	59.42%	24,664
Debt issue & Equity repurchase	67.63%	62.92%	1,211
Equity repurchase	36.86%	36.08%	2,824
Debt reduction & Equity repurchase	46.13%	47.68%	388
Debt issue & Equity issue	57.35%	58.07%	3,592

Table 7: The distribution of observations around the target leverage confidence interval

Panel A: Market leverage

	MDR <sub>T+1</sub> relative to MDR* <sub>T+1</sub>			MDR <sub>T</sub> relative to MDR* <sub>T+1</sub>		
	% below the CI	% within the CI	% above the CI	% below the CI	% within the CI	% above the CI
No transaction	60.34**	6.59**	33.08**	57.95	7.43	34.63
Equity issue	65.04**	10.78**	24.18**	57.34	12.16	30.50
Debt reduction & Equity issue	80.43**	1.85**	17.72**	45.05	3.77	51.17
Debt reduction	66.51**	1.90**	31.59**	37.39	2.83	59.78
Debt issue	29.82**	2.31**	67.87**	60.38	4.49	35.13
Debt issue & Equity repurchase	32.54**	3.06	64.41**	66.89	3.88	29.23
Equity repurchase	66.61	8.71	24.68*	67.60	9.21	23.19
Debt reduction & Equity repurchase	76.80**	3.09	20.10**	51.29	2.84	45.88
Debt issue & Equity issue	42.57**	2.48**	54.96**	60.41	6.43	33.16

Panel B: Book leverage

The table reports the percentages of post-transaction and pre-transaction debt ratios below, within, and above the 95% confidence interval (CI) around the target. MDR is market debt ratio defined as (short-term debt + long-term debt)/(assets – book equity + market equity). BDR is book debt ratio defined as (short-term debt + long-term debt)/asset. Market equity is price times shares outstanding. The target debt ratio (MDR\* or BDR\*) is defined as the predicted debt ratios from rolling first stage historical panel regressions of observed debt ratios on determinants of the target similar to those reported in Table 2. Security issues (repurchases) are identified as firm-years with net debt or equity issued (repurchased) in excess of five percent of lagged total assets. Net equity issued is equity issued minus equity repurchased, scaled by lagged total assets. Net debt issued is long-term debt issuance minus long-term debt reduction, all scaled by lagged total assets. Post-transaction (T+1) fractions significantly different from pre-transaction (T) fractions at the 5% and the 1% level are marked \* and \*\* respectively.

	BDR <sub>T+1</sub> relative to BDR* <sub>T+1</sub>			BDR <sub>T</sub> relative to BDR* <sub>T+1</sub>		
	% below the CI	% within the CI	% above the CI	% below the CI	% within the CI	% above the CI
No transaction	59.46**	7.88**	32.66**	55.42	9.11	35.48
Equity issue	64.74**	11.84**	23.42**	52.36	14.39	33.26
Debt reduction & Equity issue	79.57**	1.49**	18.93**	34.77	3.74	61.49
Debt reduction	67.23**	2.02**	30.75**	35.89	3.43	60.68
Debt issue	23.18**	2.65**	74.17**	55.53	6.04	38.44
Debt issue & Equity repurchase	21.55**	1.65**	76.8**	59.29	5.70	35.01
Equity repurchase	64.55	9.07*	26.38**	64.31	10.20	25.50
Debt reduction & Equity repurchase	74.23**	2.06	23.71**	49.48	2.84	47.68
Debt issue & Equity issue	36.11**	2.92**	60.97**	53.06	7.24	39.70

Table 8: The distribution of deviations from target leverage

Panel A: Market leverage

	MDR* <sub>T+1</sub> -MDR <sub>T+1</sub>			MDR* <sub>T+1</sub> -MDR <sub>T</sub>		
	25 <sup>th</sup> Percentile	50 <sup>th</sup> Percentile	75 <sup>th</sup> Percentile	25 <sup>th</sup> Percentile	50 <sup>th</sup> Percentile	75 <sup>th</sup> Percentile
No transaction	-0.023	0.021	0.073	-0.023	0.014	0.059
Equity issue	0.000	0.030	0.085	-0.007	0.014	0.056
Debt reduction & Equity issue	0.024	0.090	0.169	-0.063	-0.005	0.043
Debt reduction	-0.030	0.054	0.131	-0.096	-0.024	0.030
Debt issue	-0.126	-0.048	0.017	-0.023	0.019	0.070
Debt issue & Equity repurchase	-0.103	-0.035	0.023	-0.011	0.027	0.080
Equity repurchase	-0.001	0.030	0.075	0.000	0.029	0.074
Debt reduction & Equity repurchase	0.008	0.071	0.145	-0.046	0.006	0.061
Debt issue & Equity issue	-0.080	-0.014	0.057	-0.016	0.018	0.069

Panel B: Book leverage

The table reports the 25<sup>th</sup>, 50<sup>th</sup>, and 75<sup>th</sup> percentiles of deviations of post-transaction and pre-transaction debt ratios around the target. MDR is market debt ratio defined as (short-term debt + long-term debt)/(assets – book equity + market equity). BDR is book debt ratio defined as (short-term debt + long-term debt)/asset. Market equity is price times shares outstanding. The target debt ratio (MDR\* or BDR\*) is defined as the predicted debt ratios from rolling first stage historical panel regressions of observed debt ratios on determinants of the target similar to those reported in Table 2. Security issues (repurchases) are identified as firm-years with net debt or equity issued (repurchased) in excess of five percent of lagged total assets. Net equity issued is equity issued minus equity repurchased, scaled by lagged total assets. Net debt issued is long-term debt issuance minus long-term debt reduction, all scaled by lagged total assets.

	BDR* <sub>T+1</sub> - BDR <sub>T+1</sub>			BDR* <sub>T+1</sub> - BDR <sub>T</sub>		
	25 <sup>th</sup> Percentile	50 <sup>th</sup> Percentile	75 <sup>th</sup> Percentile	25 <sup>th</sup> Percentile	50 <sup>th</sup> Percentile	75 <sup>th</sup> Percentile
No transaction	-0.021	0.018	0.063	-0.024	0.010	0.050
Equity issue	0.000	0.027	0.078	-0.019	0.008	0.046
Debt reduction & Equity issue	0.027	0.112	0.200	-0.108	-0.029	0.024
Debt reduction	-0.028	0.056	0.127	-0.095	-0.025	0.026
Debt issue	-0.131	-0.059	-0.001	-0.030	0.011	0.056
Debt issue & Equity repurchase	-0.135	-0.067	-0.007	-0.025	0.021	0.075
Equity repurchase	-0.006	0.023	0.076	-0.004	0.022	0.074
Debt reduction & Equity repurchase	0.001	0.080	0.154	-0.080	0.003	0.058
Debt issue & Equity issue	-0.127	-0.034	0.034	-0.035	0.008	0.050

Figure 1: Fixed Cost of Adjustment

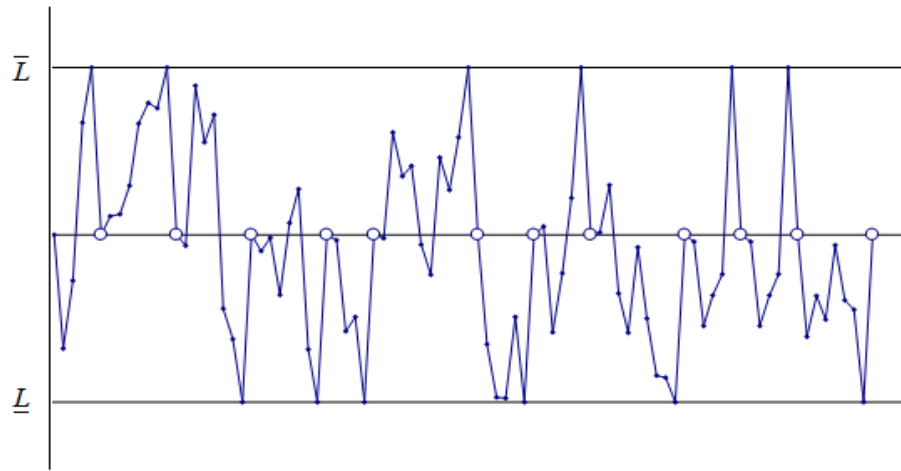


Figure 2: Proportional Cost of Adjustment

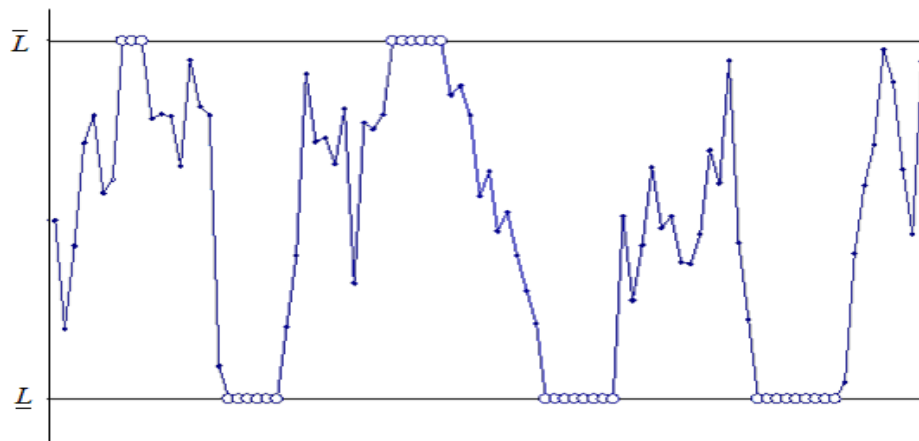
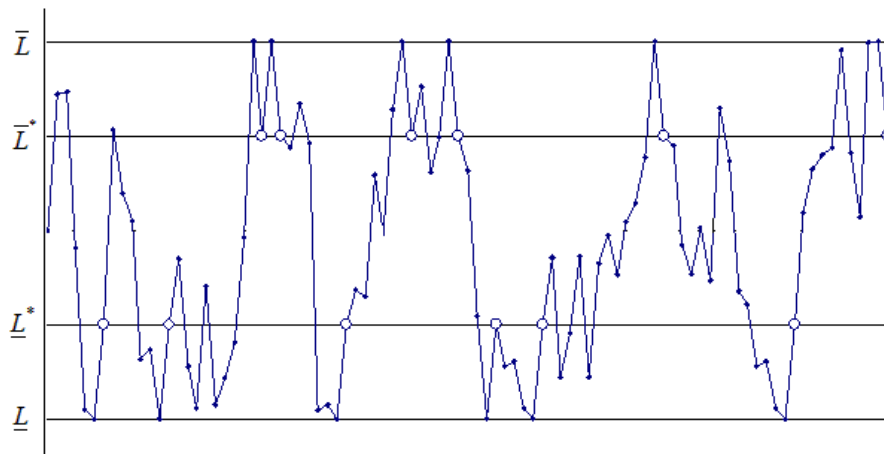


Figure 3: Fixed and Weakly Convex Cost of Adjustment



## CHAPTER 3

# Debt Maturity Structure and Firm Value

### 1. Introduction

Since Modigliani and Miller's (1958) capital structure irrelevance principle, the effect of corporate leverage on total firm market value has long been a central issue of corporate finance research. A large body of theoretical studies has explored various costs and benefits of debt and its implications for firm value. For instance, Jensen (1986) argues that debt could be value-enhancing because it reduces overinvestment problem. In contrast, Myers (1977) shows that debt can be value-reducing if it introduces underinvestment problem. Prior empirical studies also find that firms' capital structure changes convey new information to investors and therefore affect stock prices and firm value (Masulis, 1983; Bhandari, 1988; Fama and French, 1992; Safieddine and Titman, 1999; McConnell and Servaes, 1995; Lang, Ofek and Stulz, 1996).

