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Abstract

We show that corporate financial policies are highly interdependent; firms make financing decisions in large part by responding to the financing decisions of their peers, as opposed to changes in firm-specific characteristics. We identify these peer effects with a novel instrumental variables approach that uses the lagged idiosyncratic equity shocks to peer firms as a source of exogenous variation. On average, a one standard deviation change in peer firms' leverage ratios is associated with a 9% change in own firm leverage ratios — a marginal effect that is significantly larger than that of any other observable determinant and one that is driven by interdependencies among security (i.e., debt and equity) issuance decisions. Further, we find that the presence of these peer effects is consistent with information-based theoretical models of learning and reputational considerations. Finally, we show that these peer effects create an exeternality among financial policies that amplifies the effects of changes in firm-specific capital structure determinants and alters their interpretation.

Competitor, or peer, firms play a central role in shaping a variety of corporate policies ranging from executive compensation to product market strategy. However, most research on corporate financial policy assumes that firms choose their capital structures independently of their peers. That is, theoretical and empirical work typically assume that a firm's capital structure is determined by some function of its marginal tax rate, expected deadweight loss in default, information environment, or incentive conflicts among claimants. Thus, the role for competitor firms' behavior in affecting corporate capital structures is often ignored, or at most an implicit one through its unmeasured impact on these firm-specific determinants.[1]

Despite this lack of attention, there is evidence suggesting that peer effects are relevant for corporate financial policy. Empirically, median or average industry leverage is the single most important observable capital structure determinant in terms of explained variation and magnitude of marginal effect. Additionally, survey evidence indicates that CFOs often consider the financing decisions of other firms in their industry when setting financial policy. Theoretically, peer effects in corporate behavior are rationalized by models of herding, signalling, and product market competition.[2] Understanding the extent to which and why peer firms play a role in shaping corporate financial policy is important for two reasons. First, it moves us closer to answering a fundamental question in corporate finance; namely, how do firms choose their capital structures? And, second, it has important implications for empirical, as well as theoretical, research because the presence of peer effects implies the existence of externalities in corporate financial policy.

While well motivated and empirically important, peer firm financial policy and its link to corporate capital structure does not have a unique interpretation because of the reflection problem (Manski (1993)). The reflection problem refers to a specific endogene-

[1]Theoretical examples include traditional tax-bankruptcy cost tradeoff theories, (Scott (1976)), agency-based theories (Jensen and Meckling (1976)), information asymmetry (Myers and Majluf (1984)), optimal contracting (DeMarzo and Fishman (2007)). Some exceptions include Brander and Lewis (1986), Maksimovic (1988), and Maksimovic and Zechner (1990). Empirically, there are no studies of which we are aware that explicitly model the interplay between financing decisions of peer firms, though several studies have examined reduced form relations linking leverage to median industry leverage (e.g., Frank and Goyal (2007)).

[2]Studies empirical by Bradley, Jarrell, and Kim (1984), Frank and Goyal (2007), Lemmon, Roberts, and Zender (2008), all of which show that industry effects have the most economically important impact on leverage among observable leverage determinants. Graham and Harvey (2001) show that almost one quarter of surveyed CFOs identify the behavior of competitors as an important input into their financial decision making. Models of herding behavior include Bikhchandani, Hirshleifer, and Welch (1998), Scharfstein and Stein (1990), and Zwiebel (1995); signalling include Ross (1977); and product market competition include Bolton and Scharfstein (1990) and Brander and Lewis (1986).

ity problem that arises when trying to infer whether the behavior of a group influences the behavior of the individuals that comprise the group. In the current context, this problem is created by using a measure of peer firm financial policy, such as industry average leverage, as an explanatory variable for individual firm financial policy. In particular, any observed similarity in financing behavior among the firms within an industry — or any other peer group — can be attributed to two potential explanations.

The first explanation is that firms in the same industry face similar institutional environments or have similar firm characteristics, such as production technologies and investment opportunities. The inability to perfectly measure or observe these determinants generates a role for peer firm financial policy in so far as it proxies for these factors. In essence, the correlation between firms' leverage ratios and that of their peers reflects an omitted variables or measurement error bias.

The second explanation is that firms' financial policies are at least partly driven by a response to their peers. This response can be driven either by peer firm financial policy or by changes to peer firm characteristics. That is, peer firms can influence corporate financial policy via two distinct channels. The first, or direct, channel refers to firms' responding to the actions of their peers — in this case financial policy. The second, or indirect, channel refers to firms' responding to changes in the characteristics of their peers — profitability, risk, investment opportunities, etc.

The goal of this paper is to disentangle these explanations to better understand the role played by firms' peers in determining financial policy, and to identify the implications of these peer effects for empirical and theoretical research on corporate capital structure. To do so, we employ an identification strategy that uses the lagged idiosyncratic component of peer firms' stock returns as an instrument for their financing decisions. Intuitively, the key assumption behind this strategy is that an idiosyncratic shock to the stock price of firm A in period t has no affect on the financing decision of firm B in period $t+1$ but for its affect on firm A's financing decision in period $t+1$. To bolster confidence in this assumption, we estimate firm-specific, rolling regressions of stock returns on the usual asset-pricing factors and an industry factor. This specification produces an estimated residual (i.e., instrument) with a number of desirable properties.

First, the conditional correlation between firms' idiosyncratic return and that of their peers is visrtually zero, ensuring that there are no common factors driving our estimated peer effect. Second, the shocks are conditionally serially uncorrelated and serially cross-uncorrelated implying that firms' shocks do not forecast future shocks for themselves or for other firms. Third, the shocks are uncorrelated with virtually every firm characteristic typically used to explain variation in capital structure. While these features do not guarantee validity of our instrument, they are reassuring and help guide our robustness tests aimed at addressing identification threats from alternative hypotheses.

Our first stage results show that idiosyncratic stock returns are strongly negatively correlated with leverage levels and changes, primarily through their affect on security issuance decisions. Statistically speaking, the first stage F-statistics are well above weak-instrument thresholds, ensuring that the instrument relevance test is easily passed. Economically speaking, this finding shows that managers respond to the firm-specific information contained in market equity prices when making financing decisions.

The second stage results show that firms' capital structure choices are strongly positively influenced by the financing choices of their peers. For example, firms change their market leverage ratios by nine percentage points, on average, in response to a one standard deviation change in leverage by peer firms. This marginal effect is the largest among observable determinants, including profitability, tangibility, firm size, and market-to-book, as well as a host of other explanatory variables. This finding is also extremely robust, found in both book and market measures of leverage, and in both levels and changes in leverage. Further, this finding is unaffected by a number of specification changes and robustness tests examining alternative explanations. Closer inspection reveals that the commonality in leverage choices among peers is driven by a commonality in financing decisions; firms are significantly more likely to issue debt or equity when their peers issue that same security.

Somewhat less important are the effects of peer firms' characteristics on financial policy. Rather, the primary channel through which peers affect financial policy is the direct channel of financing decisions, as opposed to the indirect channel of changing characteristics. Indeed, two way sorts on peer firms' average equity shocks — our instrument — and peer firms' average leverage changes — our endogenous variable — reveal that firms only alter their leverage in response to peer firms' equity shocks when these shocks are accompanied by a change in peer firm leverage.

In addition to identifying an economically important source of variation in corporate financial policies, our results highlight the presence of externalities in those policies. To illustrate, consider a change in firm A's profitability. This change not only affects firm A's financing choice, but also every other member of firm A's peer group via the direct and indirect peer effect channels. This impact on peer firms' financial policies feeds back onto firm A's financial policy, and so on, and so on.

The implications of this feedback are twofold. First, the total derivative or marginal effect of any capital structure determinant can no longer be gleaned solely from that determinant's coefficient, even in linear models. Rather, this marginal effect is a function of an amplification term due to the direct peer effects, a spillover term due to the indirect peer effects, and the size of the peer group. We show that the amplification term varies from a low of 4% in large peer groups to a high of 22% in small peer groups. We also show that the indirect peer effects often provide a countervailing force that offsets the amplification due to the direct peer effect. Thus, the second implication of this feedback effect is to alter the interpretation of existing capital structure determinants. Factors such as size, market-to-book, profitability, etc. influence capital structure not only through their direct impact on the firm but also via their indirect impact on the firm through the firm's peers.

To understand precisely why these commonalities exist, we test the implications of several information-based models by examining which firms do the mimicing and which firms are mimiced. Consistent with models of reputational concerns (Scharfstein and Stein (1990) and Zwiebel (1995)) and learning (Banerjee (1992), Conlisk (1980), and Bikhchandani, Hirshleifer, and Welch (1998), we find that smaller, more constrained, growth firms exhibit more pronounced mimicing tendencies. We also find that lower paid CEOs that have been with the company for a shoter time exhibit more mimicig behavior; however, these results are statistically weak due largely to data limitations. While helping to shed light on the underlying mechanism behind peer effects, this analysis also reinforces our identification strategy as most alternative hypotheses leave little room for systematic heterogeneity in the peer effect.

Our study is most closely related to those documenting the importance of industry as a capital structure determinant. For example, Bradley et al. (1984) show that 54% of the cross-sectional variance in firm leverage ratios is explained by industrial classification. More recently, Frank and Goyal (2007) find that industry median leverage has the single most explanatory power for firm leverage among the 25 firm characteristics and macroeconomic variables they consider. However, these studies have left the interpretation of these industry effects largely unresolved. Indeed, Frank and Goyal (2007, 2008) explicitly note that capital structure similarities within an industry have several possible meanings. Ours is the first study to sift through these alternative meanings, identify policy interdependence as a substantial element of the industry leverage effect, and estimate the externalities induced by the presence of peer effects.[3]

[3]More broadly, our study is related to a long line of works examining peer effects in various settings including mutual fund voting (Matvos and Ostrovsky (2009)), student performance (Kremer and Levy (2003)), investment decisions (Duflo and Saez (2002)), and entrepreneurship (Lerner and Malmendier

Our study is also related to the work of Mackay and Phillips (2005), which identifies significant intra-industry variation in capital structures. Our study compliments theirs by showing that intra-industry leverage heterogeneity is accompanied by strong interdependencies in financial policy. In other words, within-industry leverage distributions may be spread out, reflecting significant dispersion; but, these distributions tend to shift over time as peer firms respond to one another, as opposed to just stretching or contracting when each firm acts in isolation.

Finally, our study is more broadly realated to a number of studies examining the link between individual outcomes and group outcomes in corporate finance (e.g., executive compensation and corporate governance). An important by-product of our study is to highlight the salient empirical issues that appear in observational studies, as opposed to randomized experiments (e.g., Duflo and Saez (2003), Lerner and Malmendier (2009)). Ordinary least squares regressions will typically not provide meaningful results because of the reflection problem, and, as such, a clear identification strategy is needed to rule out the null of ommitted or mismeasured shared characteristics. Additionally, the presence of feedback effects complicates the computation of marginal effects, which are no longer clear from the estimated coefficients.

The paper proceeds as follows. Section I introduces the data and presents summary statistics. Section II examines the explanatory power of peer firm leverage for corporate capital structures. Section III develops the empirical model, and highlights the identification challenge. Section IV discusses our identification strategy, focusing on the construction of our instrument, its economic and statistical properties, and potential identification threats. Section V presents our estimates of the peer effects and the corresponding feedback effects. Section VI examines cross-sectional heterogeneity in the effects to better understand underlying economic mechanism behind the effects. Section VII concludes.

I. Data and Summary Statistics

Corporate accounting data come from Standard & Poor's (S&P) Annual Compustat database. We draw a sample of firm-year observations during the period 1965 to 2006. We choose 1965 as the start year to mitigate the selection bias toward large, successful firms that exists in the early part of the Compustat sample. To maintain consistency with previous empirical studies and to avoid capital structures dictated by regulatory considerations, we exclude financial firms (SIC codes between 6000 and 6999) and utilities

(2009)).

(SIC codes between 4900 and 4999), as well as government entities (SIC codes greater than or equal to 9000). Similar reasons motivate us to exclude any firms that undertook a significant acquisition during the sample period as indicated by Compustat variable *aftnt1* equal to "AB"; however, all of our results are insensitive to this screen which affects less than 3% of the sample frame's observations. We also exclude any observations with missing data for the primary variables used throughout the study (see Appendix A) so that our primary analysis is conducted on a consistent sample.

Stock return data for our sample of Compustat firms are obtained from the Center for Research in Security Prices (CRSP) monthly stock price database. We merge CRSP and Compustat data using the historical header file from CRSP. Our final sample consists of firm-year observations in the intersection of our Compustat sample and CRSP. We use several other data sources for robustness tests, but postpone a discussion of these ancillary data sources until their analysis.

Table I presents summary statistics for our sample. The aforementioned screens produce 76,501 firm-year observations corresponding to 9,293 unique firms. There are 178 industries, defined by three-digit SIC code, represented in our sample. The typical industry contains approximately 18 firms, though the distribution is right skewed as indicated by the median number of firms, 12. To address potential measurement concerns regarding the definition of an industry, as well as the documented intra-industry heterogeneity (Mackay and Phillips (2005)), we investigate more refined peer groups in some of our empirical analysis below. Though, recent research by Hoberg and Phillips (2009) shows that more refined industry definitions based on data from SEC filings provides little improvement over SIC codes in the ability of industry fixed effects to explain variation in corporate investment and financing.

Summary statistics for a number of variables, in levels and first differences, used throughout this study are presented after Winsorizing all ratios at the upper and lower one percentiles. We Winsorize to mitigate the influence of extreme observations and eliminate any data coding errors. Winsorizing at the 2.5 or five percentiles have no qualitative affect on any of our results.

We group the variables into four distinct categories, to which we refer throughout the study. Outcome variables refer to the dependent variables used in our anlaysis and consist primarily of leverage measures, though we also examine measures of financing decisions such as debt and equity issuances. Direct peer effects refer to peer firm averages of the outcome variables and are constructed as the average of all firms within a three-digit SIC code industry-year combination, excluding the ith observation. Indirect peer effects are also peer firm averages, though of firm characteristics as opposed to outcome variables. Firm specific factors refer to firm i's value and correspond to the traditional empirical capital structure determinants (e.g., Rajan and Zingales (1995), Frank and Goyal (2007,2008)). All variables are formally defined in Appendix A. At this point, we simply note the similarity of several of the summary statistics (e.g., outcome variables and firm specific factors) to those found in previous studies, such as Frank and Goyal (2007).

II. The Correlation Between Firm and Industry Leverage

We begin by examining the empirical link between industry leverage and corporate capital structures using the existing empirical literature as our guide. The goal is threefold. First, we want to highlight the economic significance of this determinant. Second, we want to provide results against which we can benchmark subsequent findings. Finally, we want to ensure that this result is not spurious.

Table II presents scaled OLS estimates, t-statistics, and model statistics for several variations of the following model of leverage,

$$y_{ijt} = \alpha + \beta \bar{y}_{-ijt} + \lambda' X_{ijt-1} + \phi' \nu_t + \delta' \mu_j + \psi' \omega_i + \varepsilon_{ijt}. \tag{1}$$

We scale each coefficient estimate by the corresponding variable's standard deviation to normalize units and ease the comparison across determinants. The indices i, j, and t correspond to firm, industry, and year, respectively. The outcome variable, y_{ijt}, is financial leverage. For robustness, we examine both book and market leverage.

