

A Critique of Quantitative Structural Models in Corporate Finance*

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March 2, 2010

Abstract

I contend that structural quantitative models are better suited for situations in which the important forces are *a priori* well understood. This is rarely the case in corporate finance. It is also more difficult to detect and correct for misspecification in these models. Their existing empirical tests have largely been perfunctory. They have not entertained powerful and often simpler alternatives. They have not been tested out-of-sample. They have not been tested in the context of quasi-experiments, such as tax law changes. Moreover, even in-sample, the empirical evidence strongly rejects current models. They attribute capital structure behavior primarily to forces that seem to be, at best, of minor importance. Managers intervene in their capital structures very actively for reasons not yet understood.

*This paper would make suitable reading for a Ph.D. course in empirical methods or corporate finance, especially together with Fischer, Heinkel and Zechner (1989), Hennessy and Whited (2005), and Strebulaev (2007).

Please note that it is the purpose of a critique to focus on shortcomings. Thus, the perspective of this paper is inevitably biased.

Modern economics is guided by two principles. First, Occam's razor dictates that theories should be only as complex as necessary. Second, positive economics dictates that *theory is to be judged by its predictive power for the class of phenomena which it is intended to "explain"* (Friedman (1966, page 8)). Depending on the context, economic models can also be more reduced-form or more structural (based on deeper microfoundations), and focused more on delivering qualitative predictions (comparative statics) or quantitative predictions. Although these characteristics vary in degrees, although any combinations are feasible, and although there is some subjectivity in applying these labels to economic theories, my paper will contrast two clusters in the corporate finance literature: theories that are relatively simpler, reduced-form, and built for qualitative predictions; and theories that are relatively more complex, structural, and built for quantitative predictions.

The more complex structural modeling approach with quantitative predictions was pioneered in macroeconomics. Lucas (1976) critique that reduced form models can be unstable led economists to focus more on models with relatively deeper "structural" microfoundations. Mehra and Prescott (1985)'s critique that simple models could fit the equity premium qualitatively but not quantitatively led economists to focus more on the quantitative aspects of their models. This approach has also recently become more popular in corporate finance, where it has garnered multiple Brattle prizes in the Journal of Finance. In the capital structure literature, it is sometimes called the new "dynamic tradeoff theory" (DTT). Many prominent recent papers (e.g., Hennessy and Whited (2005), Hennessy and Whited (2007), Ju, Parrino, Poteshman and Weisbach (2005), Strebulaev (2007), Titman and Tsyplakov (2007), DeAngelo, DeAngelo and Whited (2010)) have developed such models to explain the firm's capital structure choice. The theory's most common ingredients include the presence of taxes, distress costs, and frictions (TDF). The theories have appealing normative prescriptions for managers, but my paper is concerned only with their positive use to explain corporate behavior empirically.

My paper critiques this aspect of the literature in its current form. My main objections are (A) that the DTT models have not entertained good alternatives. Moreover, our lack of *a priori* understanding of the most important underlying economic forces in corporate finance renders the quantitative structural approach problematic. And (B), that there is little evidence supporting these models. They have never been subjected to appropriately stringent tests. This deserves elaboration. Every theory must pass a set of increasing hurdles:

1. Does the theory make sense?¹

The existing quantitative structural models in the capital structure literature, especially those by Strebulaev (2007) and Hennessy and Whited (2005) which I will discuss in more detail my paper, easily pass this hurdle. They are built on a small number of

¹Friedman argues that the first hurdle is not necessary. Even unreasonable theories can be acceptable, as long as the model predicts well. I disagree. Evaluating how well the inputs fit can just be as enlightening as evaluating how well the outputs fit.

simple and plausible forces. (This is despite the fact that they have specificity in detail that can be argued with.)

Yet, even when empirically calibrated, a quantitative model that merely passes this hurdle is still only a hypothesis. It is at least since Galileo that scientists have agreed that it is the role of empirical tests to distinguish between plausible theories—and there are many potential alternatives to explain managerial capital structure choices.

If the point of these theories were only to point out that interpretations of earlier empirical work can require nuance, then they are a success. Yet, if they want to be relevant for understanding the real world (or even that this nuance is necessary), then they have to survive empirical tests themselves, too.

2. Does the theory fit the data in-sample?

Fitting data in-sample, is still a rather modest hurdle. Modelers have many degrees of freedom. Asking any model only to explain evidence in-sample (and especially if the theory was designed for it) is perfunctory. In practice, existing empirical tests of quantitative structural theories in corporate finance have employed a hurdle that is best described as analogous to judging a qualitative reduced-form theory by the r-squared of an in-sample regression with many variables at the researcher's discretion, without controls for competitive explanations and confounding variables, and without diagnostics and corrections for a whole range of possible misspecification errors. Better tests are possible.

Yet, the DTT models struggle even on their fairly modest attempts. Section III.D shows that the evidence in their favor is not strong. The TDF theory, upon which some of these models are build, fails in its most essential prediction—that non-readjustment can be explained by non-activity. Empirically, managers are very active. The “funding model” by Hennessy and Whited (2005) predicts a cash link which does hold in the data—albeit with an R-square of only 0.0008 (!). It also predicts that high-debt firms are inclined to fund more with debt (the opposite of readjustment)—a prediction which seems rejected by the data. (If a theory has two prediction, and one is rejected, then the theory is rejected.) In sum, no quantitative structural model in corporate finance has shown that its focus is on a first-order effect, and that it can help explain actual in-sample behavior, much less better so than conceptual models. I will enumerate below a number of *a priori* reasons (such as lack of *a priori* knowledge of the underlying structure and other misspecification issues) why this should not have come as a surprise.

3. Does the theory predict out-of-sample?

More complex structural quantitative models should predict better out of sample than simpler alternatives—and there are some very simple ones: any model should predict better than an alternative that states that firms do what they have always done; and it should predict the behavior of the actual firm better than that of a randomly drawn

other firm. The DTT models have not been subjected to and have therefore not passed this hurdle.

4. Does the theory fit and predict well during quasi-experiments?

When feasible, this is the most powerful test of any theory. It requires the presence of a known exogenous shock to the model's inputs (or structural parameters). There is also again an easy benchmark that any theory should beat: Any quantitative model should predict better if the known quasi-exogenous change of an independent variable is *not* ignored. This test is powerful, because it is the alternative hypothesis that receives the specific prediction: "behaving as firms always have" rejects the theory. Fortunately, such theory-relevant shocks have occurred frequently in the capital structure context: There have been changes in the tax code, changes in the costs of distributing securities, changes in the bankruptcy law, innovations in securities, profitability shocks, etc., and many other inputs that dynamic tradeoff models (or their competitors) are built on. Quasi-experimental tests can be run in-sample or, better yet, out-of-sample. The DTT models have not been subjected to and have therefore not passed this hurdle, either.

These hurdles should be viewed as relative to competitive theories, and especially simpler ones. Each successive hurdle is necessary but not sufficient for adoption of the theory.

Applying all four hurdles is not asking a model to predict everything. A model need only outpredict reasonable alternatives in the arena that it was designed for. In the optimal capital structure literature, this means that the DTT model should predict the evolution of corporate leverage ratios better than alternatives (and of course not perfectly). Because many DTT models exist not merely to illustrate an effect conceptually or to predict qualitatively (both of which can be done with more simple reduced-form models), but primarily to predict better quantitatively; and because structural micro-foundations are modeled primarily in order to give the model stability in the presence of counterfactuals, which are in a sense realized natural experiments, asking these theories to pass even the final quasi-experimental prediction hurdle is all the more appropriate.

My paper also echoes some points made (independently) in a recent critique by Angrist and Pischke (2010). They argue not only that similarly quantitative structural models have failed badly in industrial organizations, labor economics, macro-economics, etc., but also that the alternative of design-based empirical studies (including quasi-experimental designs) has offered real tests of economic forces and causality even for starkly reduced-form simple models. My paper endorses their views. In addition, I argue that quantitative models are not substitutes but complements to design based studies. *The predictions of quantitative structural models should be tested in experimental or quasi-experimental settings out-of-sample against reasonable alternatives.*

I The Optimal Capital Structure

In this section, I first go over a brief and selective history of the optimal capital structure literature, and then illustrate typical modeling tradeoffs with four variants of the same model in varying degrees of complexity.

A A Brief and Selective History

The modern optimal capital structure literature started with Williams (1938) and Modigliani and Miller (1958), which showed that the firm's leverage ratio is irrelevant in a perfect market.² Robichek and Myers (1966) introduced the main tradeoff that is still the most common in this literature today: a firm that has too little debt loses tax advantages, a firm that has too much debt suffers excessive financial distress costs. Brennan and Schwartz (1978) introduced modeling techniques that were known to have had considerable success in pricing financial derivatives, and which made it possible to analyze optimal capital structure for long-lived firms. The Brennan-Schwartz model (and many of its successors) had many quantitative aspects to them, but they were not yet empirically calibrated. Fischer et al. (1989) introduced transaction costs into a similar dynamic model of capital structure based on a tax vs. distress costs tradeoff. Their model lacked closed-form solutions and did not attempt an empirical calibration, either. However, it essentially completed the three basic ingredients of the most prominent strand of current capital structure models: a benefit vs. a cost of debt, overlaid with transaction costs. We shall refer to models based on these ingredients as the tax-distress-frictions (TDF) model. Fischer et al. showed that frictions implied that firms should change their capital structures only rarely. Leland (1994) derived the first dynamic model with closed-form solutions in the absence of transaction costs (allowing for taxes, default costs, and endogenous default), but also did not attempt an empirical calibration. Leland and Toft (1996) extended this to an endogenous bankruptcy choice. Goldstein, Ju and Leland (2001) further extended the model to allow for transaction costs and dynamic debt level changes.