Recognizing the heterogeneity of corporate debt, another strand of literature emphasizes that corporate debt varies in a number of dimensions, such as convertibility, callability, sources, priority, and maturity.<sup>33</sup> On theoretical level, the heterogeneity of corporate debt leads to substantial variations in the effects of debt on firm value. The empirical evidence however, is also mixed and inconsistent. For example, James (1987) shows that the abnormal returns

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<sup>33</sup> Rauh and Sufi (2008) provide evidence that corporate balance sheet debt does comprise significant amount of different types of debt.

associated with loan announcements are positive and significant, while other types of corporate financing generate either negative or zero excess returns. Preece and Mullineaux (1994) find that the announcement effects differ for bank versus non-bank loans. Billet, Flannery, and Garfinkel (1995) argue that the positive announcement effect varies with the quality of the lender. Billet, Flannery, and Garfinkel (2006), however, find that bank loans are not significantly different from seasoned equity offerings or public debt issues in the long run.

As one of the most important features of debt heterogeneity, the maturity structure, i.e., the time profile of corporate borrowing, is crucial to manager's corporate debt management (Lewellen and Emery, 1985) and financial system's susceptibility to crises (Diamond and Rajan, 2001). Given the importance of the issue, the existing literature has proposed that tax, agency costs of debt, signaling, and liquidity risk considerations may explain the choice of the maturity structure of corporate debt. The empirical studies have documented that firm characteristics, such as growth opportunities, credit quality, risk under conditions of asymmetric information, conflict of interests between manager and shareholder, affect the composition of long term debt and short term debt in a firm's capital structure (Barclay and Smith, 1995; Guedes and Opler, 1996; Stohs and Mauer, 1996; Datta, Datta and Raman, 2005; Fan and Titman, 2008).

The existence of an optimal debt maturity structure rests on the presumption that the mix of long/short term debt has a real effect on firm value. However, the generic relationship between debt maturity structure and firm value has been left unexplored in the literature. This paper undertakes this task and provides empirical evidence on the valuation effect of changes in debt maturity structure on firm value.

The basic empirical model adopted in this paper follows Faulkender and Wang (2006). The model has been pervasively used to examine the valuation effects of changes in cash

holdings and capital investment (see Faulkender and Wang, 2006, Dittmar and Mahrt-Smith, 2007; Masulis, Wang and Xie, 2009 ). The examination of the relationship between debt maturity structure and firm value within Faulkender and Wang (2006)'s model is complicated because debt maturity structure choice is endogenous. For example, Barclay and Smith (1995, 2003) and Guedes and Opler (1996) document the negative relationship between debt maturity structure (measured as the fraction of long term debt over total debt) and a variable measuring growth opportunity (the market to book ratio). Therefore, debt maturity structure might proxy for the growth opportunities and the effect of debt maturity structure on firm value might simply reflect the relationship between growth opportunities and firm value.<sup>34</sup>

The endogeneity of debt maturity choice can also lead to reverse causality. As shown in Flannery (1986), a separating equilibrium exists where firms with favorable private information issue short term debt at a relatively low interest rate and, subsequently, roll it over, and those with unfavorable private information issue long-term debt at a relatively high rate. In other words, shorter debt maturity structure signals management's information about future firm value. The change in debt maturity structure in this case is the outcome of manager's decisions based on private information about future firm's performance.

To address the endogeneity bias and the reverse causality problem simultaneously, I use the previous year's long term debt maturity structure as the instrumental variable to implement IV estimation. The benefits of using pervious year's long term debt maturity structure as instrumental variable are that they are predetermined and exogenous to current and future growth opportunities. As shown in section III, after taking into account the endogenous choice of debt maturity structure, I find that shorter debt maturity structure is generally associated with higher firm value. This finding is robust to different valuation models, different measures of debt

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<sup>34</sup> The similar argument applies to the relationship between leverage and growth (see Lang et al, 1995).

maturity structure and different estimation procedures.

The existing theoretical studies suggest that shorter debt maturity structure may affect firm value *via* several channels. First, shorter debt maturity structure reduces underinvestment problem as proposed by Myers (1977). Second, as summarized in Harvey, Lins and Roper (2004), short term debt could be better-suited for reducing overinvestment problem arising from inherent empire-building preferences of self-interested managers and other conflicts of interest between managers and shareholders. Last but not least, while shortening debt maturity structure mitigates agency problems, it reduces financial flexibility and increases the risk of suboptimal liquidation (Diamond, 1991, 1993; Sharpe, 1991), thus, decreasing firm value.

My sub-sample analyses show that the beneficial effects of shorter debt maturity structure increase in growth opportunities as measured by Tobin's Q. For firms with poor growth opportunities (Tobin's  $Q < 1$ ), the effect of change in debt maturity structure is insignificant. Further analysis shows that this effect is driven by low-growth opportunities, cash-rich, and low leverage ratio firms that are more likely to have an overinvestment problem. These results imply that the positive effects could arise mainly because shortening debt maturity structure alleviates the debt overhang (underinvestment) problem. For growth firms with relatively high leverage ratio, the effects of shorter debt maturity structure become insignificant. These results suggest that the liquidity effect plays an important role in the valuation effect of debt maturity structure. Corporate investment regressions and Fama and French (1998)'s valuation regressions further confirm these findings.

The paper is organized as follows. Section II summarizes the theoretical considerations on valuation effects of debt maturity structure changes. Section III presents the basic regression model and results for the relationship between debt maturity structure and firm value. Section IV

links the valuation effects to firm characteristics and examines the channel(s) of valuation effects. Section V summarizes the conclusions and its implications for corporate debt management.

## **2. Debt Maturity Structure and Firm Value: Literature Review**

The existing theoretical research regarding the valuation effects of debt maturity structure is inconclusive. In an economy without taxes and bankruptcy costs, Stiglitz (1974) shows that debt maturity is irrelevant to firm value. The recent literature, for example, Myers (1977) and Jensen (1986), argues that debt maturity structure is relevant because shorter debt maturity structure reduces agency costs of debt and/or mitigates the agency problem arising from conflicts of interest between shareholders and management. The collapse of L.A. Gear examined in DeAngelo, DeAngelo and Wruck (2002) also illustrates that debt maturity structure does matter. This section briefly summarizes the related literature and the empirical predictions regarding the valuation effects of debt maturity structure.

### **2.1 Agency costs of debt: conflicts between bondholders and shareholders**

Myers's (1977) influential work on corporate borrowing policy emphasizes the agency costs of debt originating from the conflicts between shareholders and bondholders. When investment is financed by debt, debt creates an incentive problem because the profits from the project have to be shared between shareholders and bondholders. In this case, shareholders may let pass some positive NPV projects if they perceive that most of the gains from pursuing new projects will flow to bondholders. This is the so called underinvestment problem under debt overhang, which is more severe when there is more long term debt in capital structure. Holding other things equal, the underinvestment problem is more acute for firms with ample growth

opportunities. To mitigate agency costs of debt, Myers recommends that firms either limit the total amount of debt or shorten the debt maturity structure.

Using a simulation approach, Parrino and Weisbach (1999) show that firms with relatively short term debt suffer much less from stockholder-bondholder conflict. The magnitude of underinvestment-related distortions on investment is substantially lower under shorter debt maturity. In this sense, shortening debt maturity structure can enhance firm value and the positive effect should be more prominent for firms with more growth opportunities.

## **2.2 Agency problem: conflicts between shareholders and management**

Jensen and Meckling (1976) suggest that separation of ownership and control inevitably leads to conflicts of interest between outside shareholders and managers. Debt can create value by reducing self-interested managers' overinvestment of free cash flow (Jensen, 1986; Jensen, 1993; Stulz, 1990). Short term debt in this respect could be more effective because it subjects managers to more frequent monitoring by underwriters, investors, and rating agencies at the time of the issuance (Rajan and Winton; 1995, Leland and Pyle, 1977; Stulz, 2000).<sup>35</sup> A number of studies provide empirical evidence suggesting that shortening debt maturity structure can indeed increase firm value by reducing overinvestment problem. By using emerging market data, Harvey, Lins, and Roper (2004) show that short term debt is more effective in reducing managerial agency costs. DeAngelo, DeAngelo, and Wruck (2002) show that short term debt affects the LA Gear's equity value because it subjects managers to more frequent scrutiny of the capital markets when they have to refinance principal.

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<sup>35</sup> The empirical evidence on the role of short term debt in reducing overinvestment problem is weak. La Porta et al. (2002, 2003) show that, in emerging market, a domestically issued short term debt contract is unlikely to reduce overinvestment problem because family groups or governments usually control the banks and use them for their own purpose.

### **2.3 Liquidity Risk**

While shortening debt maturity structure could increase firm value by reducing underinvestment and/or overinvestment problem, choosing shorter maturity debt structure could be cost. Specifically, shorter debt maturity structure includes higher out of pocket flotation costs, greater opportunity costs of management time in dealing with more frequent debt financing, and refinancing risk (Barclay and Smith, 1995). In addition, Diamond (1991, 1993) and Sharpe (1991) show that too much debt maturing in the short term creates a risk of suboptimal liquidation because lenders ignore the full value of control rents. Higher risk of suboptimal liquidation implies higher expected bankruptcy costs and, therefore, lower firm value. Johnson (2003) confirms empirically that short maturity structure increases liquidity risk.

However, as suggested in Dang, Thomas and Zechner (2006), the net effect of shorter debt maturity structure on liquidity risk could be ambiguous if firm's leverage decision and maturity structure are determined simultaneously. While long term maturities deinceivize equityholders to reduce leverage in response to poor firm performance, a sufficiently short term maturity commits equityholders to reduce leverage and, thereby, reduces liquidity risk.

In summary, whether shortening debt maturity structure has a beneficial effect on firm value depends on whether it can reduce underinvestment and/or overinvestment problem. More importantly, it depends on whether the benefits dominate the expected costs of inefficient liquidation.