The first independent variable, $\bar{y}_{-ijt}$, denotes the peer firm leverage. We focus on the average throughout this study, though substituting the median produces similar findings. This variable corresponds to the direct peer effect, though the extent to which its coefficient (β) captures variation in leverage due to peer firm behavior is unclear at this stage. This point is worth emphasizing. OLS estimation of equation (1) only indicates the direction and magnitude of association between leverage and average industry leverage, as well as the other explanatory variables. As such, we postpone our economic inferences until we appropriately address the identification concerns below.

Previous studies typically lag average industry leverage, and other explanatory variables, in an attempt to account for delayed responses and to mitigate the endogeneity concerns which are the focus of this study. Empirically, the choice between contemporaneous or lagged values is largely irrelevant — the estimated coefficients are similar in both signs and magnitudes. For the purposes of our study which will focus on identification of the effect, a contemporaneous measure is more appealing because it limits the amount of time for firms to respond to one another. While this makes it harder to identiy mimicing behavior, it mitigates the confounding that can occur over long periods of time.

The second term, X_{ijt-1}, is a K-dimensional vector of firm-specific determinants of financial policy, lagged one period. (Again, lagging one period has a negligible effect on the parameter estimates; nonetheless, we present estimates with both lagged and contemporaneous determinants below.) In Table II, we focus on the most common and robust determinants of capital structure (see, for example, Rajan and Zingales (1995) and Frank and Goyal (2003, 2007)). We incorporate year (ν_t), and industry (μ_j) or firm (ω_i) fixed effects to capture common components of leverage ratios. We assume that the error term, ε_{ijt}, is potentially correlated within firms and heteroscedastic. As such, all standard errors and test-statistics are robust to these two departures from the classical regression model (Petersen (2009)).

The results in Panel A show that, in a pooled regression, peer firm leverage is the most economically important observable determinant of capital structure, in terms of either marginal effect or explained variation. A comparison of the adjusted R-squares for specifications (1) and (2) reveals that peer firm average leverage, by itself, explains more variation in book leverage ratios than the other observable determinants combined. Comparing the scaled coefficients reveals a similar finding: the partial derivative of peer firm leverage is larger than any other leverage determinant. For example, column (3) reveals that a one standard deviation change in average peer firm book leverage is associated with a 5.5% change in individual firms' book leverage ratios. This effect is over 50% larger, in magnitude, than the next most important determinant, profitability, whose standard deviation scaled coefficient is -3.6%.

Specifications (4) and (5) incorporate industry and firm fixed effects to address unobserved heterogeneity concerns. Estimates of the latter specification are obtained by a within estimator, though first difference estimates produce similar findings. While no longer the most important characteristic, changes in average peer firm leverage still have an economically and statistically large impact on within-industry and within-firm variation in leverage. Interestingly, the change in the economic magnitude of the peer firm leverage arising from the inclusion of industry and firm fixed effects highlights that peer firm leverage is more important for explaining cross-sectional, as opposed to time-series, variation in leverage ratios. This finding is important because cross-sectional, as opposed to time-series, variation in leverages ratios is arguably the larger mystery in the capital structure puzzle (e.g., Myers (1984), Welch (2004), Lemmon, Roberts, and Zender (2008), and Strebulaev and Yang (2008)).

Specifications (6) through (10) are identical to (1) through (5), only replacing book leverage with market leverage. The results are strikingly similar, particularly when one accounts for the greater volatility of market leverage relative to book leverage (see Table I). Thus, the larger magnitudes of the estimated coefficients do not necessarily imply greater economic significance. Rather, they reflect greater volatility in market leverage relative to book leverage (see Table I).

In unreported analysis, we examine several additional specifications for robustness. A dynamic specification that includes lagged leverage reveals that peer firm average leverage is statistically significant and the most economically significant determinant after the lagged dependent variable. Likewise, the importance of peer firm average leverage is undiminished by the inclusion of additional determinants, such as the marginal tax rate, stock returns, earnings volatility, Altman's Z-Score, capital expenditures, research and development expenditures, and sales and general administrative expenses.

Panel B examines the impact on the estimated scaled coefficients of varying the peer group definition. In addition to quantifying the sensitivity of the estimates, these tests highlight the importance of defining peer groups in an economically meaningful manner. We re-estimate the book and market leverage specifications presented in columns (3) and (8) of Panel A using four different definitions for industry. Because the results are similar in their implications, we present and discuss only the market leverage findings.

The first definition randomly assigns firms to industries which are similar in size to industries defined by 3-digit SIC codes, approximately 18 firms per industry-year, on average. To avoid our results being driven by "one odd draw," we repeat the process of random assignment and model estimation 100 times to reduce the impact of simulation error. We then average the estimated coefficients and construct a corresponding standard error from the standard deviation of the 100 estimated coefficients. The R^2 is the average across the 100 estimations. The results reveal that there is no link — statistically or economically — between leverage and peer firm leverage, whose coefficient and t-statistic are both zero.

The second, third, and fourth definitions define industry using one-digit, two-digit, and three-digit SIC codes, respectively. The results show that the scaled coefficient of average industry leverage, as well as its precision, increases monotonically moving from the one-digit to the three-digit peer group definitions, with a particular sharp increase from two-digit to three-digit classifications. Importantly, these results are not an artifact of the scaling: the parameter estimates show a similar monotonic increase in economic and staistical signifcance. This increase in parameter estimates moving from one-digit to three-digit SIC codes coincides with the decrease in the average number of firms in each classification (239 for one-digit, 45 for two-digit, 18 for three-digit) and increase in similarities among firms. In concert with the randomly assigned industries, the results in Panel B show that the relation between leverage and industry average leverage, while still subject to multiple interpretations, is not spurious. The results in Panel A show that this relation is economically large. We now turn to understanding what this relation means.

III. Empirical Model

Our empirical framework is motivated by Manski (1993) and begins with a linear model of financial policy. We start with a linear specification to emphasize the intuition and highlight the salient econometric issues. We discuss and investigate a variety of extensions to the model further below.

Using the notation introduced in section II, we model measures of financial policy, such as leverage, by the following equation,

$$y_{ijt} = \alpha + \beta \bar{y}_{-ijt} + \lambda' X_{ijt-1} + \gamma' \bar{X}_{-ijt-1} + \delta' \mu_j + \phi' \nu_t + \varepsilon_{ijt}. \tag{2}$$

Equation (2) is similar to existing models found in the capital structure literature, such as equation (1), but for the addition of the K-dimensional vector, $\bar{X}_{-ijt-1}$. This vector contains average peer firm characteristics, each of which is constructed in a manner similar to that of the peer group leverage measure. Economically, this vector corresponds to the indirect channel of peer effects where the characteristics, as opposed to the actions, of peer firms affect the actions of firm i. Each term in this vector corresponds to a firm-specific determinant in X_{ijt} implying that the average investment opportunities, profitability, tangibility, etc. of peer firms play a role in shaping firm i's financial policy.

The parameter vector is $(\alpha, \beta, \lambda', \gamma', \delta', \phi')$. We refer to these parameters as structural parameters only to distinguish them from the composite, or reduced form, parameters that appear in the context of instrumental variables. Like the vast majority of the empirical capital structure literature, we leave unspecified the precise optimization problem undertaken by the firm.[4] Externalities are captured by β, which measures the direct peer effect occurring through peers' financial policies, and γ', which measures the indirect peer effect occurring through the characteristics of peers. The coefficients λ, δ, and ϕ measure the effect of firm specific and common factors on financial policy. In doing so, the variables corresponding to these parameters mitigate, but do not eliminate, the

[4] See Hennessy and Whited (2005, 2007) for examples of a fully specified economic model and structural estimation.

possibility that firms in the same industry have similar financial policies because they share common (possibly unobserved) characteristics or operate in the same institutional environment.

The model is easily extended along a number of dimensions. Each firm may be influenced by multiple peer groups. Direct and indirect peer effects may be transmitted via distributional features other than the mean, such as the median. The linear functional form can be relaxed to accommodate nonlinear or nonparametric specifications. These extensions, as well as others, are considered below.

A. The Identification Problem

The empirical goal is to disentangle the various explanations for capital structure heterogeneity by statistically identifying the structural parameters, $(\alpha, \beta, \lambda', \gamma', \delta')$. The primary difficulty arises from the presence of $\bar{y}_{-ijt}$ as a regressor in equation (2). Intuitively, if firms' financing decisions are influenced by one another, then firm i's capital structure is a function of firm j's and vice versa. That is, the explanatory variable encompassing firm j's capital structure, $\bar{y}_{-ijt}$, is simultaneously determined with the dependent variable representing firm i's capital structure, y_{ijt}. Thus, peer firm average leverage $\bar{y}_{-ijt}$ is an endogenous regressor.

This identification problem can be seen by focusing on the population version of equation (2) and invoking the equilibrium condition $E(y_{ijt}|\mu_j) = E(y_{-ijt}|\mu_j)$. Ignoring the time dimension for notational convenience, we can derive the following reduced form model using the results in Manski (1993):

$$E(y|X, \mu_j) = \alpha^* + \gamma^{*'} E(X|\mu_j) + \delta^{*'} \mu_j + \lambda^{*'} X. \tag{3}$$

where the superscript "*" refers to reduced form or composite parameters that are functions of the underlying structural parameters. Specifically,

$$\begin{aligned}
\alpha^* &= \frac{\alpha}{1-\beta} \\
\gamma^{*'} &= \left(\frac{\beta\lambda + \gamma}{1-\beta}\right)' \\
\delta^{*'} &= \left(\frac{\delta}{1-\beta}\right)' \\
\lambda^{*'} &= \lambda'
\end{aligned}$$

(See Appendix B for a formal derivation.) Immediately apparent is that the structural parameters cannot be recovered from the estimable composite parameters since we are left with five unknowns and only four equations. What is needed to recover the structural parameters is an exogenous source of variation in peer firm financial policy.

However, as long as the intercept, the average peer characteristics, the group fixed effects, and the firm-specific factors are linearly independent, we can identify the reduced-form parameters $(\alpha^*, \gamma^{*'}, \delta^{*'}, \lambda^{*'})$. This result is useful because estimation of the reduced form model (equation (3)) can identify the presence of a peer effect without the use of an instrument. Specifically, the coefficients on the peer firm characteristics, $\gamma^{*'}$ will be zero only if both β and $\gamma^{*'}$ are zero. Thus, as a test for the presence of peer effects we estimate several variations of equation (3) via OLS.

The results are presented in Table III. The layout and specifications mimic those found in Table II, but for the replacement of the endogenous direct peer effect $\bar{y}_{-ijt}$ with exogenous lagged average peer firm characteristics, $\bar{X}_{-ijt-1}$. Two findings are particularly relevant. First, the R-squares in Columns (1) and (6) show that average industry characteristics capture 6.4% and 16% of the variation in book and market leverage ratios, respectively. These estimates are just over half of the variation captured by the industry average leverage ratios (see the corresponding columns in Table II). The difference in variation is due to some combination of firm-specific effects and direct peer effects.

Second, in every specification at least two, and often more, average peer firm characteristics are statistically significant. Related, tests of the null hypothesis that these coefficients are jointly zero are all rejected at better than the one percent level (F-stat towards the bottom of the table). The scaled coefficients of the peer firm characteristics tend to be smaller than those of firm-specific effects, as is their net contribution to explained variation. Both of these results are expected. Peer firm characteristics, in isolation, are imperfect proxies for the industry average leverage, and the coefficients are nonlinear combinations of the underlying structural parameters.

Ultimately, these results indicate the presence of peer effects. What they cannot tell us is the channel through which peer effects operate, direct versus indirect, or the magnitude of the peer effects and associated externalities. For these features, we turn to an instrumental variables approach.

IV. The Identification Strategy

A valid instrument satisfies both the relevance and exclusion conditions. In our setting, these conditions translate into a variable that affects the peer groups' financing decisions (relevance), and affects the firm's financing decision *only* through the peer groups' financing decisions (exclusion). In other words, we require a "shock" to the decision process behind peer firms' financing decisions. Equivalently, we need a perturbation to the equilibrium condition in equation (3).

We argue that the idiosyncratic component of peer firms' equity returns from the previous year is a good candidate for such an instrument. First, the idiosyncratic component of stock returns is, by definition, firm-specific and unrelated to other firms via common factors, consistent with the requirements of the exclusion restriction. Second, a vast empirical asset pricing literature suggests that estimation of this component of stock returns is plausible and, to a degree, empirically testable. Third, the empirical relevance of stock returns for financial policy is well documented (e.g., Loughran and Ritter (1995), Baker and Wurgler (2002), and Welch (2004)) and consistent with several theories.[5]

What is unknown is whether or not the idiosyncratic component of stock returns contains information relevant for future financial policy. Fortunately though, this condition is empirically testable and all analysis below contain formal test results. What is untestable is whether this instrument satisfies the exclusion restriction. Before addressing this issue, we first describe the construction of this instrument, followed by a discussion of potential identification threats to motivate our empirical analysis.

A. Construction of The Instrument

To isolate the idiosyncratic component of stock returns, we specify the following augmented factor model for returns, r_{ijt}:

$$
\begin{aligned}
R_{ijt} = \alpha_{ijt} \quad &+ \quad \beta_{ijt}^{M}(RM_t - RF_t) + \beta_{ijt}^{SMB} SMB_t + \beta_{ijt}^{HML} HML_t + \beta_{ijt}^{MOM} MOM_t \\
&+ \quad \beta_{ijt}^{IND}(R_{jt} - RF_t) + \eta_{ijt}, \qquad (4)
\end{aligned}
$$

where R_{ijt} refers to the total return for firm i in industry j over month t. The first four factors are those typically found in empirical asset pricing studies (e.g., Fama and French (1993) and Carhart (1997)): the excess market return ($RM_t - RF_t$), the small minus big portfolio return (SMB_t), the high minus low portfolio return (HML_t), and the momentum portfolio return (MOM_t) The fifth factor is the excess return on an equal weighted industry portfolio, ($R_{jt} - RF_t$), defined like our peer groups as a three-digit SIC Code. While not a priced risk factor, this last factor is included to remove any variation

[5]For example, Myers and Majluf (1984) suggest that financial policy is linked to stock prices because of information asymmetry between managers and investors. Likewise, Myers (1977) suggests that financial policy is linked to stock prices because of debt overhang considerations.

in returns that is common across firms in the same industry. Inclusion of this factor ensures that the estimated residual, our instrument, is orthogonal to industry shocks.

We estimate equation (4) for each firm on a rolling annual basis using historical monthly returns. We require at least 24 months of historical data and use up to 60 months of data in the estimation. The observed returns, estimated coefficients, and realized factor returns enable us to compute the expected and idiosyncratic components of monthly stock returns.

For example, to obtain expected and idiosyncratic returns for January 1990 through December 1990 for IBM, we first estimate equation (4) using monthly returns from January 1985 through December 1989. Using the estimated coefficients and the factor returns from January 1990 through December 1990, we use equation (4) to compute the expected and idiosyncratic returns as follows:

$$\begin{aligned}
\text{Expected Return}_{ijt} \equiv \hat{R}_{ijt} &= \hat{\alpha}_{ijt} + \hat{\beta}^{M}_{ijt}(RM_t - RF_t) + \hat{\beta}^{SMB}_{ijt} SMB_t + \hat{\beta}^{HML}_{ijt} HML_t \\
&\quad + \hat{\beta}^{MOM}_{ijt} MOM_t + \hat{\beta}^{IND}_{ijt}(R_{jt} - RF_t) \\
\text{Idiosyncratic Return}_{ijt} \equiv \hat{\eta}_{igt} &= R_{igt} - \text{Expected Return}_{igt}
\end{aligned}$$

To obtain expected and idiosyncratic returns for 1991, we repeat the process by updating the estimation sample from 1986 through 1990 and using factor returns during 1991. This process generates betas that are firm-specific and time-varying but constant within a calendar year.[6] Thus, our construction of idiosyncratic shocks allows for heterogeneous — both cross-sectionally and longitudinally — sensitivities to aggregate shocks.