The first empirical calibrations of dynamic optimal capital structure models appeared in Hennessy and Whited (2005) and Ju et al. (2005). In Hennessy and Whited (2005), the Brattle first-prize paper of 2005, firms choose between reinvestment vs. payout and between debt vs. equity in the presence of new equity flotation costs (of 2.8%). Their point is easy to explain with an example. Firms that experience a positive earnings shock need

²My paper focuses only on a part of the optimal capital structure literature. It omits many relevant models. For example, in Titman and Tsyplakov (2007), firms choose investment and capital structure to trade off financial distress costs and a conflict between debt holders and equity holders in the presence of transaction costs. In contrast to Hennessy-Whited and Strebulaev, firms have time-varying target debt ratios. My paper also ignores a related literature that has developed to explain credit spreads (Jones, Mason and Rosenfeld (1983), Collin-Dufresne, Goldstein and Martin (2001), and Titman, Tompaidis and Tsyplakov (2004)). Credit spreads are subject to external convergence trading. Thus, explaining credit spreads falls more into the realm of asset-pricing than into the realm of corporate finance.

less external funding for their internal projects. Having their real projects thus already funded, a dollar worth of debt issuance would merely serve to increase equity distributions. Issuing more debt is thus relatively more attractive when firms need to fund their real projects and not just additional equity distributions. The model thus draws a link between firms' liquidity and leverage. (Hennessy and Whited (2007) find that equity costs of 5-10% and bankruptcy costs of 8-15% explain the data better; and DeAngelo et al. (2010) point out that firms can save cash instead of paying it out, in anticipation of potential future earnings shortfalls.) I will occasionally call these models the "funding model," because capital structure is linked to the payout/retention choices of firms. (Below, I will provide some basic empirical evidence on the funding model.) In Ju et al. (2005), firms behave much as they do in Leland (1994) and Leland and Toft (1996), but they can also manage their capital structure continuously. In addition, lenders can force a firm into bankruptcy when the firm value declines too much. Although the model does not have transaction costs, it focuses on the fact that the curvature of the objective function is modest. This implies that firms can sustain large deviations from their optimal ratios with only minimal costs. To keep my own paper show, I will not discuss these two strands in the detail they deserve.

Strebulaev (2007), the Brattle first-prize paper of 2007, adds a deeper structural basis with an empirical calibration to the TDF model. Firms face the standard tradeoff between the costs of debt (parameterized bankruptcy costs) and the benefits of debt (the tax deductibility of interest). In addition, there are transaction costs to readjusting. Strebulaev's main point is easiest to explain with an example. Consider a firm that has experienced a positive shock in profitability and that finds it too costly to pay down equity (e.g., due to a possible need of raising equity again in the future). The firm's market value increases, which lowers its leverage. With higher earnings, the firm also suffers from higher taxes. Thus, even though firms with higher taxes would optimally have higher leverage ratios, it ends up being the firms with the higher taxes that have lower leverage ratios. The model points out that slow adjustment due to transaction costs can explain [a] that firms do not seem to undo the effects of stock market return shocks; and [b] that firms with higher profitability have less debt, not more debt. Strebulaev (2007) thus shows that neither empirical finding necessarily rejected the TDF model. Quantitatively, with sufficiently high costs, readjustments can be rare. In his Table VII, he shows that only 346 out of 3,000 firms would adjust in a given year, small enough then that the tradeoff model can explain both empirical puzzles with a roughly appropriate order of magnitude. Quantitative prediction about the frequency of adjustment is a useful aspect in this context.

In sum, Ju et al. (2005), Hennessy and Whited (2005), Hennessy and Whited (2007), DeAngelo et al. (2010), Strebulaev (2007) Titman and Tsyplakov (2007) and other quantitative models can deliver good qualitative implications, e.g., similar to those in Fischer et al. (1989): Firms are slow to adjust their capital structures in the presence of even modest frictions, because the (no-friction) objective function for the tradeoff between the tax benefits and distress costs of debt is fairly flat. By providing quantitative calibrations, they give confidence that the TDF model can explain significant inertia in leverage ratios.

B The Tax-Distress-Friction Model in Varying Degrees of Complexity

Before critiquing the (quantitative) TDF model, I want to illustrate its three ingredients—taxes, transaction costs, and capital structure adjustment frictions—with varying degrees of complexity. According to Wikipedia, “In general usage, complexity tends to be used to characterize something with many parts in intricate arrangement.” In economics, one’s definition of complexity is somewhat subjective. In this paper, I label a model to be more complex if it has more input variables, and functionally more complex output forms; and, secondarily, if it seems intuitively less transparent to a first-time reader. (Complexity is *not* mathematical sophistication.) Of course, complexity has costs and benefits that demand a balance. Without some complexity, it is impossible highlight economic relations and to predict better.³

Model #1: The simplest model could be just a textual explanation:

A firm that has too little debt foregoes the tax deductibility of interest payments. A firm that has too much debt may go bankrupt. In the absence of friction, this can result in an optimum interior leverage ratio. If it is costly to change capital structure (e.g., due to the flotation costs of debt or equity) and if the objective function is relatively flat, then the firm may optimally choose not to adjust its capital structure.

This was effectively articulated by Myers (1993, page 587):

Large adjustment costs could possibly explain the observed wide variation in actual debt ratios, since firms would be forced into long excursions away from their initial debt ratios.... If adjustment costs are large, so that some firms take extended excursions away from their targets, then we ought to give less attention to refining our static tradeoff stories and relatively more to understanding what the adjustment costs are, why they are so important and how rational managers would respond to them.

Lack of mathematics may raise the danger of logical errors and make it more difficult to discover interesting relationships implied by the economics of the problem. It also offers no quantifiable guidance, even just over the envelope.

Model #2: A slightly more complex conceptual model could elaborate algebraically on the textual explanation. Assume a one-period firm with corporate tax rate τ and debt D that pays an interest rate of r . Relative to an identical firm with value V_{FE} that has

³Complexity also carries an intrinsic cost for the audience. It makes it more difficult to convey how economic forces work, even for the most gifted of writers. It makes it more difficult to replicate results and spot potential errors. It can limit the appeal of an economic argument to a smaller number of inductees. And it can make it more difficult to understand intuitively how a model would change if it were extended.

no debt and no possibility of financial distress, the value of the firm with debt would be $V_{FE} + \tau \cdot r \cdot D$. Now add a drastically reduced-form deadweight distress cost of debt, c , that increases quadratically with the firms debt-to-value ratio. To avoid irrational solutions, this distress-determining value ratio is divided by V_{FE} and not by V itself. The optimal leverage ratio is

$$\max_D V_{FE} + \tau \cdot r \cdot D - c \cdot (D/V_{FE})^2$$

The optimal debt-to-value ratio is thus $D^* = (r \cdot \tau \cdot V_{FE}) / (2 \cdot c)$. The comparative statics are simple and obvious: The firm should have more debt if the interest rate is higher ($\partial D^* / \partial r > 0$), if the corporate tax rate is higher ($\partial D^* / \partial \tau > 0$), and if the cost of distress is lower ($\partial D^* / \partial c > 0$). The value of the firm at the optimum leverage ratio is

$$V_{FE} + \frac{(r \cdot \tau \cdot V_{FE})^2}{4 \cdot c}$$

The difference between the value of a firm with optimal debt, D^* and actual debt D is thus

$$\Delta = \frac{(r \cdot \tau \cdot V_{FE}^2 - 2 \cdot c \cdot D)^2}{4 \cdot c \cdot V_{FE}^2}$$

Finally, assume that there is both a fixed and a variable cost of issuing or retiring debt, $a + b \cdot D$. The marginal firm which is indifferent between readjusting and no readjusting has a debt level that satisfies

$$\frac{(r \cdot \tau \cdot V_{FE}^2 - 2 \cdot c \cdot \bar{D})^2}{4 \cdot c \cdot V_{FE}^2} = a + b \cdot \bar{D}$$

$$\Rightarrow \bar{D} = \frac{(b + r \cdot \tau) \cdot V_{FE}^2 + V_{FE} \cdot \sqrt{4 \cdot a \cdot c + b \cdot (b + 2 \cdot r \cdot \tau) \cdot V_{FE}^2}}{2 \cdot c}$$

For example, if the financial distress cost parameter is 0.1, the cost of debt is 5%, the tax rate is 30%, and the value is normalized to \$1, then the optimal debt ratio is 7.5%. The value at the optimum is \$1.00056. With a fixed issuing cost $a = 0.01$ and a variable issuing cost of $b = 0.02$, the firm will retire debt only if its current debt ratio is above 52.86%. Vis-a-vis the textual approach, the ability of this simple model to provide numerical values means that it hints that the objective function could be fairly flat at the optimum for a wide variety of parameters and models that map into an quadratically increasing distress cost. Small deviations of the current capital structure from the optimum should not cause firms to become active.