## **3. Empirical Models and Results**

### **3.1 Model Specification**

To examine the valuation effects of debt maturity structure, Faulkender and Wang (2006)

valuation model is adopted in this paper. The model has been used to examine the value of cash (see Faulkender and Wang, 2006), the effect of corporate governance on the value of cash (Dittmar and Mahrt-Smith, 2007; Masulis, Wang and Xie, 2009) and the contribution of capital expenditure to shareholder value (Masulis, Wang and Xie, 2009). In the spirit of Fama and French (1998) change in value regressions, Fauldenker and Wang (2006)'s model is essentially a long term event study, that examines the cumulative effect of change in debt maturity structure on excess stock return over one year horizon. The model specification is

$$\begin{aligned}
r_{i,t} - R_{i,t}^B = & \beta_0 + \beta_1 \times \frac{\Delta C_{i,t}}{M_{i,t-1}} + \beta_2 \times \frac{\Delta E_{i,t}}{M_{i,t-1}} + \beta_3 \times \frac{\Delta NA_{i,t}}{M_{i,t-1}} + \beta_4 \times \frac{\Delta RD_{i,t}}{M_{i,t-1}} \\
& + \beta_5 \times \frac{\Delta I_{i,t}}{M_{i,t-1}} + \beta_6 \times \frac{\Delta D_{i,t}}{M_{i,t-1}} + \beta_7 \times \frac{C_{i,t-1}}{M_{i,t-1}} + \beta_8 \times L_{i,t} + \beta_9 \times \frac{NF_{i,t}}{M_{i,t-1}} \\
& + \beta_{10} \times \frac{C_{i,t-1}}{M_{i,t-1}} * \frac{\Delta C_{i,t}}{M_{i,t-1}} + \beta_{11} \times L_{i,t} * \frac{\Delta C_{i,t}}{M_{i,t-1}} + \alpha_1 \times \Delta Structure_{i,t} + \varepsilon_{i,t}
\end{aligned} \tag{1}$$

The dependent variable is the excess return for individual firm over fiscal year  $t$ , defined as the stock return ( $r_{i,t}$ ) for firm  $i$  during fiscal year  $t$  minus stock  $i$ 's benchmark return ( $R_{i,t}^B$ ) for the same time period. As pointed out in Faulkender and Wang (2005), subtracting the benchmark return helps control for the time series variation in risk factors and the cross sectional variation in firms' exposures to these factors.

As in Faulkender and Wang (2006), an array of variables is added into the regression to control for the effects of changes in profitability, cash, investment policy and financing policy on firm value.  $C$  is cash plus marketable securities;  $E$  is earnings before interest;  $NA$  is net asset defined as total assets minus cash plus marketable securities;  $RD$  is R&D expenditures;  $I$  is interest expense;  $D$  is total dividend; and  $NF$  is net financing defined as net equity issuance plus net long term debt issuance.  $M$  is the market value of equity.  $L$  is market leverage ratio

defined as total debt divided by market value of asset.  $\Delta X_t / M_{t-1} = (X_t - X_{t-1}) / M_{t-1}$  is the change in a variable  $X$  from time  $t - 1$  to time  $t$  scaled by the market value of equity at the end of fiscal year  $t - 1$ .

Barclay and Smith (1995) measure debt maturity as the proportion of total debt maturing in more than three years (see also Barclay and Smith, 1997, Datta, Datta and Raman, 2005). They also conduct robustness checks by using the proportion of total debt maturing in more than two years as the measure of debt maturity structure. I use the complement of this measure, the proportion of total debt maturing in two years or less, to measure debt maturity structure. Since my tests mainly focus on the use of short term debt, measuring the maturity structure as the fraction of relatively short term debt makes interpretation of the results easier.  $\Delta Structure_t$  is the change in debt maturity structure from time  $t - 1$  to time  $t$ .

### **3.2 Data**

My initial sample is drawn from Annual Compustat files covering the 1970-2007 period. Since I need information on long term debt due in one to five year maturities, the actual sample used in the regression starts from 1975. Firms with values of total assets or sales less than one million dollars are excluded from the sample. To minimize the influence of outliers and misreported data in our analysis, all variables scaled by  $M_{t-1}$  are trimmed at one percent on both tails of the distribution of the full sample, except that leverage ratio and the percentage of debt with various maturities are trimmed on zero and one.

Following Faulkender and Wang (2006), I exclude financial firms (SIC codes 6000-6999) and utility firms (SIC codes 4900-4999) from the sample. For basic regressions, I limit the sample to observations with non-missing data for all variables and for which the change in the debt maturity structure can be calculated. The resulting sample consists of 75,372 firm-year

observations in basic regressions and 73,924 firm-year observations in IV estimations. When I run separate regressions based on the previous year's firm characteristics, I only use sample with data on previous year's Tobin's Q, leverage ratio, and financial slack. 547 firm-year observations are dropped and 73,377 observations are used in Table 3-6. Table 1 presents the summary statistics for the sample with 73,924 observations. Firm characteristics of the samples used in separate regressions are summarized in the corresponding tables.

### 3.3 Basic Results

Table 2 presents the basic regression results. Column 1 of Table 2 simply replicates Faulkender and Wang (2006) analysis with qualitatively similar results. The value of cash for a firm without leverage and zero cash holdings at time  $t - 1$  is 0.96. For a firm with median leverage and median cash holdings at time  $t - 1$ , the value of cash is 0.75 ( $=0.96-0.94*0.19-0.34*0.08$ ), which is less than full value of the extra dollar of cash. The value of cash decreases in leverage and the level of cash.

As a very rough examination of the relationship between debt maturity structure and firm value, I decompose the variable  $NF_t$  into two components, net equity issuance and net long term debt issuance and then estimate the model by OLS. Column 2 of Table 2 reports the statistically significantly positive coefficient on net equity issuance and negative coefficient on net long term debt issuance. These may be interpreted as evidence that equity (long term debt) issuance generally increases (decreases) firm value. However, as the dependent variable (stock return) is contemporaneous to financing, another plausible explanation is that high stock returns encourage managers to raise additional capital by equity issuance but not debt issuance.

Column 3 of Table 2 reports OLS regression results when change in debt maturity structure is added into Faulkender and Wang's (2006) model. It shows that shorter debt maturity

structure is inversely related to firm value. The problem with OLS estimation in this regression is that change in debt maturity structure could be highly endogenous in model (1).<sup>36</sup> The endogeneity bias makes OLS estimation inconsistent and creates difficulties in interpreting the coefficient on the changes in debt maturity structure. For example, if managers prefer equity financing and avoid debt financing when firm's stock return is high, debt maturity structure tends to decrease because last year's short term debt has to be retired and may be refinanced with equity. This may lead to a negative relation between stock returns and change in debt maturity structure.

To address the endogeneity issue, IV estimation is implemented. Long term debt maturity structure is used to instrument for change in debt maturity structure. More specifically, the fractions of long term debt due in one, two, and three years at the end of fiscal year  $t-1$  are used as instrumental variables for changes in debt maturity structure. This design has certain advantages. First, long term debt maturity structure is predetermined and its information content has been incorporated into the market value of the firm at the beginning of fiscal year  $t$ .<sup>37</sup> Therefore, stock returns in year  $t$  (the changes in market value of firm) should be uncorrelated with these variables. Secondly, the time profile and the amount of maturing long term debt at  $t-1$  are exogenous with respect to the form of financing in year  $t$  (Hovakimian and Vulanvic, 2008). Finally, these variables are highly correlated with the change in debt maturity structure from  $t-1$  to  $t$ . Long term debt due in three years at time  $t-1$  becomes debt maturing in two years at time  $t$ , representing the exogenous component of the increase in my measure of short term debt. Similarly, long term debt due in two year at time  $t-1$  is an important component of

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<sup>36</sup> Numerous studies document that firm characteristics (for example, market to book, firm size, credit quality, etc) affect firm debt maturity (see Barclay and Smith, 1995, 1997, 2003; Stohs and Mauer, 1996; Fan and Titman, 2008).

<sup>37</sup> When I run simple linear regression of long term debt due in one year on the previous year's long term debt due in two years, the slope coefficient is 1.02 and  $R^2$  is 0.69. When I regress long term debt due in two years on last year's long term debt due in three years, the slope coefficient is 1.05 and  $R^2$  is 0.78.

short term debt in fiscal year  $t$ . Long term debt maturing in one year at time  $t-1$  measures the exogenous component of the reduction in short term debt and total debt in fiscal year  $t$ .

The necessity of IV estimation is highlighted by Wu-Hausman test in column 4 of Table 2. First, I run a regression of change in debt maturity structure on the instrumental variables and then add the fitted value from this regression into model (1). The significant coefficient on predicted value  $\Delta \hat{Structure}_t$  rejects the null hypothesis that the change in debt maturity structure is an exogenous variable in model (1). First stage regression results reported in Panel B show that three instrumental variables are highly significant. The correlation coefficients reported in Panel C provide further evidence on the validity of my instrumental variables. The correlation coefficients between the instrumental variables and the dependent variable are less than 0.01 and insignificant at 1% significance level. In contrast, all three instrumental variables are significantly (at 1% significance level) correlated with the change in debt maturity structure with the magnitudes of the coefficients ranging from 0.13 to 0.25.

The IV estimation results presented in column 5 of Table 2 show that, after taking into account the endogeneity of the change in debt maturity structure, shortening debt maturity structure has a positive effect on firm value. Comparing IV estimation with OLS results, I find that all other coefficients are fairly similar and only the coefficient on  $\Delta \hat{Structure}_t$  changes dramatically from -0.04 to 0.17.

In the spirit of Fama and MacBeth (1973), I also run annual IV estimations and then calculate the average coefficients from 33 regressions (1975-2007) and  $t$  statistics for the average coefficients. The results reported in the last column of Table 2 are quantitatively similar. The average coefficient is 0.143 and significant at 1% significance level. Twenty six of the thirty three coefficients are positive and the median coefficient is 0.145. These results confirm that, on

average, a shorter debt maturity structure increases firm value.

#### **4. Extensions and Robustness Checks**

Our results suggest that shortening debt maturity increases firm value. Based on discussions in Section II, shorter debt maturity may increase firm value by mitigating the underinvestment problem and (or) the overinvestment problem. Shorter debt maturity may also decrease firm value by increasing liquidity risk and expected bankruptcy costs. Hence it is worthwhile to examine the cross sectional variation in the valuation effects. We, next, examine how the valuation effects vary with growth opportunities, as measured by Tobin's Q, and liquidity risk, as measured by net debt (debt minus cash) over asset ratio.

##### **4.1 Firm characteristics and the valuation effects of shortening debt maturity structure**

Myers (1977) suggests that shortening debt maturity can alleviate the debt overhang problem. Since the underinvestment problem is more acute for firms with ample growth opportunities, I expect the positive effects to be stronger for firms with high growth opportunities.

**H1: (Underinvestment Hypothesis) The beneficial effects of shortening debt maturity structure increase with growth opportunities**

Corporate governance and capital structure literature ((Rajan and Winton; 1995, Leland and Pyle, 1977; Stulz, 2000) suggests that shorter term debt is more effective in curtailing the overinvestment problem stemming from the conflict between shareholders and managers. For this overinvestment hypothesis to explain the results in Table 2, the positive effects of short maturity structure should be more pronounced for firms suffering from overinvestment problem.

**H2: (Overinvestment Hypothesis) The positive effects of shorter maturity structure**

### **is more pronounced for firms with lower growth opportunities**

Following existing literature, I use Tobin' Q (the ratio of the market value of assets to book value of assets) to measure growth opportunities.