Table IV presents sample means and medians for the estimated coefficients. On average, each of the rolling regressions has 58 monthly observations, though the majority rely on a full five-year window. Additionally, we see that the average R-squared is approximately 30%. Unsurprisingly, the regressions load strongly positively on the industry factor, followed by the market and size factors. The average realized monthly return is 1.4%. The expected return is slightly larger at 1.5% — a difference exacerbated by rounding — which results in a slight negative average idiosyncratic monthly return. Economically speaking, these differences are negligible.

For consistency with our annual accounting data, we transform the monthly returns in two ways. First, we annualize the returns through compounding. Second, we compute average monthly returns for each calendar year and annualize by multiplying by 12. To avoid repetition, we focus attention on the former measure, though our results are qualitatively similar when using the latter.

[6] Performing the estimation on a rolling monthly basis has no effect on our results or inferences.

Our instrument is obtained from the firm-specific annual shocks by computing the average over peer firms. Using averaging as a form of aggregation ensures consistency with the direct ($\bar{y}_{-ijt}$) and indirect ($\bar{X}_{-ijt-1}$) peer effects, both of which are averages. For notational consistency, we denote the instrument by $\bar{\hat{\eta}}_{-ijt-1}$. Note that the instrument is lagged one year relative to the direct peer effect so that relevance requires that the average equity shock to peer firms from last year influences peer firms' average financing decisions this year.

Before discussing the properties of the instrument, we note that, conditional on a properly specified asset pricing model (equation (4)), the instrument need not be zero. Our instrument is a conditional average, conditional on industry and year. Additionally, the instrument is not exactly the industry average since it excludes the i^{th} observation. Panel A of Figure 1 illustrates this variation by presenting the empirical histogram for our instrument. Of course, the average of this average (i.e., the unconditional mean) should be close to zero. This conjecture is confirmed by the approximately zero average idiosyncratic return shown at the bottom of Table IV, and the zero balance point of the empirical histogram in Figure 1.[7]

B. Identification Threats

Identification threats come from correlations between our instrument and the error term, ε_{ijt}, in equation (2) due to either omitted or mismeasured variables. More precisely, the concern is that the instrument is correlated with an omitted or mismeasured firm i-specific effect (e.g., investment opportunities or bankruptcy risk) or common factor (e.g., latent risk factors or overlapping product markets), in which case our estimates of the structural parameters may still contain traces of bias. A more subtle issue arises in distinguishing the precise channel through which the peer effect occurs — directly via financial policy or indirectly via characteristics. This subsection takes a first step towards addressing these issues by examining the statistical and economic properties of our instrument.

[7]The zero unconditional mean result is also consistent with the asymptotic notion that the probability limit, as the number of firms approaches infinity, of the average industry equity shock should be zero. Of course, the notion of a peer group of infinite size is economically nonsensical, so that our economic motivation is consistent with the asymptotic properties of our instrument. More concretely, the economic notion of a peer group places a restriction on both the composition and size of the group. As the size of the group approaches infinity, any corresponding peer effect should approach zero, evidence to which is found in Panel B of Table II.

B.1. Distinguishing Peer Effects from Omitted Firm Characteristics & Common Factors

Previous empirical work shows that observable leverage determinants do a relatively poor job of controlling for systematic variation in capital structures (e.g., Welch (2004), Lemmon, Roberts and Zender (2008), and Stebulaev and Yang (2009)). These findings suggest that there are likely a number of firm characteristics or common factors that are relevant for capital structure, but that are either poorly measured or omitted from equation (2). The relevant issue for identification purposes is whether these omitted variables or measurement errors are correlated with our instrument, the average idiosyncratic equity shock to peer firms. Thus, we focus on ensuring, as much as possible, that the average idiosyncratic equity shock to peer firms is (1) not a better measure of firm i's capital structure determinants, and (2) not capturing a common factor shared among firms within the peer group.

Consider an obvious threat, such as investment opportunities, which are poorly measured and correlated with both stock returns and financial policy. In order for an alternative hypothesis based on mismeasured investment opportunities to contaminate the results, it must be that other firms' idiosyncratic returns better capture firm i's investment opportunities than do all of firm i's observable measures, which include not only the accounting measures and firm i's market-to-book ratio but also firm i's stock return. Likewise, other hard to measure or unobservable capital structure determinants, such as risk and liquidation values, can only contaminate the results in so far as they are correlated with the instrument, conditional on all of firm i's characteristics.

This argument highlights the importance of isolating the idiosyncratic component of stock returns rather than using total returns as an instrument. If the variation in individual stock prices is dominated by the idiosyncratic component, then the average total return of other firms in an industry may provide a less noisy measure of the investment opportunities facing each individual firm than their own individual market-to-book ratios or stock returns. Intuitively, the averaging of returns can net out the noise in each individual stock return. Thus, we rely solely on the idiosyncratic component of stock returns for identification.

Table V examines the extent to which our instrument, peer firm average idiosyncratic equity returns ($\bar{\hat{\eta}}_{-ijt-1}$), correlates with firm i characteristics (X_{ijt-1}). We examine the correlations with both contemporaneous and one-period lead effects, to determine whether the instrument contains information about current or future firm i characteristics.[8] Note that correlation with the characteristics is not problematic because the characteristics are all included in the regression as control variables. In other words, identification of the peer effect cannot come from variation in the instrument that is correlated with any observable firm characteristics. However, economically large associations between the instrument and firm characteristics raises potential concerns about the extent to which our instrument may be correlated with unobservable factors, and the extent to which we have removed common variation among firms' returns via equation (4).

The results reveal no statistically or economically significant associations between our instrument and firm i's characteristics measured either contemporaneously or one-period ahead. All of the scaled (and unscaled) coefficients are statistically and economically indistinguishable from zero. A joint test of coefficient significance also reveals a statistically insignificant result, as revealed by the row denoted F-Stat P-Value. Unreported analysis reveals similar findings when we expand the specification to include additional firm i controls including the marginal tax rate, stock returns, earnings volatility, and Altman's Z-Score. In other words, the instrument contains no information about firm i's observable capital structure determinants, present or near future.

With regard to an ommitted common factor, we note that each specification contains year fixed effects. However, a more salient concern is with regards to an ommitted common factor in equity returns, i.e., a misspecification of the asset pricing model. Unreported results reveal that the contemporaneous conditional correlation between the instrument and firm i's idiosyncratic equity shock is economically tiny (approximately 0.02). Further, the conditional correlation between our instrument and the one-period ahead firm i idiosyncratic equity shock is even smaller (less than 0.01).

While we take additional measures below to address concerns over misspecification of the asset pricing model, these tiny magnitudes are reassuring for three reasons. First, they show that the factor regression (equation (4)) purges most all of the intra-industry correlations present in raw returns. In other words, our instrument does not contain any information about firm i's contemporaneous shock. Second, they show that our instrument does not contain any information about firm i's future shock. Finally, they show that mismeasurement of the peer group will more likely attenuate our findings, as opposed to compromise our identification strategy.

[8] Though using future values on the right hand side of a regression is unorthodox, our goal with this analysis is not to identify the determinants of industry average idiosyncratic equity returns. Rather, we merely want to document the extent of any correlations, without attempting to draw any economic inferences or causal conclusions.

This last point is worth clarifying. If there exist peer groups within an industry, then the asset pricing model may be insufficient to remove common variation among the subgroups, thereby compromising the identification strategy. However, because the correlation between firm i's idiosyncratic equity shock and other firms' equity shocks is near zero, the existence of economically significant subgroups would require a combination of significantly positively and negatively correlated returns within the industry. To examine this possibility, we randomly select subgroups within each industry year combination and estimate the correlation between firm i and these subgroups. Fewer than 5% of the estimated correlation coefficients are negative and less than 1% of these estimates are statistically significant. Thus, any mismeasurement of the peer group will more likely attenuate our findings, as opposed to biasing our results, a conjecture we empirically investigate below.

Ultimately, this analysis and discussion illustrates that our instrument has a number of appealing properties for the purpose of identifying financing externalities. We will refer back to these properties below when we examine alternative hypotheses.

B.2. Distinguishing Direct from Indirect Peer Effects

A more subtle issue concerns distinguishing between the two channels through which a peer effect works — directly through financial policy and indirectly through characteristics. We can control for observable characteristics of peer firms via the term $\bar{X}_{-ijt-1}$. Inclusion of this term, and various fixed effects, alleviates some concern that the direct effect coefficient, β, captures indirect effects. However, the fact that firm i's relevant characteristics are hard to observe and measure, implies the same for its peers. Thus, the other identification concern is that our estimate of the direct effect of peer firm financial policy may be tainted by mismeasured or omitted peer firm characteristics.

To illustrate this problem, consider the following hypothetical example. Firm k introduces a new product, which positively impacts the idiosyncratic component of its stock return. In the following period, firm k issues equity to finance increased production, and reduces its leverage ratio towards a new optimum. In response, peer firm i, issues equity and reduces its leverage too. The question is to what is firm i responding: the introduction of the new product (the indirect peer effect), or the change in financial policy (the direct peer effect)? Relying solely on the equity shock of firm k, in conjunction with observable peer firm characteristics, to identify firm i's response may be insufficient to distinguish between these two channels. Thus, in our robustness section below, we provide additional analysis towards this end.

V. The Role and Implications of Peer Effects

A. *Leverage*

Panel A of Table VI presents the estimated standard deviation scaled coefficients, t-statistics (in parentheses), and model statistics from two-stage least squares (2SLS) regressions of equation (2). We present results for book and market leverage in both levels and first differences. The latter specification helps address concerns over omitted firm i characteristics, since it is equivalent to a levels specification that includes firm fixed effects. The level specifications uses the levels for all of the variables on both left and right hand sides of the equation. The first difference specifications uses first differences for all of the variables on both left and right hand sides of the equation. The only exception is the instrument, average peer firm idiosyncratic equity returns, which is the same across all specifications. Thus, we instrument for the endogenous direct peer effect in year t, $\bar{y}_{ijt}$, with the average idiosyncratic stock returns of peer firms in year $t-1$, $\bar{\hat{\eta}}_{-ijt-1}$.

The first stage results reveal that the average equity shock is strongly negatively associated with both the level and first difference in average industry leverage ratios. The sign of the estimate is consistent with previous findings relating total returns to leverage and with theoretical arguments relating investment opportunities and risk to optimal leverage and financing choices (e.g., Myers (1977) and Scott (1976)). The magnitude of the effects are economically significant as well, stronger than many of the included determinants (not reported). Statistically speaking, the instrument easily passes weak instrument tests (e.g., Stock and Yogo (2005)).

The second stage results reveal that peer firm financial policies are strongly positively related to leverage. The economic magnitude is slightly larger in the 2SLS estimation than the OLS estimation. For example, specification (1) in Panel A of Table VI implies that a one standard deviation change in average peer firm leverage is associated with a 6.4% change in firm i's leverage ratio. The OLS estimate found in column (4) of Panel A, Table II implies only a 2% change in firm i's leverage ratio. While this increase in magnitude may at first seem surprising, the identification discussion of section III shows that the estimated reduced form parameters are nonlinear functions of the structural parameters (see Appendix B for the derivation).

Columns (3) and (4) in Panel A of Table VI reinforce these findings by showing similar results for changes in leverage ratios. A comparison of the coefficients scaled by their corresponding variable standard deviations reveals that the direct peer effect has a larger impact on leverage ratio changes than any other included determinant. This finding is reassuring because it shows that the unobserved firm specific heterogeneity found by Lemmon, Roberts, and Zender (2008) is not responsible for our findings.

As an aside, we note that the estimated firm-specific effects are similar to those found in Table II and the existing literature (e.g., Frank and Goyal (2007)). For example, comparing column (1) in Panel A of Table VI with column (4) in Table II shows that the scaled coefficients on each firm specific characteristic are all within one percentage point of one another. Similarly, column (2) from Panel A of Table VI reveals coefficients that are quantitatively close to those in column (9) of Table II. These similarities are unsurprising in light of the orthogonality between our instrument and firm-specific characteristics (Table V), and they emphasize the fact that the identifying variation behind the estimated peer effect is specific to the idiosyncratic stock return of peer firms from the previous period.

The significant coefficients on the peer firm averages suggest that capital structure decisions are affected not only directly by the leverage choices of a firm's competitors, but also indirectly by their competitors' characteristics. That is, controlling for firm i's characteristics and peer firms' financing decisions, the results in column (1) imply that firms whose competitors are smaller, more profitable or have higher market-to-book ratios tend to have higher leverage ratios. These latter two results appear consistent with the industry equilibrium argument of Shleifer and Vishny (1992), for example. As a firm's competitors become more financially healthy, liquidation values increase. As such, debt becomes less costly and firms can take on more debt — leverage rises.

More generally, these peer characteristic findings suggest that firms consider not only their own characteristics in forming financial policy, but their characteristics *relative* to their competitors. For example, the positive coefficient on firm i's log(Sales) in column (1) suggests that larger firms on average have higher leverage ratios. However, the negative coefficient on *other* firms' size implies that a firm of a given size will use more leverage when its competitors are smaller than when its competitors are larger. This pattern of opposite signs between firm-specific and peer firm characteristics also holds for the other included and significant characteristics. However, the effect of peer firm characteristics on leverage tends to be smaller than that of the firm specific characteristics.

Unfortunately, it is difficult to place a precise interpretation on peer firm characteristics. There is little theory beyond that mentioned that speaks directly to these findings, and the proxies are relatively coarse. However, these results are consistent with the findings of MacKay and Phillips (2005), who suggest that a firm's relative position within its industry is an important determinant of capital structure. More relevant to our study, these results show that competitor characteristics represent an additional channel through which peer firms influence capital structure.

In summary, this analysis shows that peer firms play a significant role in shaping corporate capital structures, in terms of the level and change in book and market leverage ratios. Further, the primary channel through which peers affect capital structures appears to be via financial policy, as opposed to changing characteristics. The next two subsections investigate the robustness of these findings to alternative interpretations.

A.1. Robustness Tests - Peer Effects Vs. Omitted Firm Characteristics & Common Factors

In Panels B and C of Table VI we present a number of robustness checks to mitigate identification concerns related to distinguishing peer effects from omitted and mismeasured firm i characteristics and common factors. The analysis here builds on that found in section IV.B. Panel B presents results for levels, Panel C for first differences. We run all robustness tests on both book and market leverage; however, because of the similarity of findings, we report only the results for book leverage. Further, we focus attention on the key variables of interest: the first stage estimate of the instrument parameter, and the second stage estimate of the direct peer effect parameter.

The specification in Column (1) in Panel B incorporates control variables in addition to all of the ones presented in Panel A. Specifically, we expand both the firm specific factors (X_{ijt-1}) and the peer characteristics ($\bar{X}_{-jt-1}$) to also include: an indicator identifying whether a dividend was paid, Altman's Z-score, Graham's marginal tax rate, capital investment, R&D expenditures, SG&A expenditures, and intra-industry leverage dispersion. The motivation behind these additional factors comes from the parsimonious nature of our baseline specification. The results show a negligible effect on both first and second stage estimates suggesting that there are no obvious observed variables ommitted from the model.