Model #3: On its plus side, the implications of Model #2 are clear, easy to explain and directionally unambiguous. On its minus side, the model seems highly unrealistic. It had all uncertainty folded back into its ad-hoc reduced forms for the tax and distress costs. In Fischer et al. (1989), the model becomes more general. Most importantly, firms live forever, and can always delay taking on more debt today in favor of tomorrow. This means that

the problem becomes dynamic and considerably more difficult to solve, necessitating a large number of additional specific assumptions. The value of the firm follows a geometric Wiener process. Firms are taxed instantly, and issue equity to avoid financial distress cost or to make coupon payments. Firms can recapitalize at any point, and FHZ determine a critical upper and lower bound at which firms recapitalize (similar to that in an optimal inventory “(s,S)” policy problem). The optimal capital structure can be written as the solution to:

$$\begin{aligned} \max_{\underline{s}, \bar{s}, B, \delta, i} & V(s_0, B, \underline{s}, \bar{s}) - k \cdot B \\ V &= B \cdot (s_0 + k) && \text{no arbitrage} \\ E_y(s_0 = s) &\geq 0 && \text{limited liability} \\ D(s_0) &= B && \text{issue at par} \end{aligned}$$

where E is the market value of equity, D is the market value of debt with par value B , V is the value of the levered firm, s_0 is the ratio of the assets divided by the value of debt, \underline{s} and \bar{s} are the lower and upper bounds where the firm recapitalizes, k is the (proportional) cost of issuing debt, r is the instant coupon rate, and δ is a parameter summarizing the risk-free interest rate, the tax rate, and the expected value of the firm’s projects. The model does not have a closed form solution or closed-form comparative statics. (In fact, no known multi-period model with transaction costs has algebraic solutions, because the capital structure becomes path-dependent.) However, the model easily yields numerical comparative statics. FHZ assess the magnitude of their predictions by assuming a corporate tax rate of 50%, a personal tax rate of 35%, an instant variance of 0.05%, transaction costs of 1%, a riskfree rate of 2%, and bankruptcy costs of 5%. The model provides an optimal leverage ratio of 62%, and the no-adjustment range is from 29% to 175% (i.e., full equity). They then test the most straightforward qualitative implication of their model—that firms allow their capital ratios to take wide swings.

Model #4: The next step up in structure appears in Strebulaev (2007). He points out that “the benefit of having a more realistic model is that it allows for the assessment of the magnitude of economic effects.” Unfortunately, the model’s complexity makes it difficult to summarize here, but it is useful to give a flavor of its complexity.

The model has two dozen parameters, such as the present value of all future net payouts at time 0 (V_0), the initial book value of firm assets (A_0), the systematic risk of the firm’s assets (β), the volatility of monthly market returns (σ_E), the volatility of monthly 10-year T-bills (σ_D), the covariance between equity and debt returns (σ_{ED}), the average leverage (L_{av}), the volatility of idiosyncratic shocks (σ_I), the volatility of the project’s net cash flow (σ), the proportional costs incurred in selling assets q_A , the proportional adjustment costs of issuing/retiring debt q_{RC} , the proportional direct costs of external equity financing (q_E), the proportional restructuring costs (α), the fraction of assets that remains after an asset sale (k), the partial loss-offset boundary (κ), the growth rate of book assets (g), a

shift parameter in the net payout ratio estimation (a), the asset risk premium (RP_A), the loss per dollar of full offset in the case of distress (τ_K), the marginal corporate tax rate (τ_C), the marginal personal tax rate on dividends (τ_d), the marginal personal tax rate on interest income (τ_i), and an instantaneous after-tax riskless rate (r). The model can only be numerically solved. The optimization is

$$\begin{aligned}
c^* &= \arg \max_{c, \gamma_U, \gamma_{LU} \in \mathbb{R}_+^3} \frac{E^R(\delta_0) + (1 - q_R C) \cdot D(\delta_0)}{1 - \gamma_U \mathcal{E}_{\delta_0}[e^{-rT_U} | \phi_L(U) = 0] - k \gamma_{LU} \mathcal{E}[e^{-rT_{LU}} | \phi_B(LU) = 0]} \\
D(\delta_0) &= D^R(\delta_0) + \mathcal{E}_{\delta_0}[e^{-rT_U} D_0 | \phi_L^U = 0] + \mathcal{E}_{\delta_0}[e^{-rT_U} w D_0 | \phi_B^{LU} = 0] \\
\frac{\delta}{V_t} &= a + (1 - \tau_C) \cdot \frac{c}{V_0} \\
\frac{\partial E(\delta_t)}{\partial \delta_t} \Big|_{\delta_t = \delta_B} &= 0 \\
q(x) &= \begin{cases} x & \text{if } k\delta_s > wc \\ (1 + q_E)x & q_E > 0, \text{ otherwise} \end{cases} \\
E^D(\delta_0) &= E^R(\delta_0) + \mathcal{E}_{\delta_0}[e^{-rT_U} \gamma_U E^D(\delta_0) | \phi_L^U = 0] + \mathcal{E}_{\delta_0}[e^{-rT_{LU}} \gamma_{LU} k E^D(\delta_0) | \phi_B^{LU} = 0] \\
D^D(\delta_0) &= D(\delta_0) + \mathcal{E}_{\delta_0}[e^{-rT_U} \gamma_U D^D(\delta_0) | \phi_L^U = 0] + \mathcal{E}_{\delta_0}[e^{-rT_{LU}} \gamma_{LU} k D^D(\delta_0) | \phi_B^{LU} = 0] \\
E^R(\delta_0) &= \mathcal{E}_{\delta_0} \left[\int_0^{T'} e^{-rs} (1 - \tau) (\delta_s - c) ds \right] \\
&\quad + \mathcal{E}_{\delta_0} \left[\int_{T_L}^{T''} e^{-rs} q((1 - \tau)(k\delta_s - wc) - \tau_l w c \mathbf{1}_{\delta_s < \delta_t}) ds \right] \\
&\quad + \mathcal{E}_{\delta_0} \left[e^{-rT_B} \max \left[(1 - \alpha) \int_{T_B}^{\infty} e^{-rs} k(1 - \tau) \delta_s ds - w D_0, 0 \right] \Big| \phi_{LU}^B = 0 \right] \\
D^R(\delta_0) &= \mathcal{E}_{\delta_0} \left[\int_0^{T'} e^{-rs} (1 - \tau_i) c ds \right] \\
&\quad + \mathcal{E}_{\delta_0} \left[e^{-rT_L} | \phi_U^L = 0 \right] (1 - w) D_0 + \mathcal{E}_{\delta_0} \left[\int_{T_L}^{T''} e^{-rs} (1 - \tau_i) w c ds \right] \\
&\quad + \mathcal{E}_{\delta_0} \left[e^{-rT_B} \min \left[(1 - \alpha) \int_{T_B}^{\infty} e^{-rs} k(1 - \tau) \delta_s ds, w D_0 \right] \Big| \phi_{LU}^B = 0 \right]
\end{aligned}$$

where γ_U and γ_{LU} are the proportions by which the net payout increases between two refinancing points if the liquidity barrier has or has not been hit; R stands for one refinancing cycle, $T' = \min(T_L, T_U)$, and $T'' = \min(T_B, T_{LU})$, and ϕ_i^j is zero if event j occurs before event i , and one otherwise. The complexity paired with the (probable) flatness of the (sans-friction) objective function also makes it difficult to solve the model even numerically and thus to replicate the results.

Simulations of this model show that there can be stark differences between cross-sectional regressions run across firms when they are making large changes to their capital structures (they do not make small changes), and regressions run across all firms in any given year. (The time of the readjustment is not empirically calibrated, although the model has a specific prediction on it.) The former, conditioned only on activity, would show the normal tax-distress tradeoffs—firms with higher profitability and thus taxes and lower distress costs choose more debt—while the latter could show the opposite.

C Comparisons

In itself, the fact that we can model the TDF model on a more micro level, though at the cost of more specificity and complexity, should not move our priors towards the belief that the model is true or false. The four models are intrinsically very similar. On a qualitative basis, they all deliver the basic prediction that taxes push firms towards debt, financial distress costs towards equity, and transaction costs towards inertia. This is not surprising. After all, the TDF model was designed to explain the empirically observed fact that firms did not adjust their leverage ratios very actively—a prediction that the model delivers with appealing and simple intuition.

All three algebraic models make it obvious that the tax-distress tradeoff may be so flat that firms should not be very active in readjusting their capital structures—and thus that the speed of readjustment of capital structure to shocks could be close to zero. Under certain parameters, which turn out to be plausible in many situations, firms' optimal behavior is therefore similar to that under managerial neglect. The implication that frictions can induce inertia, and that inertia can generate non-readjustment and an inverse correlation between profitability and leverage ratios, are generated in all three models. Under the assumption that the model choices are correct, all three algebraic models can predict this *quantitatively*, too. But although it is not uncommon for conceptual models to provide some numerical examples to illustrate whether an effect is small or large, few would take their quantitative predictions seriously enough to test their point estimates on empirical data. The shortcomings of the simple models are too blatantly obvious. It is only the Strebulaev (2007) Model #4 that emphasizes its quantitative predictions.

The ambition of the structural models to predict quantitatively is not in itself a drawback, but an advantage. They are more easily falsifiable than conceptual models. Of course, falsification should be interpreted in quantitative terms, too. A model may provide a reasonably good description of the data, even if it is rejected statistically. No model is ever exact.

D Are Structural Models More Realistic?

Conceptual models, such as Model #2, often make their strong assumptions about functional relationships close to the phenomenon that is being explained. They justify this by viewing them as approximations of reduced-form relationships. Such an approach can often keep the model very simple. In contrast, structural models, such as Model #4, make their strong assumptions about deeper micro-foundations.⁴ The assumptions are often chosen, in part, for their tractability and can be quite specific. In the capital structure context, this can include assumptions about functional forms, cash flow processes, future taxes, dilution effects, residual homogeneity, how stakeholders, customer, and suppliers respond to financial distress, allowed financing and refinancing choices, and so on. Absent *a priori* knowledge, these assumptions are just as ad-hoc as the reduced form assumptions. Eventually, structural micro-foundations aggregate up to their own reduced forms, which may or may not be in closed form. Many structural models require quantitative testing, because qualitative tests are impossible when comparative statics cannot be signed.