Similar to Lang and Litzenberger (1989) and Danis, Denis and Sarin (1994), I use  $Q=1$  as the cutoff point to identify firms with  $Q<1$  as more likely to overinvest. Value one can also be used to identify high growth firms ( $Q>1$ ) and low growth firms ( $Q<1$ ).<sup>38</sup> Table 3 reports test results for hypotheses H1 and H2. I, first, sort firm-year observations into five quintiles based on one year lagged Tobin's Q and run IV estimations separately for these five sub-samples. The coefficients on the changes in debt maturity structure are increasing from the lowest Q quintile (0.07) to the highest Q quintile (0.30). Furthermore, the effects of changes in debt maturity structure are insignificant for the lowest Q quintile and the second lowest Q quintile. The last two columns of Table 3 present the results for firms with  $Q>1$  and  $Q<1$ . The fact that the positive effects only exist in firms with  $Q>1$  indicates that overinvestment channel is unlikely to be the reason for positive effect of shortening debt maturity structure.

So far, my analysis does not take into account firms' liquidity risk status. Shorter debt maturity structure could have a negative effect on firm value because of liquidity risk. More debt due within short time period inevitably increases liquidity risk, especially for firms with high net debt ratio. My third hypothesis is stated as follows:

**H3: (Liquidity Risk Hypothesis) The beneficial effect of shortening debt maturity structure will decrease with liquidity risk**

To examine how the valuation effects change with liquidity risk, I use net debt ratio defined as debt minus cash over asset ratio to measure liquidity risk. Table 4 reports the results

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<sup>38</sup> Lang, Ofek and Stulz (1996) and Lang, Stulz and Walking (1991) also use  $Q=1$  as the cutoff point to identify high growth firms and low growth firms.

for firms with different net debt ratios which allow us to test hypothesis H3. The results show that the positive effects of short maturity structure decrease in net debt ratio, becoming insignificant for the highest two quintiles.

Although the results reported in Table 4 are generally consistent with the liquidity risk effect, they could be also explained by overinvestment hypothesis. For example, for firms with few growth opportunities, the overinvestment problem could be more (less) prominent for firms with more (less) cash and less (more) debt, indicated by low (high) net debt ratio. Why the positive effect is insignificant for firms with high net debt ratio is because overinvestment problem is not severe for those firms. I next address this possibility below.

#### **4.2 Debt maturity effects: further evidence on channels**

When net debt ratio is used to measure liquidity risk, the implicit assumption is that cash is negative debt. However, both DeAngelo, DeAngelo, and Wruck (2002) and Acharya, Almeida, and Campello (2007) suggest that cash is not negative debt. From corporate governance perspective, it is also inappropriate to assume the perfect substitutability between cash and debt. For my purpose, it is worthwhile to examine how valuation effects of shortening debt maturity structure vary with firms' cash and debt level after controlling for growth opportunities.

As Harford (1999) suggests, cash can serve as a proxy for the agency costs of free cash flows because it is essentially a stockpiled free cash flow. He shows that cash-rich firms are more likely to make acquisitions and their acquisitions are more likely to be value-decreasing. If shorter debt maturity structure increases firm value by reducing overinvestment, the beneficial effects should be pronounced for firms with few investment opportunities ( $Q < 1$ ), high cash holdings, and low leverage.

**H4: (Overinvestment Hypothesis) For firms with low growth opportunities, the beneficial effect of shortening debt maturity structure should be more pronounced for firms with more cash and low leverage ratio.**

Myers (1977) suggests that, to reduce the agency costs of debt overhang, firms can either reduce their leverage ratios or shorten the debt maturity structure. This implies that, for high-growth firms ( $Q > 1$ ), the positive effects should increase with high leverage ratio *if liquidity risk does not matter a great deal*. Hence examining how positive effects change with leverage ratio helps us to assess the empirical importance of liquidity effect.

**H5: (Underinvestment Hypothesis versus Liquidity Risk Hypothesis) For firms with high growth opportunities, the positive effects of debt maturity structure may increase in leverage ratio (underinvestment hypothesis) or may decrease in leverage ratio (liquidity risk hypothesis).**

Based on the median values of the entire sample, I split the sample into firms with above- and below-median levels of cash and market leverage ratio. For high growth firms and low growth firms, I run separate regressions for firms with above- and below- median levels of cash and leverage ratio.

Table 5 reports the sub-sample analysis results. For firms with  $Q < 1$ , although the magnitude of coefficient on change in debt maturity structure is larger for firms with more cash and low leverage ratio, it is not statistically significant at 5% significance level. It is consistent with my previous findings that shortening debt maturity structure does not increase firm value by reducing overinvestment problem. It is also consistent with Hart and Moore's (1995) argument that short term debt obligations do not put meaningful free cash flow disciplinary pressure on management (see also Hart, 1993).

For high growth firms with  $Q > 1$ , I can only observe a significant positive effect of debt maturity structure for firms with relatively low leverage ratio. For high growth firms with relatively high leverage ratios, the effect is insignificant regardless of the levels of cash holdings. The magnitude of the coefficient from the low leverage ratio and high cash holdings group (0.308) is much larger than the coefficient for the low leverage ratio and low cash holdings group (0.173). All these results indicate that the liquidity effect plays an important role in the valuation effect of shorter debt maturity structure.

### 4.3 Debt Maturity Structure and Corporate Investment

To provide additional evidence on my findings, I next examine the effects of debt maturity structure on corporate investment. While many studies have investigated the effect of leverage ratio on investment, few have studied the effect of debt maturity structure on investment. Two recent exceptions are Aivizian, Ge, and Qiu (2005) and Almeida, Campello, Laranjeira, and Weisbenner (2009). The former runs a corporate investment regression to show that longer debt maturity structure reduces investment for growth firms. The latter uses an experiment-like design to show that debt maturity structure matters for corporate investment decision when there are credit supply shocks. My basic model, based on Fazzari, Hubbard, and Petersen (1988), is specified as:

$$I_{i,t} / K_{i,t-1} = \beta_0 + \beta_1 \times (CF_{i,t} / K_{i,t-1}) + \beta_2 \times Q_{i,t-1} + \beta_3 \times Lev_{i,t-1} + \beta_4 \times Str_{i,t-1} + \mu_i + v_t + \varepsilon_{i,t} \quad (2)$$

Following the existing literature, the dependent variable, firm investment  $I_{i,t} / K_{i,t-1}$ , is measured as firm  $i$ 's capital expenditures in fiscal year  $t$  normalized by net fixed assets at the beginning of fiscal year  $t$ .  $CF_{i,t} / K_{i,t-1}$  is defined as firm  $i$ 's cash flow (earnings before extraordinary items plus depreciation) scaled by net fixed assets at the beginning of the fiscal

year  $t$ .  $Q_{i,t-1}$  is firm  $i$ 's market to book ratio at the beginning of fiscal year  $t$ . Leverage ratio ( $Lev_{i,t-1}$ ) is firm  $i$ 's market leverage ratio at the beginning of fiscal year  $t$ .  $Str_{i,t-1}$  is firm  $i$ 's short term debt in current liabilities to total debt ratio at the beginning of fiscal year  $t$ . The sample includes all US manufacturing firms (SIC 2000-3999) from Compustat.

A number of studies document that the coefficient on leverage ratio in corporate investment regression is statistically significantly negative (Lang et al, 1996; Aivazian et al, 2005). One standard interpretation is due to debt overhang. The negative relation can also be explained by the leverage reduction action of managers who expect an increase in investment opportunities. The other interpretation is the role of capital structure in mitigating free cash flow problem. Given that empire-building managers always want to increase capital investment, debt serves as a commitment device that managers will pay out cash as interest and principal. Debt also subjects managers to more external monitoring. Both of these effects suggest that the negative relation between leverage ratio and capital investment could reflect how debt helps overcome overinvestment problem. To summarize, the negative  $\beta_3$  may reflect the value-enhancing role of debt suggested by Jensen (1986) or value-reducing role of debt described in Myers (1977).

While both the underinvestment and the overinvestment hypotheses predict a negative relationship between leverage ratio and investment, they have different predictions about the effect of short term debt/total debt ( $Str$ ). The previous year's short term debt has to be paid this year, which directly reduces the current cash flow and, therefore, can have a negative effect on capital investment. Similarly, if debt maturity structure helps reduce the overinvestment problem, I expect the negative effect on corporate investment, especially for firms with few growth opportunities. However, according to Myers (1977), shorter debt maturity structure may reduce

the underinvestment problem if debt matures before new growth opportunities arrive. Therefore, the positive  $\beta_4$  can be obtained when the underinvestment reduction effect dominates the cash flow effect. Since the underinvestment problem is more acute for firms with high growth opportunities, the value-enhancing role of shorter debt maturity structure should be more pronounced for firms with  $Q>1$ , compared to firms with  $Q<1$ . Contrarily, the overinvestment story discussed in Section II predicts negative  $\beta_4$ , especially for firms with  $Q<1$ .

It is likely that (short term debt)/(total debt) ratio at the beginning of fiscal year  $t$  in investment model (4) is endogenous. Firm with substantial growth opportunities tend to use more short term debt in capital structure (see Barclay and Smith, 1995; Guedes and Opler, 1996; Barclay, Marx and Smith, 2003; Scherr and Hulburt, 2001). To address this issue, I use the exogenous component of short term debt (long term debt due in one year) in the regression, as discussed in section III.

Panel A of Table 6 reports the regression results of Model (2). Column 1 of Panel A in Table 6 reports the fixed effects regression results for the full sample. The statistically significant positive coefficient on short term debt/total debt ratio suggests that shortening debt maturity increases investment. However, as shown in Column 2 and 3 of Panel A, the positive effects on investment can only be observed for growth firms with  $Q>1$ .<sup>39</sup> It provides additional evidence that shorter debt maturity structure mitigates the underinvestment problem but not the overinvestment problem.

Myers (1977) suggests that leverage ratio and debt maturity structure could be substitutes in terms of reducing the underinvestment problem, I further examine this possibility by estimating corporate investment regressions for different sub-samples double sorted by leverage

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<sup>39</sup> The positive and significant coefficients (0.0049 and 0.071) are comparable to the coefficients (-0.035 and -0.089) on long term debt maturity structure for full sample and firms with above-median Tobin's Q in Table III of Avizian et al. (2005).

ratio and levels of cash holdings. These results are reported in Panel B of Table 6.