The specification in column (2) addresses the concern that commonality among firms' capital structures is due to the use of common banks (commercial or investment) within the industry. In other words, firms within an industry may be behaving similarly with respect to their financial policies because they are using the same banker, who is giving similar advice. We use Thompson's SDC and Reuters Loan Pricing Corporation's Dealscan database to identify lead underwriters and arrangers or agents for public and private, debt and equity issuances.[9] We then create bank fixed effects for each firm in

[9]Specifically, SDC provides underwriter information for public debt and equity offerings, as well as

the overlap of our sample and these two databases by forward imputation. That is, we assume that the firm uses the same bank each year until either the end of the sample or until we find a different bank being used, regardless of the security being issued. For example, if IBM floated equity with Goldman Sachs as the lead underwriter in 1991, we assume that IBM used Goldman Sachs for each year including and after 1991, until the end of our sample or until they used another bank for a future equity *or* debt issuance. (Results obtained by backward imputation — assuming that the firm used the same bank in all years prior to the issuance until either the beginning of our sample or a new bank was found — are similar.)

We make three points concerning the results in column (2). First, incorporating bank fixed effects reduces the sample size by more than 50% because of the additional data requirements. Second, incorporating bank fixed effects has no impact on the first stage estimate and actually amplifies the second stage estimate. Third, bank effects explain a significant amount of variation in leverage ratios. In unreported analysis using identical samples, the difference in *adjusted* R-squares due to the bank effects is nine percentage points. So, while banks seem to have significant influence over corporate capital structures, they are not responsible for the commonality in financial policies that we are identifying.

In column (3), we incorporate firm i's lagged leverage ratio and the average lagged leverage ratio for the peer group, to capture any targeting behavior or dynamic feedback from the explanatory variables onto leverage ratios. Note, that this specification is similar to that used in papers studying targeting behavior (e.g., Flannery and Rangan (2006) and Kayhan and Titman (2007)). Specifically, a simple reparameterization shows that this specification is identical to a model in which firms adjust to a time-varying target consisting of firm and peer characteristics. (The first difference model found in Panel C implicitly incorporates firm fixed effects into the specification.) Again, both first and second stage estimates are largely unaffected.

In column (4), we incorporate contemporaneous controls, both firm-specific (X_{ijt}) and peer ($\bar{X}_{-ijt}$), in addition to the one-period lagged controls. The motivation here is to ensure that our choice of lag structure is not driving our results, as well as to account for any delays in the response of firm i to the shock to its peers. For example, competitors' contemporaneous market-to-book ratio may capture the impact of a shock to peer firms' investment opportunity set that is only fully realized one period later.

Rule 144a offerings. We rely on Dealscan to identify the lead bank (or arranger) on sole-lender and syndicated loans.

Again, we see evidence of a strong instrument, and an economically significant role for peer firm financial policy.

In column (5), we incorporate the lagged and contemporaneous realized (or total) stock return for firm i, and the lagged and contemporaneous expected stock return for the peer group.[10] This specification addresses two related concerns. The first is that the asset pricing model (equation (4)) is misspecified, in which case there may still be common factors in the estimated idiosyncratic component of stock returns. The second is that our results are capturing a sequencing or timing of returns within an industry, so that firm i is responding to its own return shortly after firm j responds to its return.

Including firm i's total return eliminates both of these concerns since only the portion of peer firms equity shocks that is orthogonal to firm i's total return is available for identification. More simply, if peer firm idiosyncratic returns are capturing a common factor shared by firm i, then this factor is better captured by firm i's total return. Likewise, if firm i is simply responding to it's own stock return following the returns of its peers, then including firm i's return should eliminate this alternative. Again, the results are largely unchanged, which is particularly reassuring since there is, by construction, no scope for identifying variation to contain information impounded in firm i's returns or prices (i.e., market-to-book ratio).

Finally, in column (6), we include quadratic and cubic polynomials of each firm-specific factor and peer firm average characteristic in our primary specification (i.e., firm size, profitability, tangibility, market-to-book). Again, we see little change in the results.

Panel C in Table VI presents similar results for the same specifications in first difference form. We note that both first and second stage results are robust across all specifications.

In unreported analysis, we employ our empirical model and identification strategy on corporate fixed investment measured by capital expenditures. The motivation behind this analysis is to further address concerns over latent investment opportunity commonalities among peer firms. More specifically, we regress firm i investment on peer firm average investment, firm specific and peer firm averages of cash flow and the market-to-book ratio, and industry and year fixed effects. We instrument for peer firm average investment with their average idiosyncratic equity shock. The results reveal no statistically or economically significant direct (or indirect) peer effect, despite a highly statistically significant

[10] Using the realized return for the peer group would confound our results since the identifying variation would be shared by the instrument and the control variable.

positive first stage estimate. Thus, the peer effect found in financial policy seems unlikely to be driven by a corresponding commonality among investment opportunities.

Additionally, we examine the effects of altering the definition of the peer groups First, we find that our results are robust to a more refined definition of peer groups based on intra-industry size groupings (e.g., Bizjak, Lemmon, and Naveen (2008) and Byrd, Johnson, and Porter (1998)). Second, we examine peer group definitions as we did in Panel B of Table II: random, one-digit SIC code, and two-digit SIC code. With randomly assigned industries designed to mimic the average number of firms found in our 3-digit SIC code definition (18 firms), we find a significantly negative first stage estimate and a statistically insignificant second stage estimate indicative of no peer effect. As we move to coarser definitions of the peer group (e.g., 2- and 1-digit SIC codes), both the first and second stage estimates are statistically insignificant. The first stage estimates become insignificant because the distribution of our instrument is collapsing around the unconditional mean of zero. This result can be seen in Panels B and C of Figure 1. The second stage estimate is insignificant because of a combination of a weak instrument and a noisier definition of the peer group. These findings reinforce the importance of the peer group defintion.

The robustness of the results may at first appear surprising. However, this feature largely reflects the instrument properties highlighted above. The instrument is conditionally orthogonal to firm i's contemporaneous and future accounting measures and stock returns. Therefore, adding additional controls such as firm characteristics, peer firm characteristics, and stock returns has little affect on our coefficient estimates. While no instrument is perfect, we believe that these results minimize the scope for alternative interpretations based on omitted or mismeasured firm i characteristics or asset pricing factors. Firms choose leverage ratios, both levels and changes, in close accord with their peers' choices.

A.2. Robustness Tests - Direct Vs. Indirect Peer Effect Channels

The results above suggest that the peer effect works through financial policy, as opposed to characteristics. The effect of average peer firm capital structure on firm i's leverage ratio is significantly larger than that of a change in any average peer firm characteristic. Further, many of the robustness tests performed above, while motivated by alternative hypotheses based on omitted firm specific characteristics, lend further evidence favoring the importance of the direct channel as opposed to the indirect channel. The inclusion of additional peer firm characteristics (column (1)), lagged peer firm average leverage ratios (column (3)), contemporaneous peer firm characteristics (column (4)), peer firm expected returns (column (5)), and nonlinear peer characteristics (column (6)) all mitigate contamination of the direct peer effect estimate (β) by omitted indirect peer effects.

As an additional robustness check, we perform a double sort of the data based on quintiles of our instrument, lagged average peer firm idiosyncratic returns, and the endogenous variable, average peer firm leverage changes. Within each quintile combination, we compute the average change in leverage. As before, we perform this analysis on both book and market leverage but present only the market leverage results for brevity. The goal with this analysis is to determine whether firms' financial policies are responding more to the equity shock or more to the subsequent capital structure change.

The results are presented in Table VII, where quintile (1) represents the lowest 20% of the distribution and quintile (5) the highest. The inner cells of the table contain the average change in leverage for each quintile combination, as well as the corresponding t-statistic of the null hypothesis that the mean is equal to zero. For example, the average change in leverage among firms in the lowest peer firm equity shock quintile (1) and the highest peer firm leverage change quintile (5) is 5.3% with a t-statistic of 24.

Looking across each row, we note a near monotonic increase in the average leverage change. In other words, holding fixed the peer firm equity shock, leverage changes are very sensitive to changes in peer firm leverage. However, the converse is not true. Looking down each column, the average leverage change is largely insensitive to variation in the peer firm equity shock. In fact, in column (3) where the average peer firm leverage change is indistinguishable from zero, we see that only one of the cells is statistically significantly different from zero. That is, firms' only change their leverage in respones to a peer firm equity shock if it is accompanied by a change in peer firm leverage. These findings further reinforce our identificaiton strategy and suggest that the direct channel of peer firm financial policies appears to be the more economically important channel through which peer effects influence capital structure.

B. Financial Policy

In Table VIII, we examine net equity and net debt issuing activity to understand whether peers are influencing specific financing decisions, such as net equity and net debt issuances, or whether leverage is changing because of passive changes in the market value of equity or accumulation of retained earnings. This latter scenario is unlikely since our results unaffected by the inclusion of firms' stock returns and measures of profitability (see Table VI). However, we wish to provide more direct evidence on the precise financing channels driving the leverage results.

Column (1) presents results where the dependent variable is an indicator equal to one if the firm performs a net equity issuance in excess of 1% of total assets, and zero otherwise. This regression models the decision by firms to issue equity in a given year. While a logit or probit model may be more appropriate from a forecasting perspective, we present results using the linear model in equation (2) to ease the interpretation and comparison with other findings. Unreported instrumental variables results using a probit model reveal quantitatively similar findings.

The first stage results reveal that the idiosyncratic component of stock returns is strongly correlated with equity issuance decisions. This effect is both economically and statistically significant, again highlighting that the idiosyncratic component of stock returns is as important for financial policy, if not more so, than the systematic component. The second stage results show that the peer effect is also significant. A one standard deviation increase in the probability of issuing equity by peer firms leads to an 9.1% increase in the probability of firm i issuing equity. In fact, other than firm i's own market-to-book ratio, the peer effect is the most economically important determinant. The other firm-specific factors show similar relations to equity issuance decisions as found in previous studies.[11] None of the peer firm average characteristics are statistically significant.

While the decision to issue equity is closely tied to peers, the relative amount to issue (or repurchase) is statistically weakly related. Column (2) shows that the first stage estimate, while economically large, is statistically insignificant. Similarly, the second stage estimate of the direct peer effect is statistically insignificant.

Looking at column (3) and the decision to issue debt, the estimated scaled coefficient implies that a one standard deviation increase in peer firms' probability of issuing debt is met with a 9.8% increase in the probability of firm i issuing debt. However, this estimate is statistically imprecise, as evidenced by the small t-statistic, despite a highly significant first stage estimate. Indeed, the direct peer effect for debt issuances dwarfs those of the firm-specific effects, the largest of which is 3.9% (Net PPE / Assets). Column (4) reveals even weaker results for the relative amount of debt issued.

One drawback of the anlaysis in columns (1) through (4) is that in many years firms do not undertake any significant financing and, therefore, there is no scope for mimicing financial policy. To address this potential concern, in columns (5) and (6) we restrict

[11]See studies by Hovakimian, Opler, and Titman (2001), and Leary and Roberts (2005).

the sample to just those firm-years in which a financing decision, debt or equity, occurs and re-estimate the stock and debt issuance decision models (columns (1) and (3), respectively). This screen reduces the sample size by 47% (76,501 to 40,258 observations), yet this reduction has little effect on the statistical significance of our findings. The first stage instrument estimates are statistically significant, as are the second stage estimates of the direct peer effect. Economically speaking, a one standard deviation increase in the likelihood of peer firms issuing equity (debt) leads to a 14% (25%) increase in the probability of firm i issuing equity (debt). A comparison to other determinants reveals that these effects are by far the largest.

This analysis shows that peer effects impact leverage through their role in shaping individual financing decisions. Firms security choice, debt or equity, is dictated to a large extent on the security choice of their peers. Finally, the relative unimportance of the peer firm characteristics here further reinforce the previous evidence pointing towards the direct channel of financial policy through which peer effects operate.

C. Amplification, Spillover, and Marginal Effects

An important implication of the empirical model in equation (2) and the estimated peer effects is the presence of externalities. A simple example will illustrate how these externalities function using the results in Panel A of Table VI to make things concrete. Assume firm A's profitability increases. This change leads to a decline in firm A's leverage, as suggested by the negative scaled coefficient estimate for firm-specific EBITDA / Assets. The decline in firm A's leverage leads to a decline in leverage for every other firm in firm A's peer group via the direct peer effect — the positive coefficient on peer firm average leverage. Additionally, the decline in firm A's profitability leads to an increase in leverage for every other firm in firm A's peer group via the indirect peer effect, or the positive coefficient on peer firm average EBITDA / Assets. These latter two effects feedback onto firm A's leverage, again via the direct and indirect peer effect channels, and so on, and so on.

The presence of these externalities implies that the total derivative is no longer equal to the partial derivative, even in a linear model, because of the presence of the outcome variable on the right hand side of the equation. Since the total deriviate is the economic quantity of interest, the effect of a change in any exogenous capital structure determinants (e.g., size, market-to-book, profitability, etc.) cannot be gleaned solely from its coefficient. To see this point and ease the presentation, consider a particular industry j and year t.

Rewriting our model, equation (2), in matrix notation produces

$$y = \frac{\beta}{N-1} Qy + X\lambda + \frac{1}{N-1} QX\gamma + Z\delta + \varepsilon. \tag{5}$$

where $y = (y_1, ..., y_N)'$ is a vector of outcomes for the N firms in an arbitrary industry-year combination, Q is an $N \times N$ matrix with zeros on the diagonal and ones everywhere else, X is an $N \times k_1$ matrix of exogenous variables that appear as both firm specific factors and peer firm averages in our model (e.g., sales, profitability, market-to-book, tangibility), Z is an $N \times k_2$ matrix of all other exogenous variables (e.g., industry and year fixed effects), and ε is an $N \times 1$ vector of residuals.

Solving equation (5) for y yields

$$y = \left(I - \frac{\beta}{N-1} Q \right)^{-1} \left(X\lambda + \frac{1}{N-1} QX\gamma + Z\delta + \varepsilon \right). \tag{6}$$

Of interest is the marginal effect or derivative of the outcome for firm $i = 1, ..., N$, y_i, with respect to a change in each $m = 1, ..., k_1$ exogenous variables for all firms $l = 1, ..., N$, x_{lm}. This derivative equals

$$\frac{\partial y_i}{\partial x_{lm}} = \begin{cases} \lambda_m \left(1 + \frac{\beta^2}{(N-1+\beta)(1-\beta)} \right) + \gamma_m \left(\frac{\beta}{(N-1+\beta)(1-\beta)} \right) & \text{for} \quad i = l \\ \lambda_m \left(\frac{\beta}{(N-1+\beta)(1-\beta)} \right) + \gamma_m \left(\frac{1}{(N-1+\beta)(1-\beta)} \right) & \text{for} \quad i \neq l \end{cases} \tag{7}$$

(See Appendix C for a derivation.)

In the typical linear model without peer effects, both β and γ are equal to zero and the derivative reduces to $\partial y_i / \partial x_{lm} = \lambda_m$ for all i and l. In other words, changing observation i's value for variable m, i.e., x_{im}, by one unit only affects observation i's outcome, y_i, and does so by amount λ_m. With peer effects, λ_m is no longer a sufficient statistic for the marginal effect of exogenous variables and externalities create a channel for cross-observation affects.