Although no comparative test has been performed, it is plausible that the Strebulaev model could deliver more accurate point predictions than the simpler models. With its specificity in structural assumptions, Model #4 seems to communicate that it is more realistic than Model #3. Model #2 is outright “flat”: most firms will exist longer than 1 period, that they can change capital structure often, that the cost of distress is not quadratic, etc. If the micro assumptions of Model #4 are correct, then it should be more stable in predicting (especially with respect to policy responses, i.e., “counterfactuals”) Yet it is not *a priori* clear that the reduced forms delivered by Model #4 are more accurate than those delivered by Model #2. It is possible that economic forces are better captured by the simple reduced-form quadratic cost than by the structural process. To the extent that Model #4’s micro foundations are misspecified and/or that Model #4 omits some forces that counter-balanced the very specific forces it develops in detail, the simple reduced-form Model #2 could actually have the more empirically appropriate and stable reduced form. It is a folk theorem in econometrics that simple econometric models often predict better than complex ones. It is possible that the same could hold true for models of capital structure.

⁴Structural modeling grew after Lucas (1976) critiqued the instability of extreme reduced form models for policy purposes. There is now universal agreement about the sickness—that reduced form models can be unstable. But ironically, it is not clear whether the proposed cure has worked. Few structural models have actually been tested empirically for their ability to provide more accurate and stable descriptions in the presence of policy changes—which was after all their *raison d’être*.

II Does The Theory Make Sense (More Than Alternatives)?

The rest of my paper will assess how complex quantitative structural models (like Model #4) perform on the four hurdles mentioned in the introduction, especially compared to simpler reduced-form qualitative models (like Model #2).

The first hurdle that a model should pass is the plausibility test. This inevitably requires some subjectivity in judgment. For example, for some economists, a reasonable model requires essentially rational behavior by its participants. For others, it merely requires behavior that does not create first-order losses in situations in which agents can easily learn their mistakes.

Most economists, including myself, would agree that the basic TDF theory, and indeed all of the existing DTT models, easily pass the plausibility test. They are based on simple, plausible forces. There could be disagreement whether the structural derived forms or simply assumed reduced-forms are more plausible.

However, it is only in the absence of alternatives that we can accept a theory at face value as the best explanation of the evidence. An important question then is whether the model is more plausible *a priori* than alternatives.

The TDF model was designed to offer two primary predictions. First, firms are inert. In turn, inertia means non-readjustment and a negative association between profitability and leverage. (There is strong empirical evidence that firms do not show much readjustment in response to shocks.) Second, when firms do adjust, taxes push them towards debt; distress costs towards equity. (The empirical evidence for these implications is weak.) The two predictions are almost separable. The friction aspect delivers the inertia implication, the tax-distress tradeoff delivers the conditional behavior explanation.

A Alternatives

The question then is whether there are models without at least one of the three base ingredients that can deliver similar implications. I would argue that this is so. TDF is *not* the only reasonable theory that survives the plausibility criterion. There is a wide range of explanations put forth in the literature that could help predict flat objective functions even without all the ingredients of the TDF theory. These could explain strong non-readjustment evidence and weak or spurious tax-distress evidence. For example:

- Could equity changes be influenced by the need to grant employee stock options and conduct acquisitions (Fama and French (2002))?
- Could firms imitate their industry peers (Roberts and Leary (2009))?
- Could the operations of the firm and industry, including their pension liabilities, play a role (Shivdasani and Stefanescu (2010))?

- Could the identity of managers and advisors matter (Bertrand and Schoar (2003))?
- Could credit ratings play a direct role (Kisgen (2006))?
- Could hubris play a role (Roll (1986))?
- Could (the belief in) market-timing play a role (Baker and Wurgler (2002))?
- Could covenant violations and conflicts of interest play a role (Roberts and Sufi (2009))?
- Could unmitigated agency concerns induce managers to prefer equity-heavy capital structures?
- Could precommitments (such as sinking funds) play an important role?
- Could managers need to assure creditors against risk-shifting (Parrino and Weisbach (1999))?
- Could managers avoid Myers' adverse selection problem? Some TDF models have mentioned but not competitively tested the pecking-order theory (which substitutes extreme adverse selection for the TDF model's tax and friction ingredients) as possible reason for non-readjustment.⁵
- Could managers readjust their capital structures *non-optimally* by following a rule, in which they pay little consideration to bankruptcy costs and tax benefits and possibly to transaction costs,
 - but simply readjust when their capital structure has become too different from that of their peers;
 - but use heuristics to decide on a reasonable band of leverage ratios within which they allow their capital structure to fluctuate;
 - but change capital structure only when they acquire other firms;
 - but be "asleep at the switch," and readjust whenever they wake up;
 - but enact random changes if they can afford to;
 - but like lower leverage ratios, so firms with poor governance would not ratchet up leverage ratios.

This list of possibly important influences is of course not exhaustive. Even if the TDF model could fit the data, the competitive benchmark needs to be whether it can fit observed leverage ratios better than alternatives, also proposed in the literature. That is, we need to design empirical tests to distinguish between explanations. In reality, there may even be multiple influences, influencing different firms differently. As far as I am aware, no tests of *quantitative* structural models have tried to control for or include the above forces. In

⁵Shyam-Sunder and Myers (1999) have entertained the pecking order theory as an embedded competitor—and promptly found it to perform as well as the TDF model. Other reduced-form tests have also tested multiple models competitively. For example, Fama and French (2002) test the tradeoff model against the pecking order model; and Huang and Ritter (2009) test the static tradeoff model, a pecking-order model, and the market timing model competitively.

contrast, most tests of *qualitative* reduced-form capital structure models include batteries of variables related to other possible influences, admittedly often ad-hoc, and often including fixed effects or difference specifications.

This is not to argue that the TDF model should explain all correlations. But the goal of the *quantitative* versions of the TDF model is to explain the magnitude of corporate leverage ratios. It attributes leverage ratio changes caused by *any* effect to its own inputs. This means that the model must either contain these other influences in its parameters (and stably so), or that the other forces must be of second-order importance vis-a-vis its tax-distress forces. Yet it seems *a priori* unlikely that the TDF model implicitly contains the most important economic forces and neglects only those of lesser importance.

Importantly, the fact that the quantitative model is based on micro-foundations does not in itself make the model more stable. A model omitting important factors would still suffer the full brunt of Lucas' critique. If the TDF model describes just some of the forces, holding the other relevant forces constant in its structural design remains essential. Correlation between the TDF model's influences and the above influences could produce Type-1 error, where the model seems to predict well, but the causes are *not* taxes, distress costs, and friction; and its predictions are not stable.⁶

B How To Choose Theories and Approach?

The presence of many potential forces and alternative models raises the question how one chooses among them. In the context of structural models, the question is how to decide on the specific micro foundations. I contend that when the economic application is simpler, i.e., when it is more likely that a researcher knows *a priori* all important structural underpinnings (not just some), the quantitative approach is more appealing, because the structural specification is more likely to be *a priori* correct.⁷ Moreover, the quantitative structural model then adds relatively more to our understanding of otherwise already well-understood qualitative behavior. Conversely, if the economic application is itself complex, and we do not know *a priori* the most important forces, it is the qualitative model that seems more appealing.

Given the large number of alternatives, given only modest *a priori* knowledge of which forces are first-order and which forces are second-order, and given only modest *a priori* knowledge of the specific functional micro-foundations, my priors are that no structural

⁶An analogy may be a "traffic accident theory" of life expectancy. Yes, traffic accidents play a role in reducing life expectancy (and they correlate with age and disease), but accidents are *not* the most important input into life expectancy—and although extensions and generalizations of the theory will be able to fit some moments better, ultimately this theory will not explain the evidence. The fact is that traffic accidents are not the primary, much less the only important cause of death.

⁷A special case in asset pricing often arises when behavioral errors can be arbitrated by third parties. Then, even complex behavior can often be characterized by much simpler optimization approaches. However, this is not the case in the capital structure arena, where incorrect capital structure choices cannot easily be arbitrated.

model is likely to predict well in the capital structure context specifically and in corporate finance generally. Moreover, it is a practical drawback of micro-founded quantitative modeling that it is more difficult to incorporate the multitude of plausible competitive theories simultaneously to permit competitive testing.

C What is the Null?

This raises the question whether any DTT model is itself the null hypothesis (that needs to be rejected with 95% confidence) or whether there is another null hypothesis (that needs to be rejected by the DTT theory with 95% confidence). Attributing null status to any theory is in effect “half assuming” the model, rather than testing it. Although it is not necessarily true that a researcher should use the simplest theory as the null, it seems reasonable to suggest it. The TDF theory is *not* simpler than a number of the competitive theories mentioned above. Consequently, it is not at all clear that it deserves null status.

The view that a quantitative structural model (which maintains to have covered all first-order influences) cannot be rejected by the empirical evidence can easily lead to conclusions that seem too strong. For example, Hennessy (2004) observes that

Incorporating debt in a dynamic real options framework, we show that underinvestment stems from truncation of equity’s horizon at default.

Underinvestment is presented as a fact. There are no plausible alternative explanations put forth, there is no out-of-sample prediction, and no quasi experiment to test for causality. For another example, Li, Livdan and Zhang (2009), the lead article in the November 2009 issue of the *Review of Financial Studies*, begins both its abstract and introduction with the statement

We take a simple q-model and ask how well it can explain external financing anomalies both qualitatively and quantitatively. Our central insight is that optimal investment is an important driving force of these anomalies.