For firms with  $Q < 1$ , the direct effects of debt maturity structure on investment are insignificant for all four sub-samples. This implies that shortening debt maturity structure *per se* does not help reduce the overinvestment problem. In contrast, for firms with  $Q > 1$ , the effects of debt maturity structure on corporate investment are positive and significant for firms with low leverage ratios. The magnitude of the coefficient increases with the level of cash holdings. While this is consistent with my previous finding that beneficial effects of shortening debt maturity structure of firm value is higher for growth firms with more cash holdings and lower leverage ratio, this also implies that liquidity effects of debt maturity structure are important

#### 4.4 Debt maturity structure and firm value: robustness checks

Another valuation model which has been widely used in the literature is Fama and French (1998). This model has been used by Pinkowitz and Stulz (2006) and Dittmar and Mahrt-Smith (2007) among others to examine the value of cash. The change model is specified as:

$$\begin{aligned}
 d(V_t - A_t) / A_t = & a + a_1 dE_t / A_t + a_2 dE_{t+2} / A_t + a_3 dA_t / A_t + a_4 dA_{t+2} / A_t + a_5 dRD_t / A_t \\
 & + a_6 dRD_{t+2} / A_t + a_7 d(D_t / A_t) + a_8 d(D_{t+2} / A_t) + b_1 d(I_t / A_t) + b_2 d(I_{t+2} / A_t) \\
 & + c_1 dV_{t+2} / A_t + d_2 d(LTD_t / A_t) + d_3 d(LTD_{t+2} / A_t) + e_t
 \end{aligned}
 \tag{4}$$

and

$$\begin{aligned}
 d(V_t - A_t) / A_t = & a + a_1 dE_t / A_t + a_2 dE_{t+2} / A_t + a_3 dA_t / A_t + a_4 dA_{t+2} / A_t + a_5 dRD_t / A_t \\
 & + a_6 dRD_{t+2} / A_t + a_7 dD_t / A_t + a_8 dD_{t+2} / A_t + b_1 dI_t / A_t + b_2 dI_{t+2} / A_t \\
 & + c_1 dV_{t+2} / A_t + d_2 dLTD_t / A_t + d_3 dLTD_{t+2} / A_t + e_t
 \end{aligned}
 \tag{5}$$

Following Fama and French (1998),  $V_t$  is market value of total assets and  $A_t$  is book value of total assets at fiscal year  $t$ . Long term debt ( $LTD_t$ ) is defined as the debt with maturity more

than two years at fiscal year  $t$ . In this model, the leverage ratio is measured as the ratio of interest expense to total assets. The definitions of other variables are the same as section III.A.  $dX_t = (X_t - X_{t-2}) / A_t$  is the change in a variable from time  $t-2$  to time  $t$  scaled by total assets.  $d(X_t) = X_t / A_t - X_{t-2} / A_{t-2}$  is the ratio change in a variable from time  $t-2$  to time  $t$ . Panel A of Table 8 reports the Fama-MacBeth Regressions results of model (4) and (5).

In Panel B of Table 7I replace the ratio of interest expense to total assets in model (4) and (5) with the ratio of total debt to assets. This change allows me to directly control for the effect of total debt. Following Fama and French (1998), I use the value 3 as the rough cutoff point for the significance of the coefficients. If shortening debt maturity structure increases firm value, then the coefficients ( $d_2$ ) on the change in long term debt should be statistically significantly negative.

The full sample regression results for different models and definitions of leverage ratio are presented in Table 7. The results show that controlling for the effects of total debt, more long term debt in capital structure is associated with lower firm value.

I next reestimate regression (4) and (5) for firms with  $Q < 1$  and  $Q > 1$  in Table 7. The negative effects of change in long term debt are only significant for firms with  $Q > 1$ . This indicates that short debt term tends to alleviate the underinvestment rather than the overinvestment problem.

## 5. Conclusions

The choice of debt maturity structure is an important issue in corporate finance research. A number of existing theoretical studies suggest that debt maturity structure is value-relevant. While the determinants of debt maturity structure and the effects of debt maturity structure on

investment have been widely examined in the literature, the relation between debt maturity structure and firm value remains largely unexplored.

Using two recently developed valuation models of Faulkender and Wang (2006) and Fama and French (1998), this paper shows that shortening debt maturity structure increases firm value and that the beneficial effects vary with firm characteristics. The sub-sample analysis further shows that the positive effect only exists for growth firms with relatively low leverage ratios. For firms with  $Q < 1$ , I fail to observe any significant effect of debt maturity structure. These findings suggest that shortening debt maturity structure increases firm value by reducing the underinvestment problem but not the overinvestment problem. For growth firms ( $Q > 1$ ), the positive effects become insignificant when the leverage ratio is relatively high and the effects increase in the level of cash holdings. These results suggest that liquidity effect is an important factor in the valuation effects of shorter debt maturity structure.

Table 1: Summary Statistics

This table provides summary statistics for the sample. The data set includes all COMPUSTAT firms with data for all variables in model (1). Excess stock return is defined as  $r_{i,t} - R_{i,t}^B$ , where  $r_{i,t}$  is the stock return for firm  $i$  during fiscal year  $t$  and  $R_{i,t}^B$  is stock  $i$ 's benchmark return at year  $t$ . Firm characteristics are measured using Compustat data items as follows:  $C$  is cash plus marketable securities;  $E$  is earnings before interest;  $NA$  is net asset defined as total assets minus cash plus marketable securities;  $RD$  is R&D expenditures;  $I$  is interest expense;  $D$  is total dividend; and  $NF$  net financing defined as net equity issuance plus net long term debt issuance. All firm characteristics variables are scaled by the market value of equity at the end of fiscal year  $t - 1$ .  $Lev$  is market leverage ratio defined as total debt divided by market value of asset.  $\Delta X_t / M_{t-1} = (X_t - X_{t-1}) / M_{t-1}$  is the change in a variable from time  $t - 1$  to time  $t$ .  $\Delta St_t$  is defined as the change in the ratio of short term debt to total debt from time  $t - 1$  to time  $t$ . Short term debt is defined as short term debt in current liability plus long term debt due in two years.

	<b>Mean</b>	<b>St. Dev</b>	<b>25<sup>th</sup> Percentile</b>	<b>Median</b>	<b>75<sup>th</sup> Percentile</b>	<b>obs</b>
$r_{i,t} - R_{i,t}^B$	-0.04	0.47	-0.35	-0.10	0.17	73924
$\Delta C_t$	0.01	0.14	-0.03	0.00	0.04	73924
$\Delta E_t$	0.01	0.19	-0.03	0.01	0.04	73924
$\Delta NA_t$	0.11	0.47	-0.03	0.07	0.23	73924
$\Delta RD_t$	0.00	0.01	0.00	0.00	0.00	73924
$\Delta I_t$	0.00	0.03	0.00	0.00	0.01	73924
$\Delta D_t$	0.00	0.01	0.00	0.00	0.00	73924
$C_{t-1}$	0.15	0.21	0.03	0.08	0.19	73924
$Lev_t$	0.22	0.17	0.08	0.19	0.33	73924
$NF_t$	0.03	0.23	-0.04	0.00	0.08	73924
$\Delta Structure_t$	0.00	0.22	-0.06	0.00	0.08	73924

Table 2: Debt maturity structure and equity value

The dependent variable is the excess stock return  $r_{i,t} - R_{i,t}^B$ .  $C_t$ ,  $E_t$ ,  $NA_t$ ,  $RD_t$ ,  $I_t$ ,  $D_t$ ,  $NF_t$ ,  $NEF_t$  and  $NDF_t$  are cash plus marketable securities, earnings before interest, total assets minus cash holdings, R&D expenditures, interest expense, dividend paid, net financing, net equity financing and net long term debt financing for fiscal year  $t$ , respectively. All above variables are scaled by  $M_{t-1}$ , the market value of equity at the end of fiscal year  $t - 1$ .  $\Delta X_t = (X_t - X_{t-1}) / M_{t-1}$  is the change in a variable from time  $t - 1$  to time  $t$  scaled by  $M_{t-1}$ .  $Lev_t$  is market leverage ratio defined as total debt divided by market value of asset.  $\Delta Structure_t$  is defined as the change in the ratio of short term debt to total debt at the end of fiscal year  $t$ . Short term debt is defined as short term debt in current liability plus long term debt due in two years. The model is estimated by using all COMPUSTAT firms with data for all variables in any regressions. White heteroscedastic-consistent t-statistics are in parentheses. “\*\*\*” and “\*\*” denote statistical significance at the 1% and 5%, respectively.

<b>Panel A</b>	<b>OLS</b>	<b>OLS</b>	<b>OLS</b>	<i>Wu – Hausman Test</i>	<b>IV</b>	<b>Fama-MacBeth</b>
$\Delta C_t$	0.96 (31.59)**	0.91 (28.99)**	0.96 (31.54)**	0.95 (31.04)**	0.96 (31.24)**	1.03 (17.90)**
$\Delta E_t$	0.49 (35.90)**	0.49 (35.30)**	0.49 (35.63)**	0.49 (35.22)**	0.51 (35.91)**	0.54 (25.86)**
$\Delta NA_t$	0.22 (34.21)**	0.22 (34.05)**	0.22 (34.41)**	0.22 (33.91)**	0.21 (32.26)**	0.23 (19.06)**
$\Delta RD_t$	1.07 (8.24)**	1.07 (8.18)**	1.07 (8.21)**	1.04 (7.91)**	1.08 (8.16)**	0.99 (5.31)**
$\Delta I_t$	-0.70 (9.15)**	-0.68 (8.68)**	-0.69 (9.06)**	-0.67 (8.74)**	-0.72 (9.30)**	-0.60 (3.04)**
$\Delta D_t$	2.03 (10.87)**	2.11 (11.28)**	2.02 (10.86)**	2.01 (10.73)**	2.03 (10.74)**	1.83 (5.76)**
$C_{t-1}$	0.13	0.13	0.13	0.13	0.12	0.13

	(11.95)**	(11.55)**	(12.00)**	(12.05)**	(11.47)**	(6.92)**
$Lev_t$	-0.61	-0.61	-0.61	-0.62	-0.61	-0.63
	(58.87)**	(58.80)**	(58.89)**	(59.06)**	(57.91)**	(20.63)**
$NF_t$	-0.07		-0.07	-0.07	-0.03	-0.08
	(6.02)**		(6.65)**	(6.65)**	(2.59)**	(3.83)**
$NEF_t$		0.20				
		(7.41)**				
$NDF_t$		-0.10				
		(8.13)**				
$\Delta C_t * Lev_t$	-0.94	-0.84	-0.94	-0.93	-0.93	-1.00
	(12.05)**	(10.57)**	(12.05)**	(11.90)**	(11.84)**	(10.38)**
$\Delta C_t * C_{t-1}$	-0.34	-0.32	-0.33	-0.33	-0.35	-0.37
	(8.72)**	(8.20)**	(8.64)**	(8.55)**	(8.97)**	(8.65)**
$\Delta Structure_t$			-0.04	-0.08	0.17	0.14
			(5.35)**	(8.93)**	(7.63)**	(6.05)**
$\Delta \hat{Structure}_t$				0.25		
				(10.14)**		
$Intercept$	0.03	0.03	0.03	0.03	0.03	0.03
	(10.33)**	(8.27)**	(10.40)**	(10.85)**	(9.99)**	(3.29)**
$Instrument\ Variable$				$[\frac{DD1_{t-1}}{Debt_{t-1}}, \frac{DD2_{t-1}}{Debt_{t-1}}, \frac{DD3_{t-1}}{Debt_{t-1}}]$		
$Observations$	75372	74224	75372	73924	73924	73924
$R^2$	0.19	0.19	0.19	0.19	0.18	0.21