Looking more closely at equation (7) when $i = l$, we note that the direct peer effect, β, amplifies the effect of a change in an exogenous variable on y. This amplification mechanism is represented by the parenthetical expression multiplying λ_m. For β in the open unit interval and $N > 1$, this expression is strictly greater than 1.[12] Thus, changes in x in the presence of direct peer effects lead to even larger changes in y because of the feedback among peer outcomes. In addition to this amplification effect is the second

[12]Stationarity requires that β lie within $[0, 1)$. In fact, this is empirically true in all of our models. The coefficient estimate of β ranges from 0.73 to 0.92 for our leverage models found in Table VI, and is never statistically distinguishable from a value greater than or equal to 1.0.

term, which captures spillovers created by the presence of peer firm characteristics. This term can either further amplify ($\gamma_m > 0$) or compress ($\gamma_m < 0$) the marginal effect of a change in x_{im} on y_i.

The direct peer effect also creates a role for cross-observation externalities, as seen by the first term in equation (7) for the case in which $i \neq l$. This term captures the change in firm i's outcome arising from an exogenous change in x_{lm}. Amplifying or compressing this first spillover term is a second spillover term due solely to the indirect effects. Thus, cross-observation spillover effects become a potentially important element of financial policy once peer effects — direct and indirect — enter into the model.

Table IX presents estimates of the standard deviation scaled coefficients and corresponding t-statistics from the model of market leverage found in column (2) of Panel A, Table VI. Also presented are estimates of the derivatives, amplification terms, and spillover terms from equation (7), as well as corresponding chi-square statistics in brackets. The columns labeled ($\lambda \times \sigma_x$) and ($\gamma \times \sigma_x$) simply repeat the firm specific and peer firm average characteristic parameter estimates, as well as the direct peer effect estimate (β), found in Table VI for ease of reference.

Equation (7) shows that the amplification and spillover terms are all decreasing functions of N, the size of the industry. Intuitively, each firm has a smaller effect on its peers, the larger is the peer group. To show the consequences of this variation, we present estimates of these terms for three different size industries based on the fifth (6 firms), fiftieth (12 firms), and ninety fifth (33 firms) percentiles of the industry size distribution. We see that the amplification term, the first parenthetical term in equation (7) for $i = l$, declines from 1.22 to 1.039 as we move from the small to the large peer group. In other words, the marginal effect of a change in any exogenous variable is amplified by 22% in small industries and 4% in large industries because of feedback among financing decisions. We also note that the two distinct spillover terms show similar declining patters; though, only the second parenthetical term in equation (7) for the case $i \neq l$ is statistically significaint.

This sensitivity to peer group size implies that all of the derivatives are a function of industry size, as well. However, the deriviatives may be increasing or decreasing functions depending upon the sign and magnitide of the firm specific peer firm average characteristic, (λ) and (γ) resptively. For example, the firm size scaled derivatives are increasing functions of the peer group size, largely because of the relatively large negative coefficient on the peer firm average size characteristic (γ). On the other hand, the small negative coefficient on peer firm average tangibility results in a scaled derivative that increases with peer group size. We also note that none of the cross-observation deriviates appear to be significnat, statistically or economically. This result is largely due to a combination of two forces: (1) indirect peer firm effects that are small relative to firm-specific effects, and (2) the derivative measures the response to a change in only one peer firm's characteristic.

Ultimately, there are three key messages concerning the marginal effects of exogenous variables that come from Table IX. First, the marginal effects of the exogenous variables differ from that implied by the firm-specific coeffecient. Second, the marginal effects vary as a function of the size of the peer group. Third, existing empirical results can be reinterpreted as embedding different feedback and spillover effects. Specifically, when the market-to-book ratio, for example, of firm i changes, it affects firm i's capital structure not only because of firm i's response to this changing characteristic, but also because of firm i's response to its peers that are also affected by firm i's changing characteristic and policy response.

VI. Why do Firms Mimic One Another?

Given the importance of peer firm behavior for firms' capital structures, we now turn to understanding *why* firms mimic one another. We begin with a brief discussion of the relevant theory to motivate the subsequent empirical analysis aimed at identifying the underlying economic mechanism behind the peer effects.

A. Theoretical Motivation

Mimicking behavior in economic models is typically motivated either by an attempt to elicit the private information of other agents (information inflow) or by an effort to influence the perception of one's own private information (information outflow). An example of the former in our context is when firms learn about optimal capital structure in their industry by observing the financing decisions of other firms. As shown by Banerjee (1992) and others, when a firm's own signal is noisy and optimization is costly or time-consuming (Conlisk, 1980), managers will rationally put more weight on the decisions of others than on their own information. This is especially likely when other firms in the industry are perceived as having greater expertise (Bikhchandani, Hirshleifer and Welch, 1998). Bikhchandani et al. refer to this as observational learning or social learning.

Managers may also mimic other firms' policies to influence the perceived quality of either the manager or the firm. For example, Scharfstein and Stein (1990) and Zwiebel (1995) both use corporate investment decisions as a vehicle to illustrate how managers' reputational concerns can lead to herd behavior. In the former study, higher quality managers receive correlated signals about investment opportunities, while lower quality managers receive independent signals. Managers therefore mimic the investment choice of others in order to increase their perceived type. In this environment, herding is more important than making efficient investment choices because blame is shared in the event of a bad outcome. However, herding behavior can be mitigated by short-term incentive contracts and relative performance evaluation, or when managers outside opportunities are greater.

In Zwiebel's model, managers' types are inferred from their relative performance. Because managers perceived to be below a cutoff type are fired, they prefer to mimic the investment choices of others in order to minimize the volatility of their relative performance. The exceptions are low-skilled managers, for whom volatility decreases the likelihood of being fired, and the highest-skilled managers, who face little chance of firing and value the efficiency gain of innovating. Thus, in contrast to Scharfstein and Stein (1990), relative performance evaluation generates, rather than mitigates, herding behavior.

Complementing these theories is the model of Ross (1977) that shows how financial policy can be used to influence the perceived quality of the firm. Specifically, Ross shows that when insiders have better information about firm value than outside investors, insiders may try to use financial structure to signal this information to the market. However, if the signal is not sufficiently costly, low quality firms will imitate the financial structure of the high quality firms to avoid having their type detected. A pooling equilibrium results in which all firms make the same financing choices.

Finally, while models that explicitly predict mimicking or herding behavior tend to be based on informational frictions, interactions between financial structure and product market competition may also lead to mimicking of financial policies. One motivation is fear of predation. For example, in Bolton and Scharfstein (1990), high leverage invites predatory price competition from less levered rivals; in Chevalier and Scharfstein (1996), firms with high leverage under-invest during an industry downturn and lose market share to more conservatively financed competitors. If the expected cost of this predatory behavior is severe enough, highly levered firms will mimic the capital structures of their less-levered rivals. Alternatively, firms may increase their leverage in response to a levered buyout of industry peers, either as a takeover defense or as a commitment to compete aggressively (Brander and Lewis, 1986).

B. Empirical implications

To distinguish among these alternative mechanisms we ask two questions: which types of firms are most influenced by the financial policy choices of their peers? And, which types of firms are doing the influencing?

From the discussion above, if firms mimic their peers in an effort to learn about optimal capital structure, then we expect mimicking behavior to be most pronounced among firms that view their peers as having superior information, e.g., younger firms, new entrants, and those with lower market share or weaker past performance. Likewise, managerial reputation models suggest that managers with greater reputational concerns are likely to be younger CEOs with less tenure, but also those managing younger, less successful firms.

Additionally, according to Scharfstein and Stein (1990), mimicking behavior should be weaker when managers have better outside opportunities or are evaluated relative to their peers, and when compensation is tied to (short-term) firm performance. For reasons mentioned above, Zwiebel's (1995) model does not share this last prediction, though it does imply a nonlinear relationship between past success and herding tendencies where the least and most skilled managers have the greatest incentive to deviate.

Finally, the pooling explanation based on signaling models relies on the signal (e.g. debt issuance) being relatively low-cost. We would thus expect peer effects to be stronger among firms with low cost access to external capital, i.e. those with looser financing constraints. Finally, if mimicking is driven by a fear of predation, it should be more pronounced among firms for which predation would be more costly: those with higher market share and greater distress costs.

C. Results

Table X presents the first set of results. To maintain consistency with the theory, we focus our analysis on the change in market leverage. Our empirical strategy for identifying which types of firms mimic is to interact the direct peer effect variable with one-period lagged indicators identifying the firm type for firm i. For example, the first column in Panel A interacts the presence of a credit rating for firm i with the direct peer effect variable, peer firm average leverage change. As indicated at the top of the column, Group 1 corresponds to firms without a rating, Group 2 with. For continuous variables (e.g., market share), we sort firms within each industry-year into tertiales and focus only on the lower and upper third of the distribution for reasons concnering statistcal power.

To ensure proper identification, we estimate all of the models using two stage least squares and instrument for the now two endogeneous variables by interacting the indicator variable identifying the firm type with our instrument, the average idiosyncratic stock return for peer firms. While this strategy preserves proper identification, it comes at the price of statistical power. The interactions not only "split" the identifying variation, they also create a significant amount of multicollinearity (e.g., Greene (2008), Wooldridge (2006)). Thus, we emphasize differences that are economically significant, as opposed to statistically significant, hoping that future research can move beyond the analysis here.

To ease the presentation and maintain focus on the variables of interest, we have suppresed the coefficient estimates of the firm-specific factors and the peer firm average characteristics, both of which are included in every model along with industry and year fixed effects. Focusing on Panel A, we see that younger, smaller (market share), non-dividend paying, high growth (market-to-book) firms without a credit rating tend to mimic their peers more strongly than their counterparts. Similarly, more financially constrained (Whited-Wu) firms tend to mimic more. Interestingly, we don't see much variation in mimicing behavior across industries classified by the degree of product market competition (industry concentration), or across periods classified by the strength of the stock market (market return).

Panel B examines the subsample of our firms with information on CEOs from Execucomp. The model and estimation procedures are similar to those found in Panel A, only now the indicator variable interacted with the direct peer effect identifies the type of CEO, as opposed to firm. A quick glance at the table reveals weak instruments and statistically noisy estimates — unsurprising consequences of an approximate 87% reduction in the sample size. Nonetheless, the results do offer some potentially interesting insights for future research to investigate using alternative data and/or techniques. Lowered paid CEOs experiencing smaller raises tend to mimic more than their counterparts. Also, CEOs with less time at their current company tend to mimic much more strongly than their more experienced counterparts.

Table XI addresses the second question raised at the start of this section: which types of firms are mimiced? Again, we focus on the change in market leverage, though our empirical strategy now revolves around determining if one group of firms (followers) more strongly mimic another group of firms (leaders) within an industry-year. We do this by sorting firms within each industry-year into teritales based on various measures. We then define the followers as those firms in the bottom two thirds and the leaders as those firms in the top third of the distribution. We then exclude the top third of the distribution from the estimation, focusing only on the follower firms, and replace the direct peer effect of the followers with that of the leaders. In essence, we are estimating the extent to which follower firms are sensitive to the financial policies of leader firms. As before, we estimate the model using two stage least squares, where the instrument for the leaders' average leverage change is their average idiosyncratic equity shock.

Panel A shows that smaller (market share), less profitable firms with low stock returns and low earnings growth are very sensitive to the financial policies of their counterparts. Indeed, these results are not only economically signifcant, but statistically significant, as well. All of the first stage estimates, but for the firm age and earnings growth classifications, pass the weak instruments tests. (The stock return classification is marginal.) However, the second stage estimates suggest very large marginal effects: one standard deviation changes in leader firm financial policies lead to a 4% to 5% change in leverage — a full standard deviation (see Table I).

In light of the theory, these results suggest that varying degrees of learning and reputation considerations play a role in mimicing behavior. The fact that more financially constrained firms are more likely to mimic suggests that costly signalling stories are less likely responsible for mimcing behavior. Likewise, the independence between industry concentration and mimicing behavior suggests that financial policy-product market interactions are not the underlying mechanism. To be clear, these results do not imply that product market competition or signalling are irrelevant for capital structure, more generally. Rather, these results suggest that the mechanism driving the estimated peer effects is more likely due the learning and reputation motives.

VII. Conclusions

This study has shown that firms do not make financing decisions in isolation. Rather, the financing decisions of firms' peers are an important determinant of corporate capital structures and financial policies. We find that not only are peer effects statistically significant, they are economically large. Marginal effects of peer decisions on the level and change in book and market leverage are greater than any other observable capital structure determinants. Behind these leverage effects is a strong peer effect among security (debt and equity) issuance decisions — firms are significantly more likely to issue the security issued by their peers.

We also find a significant, albeit economically smaller, role for peer firm characteristics in shaping capital structure. While more difficult to interpret, the results suggest that a firm's position relative to its industry is relevant for its capital structure choice, consistent with the findings of MacKay and Phillips (2005). Thus, while peer firm financing decisions drive firms in the same industry to similar capital structures, peer firm characteristics help explain the distribution of capital structures *within* industries.

Finally, the economic mechanism driving the peer effects appears to be learning and reputational, both firm and manager, motives. Though our evidence on this front is more suggestive at this stage; and, consequently, we hope will inspire further investigation.

That said, an important implication of our findings here is the presence of amplification and spillover effects. Changes affecting one firms capital structure affect peer firms' capital structures, which feedback onto the original firms capital structure, and so on, and so on. Thus, the marginal effect of capital structure determinants has a different interpretation in the presence of interactive effects, as there are multiple channels through which changes to one determinant influence the capital structure decision.

Given the economic importance of peer effects documented here, we hope that future research, both theoretical and empirical, will explore more closely the implications for this feedback and the mechanisms behind this capital structure determinant.

Appendix A: Variable Definitions

Compustat variable names denoted by "dataXXX." Time periods are denoted by (t) or (t-1) suffixes. We screen firm-year observations based on nonmissing data for the levels and first differences of the following variables: net equity issuances, net debt issuances, book leverage, market leverage, sales, market-to-book ratio, profitability, tangibility, and idiosyncratic component of stock returns.

Total Book Assets = data6.

Total Debt = Short-Term Debt + Long-Term Debt = data9 + data34.

Book Leverage = Total Debt / Total Book Assets.

Market Value of Assets (MVA) = data199 * data54 + data34 + data9 + data10 - data35.

Market Leverage = Total Debt / MVA.

Net Debt Issuances = [(data9(t) + data34(t)) - (data9(t-1) + data34(t-1))] / data6(t-1).

Debt Issuance Indicator = 1 if Net Debt Issuances > 1%; 0 otherwise.

Net Equity Issuances = (data108 - data115(t) / data6(t-1).

Equity Issuance Indicator = 1 if Net Equity Issuances > 1%; 0 otherwise.

Firm Size = Log(Sales) = Log(data12).

Tangibility = Net PPE / Assets = data8 / data6.

Profitability = EBITDA / Assets = data13 / data6.

Market-to-Book Ratio = MVA / Total Book Assets.

Common Dividends = data21.

Common Dividend Indicator = 1 if data21 > 0; 0 otherwise.

Sales, General, and Administrative Expenses = data189 / Firm Size.

Research and Development Expenses = data46 / Firm Size.

Capital Expenditures = data128.

Capital Investment = Capital Expenditures(t) / Net PPE(t-1).