LLZ reach this conclusion based on the fact that the model can be calibrated in-sample to some (but not all⁸) of the empirical moments in the data. They conduct no out-of-sample tests and do not consider the hypotheses put forth in earlier literature (i.e., non-rational behavior) as the null hypothesis that their own model needs to reject with 95% confidence. Instead the burden of proof is pushed onto alternatives to their model, which have not been considered. Their model has in effect usurped the null hypothesis.

⁸The conclusion is a logical error. If theory A makes two predictions, B and C, it is wrong to credit it as a success when we observe “B” but “not C.” The correct conclusion is “not A.” (The Ptolemaic geocentric theory could explain some but not all moments, too.) Note that this logical inference issue is different from the fact that if a model suffers noise, it may fit all the empirical moments only moderately well.

III Does the Theory fit the Data In-Sample?

A Other Misspecification

The omitted economic forces mentioned in the previous section are only one of many misspecification issues that empirical tests have to be concerned about. All models are subject to them. After all, a model is only a model. Capital structure models must also face the almost inevitable presence of such problems (from the perspective of the theory) as:

- whether proxies have errors-in-variables;
- whether there is residual heterogeneity across firms;
- whether there is residual auto-correlation;
- whether there are selection biases, how they are correlated with variables of the theory, and what their effects are;
- whether the data has been overfitted with too many degrees of freedom;

and so on. As with the potentially important omitted-variables misspecification problem discussed in the previous section, empirical tests of the theory must be capable of recognizing whether these issues are important, and they must potentially correct for them. Merely assuming that a specification is correct is logically identical to assuming by acclamation that the model is correct. This can be appropriate for a theory, but not for an empirical test.

First, consider tests of the qualitative reduced-form versions of the TDF model. These are typically tests of comparative statics explaining leverage ratios (or ranges) in regressions. There are standard techniques to assess and correct for these misspecification issues (even if doing so remains difficult).

- Other economic theories can readily be included if proxy variables for their effects exist. They simply become control variables in the regression. If there are economic forces omitted from the model that are stable over time for a given firm, then differencing and fixed effects can aid in controlling for their contaminating influences.
- The corporate tax rate, distress costs, and frictions are almost surely measured with error. This reduces the power (coefficient) of tests predicting leverage ratios, but usually leaves estimated coefficients with the same sign. Grouping can reduce proxy measurement errors.
- Firms are known to be heterogeneous in size. Thus, regressions can be run using WLS instead of OLS; standard errors can be adjusted via the White-Hansen method; or one could use size or size category interaction terms.

- Firms are likely to have similar leverage ratios, tax brackets, and distress costs over multiple years, which could possibly also cause residual auto-correlation. The standard errors can be adjusted via the Newey method.
- About one in ten firms drops out of CRSP/Compustat every year. Other data restrictions can further limit a sample. For example, in Hennessy and Whited (2005), firms with low assets, capital stocks, or sales are deleted. Their sample is 592 to 1128 firms per year, which leaves only about 20% of the total population. Selection issues can be handled via Hausman tests and Heckman corrections.
- Researchers have looked at the data many times. White and others have developed techniques to assess and correct data-snooping biases, the result of overfitting.

Not only is it possible in principle to address many of these issues, it is also common in practice for empirical papers testing qualitative model predictions to do so. That is, most papers acknowledge explicitly that they require misspecification tests and corrections, and usually include a battery of ad-hoc variables and robustness tests that are not suggested by the theory itself.

Now, contrast these “marginal coefficient” tests of qualitative reduced-form versions of the TDF model to the more exacting prediction tests of its quantitative versions. The latter are more ambitious, predicting not just directional effects but precise estimates. Misspecification can then change the inference on the underlying structural parameters. For example, errors-in-variables usually reduce the implied effect. Even when the error-in-variables problem does not change the qualitative direction of the quantitative model’s prediction, it does change the inference about structural parameters that need to be backed out. Thus, even if the quantitative model still retains the same qualitative implications as its simpler counterpart, misspecification can nullify its quantitative prediction advantage. Quantitative tests are thus relatively more sensitive to misspecification. This makes it even more important to assess and correct for misspecification.

However, this is difficult—and it is certainly not common. There is no standard econometric toolbox for non-linear quantitative model tests. Besides, the ad-hoc nature of misspecification assessments and corrections is at odds with the appealing micro-foundations of many quantitative models that assume that they can explain how firms behave. That is, ad-hoc control for other forces seems more appealing within the context of a more ad-hoc reduced-form model. The alternative, incorporating the sources of misspecification into the quantitative model itself (“going back to square one”), would complexify many of them to the point where they would become incomprehensible and untractable.⁹

⁹One ad-hoc way to diagnose misspecification issues would be to analyze the residuals of the quantitative model in a second step in a linear regression. For example, one can test whether there are still firm-fixed effects in them. The drawback is that this still treats the quantitative TDF model as the null hypothesis. Common explanatory power is attributed to the TDF model. A more stringent test would first purge leverage ratios of firm fixed effects, and then see if the model can explain the time-variation in purged leverage ratios. The drawback is that this treats the firm-fixed effects as the null hypothesis. Neither is appealing.

B Explained Moments

Quantitative predictions take a model more literally than qualitative predictions. If the model's structural parameters are not known, researchers often lean hard on the model itself: Under the assumption that the model is true, a maximum-likelihood or matching-moments calibration allows one to back out an estimate of the distress costs that is consistent with the model. Obviously, although calibrations are based on empirical data, they are *not* tests of the model. Tests then have to focus on other moments, given the calibrated moments.

It is not the case that the TDF theory is still only an empirically calibrated theory without empirical evidence to support it. The TDF theory can fit some empirical moments that were not calibrated. It can explain some qualitative evidence about leverage ranges (Fischer et al. (1989)). It can also explain some of the evidence in Welch (2004) (non-readjustment). Thus, in its defense, the TDF model has made predictions about some empirical moments that have matched the data.

C Degrees of Freedom

Degrees of freedom can be added or subtracted via explicit parameters or the choice of a model's particular functional form. With enough degrees of freedom, it is possible to engineer models that explain almost everything. But a model that can explain everything is not a model—it is a tautology. It can explain the evidence, or the opposite. A model whose predictions are too sharp is by definition wrong. A good model has reasonably specific and robust predictions. To the extent that in-sample tests are used to assess the model's performance, the model should fit a number of empirical moments which is much larger than the number of parameters.

Micro-founded structural versions of the TDF theory require a large number of parameters. For example, Strebulaev (2007) has 23 parameters used to explain fewer than a handful of empirical moments. Importantly, in its defense, the parameters are not really free, because the author chose plausible values *a priori* for many of them.¹⁰ He shows that his version is robust with respect to changes of individual parameters, one at a time.¹¹ This is because the model has multiple parameters that render capital structure readjustments costly. Removing any single friction leaves enough other frictions to provide the same slow adjustment implication. To change the slow-adjustment prediction in his version of the model would require a constellation of multiple parameter changes. In fact, logically,

¹⁰There is some latitude in the (multiple) choices of parameters, though. Some of the parameters are chosen based on prior research, which in turn may have been based on similar data and prior research. Therefore, the parameters are not exactly perfectly fixed, either. Nevertheless, it is not likely that the TDF model explains slow-adjustment because it has too many free parameters. It explains slow adjustment because the model was *designed* to explain it.

¹¹In Table VIII, Strebulaev reports profitability-leverage coefficients between -0.29 and -1.32, and adjustment coefficients between 0.01 and -0.05. (I am quoting them as one minus the value in Welch (2004) regressions.)

if the combined cost of readjustment approaches zero, firms must adjust instantly and more profitable firms should have more (not less) leverage. It is the choice of specific input parameters that gives the TDF model its specificity. Otherwise, the model can explain adjustment just as well as it can explain non-readjustment, or any degree of adjustment in between.¹² Fortunately, empirically reasonable parameters can predict reasonable non-readjustment. The TDF theory is appealing in this respect. It provides clear predictions with just a few degrees of freedom. It is both specific in its predictions, and robust with respect to alterations. The Hennessy-Whited funding model is similarly appealing.¹³

D In-Sample Evidence Rejecting TDF and Funding as First-Order Determinants

Yet, the main problems with the existing DTT models is not the fact that they are not plausible, that they explain too few moments given their degrees of freedom, that they cannot explain some moments in the data (they do!), and maybe not even that they do not conduct any misspecification tests. Instead, it is that there is prima-facie evidence suggesting that they cannot fit the most important moments that are implied by their basic premises.

D.1 The Tax-Distress-Friction Hypothesis

The TDF model predicts first and foremost that firms are inactive most of the time. For example, in his Table VII, Strebulaev suggests that firms should be active about one in eight firm-years. Predicted non-readjustment is a derived consequence of predicted non-activity. In this section, I will show that firms are typically not inactive, but active. Consequently, the TDF theory fails. Consequently, it cannot explain non-readjustment.

My test focuses on net changes in capital structure. It is a weak test, fraught with Type-1 error. If firms issue and retire debt simultaneously (which is common, as shown by Rauh and Sufi (2010)), and/or issue and retire equity simultaneously, then the net debt and net equity activity could both be zero, even if firms are very active. Similarly, if firms issue and retire debt and equity in proportion to the current capital structure, we could again see no changes in capital structure, even if firms are very active. Net capital structure changes thus understate firms' real capital structure activities.¹⁴

¹²In my opinion, overfitting is not as much an aspect of the current incarnations of the TDF model, but a concern if the model will be "extended." For example, the model is known not to predict well conditionally (i.e., the behavior of firms that start with zero leverage ratios). Grafting on additional forces will be more appealing only if the model then explained not only the moment that it was extended for, but also additional novel moments.