Table 2 (cont'd): Debt maturity structure and equity value

<b>Panel B</b>	<b>First Stage Regression</b>
$\Delta C_t$	-0.048 (3.82)**
$\Delta E_t$	-0.074 (14.53)**
$\Delta NA_t$	0.032 (13.04)**
$\Delta RD_t$	-0.146 (2.42)*
$\Delta I_t$	0.187 (6.83)**
$\Delta D_t$	-0.118 -1.51
$C_{t-1}$	0.030 (8.90)**
$Lev_t$	-0.045 (10.24)**
$NF_t$	-0.173 (32.67)**
$\Delta C_t * Lev_t$	-0.000 (0.00)
$\Delta C_t * C_{t-1}$	0.073

	(5.73)**
$DD1_{t-1} / Debt_{t-1}$	-0.135
	(20.63)**
$DD2_{t-1} / Debt_{t-1}$	-0.386
	(43.05)**
$DD3_{t-1} / Debt_{t-1}$	0.621
	(64.45)**
<i>Intercept</i>	0.017
	(10.47)**
<i>Observations</i>	73924
$R^2$	0.16
$F \text{ test}(DD1_{t-1} / Debt_{t-1} = DD2_{t-1} / Debt_{t-1} = DD3_{t-1} / Debt_{t-1} = 0)$	1856.97(P=0.000)

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Table 2 (cont'd): Debt maturity structure and equity value

<b>Panel C: The correlation coefficient matrix</b>					
	$r_{i,t} - R_{i,t}^B$	$\Delta St_t$	$DD1_{t-1} / Debt_{t-1}$	$DD2_{t-1} / Debt_{t-1}$	$DD3_{t-1} / Debt_{t-1}$
$r_{i,t} - R_{i,t}^B$	1.000				
$\Delta Structure_t$	-0.036	1.000			
$DD1_{t-1} / Debt_{t-1}$	-0.009	-0.132	1.000		
$DD2_{t-1} / Debt_{t-1}$	0.002	-0.174	0.220	1.000	
$DD3_{t-1} / Debt_{t-1}$	0.008	0.239	0.062	0.279	1.000

Table 3: Debt maturity structure, investment opportunity and equity value

The dependent variable is the excess stock return  $r_{i,t} - R_{i,t}^B$ .  $C_t$ ,  $E_t$ ,  $NA_t$ ,  $RD_t$ ,  $I_t$ ,  $D_t$ ,  $NF_t$ ,  $NEF_t$  and  $NDF_t$  are cash plus marketable securities, earnings before interest, total assets minus cash holdings, R&D expenditures, interest expense, dividend paid, net financing, net equity financing and net long term debt financing for fiscal year  $t$ , respectively. All above variables are scaled by  $M_{t-1}$ , the market value of equity at the end of fiscal year  $t - 1$ .  $\Delta X_t = (X_t - X_{t-1}) / M_{t-1}$  is the change in a variable from time  $t - 1$  to time  $t$  scaled by  $M_{t-1}$ .  $Lev_t$  is market leverage ratio defined as total debt divided by market value of asset.  $\Delta Structure_t$  is defined as the change in the ratio of short term debt to total debt at the end of fiscal year  $t$ . Short term debt is defined as short term debt in current liability plus long term debt due in two years. The table presents IV regression results. The instrumental variables in the first stage regression are the ratio of long term debt due in one year to total debt, the ratio of long term debt due in two year to total debt and the ratio of long term debt due in three year to total debt at time  $t - 1$ . White heteroscedastic-consistent t-statistics are in parentheses. “\*\*\*” and “\*\*” denote statistical significance at the 1% and 5%, respectively.

<b>Panel A</b>	<b>Q(Q<sub>t-1</sub>)=1</b>	<b>Q(Q<sub>t-1</sub>)=2</b>	<b>Q(Q<sub>t-1</sub>)=3</b>	<b>Q(Q<sub>t-1</sub>)=4</b>	<b>Q(Q<sub>t-1</sub>)=5</b>	<b>Q<sub>t-1</sub>&lt;1</b>	<b>Q<sub>t-1</sub>&gt;1</b>
$\Delta C_t$	0.49 (10.24)**	0.71 (11.58)**	1.00 (13.55)**	1.18 (11.71)**	1.90 (16.70)**	0.56 (13.76)**	1.22 (26.91)**
$\Delta E_t$	0.44 (20.08)**	0.47 (20.18)**	0.56 (17.69)**	0.73 (13.37)**	0.85 (9.24)**	0.44 (25.15)**	0.62 (27.65)**
$\Delta NA_t$	0.15 (15.58)**	0.18 (16.74)**	0.28 (18.54)**	0.33 (16.03)**	0.70 (13.03)**	0.16 (19.69)**	0.29 (27.11)**
$\Delta RD_t$	1.01 (3.75)**	0.57 (1.99)*	0.95 (3.16)**	1.13 (3.63)**	1.48 (4.77)**	0.76 (3.47)**	1.23 (7.48)**
$\Delta I_t$	-0.65 (5.87)**	-0.65 (4.78)**	-0.59 (3.07)**	0.63 (1.85)	0.83 (1.28)	-0.58 (6.17)**	-0.75 (5.45)**
$\Delta D_t$	2.63	2.09	1.37	1.07	2.76	2.53	1.75

	(9.32)**	(5.78)**	(2.73)**	(1.86)	(3.53)**	(10.41)**	(5.91)**
$C_{t-1}$	0.06	0.09	0.06	-0.07	-0.03	0.07	0.12
	(3.93)**	(4.92)**	(2.12)*	(1.83)	(0.55)	(5.52)**	(6.52)**
$Lev_t$	-0.57	-0.64	-0.83	-1.08	-1.45	-0.58	-0.73
	(23.76)**	(27.85)**	(31.63)**	(35.29)**	(35.23)**	(30.79)**	(50.19)**
$NF_t$	-0.05	-0.05	-0.05	0.01	-0.14	-0.05	-0.04
	(2.56)*	(2.45)*	(1.82)	(0.33)	(2.10)*	(3.25)**	(2.06)*
$\Delta C_t * Lev_t$	-0.32	-0.63	-0.78	-1.85	-2.80	-0.38	-1.38
	(2.61)**	(4.59)**	(4.42)**	(7.27)**	(7.43)**	(3.78)**	(12.08)**
$\Delta C_t * C_{t-1}$	-0.17	-0.15	-0.49	-0.29	-1.50	-0.18	-0.43
	(3.14)**	(2.67)**	(4.14)**	(1.09)	(2.78)**	(4.19)**	(5.44)**
$\Delta Structure_t$	0.07	0.09	0.18	0.17	0.30	0.06	0.22
	(1.19)	(1.71)	(3.94)**	(3.94)**	(6.27)**	(1.31)	(8.61)**
<i>Intercept</i>	0.08	0.09	0.10	0.09	-0.01	0.09	0.03
	(8.73)**	(10.58)**	(11.99)**	(11.10)**	-0.72	(11.23)**	(6.74)**
<i>Observations</i>	14676	14675	14676	14675	14675	22545	50832
$R^2$	0.2	0.21	0.23	0.23	0.21	0.21	0.19

Table 3 (cont'd): Debt maturity structure, investment opportunity and equity value

Panel B: Summary Statistics

Firm characteristics at fiscal year $t$	$Q(Q_{t-1})=1$	$Q(Q_{t-1})=2$	$Q(Q_{t-1})=3$	$Q(Q_{t-1})=4$	$Q(Q_{t-1})=5$
Debt less than three years	0.48	0.44	0.44	0.44	0.52
Debt less than two years	0.39	0.35	0.35	0.36	0.44
Assets	670.59	1771.23	1803.65	1673.17	1872.17
ROA	0.11	0.13	0.14	0.16	0.16
Market Leverage	0.30	0.30	0.24	0.17	0.09
Tobin's Q	0.85	1.06	1.27	1.59	2.65
R&D/asset	0.01	0.01	0.02	0.03	0.06
CapEx/asset	0.06	0.07	0.08	0.09	0.10
Financial Slack	0.08	0.07	0.08	0.11	0.18
One-year abnormal return	-0.04	-0.05	-0.04	-0.04	-0.05

Table 4: Short term debt, liquidity risk and equity value

The dependent variable is the excess stock return  $r_{i,t} - R_{i,t}^B$ .  $C_t$ ,  $E_t$ ,  $NA_t$ ,  $RD_t$ ,  $I_t$ ,  $D_t$ ,  $NF_t$ ,  $NEF_t$  and  $NDF_t$  are cash plus marketable securities, earnings before interest, total assets minus cash holdings, R&D expenditures, interest expense, dividend paid, net financing, net equity financing and net long term debt financing for fiscal year  $t$ , respectively. All above variables are scaled by  $M_{t-1}$ , the market value of equity at the end of fiscal year  $t - 1$ .  $\Delta X_t = (X_t - X_{t-1}) / M_{t-1}$  is the change in a variable from time  $t - 1$  to time  $t$  scaled by  $M_{t-1}$ .  $Lev_t$  is market leverage ratio defined as total debt divided by market value of asset.  $\Delta Structure_t$  is defined as the change in the ratio of short term debt to total debt at the end of fiscal year  $t$ . Short term debt is defined as short term debt in current liability plus long term debt due in two years. Net debt is defined as debt minus cash over asset. The table presents IV regression results. The instrumental variables in the first stage regression are the ratio of long term debt due in one year to total debt, the ratio of long term debt due in two year to total debt and the ratio of long term debt due in three year to total debt at time  $t - 1$ . White heteroscedastic-consistent t-statistics are in parentheses. “\*\*\*” and “\*\*” denote statistical significance at the 1% and 5%, respectively.

Independent Variables	Q(Net Debt <sub>t-1</sub> )=1	Q(Net Debt <sub>t-1</sub> )=2	Q(Net Debt <sub>t-1</sub> )=3	Q(Net Debt <sub>t-1</sub> )=4	Q(Net Debt <sub>t-1</sub> )=5
$\Delta C_t$	1.05 (15.88)**	0.74 (11.15)**	0.79 (10.69)**	0.72 (8.05)**	0.65 (5.77)**
$\Delta E_t$	0.59 (12.84)**	0.42 (10.95)**	0.40 (12.99)**	0.37 (13.14)**	0.35 (17.82)**
$\Delta NA_t$	0.40 (12.78)**	0.29 (15.29)**	0.24 (14.41)**	0.22 (17.76)**	0.16 (17.15)**
$\Delta RD_t$	1.06 (4.32)**	1.59 (5.49)**	0.76 (2.64)**	0.59 (1.93)	1.18 (3.69)**
$\Delta I_t$	0.06 (0.10)	0.11 (0.38)	-0.07 (0.39)	-0.29 (1.91)	-0.35 (3.48)**