Altman's Z-Score = (3.3 * data170 + data12 + 1.4 * data36 + 1.2 * (data4 - data5)) / data6

Earnings Volatility is computed each year as the historical standard deviation of EBITDA / Assets. We require at least three years of nonmissing data.

Marginal Tax Rates were downloaded from John Graham's website.

Appendix B: The Identification Problem

This appendix provides a formal derivation of the identification problem discussed in section IV B. Ignoring the time fixed effects for notational convenience, consider the population version of equation (2),

$$y = \alpha + \beta E(y|\mu_j) + \lambda' X + \gamma' E(X|\mu_j) + \delta' \mu_j + \varepsilon. \tag{8}$$

The two conditional expectations on the right hand side of equation (8) are peer group means, such as industry averages, and correspond to the direct and indirect peer effects.

The corresponding mean regression of y on X and μ_j (the conditional expectations are functions of μ_j) is therefore

$$E(y|X, \mu_j) = \alpha + \beta E(y|\mu_j) + \lambda' X + \gamma' E(X|\mu_j) + \delta' \mu_j. \tag{9}$$

Taking expectations of this equation with respect to the firm characteristics, X, conditional on μ_j yields the equilibrium condition

$$E(y|\mu_j) = \alpha + \beta E(y|\mu_j) + \lambda' E(X|\mu_j) + \gamma' E(X|\mu_j) + \delta' \mu_j. \tag{10}$$

Assuming that $\beta \neq 1$, this equilibrium has a unique solution

$$E(y|\mu_j) = \frac{\alpha}{1-\beta} + \left(\frac{\gamma + \lambda}{1-\beta}\right)' E(X|\mu_j) + \left(\frac{\delta}{1-\beta}\right)' \mu_j. \tag{11}$$

Equation (11) is the mean regression of y on μ_j ($E(X|\mu_j)$ is by definition a function of μ_j). Assuming the intercept, conditional expectation of X, and the group fixed effects are linearly independent, the composite parameters, $\alpha/(1-\beta)$, $[(\gamma+\lambda)/(1-\beta)]'$, and $[\delta/(1-\beta)]'$ are identified. However, the structural parameters $(\alpha, \beta, \gamma', \lambda')$ are not identified since we have fewer equations than unknowns. Therefore, without further information or parameter restrictions, one cannot distinguish direct peer effects from indirect peer effects from firm-specific effects.

Appendix C: Exogenous Variable Derivatives

For ease of reference, we repeat equation (6) here:

$$y = \left(I - \frac{\beta}{N-1}Q\right)^{-1}\left(X\lambda + \frac{1}{N-1}QX\gamma + Z\delta + \varepsilon\right),$$

where $y = (y_1, ..., y_N)'$ is a vector of outcomes for the N firms in an arbitrary industry-year combination, Q is an $N \times N$ matrix with zeros on the diagonal and ones everywhere else, X is an $N \times k_1$ matrix of exogenous variables that appear as both firm specific factors and peer firm averages in our model (e.g., sales, profitability, market-to-book, tangibility), Z is an $N \times k_2$ matrix of exogenous variables that appear only as firm specific factors (e.g., industry and year fixed effects), and ε is an $N \times 1$ vector of residuals. and ones everywhere else.

The goal is to derive a closed form solution for the derivative of an arbitrary element y_i in the vector y with respect to an arbitrary element x_{lm} in the matrix X. To accomplish this, we need expressions for the two $N \times N$ matrices multiplying X:

$$\left(I - \frac{\beta}{N-1}Q\right)^{-1} \text{ and } \left(I - \frac{\beta}{N-1}Q\right)^{-1}\frac{1}{N-1}Q.$$

Induction and matrix algebra shows that the first matrix is symmetric and has two distinct elements on and off the main diagonal.

On-Diagonal: $\frac{N-1-\beta(N-2)}{(N-1+\beta)(1-\beta)}$

Off-Diagonal: $\frac{\beta}{(N-1+\beta)(1-\beta)}$

Using this result and the definition of Q, the second matrix is also symmetric and has two distinct elements on and off the main diagonal.

On-Diagonal: $\frac{\beta}{(N-1+\beta)(1-\beta)}$

Off-Diagonal: $\frac{1}{(N-1+\beta)(1-\beta)}$

Therefore, the derivative of an arbitrary element y_i in the vector y with respect to an arbitrary element x_{lm} in the matrix X is therefore equal to

$$\frac{\partial y_i}{\partial x_{lm}} = \begin{cases} \lambda_m \left(1 + \frac{\beta^2}{(N-1+\beta)(1-\beta)}\right) + \gamma_m \left(\frac{\beta}{(N-1+\beta)(1-\beta)}\right) & \text{for} \quad i = l \\ \lambda_m \left(\frac{\beta}{(N-1+\beta)(1-\beta)}\right) + \gamma_m \left(\frac{1}{(N-1+\beta)(1-\beta)}\right) & \text{for} \quad i \neq l \end{cases}$$

where we used the equality

$$\frac{N-1-\beta(N-2)}{(N-1+\beta)(1-\beta)} = \left(1 + \frac{\beta^2}{(N-1+\beta)(1-\beta)}\right)$$

to rewrite the amplification term multiplying λ_m in the case $i = l$.

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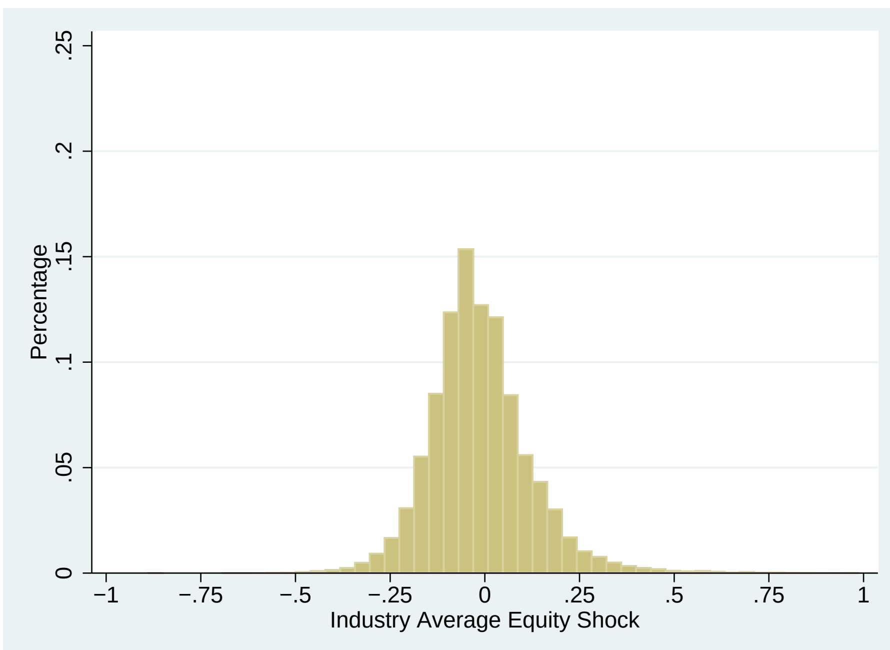
Figure 1

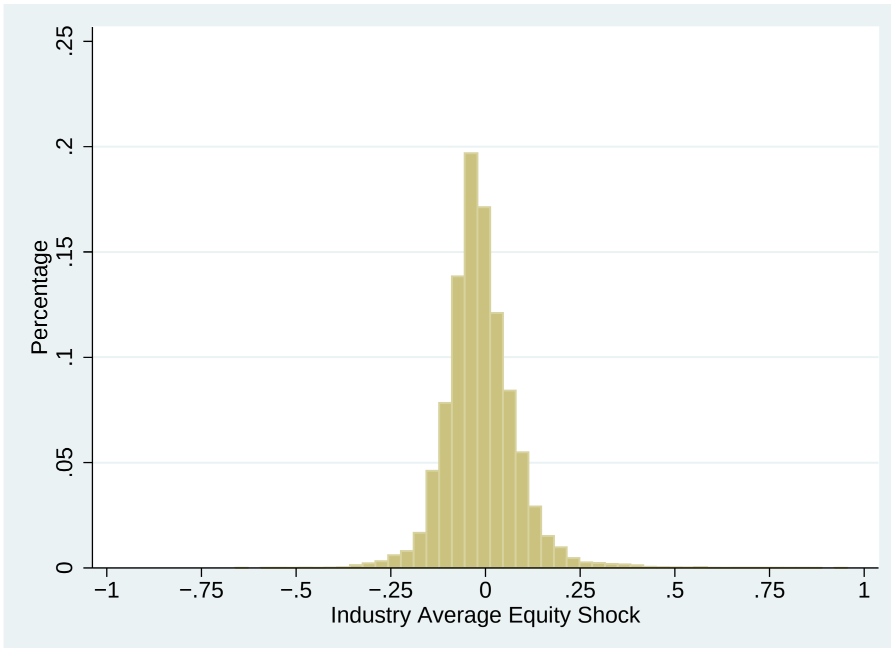
Industry Average Idiosyncratic Stock Returns Distribution

The sample consists of all nonfinancial, nonutility firms in the annual Compustat database between 1965 and 2006 with nonmissing data for all analysis variables. The figure presents the empirical distribution of our instrument, peer firm average idiosyncratic annual equity returns, for three definitions of peer groups based on three-digit SIC code (Panel A), two-digit SIC code (Panel B), and one-digit SIC code (Panel C). Peer firm averages are defined as the 3-digit SIC code-year average excluding the i^{th} observation. The data has been truncated at -1 and +1 to ease the presentation. The peer firm average for the firm i, year t observation is defined as the sample mean of all firms in the peer group excluding the it observation.

Panel A: Three-Digit SIC Code Peer Groups



Panel B: Two-Digit SIC Code Peer Groups



Panel C: One-Digit SIC Code Peer Groups

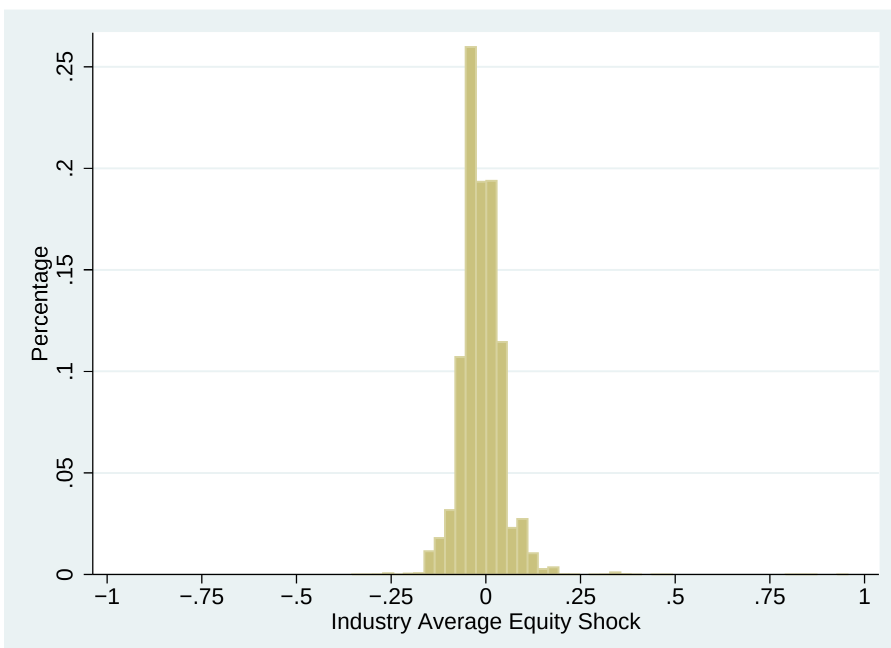


Table I

Summary Statistics

The sample consists of all nonfinancial, nonutility firms in the annual Compustat database between 1965 and 2006 with nonmissing data for all analysis variables (see Appendix A). The table presents means, standard deviations (SD), and medians. All variables are formally defined in Appendix A. Peer firm averages are defined as the 3-digit SIC code-year average excluding the i^{th} observation.

	Levels			First Differences		
	Mean	Median	SD	Mean	Median	SD
Outcome Variables						
Book Leverage (Total Debt / Book Assets)	0.241	0.218	0.199	0.004	-0.000	0.095
Market Leverage (Total Debt / Market Assets)	0.277	0.220	0.247	0.004	0.000	0.116
Direct Peer Effects (Peer Firm Avgs)						
Book Leverage	0.243	0.237	0.088	0.008	0.008	0.026
Market Leverage	0.268	0.257	0.133	0.008	0.007	0.047
Indirect Peer Effects (Peer Firm Avgs)						
Log(Sales)	4.592	4.446	1.311	0.118	0.120	0.108
Market-to-Book	1.540	1.345	0.775	-0.053	-0.026	0.356
EBITDA / Assets	0.080	0.102	0.089	-0.005	-0.003	0.030
Net PPE / Assets	0.312	0.263	0.172	0.001	0.000	0.015
Firm Specific Factors						
Log(Sales)	4.986	4.921	2.158	0.096	0.091	0.288
Market-to-Book	1.382	0.961	1.355	-0.035	-0.005	0.878
EBITDA / Assets	0.105	0.128	0.161	-0.003	0.000	0.103
Net PPE / Assets	0.322	0.272	0.221	-0.001	-0.002	0.054
Industry Characteristics						
# of Firms per Industry-Year	17.988	12.000	22.211			
Total # of Industries	178					
Sample Characteristics						
Observations	76,501					
Firms	9,293					

Table II

OLS Leverage Regressions

The sample consists of all nonfinancial, nonutility firms in the annual Compustat database between 1965 and 2006 with nonmissing data for all analysis variables (see Appendix A). The table presents estimated coefficients scaled by the corresponding variable's standard deviation, and t-statistics robust to heteroskedasticity and within firm dependence in parentheses. Peer firm averages are defined as the 3-digit SIC code-year average excluding the i^{th} observation. All models are estimated by OLS. All variables are in levels and all right hand side variables are lagged one year relative to the dependent variable, either book or market leverage as indicated above the columns, with the exception of peer firm averge leverage which is contemporaneous with the dependent variable. Panel B presents market leverage regression results for four different definitions of industry: randomly assigned, 1-digit SIC, and 2-digit SIC. The randomly assigned industries are constructed to mimic the size of 3-digit SIC codes. The estimates for the randomly assigned industries are constructed by an iterative procedure that (1) randomly assigns observations to industries and (2) estimates the model. This procedure is repeated 100 times. The displayed coefficients, t-statistics, and R-squared are averages over the 100 iterations. The adjusted R-squared for the firm fixed effect specification measures the explained variation after partialling out the firm fixed effects. Statistical significance at the 5% and 1% levels are denoted by "*" and "**", respectively. All variables are formally defined in Appendix A.