¹³Moreover, both models may also provide predictions on variables other than capital structure that can help to test the model. These may limit further simplification. My own paper is focused on the capital structure literature, however, where the main question is the prediction of leverage ratios with different variables.

¹⁴Although this test of the TDF theory is about any issuing or repurchasing activity, capital structure theory more generally is about the final leverage ratio. It is not about issuing activity or repurchasing activity or

My empirical sample are all firm-years from Compustat from 1963 to 2007. I exclude firm-years with lagged debt-to capital ratios of less than 1%. There is a large population of such firms that are highly profitable and almost never take on debt. (It is not clear whether their presence favors or rejects the TDF theory.) The leaves 107,361 firm-years with non-zero starting leverage.

I decompose changes in the firm's debt-to-capital ratio into passive changes caused by stock returns, active changes caused by managerial intervention, and total changes caused by both:

$$\begin{aligned}
 dcp_{t-1,t} &= \frac{D_{t-1}}{D_{t-1} + E_{t-1} \cdot (1 + x_{t-1,t})} - \frac{D_{t-1}}{D_{t-1} + E_{t-1}}, \\
 dca_{t-1,t} &= \frac{D_t}{D_t + E_t} - \frac{D_{t-1}}{D_{t-1} + E_{t-1} \cdot (1 + x_{t-1,t})}, \\
 dct_{t-1,t} &= \frac{D_t}{D_t + E_t} - \frac{D_{t-1}}{D_{t-1} + E_{t-1}},
 \end{aligned} \tag{1}$$

so that $dct = dcp + dca$. dca can be measured counting dividends as discretionary or non-discretionary. It makes no difference to any of the results, so we mostly report dca , which is based on the capital gain (x) instead of the stock return (r). The pooled distribution of these variables in the sample is

	Min	Median	Unwinsorized			Winsorized at 0.5	
			Max	Mean	Sdv	Mean	Sdv
dct	-0.89	0.00	0.94	0.0115	0.129	0.0113	0.127
dcp	-0.60	-0.00	0.79	0.0017	0.093	0.0016	0.093
dca (excl. divs)	-0.94	0.00	0.94	0.0097	0.087	0.0098	0.085
dca ⁺ (with divs)	-0.94	0.00	0.94	0.0126	0.087	0.0126	0.085

The relative sizes of standard deviations of the three measures do not favor the TDF theory. The standard deviation of active changes (dca) is about the same as the standard deviation of stock-return induced shocks (dcp), but managers do not use the former to counteract the latter. There seems to be almost no correlation between shocks caused by stock returns and managerial activity. Consequently, the standard deviation of total debt-to-capital changes (dct) is about $\sqrt{2}$ as high as the standard deviation of stock-return caused shocks.

The remainder of this section investigates dca , which is the variable that is completely under the control of management. (dct is a far noisier Qmeasure, whose dcp component [i.e., the part that is not due to dca] is due to stock market returns and not under the full control of management.¹⁵)

Under the one-in-eight-years activity presumption, the center of a histogram of dca should contain about eight times more mass than the entire rest of the histogram. There debt activity or equity activity. Thus, the net change in capital structure may well be the most important

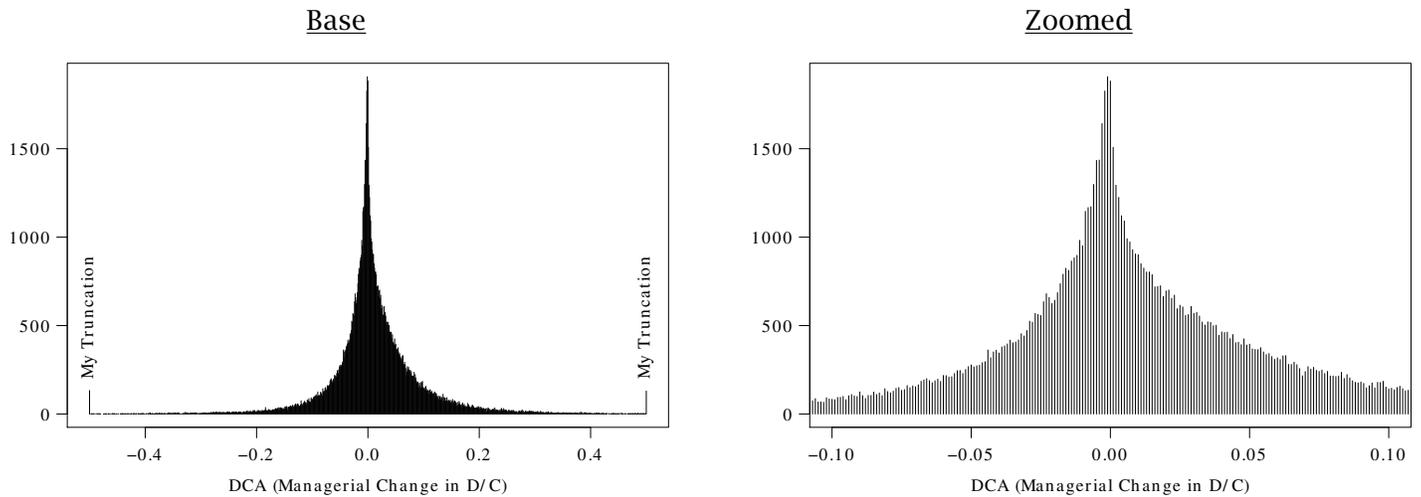


Figure 1: Histogram of Managerial Changes To Debt-To-Capital Ratios (*dca*)

should be little density to the immediate left and right of center. The remaining 15% that are not at zero should be density that is far off center.

The histograms in Figure 1 show that although *dca* does not follow a normal distribution—and, in fact, it cannot, because it is a difference of two ratios—it also does not show evidence of much inertia (exactly zero managerial activity). Managers engage in plenty of modest and not so modest capital structure changes. There is not even suggestive evidence that exact 0% inertia (-0.05% to $+0.05\%$) is more common than changes of around -0.1% , 0.1% , 0.2% , and so on. The center does not contain probability mass close to the 80-90% that the TDF theory relies on to explain non-readjustment. Moreover, there are no other steep declines in probability mass off the zero center, nor are there farther-out increases in probability mass. Changes between, say 3% and 10% are quite common.

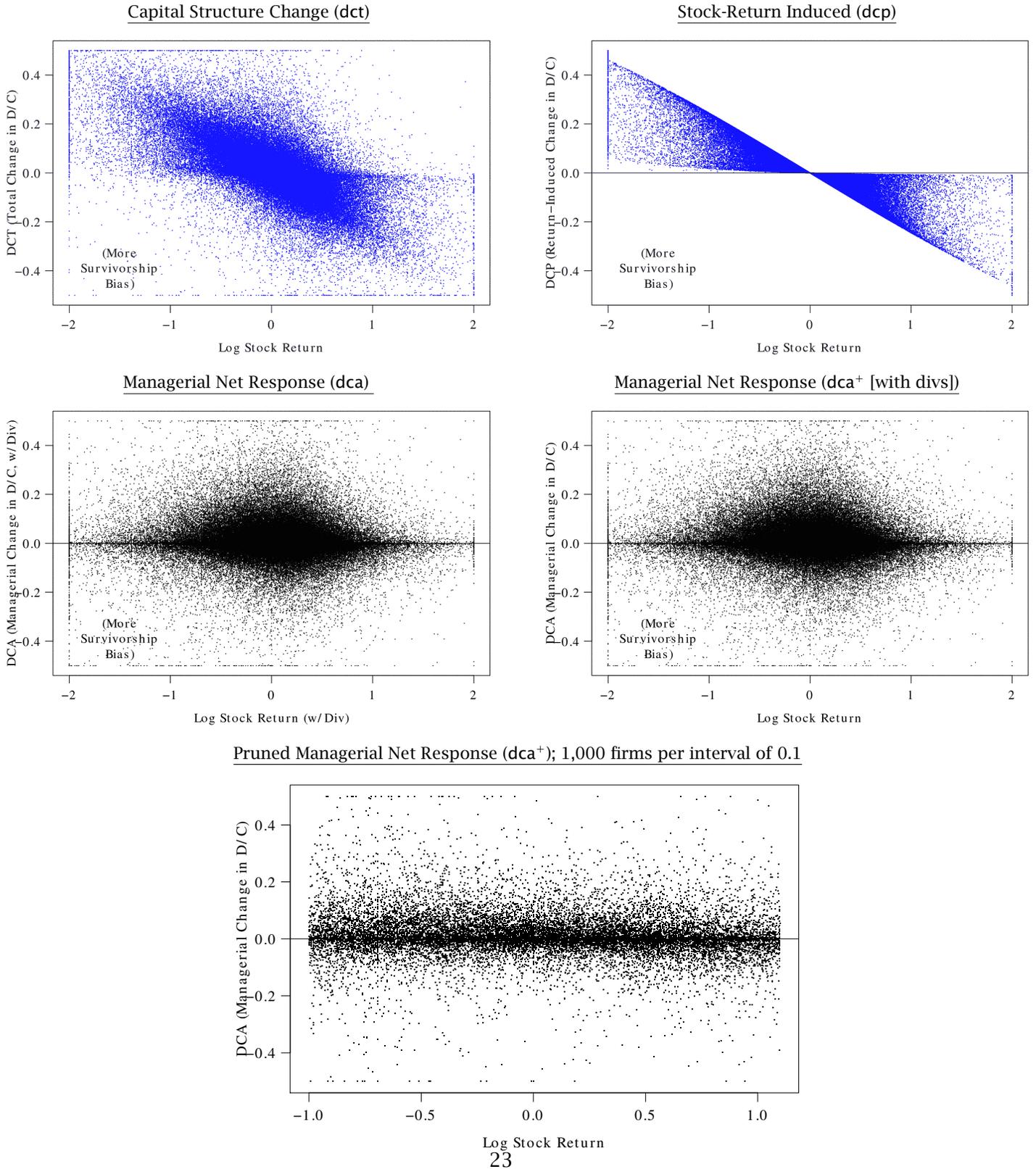
In sum, the presumption that firms cannot readjust their capital structures because it is too expensive, and that they therefore choose optimally to remain inactive, is not supported by the empirical evidence. Firms frequently intervene in their capital structures, and could use such occasions as opportunities to rebalance shocks at the same time, if they so desired.