$\Delta D_t$	2.38 (4.33)**	2.14 (4.69)**	2.25 (5.44)**	0.86 (2.58)**	1.17 (3.08)**
$C_{t-1}$	0.21 (8.58)**	0.55 (13.63)**	0.63 (16.74)**	0.56 (13.14)**	0.43 (14.44)**
$Lev_t$	-1.54 (25.33)**	-1.95 (31.60)**	-1.96 (42.58)**	-1.71 (42.87)**	-1.26 (42.26)**
$NF_t$	0.20 (3.67)**	0.15 (4.03)**	0.14 (4.61)**	0.02 (0.75)	-0.04 (2.26)*
$\Delta C_t * Lev_t$	-1.05 (3.90)**	-0.93 (2.82)**	-1.29 (4.65)**	-0.85 (3.31)**	-0.31 (1.41)
$\Delta C_t * C_{t-1}$	-0.50 (5.83)**	-0.26 (1.57)	-0.04 (0.34)	0.03 (0.28)	-0.23 (2.78)**
$\Delta Structure_t$	0.28 (5.16)**	0.11 (2.36)*	0.09 (2.06)*	0.01 (0.29)	0.08 (1.68)
<i>Intercept</i>	-0.05 (6.86)**	0.08 (11.74)**	0.24 (28.84)**	0.34 (32.35)**	0.38 (30.15)**
<i>Observations</i>	14674	14674	14674	14674	14673
$R^2$	0.18	0.24	0.30	0.32	0.27

Table 4 (cont'd): Short term debt, liquidity risk and equity value

Panel B: Summary Statistics

Firm characteristics at fiscal year $t$	<b>Q(Net Debt<sub>t-1</sub>)=1</b>	<b>Q(Net Debt<sub>t-1</sub>)=2</b>	<b>Q(Net Debt<sub>t-1</sub>)=3</b>	<b>Q(Net Debt<sub>t-1</sub>)=4</b>	<b>Q(Net Debt<sub>t-1</sub>)=5</b>
Debt less than three years	0.50	0.40	0.34	0.32	0.33
Debt less than two years	0.38	0.30	0.24	0.22	0.22
Asset	744.27	1439.23	1866.00	1902.36	1839.57
ROA	0.12	0.16	0.15	0.14	0.12
Market Leverage	0.09	0.16	0.24	0.32	0.48
Tobin's Q	1.98	1.51	1.37	1.27	1.27
R&D/asset	0.06	0.03	0.02	0.01	0.01
CapEx/asset	0.08	0.08	0.08	0.08	0.08
Financial Slack	0.26	0.10	0.07	0.05	0.04
One-year abnormal return	-0.03	-0.03	-0.03	-0.05	-0.08

Table 5: Debt maturity structure, cash holdings and equity value: sub-sample analyses

The dependent variable is the excess stock return  $r_{i,t} - R_{i,t}^B$ .  $C_t$ ,  $E_t$ ,  $NA_t$ ,  $RD_t$ ,  $I_t$ ,  $D_t$ ,  $NF_t$ ,  $NEF_t$  and  $NDF_t$  are cash plus marketable securities, earnings before interest, total assets minus cash holdings, R&D expenditures, interest expense, dividend paid, net financing, net equity financing and net long term debt financing for fiscal year  $t$ , respectively. All above variables are scaled by  $M_{t-1}$ , the market value of equity at the end of fiscal year  $t - 1$ .  $\Delta X_t = (X_t - X_{t-1}) / M_{t-1}$  is the change in a variable from time  $t - 1$  to time  $t$  scaled by  $M_{t-1}$ .  $Lev_t$  is market leverage ratio defined as total debt divided by market value of asset.  $\Delta Structure_t$  is defined as the change in the ratio of short term debt to total debt at the end of fiscal year  $t$ . Short term debt is defined as short term debt in current liability plus long term debt due in two years. The table presents IV regression results. The instrumental variables in the first stage regression are the ratio of long term debt due in one year to total debt, the ratio of long term debt due in two year to total debt and the ratio of long term debt due in three year to total debt at time  $t - 1$ . White heteroscedastic-consistent t-statistics are in parentheses. “\*\*\*” and “\*\*” denote statistical significance at the 1% and 5%, respectively.

Panel A	Q <sub>t-1</sub> <1			Q <sub>t-1</sub> >1				
	C <sub>t-1</sub> <Median(C <sub>t-1</sub> ) & lev <sub>t-1</sub> <Median(lev <sub>t-1</sub> )	C <sub>t-1</sub> <Median(C <sub>t-1</sub> ) & lev <sub>t-1</sub> >Median(lev <sub>t-1</sub> )	C <sub>t-1</sub> >Median(C <sub>t-1</sub> ) & lev <sub>t-1</sub> <Median(lev <sub>t-1</sub> )	C <sub>t-1</sub> >Median(C <sub>t-1</sub> ) & lev <sub>t-1</sub> >Median(lev <sub>t-1</sub> )	C <sub>t-1</sub> <Median(C <sub>t-1</sub> ) & lev <sub>t-1</sub> <Median(lev <sub>t-1</sub> )	C <sub>t-1</sub> <Median(C <sub>t-1</sub> ) & lev <sub>t-1</sub> >Median(lev <sub>t-1</sub> )	C <sub>t-1</sub> >Median(C <sub>t-1</sub> ) & lev <sub>t-1</sub> <Median(lev <sub>t-1</sub> )	C <sub>t-1</sub> >Median(C <sub>t-1</sub> ) & lev <sub>t-1</sub> >Median(lev <sub>t-1</sub> )
$\Delta C_t$	0.708 (3.96)**	0.316 (3.74)**	0.719 (8.18)**	0.592 (8.17)**	0.769 (4.63)**	0.617 (5.24)**	1.441 (18.31)**	0.780 (8.95)**
$\Delta E_t$	0.362 (5.86)**	0.392 (17.64)**	0.518 (9.24)**	0.394 (12.31)**	0.697 (9.58)**	0.425 (14.40)**	0.729 (12.75)**	0.434 (11.53)**
$\Delta NA_t$	0.269 (9.09)**	0.151 (14.61)**	0.278 (8.29)**	0.142 (9.42)**	0.391 (10.34)**	0.218 (16.66)**	0.513 (16.34)**	0.213 (10.93)**
$\Delta RD_t$	1.536	0.421	0.477	1.028	1.891	0.746	1.253	0.709

	(1.94)	(1.25)	(1.09)	(2.53)*	(4.34)**	(2.25)*	(5.40)**	(1.81)
$\Delta I_t$	-0.438	-0.553	-0.084	0.013	0.152	-0.399	0.278	0.124
	(0.88)	(4.89)**	(0.11)	(0.07)	(0.27)	(2.26)*	(0.61)	(0.50)
$\Delta D_t$	2.516	2.085	3.005	2.503	2.208	-0.583	2.448	1.303
	(2.58)*	(6.72)**	(3.94)**	(5.31)**	(2.99)**	(1.16)	(4.75)**	(1.98)*
$C_{t-1}$	0.392	0.400	0.029	0.166	0.149	1.097	0.108	0.252
	(2.25)*	(8.22)**	(1.03)	(7.88)**	(0.60)	(7.73)**	(3.97)**	(6.81)**
$Lev_t$	-1.404	-1.012	-1.469	-1.093	-1.991	-1.374	-1.512	-1.444
	(9.22)**	(29.75)**	(13.97)**	(22.18)**	(33.60)**	(39.26)**	(36.65)**	(29.22)**
$NF_t$	0.101	-0.034	0.068	-0.002	0.232	-0.002	0.004	0.011
	(1.12)	(1.72)	(0.82)	(0.05)	(3.93)**	(0.09)	(0.07)	(0.32)
$\Delta C_t * Lev_t$	-0.094	0.158	-0.406	-0.727	-0.515	-0.259	-2.062	-0.737
	(0.09)	(0.76)	(1.07)	(4.19)**	(0.66)	(0.92)	(6.47)**	(3.50)**
$\Delta C_t * C_{t-1}$	-2.205	-0.616	-0.453	-0.037	-3.669	-0.187	-0.704	-0.201
	(3.06)**	(3.94)**	(4.65)**	(0.74)	(1.38)	(0.36)	(5.59)**	(1.61)
$\Delta Structure_t$	0.052	-0.016	0.074	0.122	0.173	0.077	0.308	0.039
	(0.36)	(0.28)	(0.65)	(1.16)	(4.17)**	(1.70)	(7.43)**	(0.51)
<i>Intercept</i>	0.034	0.258	0.066	0.239	0.142	0.297	0.002	0.304
	(1.28)	(18.70)**	(3.78)**	(13.33)**	(13.21)**	(25.06)**	(0.30)	(18.54)**
<i>Observations</i>	1792	11340	3526	5887	10693	12863	20677	6597
$R^2$	0.23	0.25	0.23	0.25	0.25	0.29	0.2	0.31

Table 6: Debt maturity structure and capital investment

The dependent variable is firm investment, measured as capital expenditure scaled by net fixed assets at the beginning of the year.  $MB_{t-1}$  is the market to book ratio at the beginning of the year.  $Lev_{t-1}$  is market leverage ratio at the beginning of the year.  $\Delta EXO_t$  is the exogenous change in short term debt scaled by total debt at the beginning of the year. The exogenous change is measured as long term debt due in three years minus long term debt due in one year at the beginning of the year.  $DD1_{t-1}$  is long term debt due in one year at the beginning of the year. Panel A and Panel C report fixed effects regression results of Model (3). White heteroscedastic-consistent t-statistics are in parentheses. Panel B reports GMM estimation results. “\*\*\*” and “\*\*” denote statistical significance at the 1% and 5%, respectively.

Panel A	Full Sample	$Q_{t-1} < 1$	$Q_{t-1} > 1$
$CF_t / K_{t-1}$	0.020 (9.67)**	0.033 (6.10)**	0.017 (7.91)**
$Q_{t-1}$	0.063 (24.21)**	0.112 (7.17)**	0.056 (20.50)**
$Lev_{t-1}$	-0.401 (34.25)**	-0.276 (22.32)**	-0.447 (28.12)**
$DD1_{t-1} / Debt_{t-1}$	0.049 (5.23)**	0.006 (0.49)	0.071 (6.13)**
<i>Intercept</i>	0.349 (43.46)**	0.227 (16.46)**	0.390 (26.96)**
Fixed firm effects	Yes	Yes	Yes
Fixed year effects	Yes	Yes	Yes
<i>Observations</i>	5425	18067	41358
$R^2$	0.13	0.10	0.12