Panel A: Different Leverage Specifications

	Book Leverage					Market Leverage				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Peer Firm Avg Leverage	0.068**		0.055**	0.021**	0.022**	0.107**		0.077**	0.039**	0.054**
	(37.847)		(28.005)	(8.753)	(11.125)	(45.225)		(32.219)	(13.199)	(21.308)
Log(Sales)		0.022**	0.017**	0.020**	0.038**		0.033**	0.021**	0.023**	0.079**
		(11.722)	(9.340)	(9.923)	(7.303)		(14.933)	(9.824)	(9.768)	(14.318)
Market-to-Book		-0.023**	-0.016**	-0.017**	-0.004**		-0.079**	-0.064**	-0.065**	-0.028**
		(-16.554)	(-11.517)	(-11.709)	(-2.690)		(-46.943)	(-41.163)	(-40.800)	(-23.343)
EBITDA / Assets		-0.035**	-0.035**	-0.037**	-0.031**		-0.049**	-0.047**	-0.047**	-0.043**
		(-20.753)	(-21.213)	(-21.974)	(-18.253)		(-29.499)	(-29.415)	(-29.478)	(-25.554)
Net PPE / Assets		0.048**	0.030**	0.044**	0.036**		0.047**	0.028**	0.039**	0.041**
		(24.675)	(15.363)	(16.652)	(11.954)		(21.597)	(13.015)	(13.563)	(12.452)
Firm Fixed Effects	No	No	No	No	Yes	No	No	No	No	Yes
Industry Fixed Effects	No	No	No	Yes	No	No	No	No	Yes	No
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	76,501	76,501	76,501	76,501	76,501	76,501	76,501	76,501	76,501	76,501
Adj. R^2	0.123	0.112	0.170	0.192	0.062	0.213	0.247	0.307	0.322	0.154

Panel B: Different Peer Group Definitions

	Market Leverage			
	Random Industry	1-Digit SIC Industry	2-Digit SIC Industry	3-Digit SIC Industry
Peer Firm Avg Leverage	-0.016	0.023**	0.056**	0.077**
	(-0.266)	(9.557)	(23.629)	(32.219)
Log(Sales)	0.033**	0.030**	0.023**	0.021**
	(14.930)	(13.657)	(10.483)	(9.824)
Market-to-Book	-0.079**	-0.077**	-0.070**	-0.064**
	(-46.932)	(-46.435)	(-43.716)	(-41.163)
EBITDA / Assets	-0.049**	-0.048**	-0.046**	-0.047**
	(-29.505)	(-29.131)	(-28.524)	(-29.415)
Net PPE / Assets	0.047**	0.040**	0.030**	0.028**
	(21.589)	(17.521)	(13.975)	(13.015)
Firm Fixed Effects	No	No	No	No
Industry Fixed Effects	No	No	No	No
Year Fixed Effects	Yes	Yes	Yes	Yes
Obs	76,407	76,323	76,323	76,501
Adj. R^2	0.247	0.254	0.288	0.307

Table III

Reduced Form OLS Leverage Regressions

The sample consists of all nonfinancial, nonutility firms in the annual Compustat database between 1965 and 2006 with nonmissing data for all analysis variables (see Appendix A). The table presents estimated coefficients scaled by the corresponding variable's standard deviation, and t-statistics robust to heteroskedasticity and within firm dependence in parentheses. All models are estimated by OLS. All variables are in levels and all right hand side variables are lagged one year relative to the dependent variable, either book or market leverage as indicated above the columns. Indirect Effects refer to peer firm averages, defined as the 3-digit SIC code-year average excluding the i^{th} observation. Firm Specific Factors refer to the i^{th} observation's characteristic. F-stat is the test statistic of the null hypothesis that all of the indirect effects' coefficients equal zero. Statistical significance at the 5% and 1% levels are denoted by "*" and "**", respectively. All variables are formally defined in Appendix A.

	Book Leverage					Market Leverage				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Indirect Effects (Peer Firm Avgs)										
Log(Sales)	0.003		-0.007**	-0.021**	-0.014**	0.022**		0.009**	-0.012*	-0.012**
	(1.229)		(-2.783)	(-4.711)	(-4.017)	(6.569)		(2.736)	(-2.367)	(-2.920)
Market-to-Book	-0.020**		-0.013**	0.003	-0.003	-0.058**		-0.032**	0.003	-0.010**
	(-9.047)		(-6.011)	(1.541)	(-1.621)	(-21.364)		(-12.712)	(1.124)	(-5.129)
EBITDA / Assets	-0.001		0.011**	0.017**	0.003	-0.015**		-0.001	0.008**	-0.002
	(-0.445)		(5.146)	(7.359)	(1.728)	(-5.670)		(-0.393)	(2.841)	(-0.824)
Net PPE / Assets	0.035**		0.002	0.008	0.009*	0.033**		0.003	0.023**	0.019**
	(15.894)		(0.709)	(1.485)	(2.244)	(12.502)		(1.032)	(3.583)	(4.039)
Firm Specific Factors										
Log(Sales)		0.022**	0.022**	0.019**	0.040**		0.033**	0.025**	0.022**	0.083**
		(11.722)	(10.770)	(9.863)	(7.669)		(14.933)	(10.353)	(9.717)	(14.800)
Market-to-Book		-0.023**	-0.018**	-0.018**	-0.004**		-0.079**	-0.068**	-0.066**	-0.029**
		(-16.554)	(-12.333)	(-12.033)	(-2.714)		(-46.943)	(-41.414)	(-40.818)	(-23.329)
EBITDA / Assets		-0.035**	-0.038**	-0.037**	-0.032**		-0.049**	-0.049**	-0.048**	-0.043**
		(-20.753)	(-22.047)	(-22.168)	(-18.272)		(-29.499)	(-29.690)	(-29.598)	(-25.418)
Net PPE / Assets		0.048**	0.044**	0.044**	0.036**		0.047**	0.039**	0.039**	0.042**
		(24.675)	(16.316)	(16.555)	(12.030)		(21.597)	(13.133)	(13.549)	(12.814)
Firm Fixed Effects	No	No	No	No	Yes	No	No	No	No	Yes
Industry Fixed Effects	No	No	No	Yes	No	No	No	No	Yes	No
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat	146.636**		22.144**	15.833**	5.511**	350.862**		81.229**	5.670**	15.477**
Obs	76,501	76,501	76,501	76,501	76,501	76,501	76,501	76,501	76,501	76,501
Adj. R^2	0.064	0.112	0.117	0.191	0.056	0.160	0.247	0.261	0.318	0.134

Table IV
Stock Return Factor Regression Results

The table presents mean factor loadings and adjusted R-squares from the regression

$$
\begin{aligned}
r_{ijt} = \alpha \quad &+ \quad \beta_{it}^{m}(rm_t - rf_t) + \beta_{it}^{SMB} SMB_t + \beta_{it}^{HML} HML_t + \beta_{it}^{MOM} MOM_t \\
&+ \quad \beta_{it}^{j}(rj_t - rf_t) + \eta_{ijt},
\end{aligned}
$$

where r_{ijt} is the return to firm i in industry j during period t, $(rm_t - rf_t)$ is the excess return on the market, SMB_t is the small minus big portfolio return, and HML_t is the high minus low portfolio return, MOM_t is the momentum portfolio return, $(rj_t - rf_t)$ is the excess return on an equal-weighted portfolio of stocks in the same industry as defined by three-digit SIC code. The regression is estimated for each firm on a rolling annual basis using historical monthly returns data from the CRSP database. We require at least 24 months of historical data and use up to 60 months of data in the estimation. Expected returns are computed using the estimated factor loadings and realized factor returns. Idiosyncratic returns are computed as the difference between realized and expected returns.

	Mean	Median	SD
α_{it}	0.766	0.683	1.552
β_{it}^{M}	0.215	0.288	0.812
β_{it}^{SMB}	0.120	0.107	0.935
β_{it}^{HML}	0.002	0.022	0.838
β_{it}^{IND}	0.801	0.700	0.680
β_{it}^{MOM}	-0.014	-0.016	0.570
Obs Per Regression	58	60	5
Adjusted R^2	0.296	0.287	0.177
Avg Monthly Return	0.014	0.000	0.181
Expected Monthly Return	0.015	0.013	0.118
Idiosyncratic Monthly Return	-0.001	-0.007	0.175

Table V
Instrument Properties

The sample consists of all nonfinancial, nonutility firms in the annual Compustat database between 1965 and 2006 with nonmissing data for all analysis variables (see Appendix A). The table presents estimated coefficients scaled by the corresponding variable's standard deviation, and t-statistics robust to heteroskedasticity and within firm dependence in parentheses. Peer firm averages are defined as the 3-digit SIC code-year average excluding the i^{th} observation. The dependent variable is our instrument, peer firm average idiosyncratic equity returns, defined as the 3-digit SIC code-year average excluding the i^{th} observation. All independent variables are in levels and are either contemporaneous with or a one-period lead relative to the dependent variable. Firm Specific Factors refer to the i^{th} observation's characteristic. Indirect Effects refer to peer firm averages of each firm specific factor. The F-stat P-value is the P-value for F-statistic testing the null hypothesis that the firm-specific factor coefficients are jointly equal to zero. The partial adjusted R-squared is the variation in the dependent variable explained by all of the firm-specific factors and indirect effects after orthogonalizing all varibles with respect to the fixed effects. Statistical significance at the 5% and 1% levels are denoted by "*" and "**", respectively. All variables are formally defined in Appendix A.

	Peer Firm Average Equity Shock	
	Contemporaneous Independent Vars	1-Period Lead Independent Vars.
Firm Specific Factors		
Log(Sales)	-0.001	-0.000
	(-1.016)	(-0.030)
Market-to-Book	-0.000	0.001
	(-0.166)	(0.823)
EBITDA / Assets	-0.000	-0.001
	(-0.607)	(-0.958)
Net PPE / Assets	0.000	0.000
	(0.191)	(0.295)
Indirect Effects (Peer Firm Avgs)	Yes	Yes
Industry Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Obs	76,501	76,300
F-Stat P-value	0.599	0.788
Adj. R^2	0.095	0.097
Partial Adj. R^2	-0.000	-0.000

Table VI

Two Stage Least Squares Leverage Regressions

The sample consists of all nonfinancial, nonutility firms in the annual Compustat database between 1965 and 2006 with nonmissing data for all analysis variables (see Appendix A). The table presents estimated coefficients scaled by the corresponding variable's standard deviation, and t-statistics robust to heteroskedasticity and within firm dependence in parentheses. All models are estimated by linear two stage least squares (2SLS) where the endogenous variable is the peer firm average leverage ratio, and the instrument is the one period lagged peer firm average idiosyncratic component of stock returns. Peer firm averages are defined as the 3-digit SIC code-year average excluding the i^{th} observation. All variables are in levels or first differences as indicated at the top of the columns. All right hand side variables, including the instrument but excluding the endogenous variable, are lagged one year relative to the dependent variable, book leverage (columns (1) and (3)) or market leverage (columns (2) - (4)). Indirect Effects refer to peer firm averages. Firm Specific Factors refer to the i^{th} observation's characteristic. In Panel B, all specifications include firm-specific and indirect effects for firm size, profitability, tangibility, and the market-to-book ratio and are estimated by 2SLS using the same instrumenting procedure as in Panel A. Investment Bank Indicators refer to indicator variables for the primary or lead underwriter for the firm's past security issuances, debt or equity. Additional Control Variables include lagged firm specific and indirect effects for cash flow volatility, a dividend payer indicator, Altman's Z-score, Graham's marginal tax rate, capital expenditures divided by the capital stock as of the previous period, R&D expenditures divided by sales, and SG&A expenditures divided by sales as well as the intra-industry standard deviation of leverage. Stock Return Controls includes firm i's lagged and contemporaneous total stock return, and the industry average expected stock return. Polynomials of Control Variables (Vars) include quadratic and cubic terms of all right hand side variables other than industry average leverage. Contemporaneous indirect Effects (I.E.s) adds contemporaneous indirect effects to the specification in addition to the lagged indirect effects. Panel C is identical to Panel B but for the use of first differences for all variables other than equity returns. Statistical significance at the 5% and 1% levels are denoted by "*" and "**", respectively. All variables are formally defined in Appendix A.

Panel A: Leverage Regressions

	Levels		1^{st} Differences	
	Book Leverage (1)	Market Leverage (2)	Book Leverage (3)	Market Leverage (4)
Direct Effect				
Peer Firm Avg Leverage	0.064**	0.088**	0.023*	0.064**
	(3.307)	(4.091)	(2.317)	(3.601)
Indirect Effects (Peer Firm Avgs)				
Log(Sales)	-0.018**	-0.017**	-0.002	-0.006*
	(-3.988)	(-3.307)	(-1.296)	(-1.976)
Market-to-Book	0.013**	0.028**	0.001	0.001
	(3.416)	(4.090)	(0.840)	(0.836)
EBITDA / Assets	0.019**	0.020**	0.001	0.002*
	(7.683)	(4.747)	(1.346)	(2.567)
Net PPE / Assets	-0.020	-0.007	-0.001	-0.002
	(-1.940)	(-0.731)	(-0.727)	(-1.696)
Firm Specific Factors				
Log(Sales)	0.019**	0.023**	0.002*	0.005**
	(9.701)	(9.620)	(2.564)	(7.752)
Market-to-Book	-0.017**	-0.065**	-0.002**	0.000
	(-11.487)	(-40.114)	(-2.785)	(0.406)
EBITDA / Assets	-0.038**	-0.048**	-0.003**	-0.003**
	(-22.191)	(-29.074)	(-4.289)	(-5.664)
Net PPE / Assets	0.044**	0.039**	0.003**	0.005**
	(16.292)	(13.159)	(7.365)	(9.615)
Equity Shock	-0.002*	-0.003**	-0.001	0.003**
	(-2.076)	(-4.451)	(-1.948)	(6.427)
First Stage Instrument				
Peer Firm Avg Equity Shock	-0.021**	-0.032**	-0.008**	-0.009**
	(-15.211)	(-18.337)	(-8.611)	(-7.461)
Industry Fixed Effects	Yes	Yes	No	No
Year Fixed Effects	Yes	Yes	Yes	Yes
Obs	76,501	76,501	76,501	76,501

Panel B: Leverage Levels Robustness Tests

	Market Leverage - Level					
	(1)	(2)	(3)	(4)	(5)	(6)
Direct Effect						
Peer Firm Avg Leverage	0.078**	0.134**	0.120**	0.070*	0.056*	0.080**
	(3.696)	(4.981)	(5.753)	(2.246)	(2.482)	(3.986)
First Stage Instrument						
Peer Firm Avg Equity Shock	-0.032**	-0.034**	-0.022**	-0.021**	-0.029**	-0.033**
	(-18.288)	(-11.956)	(-18.100)	(-13.355)	(-17.469)	(-19.052)
Indirect Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Specifc Factors	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Control Variables	Yes	No	No	No	No	No
Bank Fixed Effects	No	Yes	No	No	No	No
Lagged Dependent Variable	No	No	Yes	No	No	No
Contemporarneous Controls	No	No	No	Yes	No	No
Stock Return Controls	No	No	No	No	Yes	No
Polynomials of Controls	No	No	No	No	No	Yes
Obs	72,550	33,321	76,501	76,300	75,316	76,501

Panel C: Leverage First Differences Robustness Tests

	Market Leverage - 1^{st} Differences					
	(1)	(2)	(3)	(4)	(5)	(6)
Direct Effect						
Peer Firm Avg Leverage Change	0.061**	0.104*	0.064*	0.081*	0.050**	0.063**
	(3.427)	(2.263)	(1.980)	(2.097)	(2.833)	(3.261)
First Stage Instrument						
Peer Firm Avg Equity Shock	-0.010**	-0.006**	-0.005**	-0.004**	-0.008**	-0.009**
	(-7.510)	(-3.130)	(-4.013)	(-3.849)	(-6.978)	(-6.899)
Indirect Effects	Yes	Yes	Yes	Yes	Yes	Yes
Firm Specifc Factors	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional Control Variables	Yes	No	No	No	No	No
Bank Fixed Effects	No	Yes	No	No	No	No
Lagged Dependent Variable	No	No	Yes	No	No	No
Contemporarneous Controls	No	No	No	Yes	No	No
Stock Return Controls	No	No	No	No	Yes	No
Polynomials of Controls	No	No	No	No	No	Yes
Obs	68,619	33,321	76,500	76,300	75,316	76,501

Table VII
Average Leverage Changes by Peer Firm Equity Shock and Peer Firm Leverage Change

The sample consists of all nonfinancial, nonutility firms in the annual Compustat database between 1965 and 2006 with nonmissing data for all analysis variables (see Appendix A). The table presents average book leverage changes for 25 groups of observations. The groups are formed by the intersection of two quintiles: (1) one period lagged peer firm average idiosyncratic component of stock returns, and (2) peer firm average change in book leverage, excluding firm i. Group averages are presented in brackets next to the quantile number. For example, the average peer firm average leverage change in quantile 2 is -0.01, and the average lagged peer firm average equity shock for quantile 4 is 0.04. The columns labeled "1 - 3" and "5 - 3" present the difference in means for columns 1 and 3 and 5 and 3, respectively. t-statistics robust to heteroskedasticity and within firm dependence are in parentheses. Statistical significance at the 5% and 1% levels are denoted by "*" and "**", respectively. All variables are formally defined in Appendix A.