Next, Figure 2 identifies the managerial response to shocks caused by stock returns. Each dot is one firm-year.

moment in the capital structure literature. It is just not the most powerful test for testing this particular inertia prediction of the TDF theory. Fortunately, it is more than powerful enough.

¹⁵I would recommend *dca*, and not *dct*, as dependent variable in empirical work whenever the empirical question is about *managerial* capital structure activity. The advantages are, first, that it reduces noise; and second, that it reduces spurious correlation induced by the presence of stock return anomalies (such as the book-to-market effect), which are not under the active control of management. The disadvantage is that it assumes that firms do not manage their capital structures by managing their company's stock returns. This is often but not always reasonable.

Figure 2: Changes Of Debt-To-Capital Ratios As Function of Stock Returns



Capital structure variables are defined on Page 21. Capital structure changes are winsorized at -0.5 and +0.5. Log stock returns are winsorized at -2 and +2.

- The top left figure plots the total change in firms' capital structures (dct). It shows that firms with higher stock returns end up with lower debt-to-capital ratios.
- The top right figure plots the purely stock-return induced change in capital structure (dcp), absent managerial capital structure adjustment.

Comparing these figures suggests that stock returns can account for most of the *average* changes in leverage ratios from year to year. The average slope similarity is again evidence for lack of readjustment. However, the visual differences between the two figures suggests that managers are not idle. The two middle figures plots this difference, i.e., the managerial responses (dca) to stock returns in the same year.¹⁶ The bottom figure leaves only 1,000 random firm-years in each 10% return interval to make the center visually easier to compare with the edges. The left figure considers dividends to be under the discretion of the manager (it is not part of the return in the computation of dca), the right figure does not.

The dca figures show that managers do not readjust: The distribution of active changes in capital structure is centered at around zero, regardless of shock. Firms are as likely to undertake actions to increase their leverage ratios as they are likely to undertake actions to decrease their leverage ratios. The fact that the *average* activity is zero is non-readjustment (Welch (2004)). This mean could indeed be explained by the TDF theory if we saw very little activity surrounding it. It is the considerable managerial activity (the variance) around zero that *cannot* be explained by the TDF theory.

- Even firms experiencing almost no capital structure shocks, i.e., whose stock returns are close to zero, are very capital-structure active.
- Firms experiencing negative shocks (that increase their debt-to-capital ratios) do not appear to use their capital structure activities systematically to counterbalance them. Almost as many such firms seem to undertake managerial activities that further increase their debt-to-capital ratios as there are firms that decrease them again.¹⁷
- Firms experiencing positive shocks do not appear to use their capital structure activities systematically to counterbalance them, either. Just as many such firms seem to undertake managerial activities that further decrease their debt-to-capital ratios as there are firms that decrease them again.

There is no evidence for probability mass at the bottom left or top right, suggesting readjustment responses when shocks have been very strong, either. Consequently, the empirically observed year-to-year changes in capital structures are *not* primarily due to

¹⁶If we plotted the following year's response, the figures would look similar. Also, note that firms experiencing an extreme stock return may not experience a large capital structure change if they had very little debt in their capital structures to begin with.

¹⁷This is only approximately true. A regression would show a mild average counterbalancing response for firms with strong negative stock returns (Roberts and Leary (2009)). However, this average response is dwarfed by the variance.

passivity. They are as much due to stock return shocks as they are due to non-readjusting managerial activities.

D.2 The Funding Hypothesis

The Hennessy-Whited funding hypothesis is based on observed productivity shocks. Unlike the frictions theory, which offers a “smoking gun” test (of common non-activity), the funding theory is difficult to reject. It is always possible that there are systematically correlated differences in unobservable productivity. Yet, the proof of burden is on the theory itself to show first that productivity differences systematically affect its inferences and second that it makes empirically useful predictions.

With this caveat, as empirical tests of the funding-capital structure link hypothesis, Hennessy and Whited (2005, page 1131) suggest the following:

We highlight the main empirical implications. First, absent any invocation of market timing or adverse selection premia, the model generates a negative relationship between leverage and lagged measures of liquidity, consistent with the evidence in Titman and Wessels (1988), Rajan and Zingales (1995), and Fama and French (2002).

A simple regression of dca on the lagged log of one plus the ratio of cash divided by capital indeed confirms this. It yields a coefficient of -0.025 , highly statistically significant with a T of 11. However, the R^2 of this regression is only 0.0008. The left panel in Figure 3 illustrates the noise level. (Winsorization of dca shows up in some bands on the top and bottom.) The managerial change in leverage may well be marginally related to liquidity, but cash holdings are simply not strongly related to, much less a first-order determinant of managerial capital structure changes. Hennessy-Whited continue with

Second, even though the model features single-period debt, leverage exhibits hysteresis, in that firms with high lagged debt use more debt than otherwise identical firms. This is because firms with high lagged debt are more likely to find themselves at the debt versus external equity margin.

This is the exact opposite of readjustment, although their prediction applies only to high-debt firms. The right panel in Figure 3 shows that the empirical relationship may even be negative, not positive. However, this is not clear. In Iliev and Welch (2010), we document that the relation between lagged leverage and current leverage is very weak; the negative pattern in the right panel here comes about [a] because firms with zero lagged leverage can only increase leverage, while firms with 100% lagged leverage can only decrease leverage; and [b] because the stock-induced change, dcp , is also a function of past leverage ratio. It is clear that lagged leverage is not a first-order determinant of managerial activity.

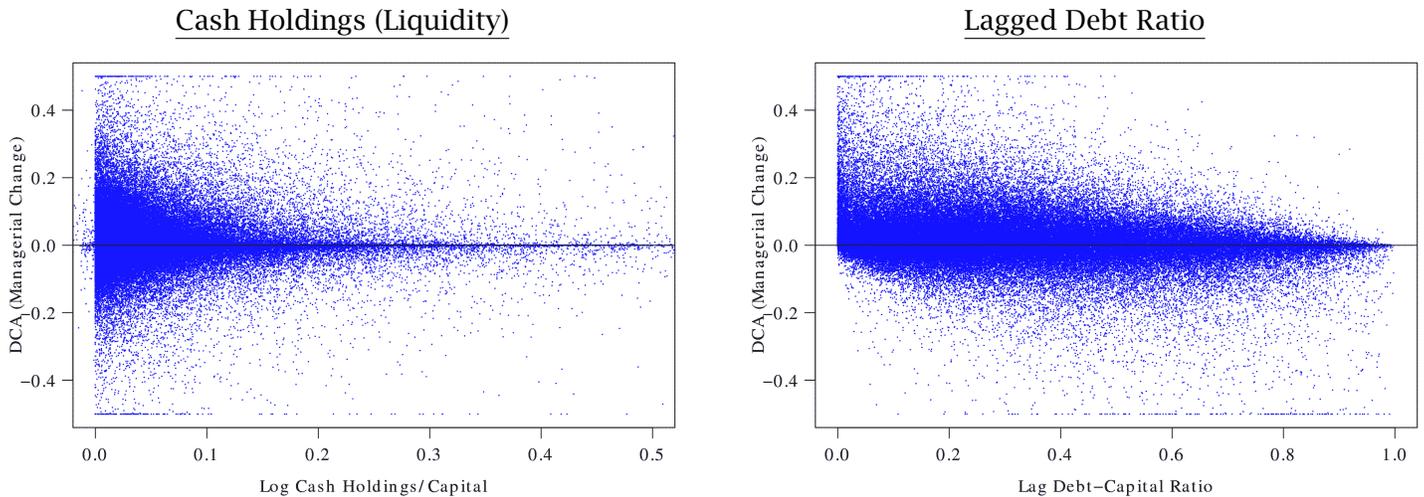


Figure 3: Managerial Changes Of Debt-To-Capital Ratios (dca) Vs Liquidity and Lagged Debt

D.3 Autocorrelation

Another hysteresis-related hypothesis could be that firms that increase their debt ratios in one year because they are on the margin will find themselves again at the margin the following year, and thus will issue debt again. We can divide the sample based on whether lag dca (all x divs) was positive or negative.

	Current dca		$dca_{t-1,t} = a + b \cdot dca_{t-1,t-2}$		
	Mean	Sdv	a	b	R^2
All	0.0098	0.0846	0.0075	-0.01698	0.0003
Extreme Neg Lag dca (< -10%)	0.0116	0.1213	-0.0071	-0.10478	0.0059
All Neg Lag dca	0.0052	0.0854	0.0036	-0.03943	0.0007
All Pos Lag dca	0.0092	0.0797	0.0128	-0.04946	0.0028
Extreme Pos Lag dca (> 10%)	0.0029	0.0946	0.0236	-0.11573	0.0101

The first line shows that the autocoefficient is negative. This is the opposite of the prediction. The next four lines show that this holds non-parametrically, too. The means are declining, although neither strongly nor monotonically so.