Table 6 (cont'd): Debt maturity structure and capital investment

Panel C	Q <sub>t-1</sub> <1				Q <sub>t-1</sub> >1			
	<Median(C <sub>t-1</sub> ) &	<Median(C <sub>t-1</sub> ) &	>Median(C <sub>t-1</sub> ) &	>Median(C <sub>t-1</sub> ) &	<Median(C <sub>t-1</sub> ) &	<Median(C <sub>t-1</sub> ) &	>Median(C <sub>t-1</sub> ) &	>Median(C <sub>t-1</sub> ) &
	<Median(lev <sub>t-1</sub> )	>Median(lev <sub>t-1</sub> )	<Median(lev <sub>t-1</sub> )	>Median(lev <sub>t-1</sub> )	<Median(lev <sub>t-1</sub> )	>Median(lev <sub>t-1</sub> )	<Median(lev <sub>t-1</sub> )	>Median(lev <sub>t-1</sub> )
$CF_t / K_{t-1}$	0.056 (3.36)**	0.042 (6.19)**	0.026 (2.53)*	0.031 (3.02)**	0.034 (6.28)**	0.026 (4.22)**	0.013 (4.69)**	0.006 (1.07)
$Q_{t-1}$	0.146 (3.20)**	0.067 (3.43)**	0.187 (5.33)**	0.126 (4.16)**	0.046 (8.28)**	0.036 (4.35)**	0.052 (15.62)**	0.042 (3.82)**
$Lev_{t-1}$	-0.043 (0.32)	-0.260 (15.47)**	-0.444 (4.85)**	-0.201 (6.41)**	-0.400 (4.56)**	-0.304 (13.21)**	-1.024 (13.42)**	-0.283 (8.46)**
$DD1_{t-1} / Debt_{t-1}$	0.075 (1.71)	-0.020 (1.36)	0.034 (1.47)	-0.031 (1.06)	0.059 (2.11)*	-0.018 (0.96)	0.076 (4.56)**	-0.028 (0.99)
<i>Intercept</i>	0.117 (2.92)**	0.243 (13.44)**	0.176 (5.24)**	0.201 (6.88)**	0.318 (10.53)**	0.346 (13.78)**	0.449 (14.49)**	0.386 (11.38)**
Fixed firm effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed year effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
<i>Observations</i>	1401	9458	2953	4255	7993	10857	17365	5142
$R^2$	0.11	0.13	0.05	0.10	0.08	0.10	0.11	0.08

Table 7 Regression Results for the change: Fama and French (1998) model

Regression results for the models

$$d(V_t - A_t) / A_t = a + a_1 dE_t / A_t + a_2 dE_{t+2} / A_t + a_3 dA_t / A_t + a_4 dA_{t+2} / A_t + a_5 dRD_t / A_t + a_6 dRD_{t+2} / A_t + a_7 d(D_t / A_t) + a_8 d(D_{t+2} / A_t) \\ + b_1 d(I_t / A_t) + b_2 d(I_{t+2} / A_t) + c_1 dV_{t+2} / A_t + d_2 dLTD_t / A_t + d_3 dLTD_{t+2} / A_t + e_t$$

and

$$d(V_t - A_t) / A_t = a + a_1 dE_t / A_t + a_2 dE_{t+2} / A_t + a_3 dA_t / A_t + a_4 dA_{t+2} / A_t + a_5 dRD_t / A_t + a_6 dRD_{t+2} / A_t + a_7 dD_t / A_t + a_8 dD_{t+2} / A_t \\ + b_1 dI_t / A_t + b_2 dI_{t+2} / A_t + c_1 dV_{t+2} / A_t + d_2 d(LTD_t / A_t) + d_3 d(LTD_{t+2} / A_t) + e_t$$

$D_t$ ,  $I_t$ ,  $RD_t$  and  $E_t$  are dividend paid, interest expense, R&D expenditures, earnings before interest for fiscal year t, respectively.  $V_t$  and  $A_t$  are the market value of the firm and total book assets at the end of fiscal year t.  $dX_t = (X_t - X_{t-2}) / A_t$  is the change in a variable from time t-2 to time t.  $d(X_t) = X_t / A_t - X_{t-2} / A_{t-2}$  is the ratio change in a variable from time t-2 to time t. The model is estimated by using all COMPUSTAT firms with data for all variables in any regressions. The table presents Fama-MacBeth regression results. t-statistics for the average coefficients are reported in parentheses. The sample period is from 1971-2007. The time period for t is 1973-2005, 33 years. “\*\*\*” denote t-statistics larger than 3.

Panel A: Average Regression Coefficients and t-statistics for the Average

	int	$\frac{dE_t}{A_t}$	$\frac{dE_{t+2}}{A_t}$	$\frac{dA_t}{A_t}$	$\frac{dA_{t+2}}{A_t}$	$\frac{dRD_t}{A_t}$	$\frac{dRD_{t+2}}{A_t}$	$\frac{dI_t}{A_t}$	$\frac{dI_{t+2}}{A_t}$	$\frac{dD_t}{A_t}$	$\frac{dD_{t+2}}{A_t}$	$\frac{dV_{t+2}}{A_t}$	$dLTD_t / A_t$	$dLTD_{t+2} / A_t$
Full	-0.05 (2.09)*	1.15 (15.77)*	0.35 (7.29)*	0.25 (5.74)**	0.33 (9.65)**	0.49 (1.26)	1.99 (6.37)**	-2.54 (6.70)*	-1.17 (3.99)**	3.05 (5.81)*	4.11 (7.18)**	-0.11 (5.24)*	-0.26 (6.40)**	-0.14 (5.04)**
Q <sub>t-1</sub> <1	-0.02 (0.98)	0.58 (9.18)**	0.21 (4.22)*	-0.11 (6.56)**	0.21 (6.50)**	0.19 (0.71)	1.17 (3.92)**	-0.39 (1.45)	-0.19 (0.54)	2.26 (4.55)*	1.54 (4.19)**	-0.11 (4.91)*	-0.02 (0.95)	-0.08 (2.79)
Q <sub>t-1</sub> >1	-0.12	1.50	0.39	0.42	0.39	0.35	1.86	-3.07	-1.35	3.70	4.60	-0.11	-0.35	-0.18

	(3.17)* *	(15.52)* *	(5.61)* *	(10.78)* *	(11.27)* *	(0.87)	(5.05)**	(6.03)* *	(3.95)**	(6.05)* *	(6.89)**	(5.38)* *	(6.80)**	(5.28)**
int	$\frac{dE_t}{A_t}$	$\frac{dE_{t+2}}{A_t}$	$\frac{dA_t}{A_t}$	$\frac{dA_{t+2}}{A_t}$	$\frac{dRD_t}{A_t}$	$\frac{dRD_{t+2}}{A_t}$	$d\left(\frac{I_t}{A_t}\right)$	$d\left(\frac{I_{t+2}}{A_t}\right)$	$d\left(\frac{D_t}{A_t}\right)$	$d\left(\frac{D_{t+2}}{A_t}\right)$	$\frac{dV_{t+2}}{A_t}$	$d(LTD_t / A_t)$	$d(LTD_{t+2} / A_t)$	
Full	-0.04 (1.50)	1.24 (20.73)* *	0.41 (8.04)* *	0.16 (4.56)**	0.27 (9.75)**	0.74 (1.94)	2.11 (6.37)**	-1.56 (4.99)* *	-0.07 (0.20)	2.98 (8.40)* *	3.15 (6.89)**	-0.10 (4.81)* *	-0.26 (5.49)**	-0.15 (5.28)**
$Q_{t-1} < 1$	-0.01 (0.73)	0.61 (10.43)* *	0.23 (4.87)* *	-0.12 (9.72)**	0.19 (6.57)**	0.57 (1.62)	1.25 (4.07)**	-0.14 (0.44)	0.72 (2.94)**	2.35 (6.48)* *	1.78 (5.25)**	-0.10 (4.72)* *	-0.01 (0.46)	-0.07 (2.85)
$Q_{t-1} > 1$	-0.11 (2.47)*	1.62 (19.94)* *	0.45 (6.46)* *	0.30 (9.01)**	0.32 (11.12)* *	0.62 (1.56)	2.02 (5.33)**	-1.93 (5.08)* *	-0.53 (1.20)	3.63 (8.79)* *	3.33 (5.67)**	-0.11 (4.90)* *	-0.37 (5.62)**	-0.21 (4.41)**

Table 7 (cont'd): Regression Results for the change: Fama and French (1998) model

Panel B: Average Regression Coefficients and t-statistics for the Average

	int	$\frac{dE_t}{A_t}$	$\frac{dE_{t+2}}{A_t}$	$\frac{dA_t}{A_t}$	$\frac{dA_{t+2}}{A_t}$	$\frac{dRD_t}{A_t}$	$\frac{dRD_{t+2}}{A_t}$	$\frac{dDebt_t}{A_t}$	$\frac{dDebt_{t+2}}{A_t}$	$\frac{dD_t}{A_t}$	$\frac{dD_{t+2}}{A_t}$	$\frac{dV_{t+2}}{A_t}$	$dLTD_t / A_t$	$dLTD_{t+2} / A_t$
Fu11	-0.05 (2.12)*	1.09 (14.12)**	0.32 (5.62)**	0.27 (5.24)**	0.35 (9.77)**	0.46 (1.21)	1.94 (6.32)**	-0.38 (5.77)**	-0.19 (4.82)**	3.09 (5.87)**	3.91 (7.01)**	-0.11 (5.32)**	-0.11 (3.85)**	-0.04 (1.88)
Q <sub>t-1</sub> <1	-0.01 (0.88)	0.58 (8.95)**	0.19 (3.29)**	-0.14 (6.47)**	0.22 (6.36)**	0.22 (0.82)	1.11 (3.89)**	0.05 (1.43)	-0.09 (2.28)*	2.38 (4.69)**	1.55 (4.04)**	-0.11 (4.95)**	-0.06 (2.10)	-0.02 (0.71)
Q <sub>t-1</sub> >1	-0.12 (3.34)**	1.43 (14.01)**	0.34 (4.42)**	0.44 (8.61)**	0.42 (11.65)**	0.33 (0.84)	1.79 (5.06)**	-0.44 (5.07)**	-0.22 (5.39)**	3.79 (6.05)**	4.38 (6.74)**	-0.11 (5.43)**	-0.16 (3.14)**	-0.06 (1.96)
	int	$\frac{dE_t}{A_t}$	$\frac{dE_{t+2}}{A_t}$	$\frac{dA_t}{A_t}$	$\frac{dA_{t+2}}{A_t}$	$\frac{dRD_t}{A_t}$	$\frac{dRD_{t+2}}{A_t}$	$d(\frac{Debt_t}{A_t})$	$d(\frac{Debt_{t+2}}{A_t})$	$d(\frac{D_t}{A_t})$	$d(\frac{D_{t+2}}{A_t})$	$\frac{dV_{t+2}}{A_t}$	$d(LTD_t / A_t)$	$d(LTD_{t+2} / A_t)$
Fu11	-0.04 (1.58)	1.19 (20.88)**	0.42 (7.26)**	0.18 (5.21)**	0.26 (9.68)**	0.69 (1.81)	2.09 (6.34)**	-0.29 (5.72)**	0.00 (0.07)	2.94 (8.69)**	3.14 (6.96)**	-0.10 (4.77)**	-0.13 (3.57)**	-0.13 (4.71)**
Q <sub>t-1</sub> <1	-0.01 (0.68)	0.60 (9.89)**	0.23 (4.00)**	-0.12 (8.94)**	0.18 (6.36)**	0.52 (1.58)	1.21 (4.14)**	0.08 (1.96)	0.03 (0.60)	2.53 (6.87)**	1.81 (5.14)**	-0.10 (4.77)**	-0.06 (1.95)	-0.05 (1.94)
Q <sub>t-1</sub> >1	-0.11 (2.63)*	1.57 (19.58)**	0.45 (6.01)**	0.33 (10.35)**	0.31 (11.41)**	0.55 (1.37)	2.02 (5.48)**	-0.34 (5.04)**	-0.04 (0.82)	3.62 (9.24)**	3.32 (5.71)**	-0.10 (4.85)**	-0.22 (3.16)**	-0.19 (3.74)**

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