Lagged Peer Firm	Peer Firm Avg Leverage Change Quantiles						
Avg Equity Shock	1	2	3	4	5 (High)	1 - 3	5 - 3
1 (Low)	-0.036**	-0.009**	-0.002	0.018**	0.053**	-0.034**	0.055**
	(-17.955)	(-4.328)	(-1.056)	(8.741)	(23.836)		
2	-0.041**	-0.008**	0.004*	0.018**	0.050**	-0.045**	0.046**
	(-18.606)	(-3.520)	(2.394)	(10.190)	(20.935)		
3	-0.041**	-0.013**	0.001	0.016**	0.055**	-0.042**	0.055**
	(-20.818)	(-7.073)	(0.461)	(7.598)	(25.874)		
4	-0.044**	-0.013**	0.008**	0.015**	0.060**	-0.052**	0.052**
	(-20.418)	(-7.778)	(4.765)	(7.364)	(24.913)		
5 (High)	-0.044**	-0.010**	-0.004	0.012**	0.057**	-0.040**	0.061**
	(-22.208)	(-5.007)	(-1.678)	(5.893)	(25.297)		

Table VIII

2SLS Security Issuance Decision Regressions

The sample consists of all nonfinancial, nonutility firms in the annual Compustat database between 1965 and 2006 with nonmissing data for all analysis variables (see Appendix A). The table presents estimated coefficients scaled by the corresponding variable's standard deviation, and t-statistics robust to heteroskedasticity and within firm dependence in parentheses. All models are estimated by linear 2SLS where the endogenous variables is the peer firm average leverage ratio, and the instrument is the one period lagged peer firm average idiosyncratic component of stock returns. Peer firm averages are defined as the 3-digit SIC code-year average excluding the i^{th} observation. The dependent variable is indicated at the top of the columns in both panels. All right hand side variables are lagged one period. Indirect Effects refer to peer firm averages. Firm Specific Factors refer to the i^{th} observation's characteristic. Issue Stock (Debt) is an indicator variable equal to one if Net Stock (Debt) Issuances normalized by lagged book assets is greater than 1%. Columns (5) and (6) isolate the subsample of observations in which either an equity or debt issuance occurred. Statistical significance at the 5% and 1% levels are denoted by "*" and "**", respectively. All variables are formally defined in Appendix A.

	Issue Stock	Net Stock Issuances	Issue Debt	Net Debt Issuances	Subsample of Issuances: Issue Stock	Subsample of Issuances: Issue Debt
	(1)	(2)	(3)	(4)	(5)	(6)
Direct Effect						
Peer Firm Avg Dependent Var	0.091**	0.163	0.098	-0.032	0.136**	0.253*
	(4.623)	(1.046)	(1.273)	(-0.350)	(4.379)	(2.182)
Indirect Effects (Peer Firm Avgs)						
Log(Sales)	-0.001	-0.017	-0.007	-0.004	0.010	0.002
	(-0.077)	(-1.367)	(-0.977)	(-0.511)	(0.920)	(0.244)
Market-to-Book	-0.020	-0.007	0.010	0.010**	-0.049**	-0.039*
	(-1.919)	(-0.498)	(0.839)	(3.850)	(-3.040)	(-2.099)
EBITDA / Assets	0.007	0.014	0.016	0.011**	-0.017**	-0.025
	(1.705)	(1.781)	(0.862)	(3.024)	(-2.604)	(-0.864)
Net PPE / Assets	0.007	0.014*	-0.019	0.002	0.008	-0.042*
	(0.782)	(2.273)	(-1.520)	(0.120)	(0.607)	(-2.123)
Firm Specific Factors						
Log(Sales)	-0.027**	-0.014**	0.029**	-0.010**	-0.054**	0.034**
	(-9.201)	(-7.030)	(10.392)	(-4.762)	(-12.422)	(8.091)
Market-to-Book	0.095**	0.070**	0.005*	0.020**	0.092**	-0.062**
	(33.655)	(12.445)	(2.124)	(8.902)	(28.699)	(-17.759)
EBITDA / Assets	-0.036**	-0.070**	-0.008**	0.001	-0.019**	0.003
	(-15.218)	(-13.089)	(-2.808)	(0.364)	(-5.700)	(0.904)
Net PPE / Assets	0.008*	0.014**	0.039**	-0.002	-0.018**	0.023**
	(2.445)	(6.732)	(11.538)	(-1.005)	(-3.880)	(5.422)
Equity Shock	0.029**	0.015**	0.010**	0.007**	0.028**	-0.012**
	(18.295)	(8.192)	(5.444)	(4.366)	(12.621)	(-4.667)
First Stage Instrument						
Peer Firm Avg Equity Shock	0.088**	0.172	-0.026**	2.510*	0.099**	-0.022**
	(26.245)	(1.353)	(-7.030)	(2.178)	(20.747)	(-4.262)
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Obs	76,501	76,501	76,501	76,501	40,258	40,258

Table IX

Exogenous Variable Derivatives, Marginal Effects, and Leverage Multipliers

The sample consists of all nonfinancial, nonutility firms in the annual Compustat database between 1965 and 2006 with nonmissing data for all analysis variables (see Appendix A). The table presents estimated coefficients and derivatives, both scaled by the corresponding variable standard deviation, from a regression of market leverage on peer firm leverage, peer firm characteristics, and firm specific factors. All variables are in levels. The last two groups include log(sales), the market-to-book ratio, EBITDA / Assets, and Net PPE / Assets. Peer firm averages are defined as the 3-digit SIC code-year average excluding the i^{th} observation. Derivatives are computed for three peer groups differing in their size: small (6 firms), medium (12 firms), and large (33 firms). The derivative $\partial y_i/\partial x_{im}$ present the estimated derivative showing the change to the outcome of ovservation i (y_i) following a one unit change to variable x_m for observation i (x_{im}). The derivative $\partial y_i/\partial x_{km}$ present the estimated derivative showing the change to the outcome of ovservation i (y_i) following a one unit change to variable x_m for observation k (x_{km}). Both derivatives are scaled by the standard deviation of the corresponding x variable, (σ_x). Amplification term is the multiplicative factor due to the direct peer effect. The terms Spillover 1 and Spillover 2 are the additive factors due to the direct and indirect peer effect. t-statistics robust to heteroskedasticity and within firm dependence are in parentheses. chi-square statistics robust to heteroskedasticity and within firm dependence are in brackets. Statistical significance at the 5% and 1% levels are denoted by "*" and "**", respectively. All variables are formally defined in Appendix A.

	Firm-Specific Factor	Peer Firm Average	Peer Group Size - Small		Peer Group Size - Medium		Peer Group Size - Large	
Variable	Scaled Coefs ($\lambda \times \sigma_x$)	Scaled Coefs ($\gamma \times \sigma_x$)	$\frac{\partial y_i}{\partial x_{im}} \times \sigma_x$	$\frac{\partial y_i}{\partial x_{km}} \times \sigma_x$	$\frac{\partial y_i}{\partial x_{im}} \times \sigma_x$	$\frac{\partial y_i}{\partial x_{km}} \times \sigma_x$	$\frac{\partial y_i}{\partial x_{im}} \times \sigma_x$	$\frac{\partial y_i}{\partial x_{km}} \times \sigma_x$
Log(Sales)	0.023**	-0.017**	0.018**	-0.007	0.020**	-0.003	0.022**	-0.001
	(9.620)	(-3.307)	[15.052]	[2.054]	[43.070]	[2.013]	[77.116]	[1.987]
Market-to-Book	-0.065**	0.028**	-0.063**	0.003	-0.064**	0.001	-0.065**	0.001
	(-40.114)	(4.090)	[592.407]	[1.282]	[1133.541]	[1.256]	[1515.577]	[1.240]
EBITDA / Assets	-0.048**	0.020**	-0.047**	0.002	-0.047**	0.001	-0.048**	0.000
	(-29.074)	(4.747)	[366.855]	[0.625]	[624.513]	[0.626]	[791.315]	[0.627]
Net PPE / Assets	0.039**	-0.007	0.044**	0.008	0.041**	0.004	0.040**	0.001
	(13.159)	(-0.731)	[86.507]	[3.333]	[142.551]	[3.261]	[173.245]	[3.217]
Peer Firm Avg. Leverage (β)	0.088**							
	(4.091)							
Amplification Term			1.222*		1.108*		1.039*	
			[4.183]		[4.183]		[4.183]	
Spillover 1			0.339		0.164		0.059	
			[2.136]		[2.047]		[1.996]	
Spillover 2			0.515*		0.250*		0.089*	
			[5.169]		[4.842]		[4.659]	

Table X
2SLS Regressions: Which Firms Mimic?

The sample consists of all nonfinancial, nonutility firms in the annual Compustat database between 1965 and 2006 with nonmissing data for all analysis variables (see Appendix A). We also use a subsample conditional on data from Execucomp in Panel B. The table presents estimated coefficients scaled by the corresponding variable's standard deviation, and t-statistics robust to heteroskedasticity and within firm dependence in parentheses. Peer firm averages are defined as the 3-digit SIC code-year average excluding the i^{th} observation. All models are estimated by linear 2SLS where the endogenous variables are the peer firm average change in leverage ratio interacted with different indicator variables, and the instruments are the one period lagged peer firm average idiosyncratic component of stock returns interacted with the same indicator variables. All variables are in first differences We rank industry-firm-year observations into three groups based on the lower, middle, and upper third of the distribution of lagged values for firm age, market share (sales as a fraction of industry sales), the market-to-book ratio, and Whited-Wu Index of financial constraints. We exclude the middle third of the distribution for each of these regressions. We also classify firms according to the presence of a credit rating and whether or not they paid a dividend the previous year. The table also presents the heteroskedasticity corrected Cragg-Donald statistic testing for weak instruments (First Stage Multivariate F-stat). Indirect Effects refer to industry averages excluding the i^{th} observation. Firm Specific Factors refer to the i^{th} observation's characteristic. All test statistics are computed using standard errors that are robust to within firm correlation and heteroskedasticity. Statistical significance at the 5% and 1% levels are denoted by "*" and "**", respectively. All variables are formally defined in Appendix A.

	Credit Rating (2=Yes)	Dividend Payer (2=Yes)	Firm Age (2=Old)	Market Share (2=Large)	Market-to-Book (2=High)	Whited-Wu Index (2=Cnstrd)	Industry Concentration (2=Conc)	Market Return (2=High)
Direct Effects								
Peer Firm Avg Leverage Change × Group 1	0.067**	0.055**	0.106	0.063**	0.035	0.034	0.035	0.050
	(2.745)	(3.202)	(1.113)	(2.644)	(1.880)	(1.950)	(1.389)	(1.315)
Peer Firm Avg Leverage Change × Group 2	0.020*	0.034**	0.025	0.024	0.069**	0.059**	0.043	0.062**
	(2.074)	(2.649)	(1.271)	(1.377)	(3.702)	(2.622)	(1.866)	(2.944)
First Stage Multivariate F-stat	14.190**	25.414**	0.719	10.648**	12.371**	21.484**	11.306**	4.468
Indirect Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Specific Factors	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Obs	76,501	76,501	46,861	51,485	51,122	51,692	47,929	51,072

Panel B: Interactions with CEO Characteristics

	CEO Age (2=Old)	CEO Total Pay (2=High)	CEO Total Pay Growth (2=High)	CEO Tenure (2=Long)	Company Tenure (2=High)
Direct Effects					
Peer Firm Avg Leverage Change × Group 1	0.017	0.064	0.072	0.016	0.189
	(0.509)	(1.864)	(1.092)	(0.372)	(0.446)
Peer Firm Avg Leverage Change × Group 2	0.044	0.006	0.010	0.037	-0.049
	(1.678)	(0.227)	(0.197)	(1.411)	(-0.314)
First Stage Multivariate F-stat	2.758	3.268	0.881	1.534	0.105
Indirect Effects	Yes	Yes	Yes	Yes	Yes
Firm Specific Factors	Yes	Yes	Yes	Yes	Yes
Industry Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Obs	11,280	10,366	9,371	12,227	6,985

Table XI

2SLS Regressions: Which Firms are Mimiced, Industry Leaders or Followers?

The sample consists of all nonfinancial, nonutility firms in the annual Compustat database between 1965 and 2006 with nonmissing data for all analysis variables (see Appendix A). The table presents estimated coefficients scaled by the corresponding variable's standard deviation, and t-statistics robust to heteroskedasticity and within firm dependence in parentheses. All models are estimated by linear 2SLS where the endogenous variable is the industry average leverage change, Industry Avg., and the instrument is the one period lagged industry average idiosyncratic component of stock returns, Avg. Equity Shock. Indirect Effects refer to peer firm averages. Firm Specific Factors refer to the i^{th} observation's characteristic. All specifications include indirect effects and firm-specific factors for firm size, profitability, tangibility, and the market-to-book ratio. Firms are classified as either "Leaders" or "Followers" based on: age, profitability, market share (sales as a fraction of industry sales), stock returns, and earnings growth. Leaders are defined as those firms falling in the upper third of the distribution; followers are defined as those firms falling in the lower and middle thirds. The table restricts attention to the sample of followers and regresses their change in market leverage ratio on the average change in market leverage of Leaders, as well as the control variables indicated towards the bottom of the table. First stage coefficients and t-statistics for the instrument are also presented. Statistical significance at the 5% and 1% levels are denoted by "*" and "**", respectively.

	Market Leverage				
	Age	Profitability	Market Share	Stock Return	Earnings Growth
Direct Effect					
Leader Firm Avg Leverage Change	-0.301	0.044**	0.040**	0.055*	0.084
	(-1.615)	(3.482)	(3.356)	(1.965)	(1.847)
First Stage Instrument					
Leader Firm Avg Equity Shock	0.003	-0.011**	-0.012**	-0.004**	-0.003**
	(1.820)	(-9.483)	(-9.507)	(-4.542)	(-2.885)
Contextual Effects	Yes	Yes	Yes	Yes	Yes
Firm Specific Factors	Yes	Yes	Yes	Yes	Yes
Industry Effects	No	No	No	No	No
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Obs	38,103	47,997	44,161	49,495	50,975

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