D.4 Empirical Conclusion

My tests have ignored all the diagnostics that I recommended earlier. Better tests can be designed. But the visual nature is appealing, and my main point will almost surely be robust: It is not that the directional mean relationships could not be as suggested by these

hypotheses. The theories may well be built on applicable marginal forces. Instead, it is that the empirical forces upon which the TDF and funding theories are built are likely to explain only a minute fraction of the managerially active part of capital structure changes. (In better tests, they may even be [more] definitively rejected.)

Going the step from “marginal contributory force” to “encompassing force,” i.e., attributing the evolution of capital structure behavior primarily to the forces in their models, does not seem warranted by the empirical evidence. Any structural model that attributes all capital structure dynamics to them is likely to be misspecified. We still need to learn what the right ingredients—the first-order determinants of managerial behavior—are.

IV Out-of-Sample Prediction

Fitting an empirical moment *in-sample* with a theory designed to explain it, is not a powerful tests. It is equivalent to judging a theory by the r-squared of an in-sample regression with many variables at the researcher’s discretion, without controls for other theories, and without diagnostics and corrections for a whole range of possible misspecification errors. Out-of-sample prediction is a better test of a model. Accounting for sampling variation, it is a more stringent test if and only if the model is false.

Quantitative models make point predictions, which are especially suitable to such tests. All that is needed is an objective criterion, such as the mean-squared-error to test performance. Moreover, out-of-sample prediction can also provide a test of the stability of the model. In-sample prediction errors should not be too far from out-of-sample prediction errors.

It is an additional advantage of the out-of-sample approach that alternative theories need not be embedded into the model. They can compete on equal grounds in explaining leverage ratios. If the quantitative model predicts better than simpler alternatives in out-of-sample tests, it matters less how many other theories could also be true (Section II), how many degrees of freedom the model needs (Section III), or whether it suffers from misspecification problems (Section III). The model is then eminently useful.

There are also appealing naive null hypothesis for out-of-sample tests: the model should predict the behavior better of the specific firm than that of a randomly drawn firm; and it should predict better than the statement that firms behave just as they have always behaved. While this is usually easy to reject in-sample, it is often surprisingly difficult to reject out-of-sample (Goyal and Welch (2008)). In the capital structure context, this simplest of all alternatives would be that firms do not respond systematically. The TDF model has similar but *not* identical prescriptions. There are years in which the model predicts an increase in the probability of capital structure activity. This could provide a good empirical test.

Unfortunately, there is no existing empirical evidence that suggests that structural quantitative TDF models (or the broader class of DTT models) can predict out-of-sample.

V Quasi-Experimental Tests

More recently, the Lucas critique has also influenced how theories are being tested. Economic models suggest that some independent variables x *cause* an effect on some dependent variables y . In turn, this causes correlations among exogenous x 's and endogenous y 's, or among different y 's. The tests Lucas critiqued were such correlation tests among y 's, some of which mistakenly considered y 's to be x 's. Correlation tests are minimal—necessary, but not sufficient. In the real world, spurious, unstable correlations between interesting variables are common, and many past empirical tests of economic models based merely on correlations have been invalidated by later data—and many even in the absence of policy changes. Lucas' remedy was to suggest micro-based structural models. If the underlying structural assumptions are correct, this will improve the stability of model predictions.

However, a recent paradigm change in empirical economics has offered a direct alternative that is based on the very counterfactuals that Lucas was so concerned about. This approach is now called “quasi-experimental.” It requires a direct identification of the source of exogenous variation—a shock to the model's x . Good identification, especially based on sudden shocks that influence different firms predictably differently, can make such tests relatively less sensitive to misspecification, such as omitted variables and unrelated structural shifts. An identified exogenous shock to a parameter or input that is unique to the tested economic model, can help distinguish it from other models with otherwise similar correlation predictions. In a sense, quasi-experimental designs emphasize causation over correlation.¹⁸ Identification has moved from having been an afterthought to being at the heart and center of many empirical papers today.

Structural quantitative models are exquisitely well suited to tests in the context of quasi-experiments, *because* they provide for point predictions intended to be stable. Their micro foundations were built so that they can predict exactly what resulting changes should occur. Quasi experiments are in effect the “realized counterfactuals” or “realized policy changes” for which structural micro-founded models have been constructed in the first place.

Quasi-experimental tests can be performed in-sample, but they are even more powerful out-of-sample. The model parameters would be fit up to the point of the change, and the model would then be used to predict subsequent capital structure changes.

¹⁸ Angrist and Pischke (2010) list the main econometric tools as instrumental variables techniques, regression discontinuities, and diff-in-diff. (I would add out-of-sample prediction as the simplest but also weakest form of this approach.) Their paper appears in the Journal of Economic Perspectives, with comments by Leamer, Sims, Stock, Nevo, and Keane, as well as a de-facto response by Einav and Levin (2009).

Yet, this new approach has also made empirical analyses with even modest theoretical underpinnings more believable, but it has also made it more difficult and constraining for empiricists. Quasi-experimental tests are feasible only when realized counterfactuals can be found. Some authors, notably Deaton and Heckman, have accused quasi-experimental economics to be driven more by the search for good experiments than by the search for answers to good questions. Nevertheless, the profession's consensus now seems to be that correlation tests seem more appropriate for the early stages in a literature, when theories and data are still new and no good source of exogenous variation can be identified; while quasi-experimental tests seem more appropriate for later stages of a literature and when good quasi-experiments can be found. Fortunately, relevant quasi-experiments seem plenty in the corporate finance arena.

There have been many known exogenous shocks to the TDF model's input parameters over the last fifty years. There have been repeated tax changes. Some have even affected different firms differently. Hennessy and Whited (2005) mention one good such experiment: they begin their sample in 1993, because Graham (2000) showed that different tax parameters applied earlier. This suggests that the model fitted after 1993 and then adjusted for the change in the tax rate should explain the 1992 evidence better than a model for which the tax rate would be presumed unchanged. Of course, such tests still need to hold constant the influence of other simultaneous events and address misspecification concerns—but doing so is part and parcel of standard reduced-form tests.

Tax changes are not the only quasi-experiments. Even the slower changes of distress costs and frictions can be used in empirical tests. For example, the costs of issuing have decreased over the decades. They are higher in bear markets than in bull markets. The TDF model should predict better with parameters fitted to the current situation, than with parameters fitted to data from other years.

Unfortunately, there is no existing empirical evidence that suggests that structural quantitative TDF models (or the broader class of DTT models) can predict empirical quantities in the presence of realized quasi experiments.

VI Conclusion

The choice of modeling approach comes down to the relative costs and benefits in a particular economic application. Quantitative structural modeling seems more attractive than qualitative reduced-form modeling when the economic phenomenon is relatively simple or when there are stark consequences to deviation (such as arbitrage); when the first-order forces are *a priori* known; and when there is less risk of model misspecification (such as omitted variables, exogenous variation identification, error in variables, survivorship bias, aggregation problems, functional form mistakes etc.). Structural models are rarely the right tool merely to illustrate that certain derived comparative statics are possible or that an effect is strong. Reduced-form qualitative models can do this more simply. Structural models are also rarely the right tool to ascertain what variables are causal. Quasi-experimental tests of simple reduced form models, holding multiple effects constant, can do this more convincingly. Causality in structural models is by assumption.

I have argued that corporate behavior is a highly complex phenomenon, where we do not understand even the basics of managerial motivations. Mistakes are not inherently subject to easy correction by third party arbitrage. Potential misspecifications loom large. Thus, I contend that corporate finance does not seem like fertile ground for the quantitative approach.

It is tempting to infer from the increasing number of award-winning papers that we can settle on a specific theory and that we already have good evidence—that all we now have to do is develop these models further if we want to understand the capital structure choices of firms better. Not surprisingly, some papers proposing structural models conclude on rather optimistic notes. Hennessy and Whited (2005) remark that “our theoretical and empirical results underline the importance of understanding corporate financial decisions in dynamic settings, as well as the importance of having a tight connection between theory and empirical work. Given the power of our theoretical and empirical framework to explain observed leverage phenomena, it appears likely that similar success is possible in other areas of corporate finance.” Strebulaev (2007) closes with “Research that combines these two strands [real cash flow models and capital structure models] is likely to be a fruitful avenue for future research in capital structure, and more generally, corporate finance.”

I would agree with these conclusions if the structural models had proven superior predictive ability out-of-sample in quasi experiments. Necessary structural complexity to explain data is commendable. Yet, the existing theories seem only to have passed the first hurdle, plausibility. They have failed the other three—there is no strong empirical support for them. On the contrary. As of 2010, even in-sample empirical evidence suggests that, at best, they do not model effects that are of first-order importance; and, at worst, that the data rejects them. Therefore, I draw rather different conclusions:

- Most importantly, all models deserve real and not just perfunctory tests. This means out-of-sample prediction in the context of quasi experiments against competitive explanations.
- I would not recommend the tradeoff theory as the null hypothesis that needs to be rejected. I would suggest that the simplest null hypotheses to reject are random behavior first, and time-invariant behavior second.
- To explain non-readjustment, any model has to explain how it comes about in the presence of high levels of capital structure activity. Consequently, the TDF model *cannot* explain lack of readjustment. Lack of readjustment remains a puzzle.
- Identifying the first-order determinants that drive managers to be so capital-structure active remains an important unsolved puzzle.

We should not only be prepared to adopt a specific model or even entire modeling approach if it shows itself to be superior in prediction, but also be prepared to reject and discard it if it cannot do so. Failure is not always just a challenge to push ahead.

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