A Critique of Recent Quantitative and Deep-Structure Modeling in Capital Structure Research and Beyond*

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ABSTRACT

This paper highlights four shortcomings of recent quantitative and deep-structure models in corporate finance: (1) These models have omitted too many plausible forces not based on evidence but based on authors' priors. (2) The link between their unobserved structures and their reduced-form empirical evidence has been too weak (even orthogonal forces could have affected their inference), (3) The existing tests have largely ignored many important econometric issues, such as selection and survivorship biases. (4) The models have never been held to reasonable test standards, such as performance in quasi-experimental settings. Constructively, my paper offers two primary suggestions: The first is to search for more direct empirical proxies instead of relying on "assumed" first-order conditions. The second is to design quasi-experimental tests of structural models. It illustrates these points in the context of Hennessy and Whited (2005) and Strebulaev (2007).

Two macroeconomic papers have had great influence on modern economics. Lucas (1976) critiqued that reduced-form models can be useless for macroeconomic policy evaluation. He pointed out that such interventions, a type of counterfactuals, may require analysis of the model in its deeper micro-foundations (the structure) and not in its reduced form. (Of course, incorrect deep-structure models are as susceptible to instability as reduced-form models.) Mehra and Prescott (1985) showed that simple models could fit the equity premium only qualitatively but not quantitatively. It pointed out that research should conduct at least basic calibrations to appraise whether a model can explain a phenomenon. Although deep-structure and quantitative modeling need not be linked, in practice, they frequently are.

Econometrics has also progressed over the last decades. In particular, it has become clear that identification problems — the confusion in empirical work about what variables are endogenous and what variables are exogenous (Leamer, 1983) — can be reduced by focusing on what is now called quasi-experimental methods (Angrist and Pischke, 2008). These methods investigate the evidence in situations in which researchers can identify shocks as "exogenously caused" based on economics. For example, instead of the overall correlation between taxes and capital structure, quasi-experimental research may investigate how a sudden change in federal tax rates influences changes in the capital structures of firms of which the net income is either just above vs just below the border where they are taxed. Quasi-experiments can be viewed as examples of realized counterfactuals in Lucas' sense.

Deep-structure and quantitative models have been commanding an increasing market share of theoretical corporate finance. Examples of structural quantitative models in corporate finance are Leland (1994), Leland (1998), Leland and Toft (1996), Goldstein *et al.* (2001), Hennessy and Whited (2005), Ju *et al.* (2005), Hennessy and Whited (2007), Strebulaev (2007), Titman and Tsyplakov (2007), and DeAngelo *et al.* (2010b). My paper offers a critical perspective on this quantitative and structural modeling trend in corporate finance. One can disagree about model esthetics, such as the tradeoffs between complexity, realism, and simplicity, especially when it comes to merely highlighting comparative statics and intuition. But there should be little disagreement that existing quantitative and deep-structure models in corporate finance have the

following shortcomings:

1. Although built on plausible forces, they have ignored many other plausible forces not based on the data but based on their authors' priors.
2. The reduced form of structural models that is put to the data often maps into a vast number of plausible alternative explanations that are consistent with the same reduced-form findings. This is particularly problematic in structural models, because even forces orthogonal with the underlying hypothesized structural forces can distort their inference (Section 2.2.1.1). The gap between the theory and the evidence is so unusually large because the model tests lean heavily on inversions of structural first-order conditions, often linking entirely different kinds of variables together. My paper argues that, in the corporate finance context, direct empirical proxies for structural variables can often be found, and direct variables are likely to yield better tests.
3. Their tests have largely ignored other econometric issues of importance in the corporate finance context, such as selection biases, survivorship biases, controls, and so on.
4. The models have not been held to appropriately high test standards (in-difference estimation, quasi-experimental identification, out-of-sample prediction), and few tests have allowed for good alternative explanations.

My paper elaborates on these points in great detail. To illustrate the arguments, I critique two prominent models: Hennessy and Whited (2005) and Strebulaev (2007). I argue that they convey a confidence that is not justified by an objective evaluation of the evidence. However, my points are not specific but broad. I argue that it is unlikely that *any* models in their vein are likely to succeed anytime soon. To be clear, many papers built on simpler approaches suffer from many of these weaknesses as well. However, I argue that the shortcomings are less likely to be overcome in state-of-the-art models in the Hennessy and Whited (2005) and Strebulaev (2007) approach than they are likely to be overcome in state-of-the-art models using simpler approaches.

Some of my points echo similar ones made independently in recent critiques by Angrist and Pischke (2010) and Caballero (2010). Angrist and Pischke argue that structural models have failed badly in industrial organizations and labor economics, and propose that the alternative of

"design-based" empirical studies (including quasi-experimental designs) has offered real tests of economic forces and causality even for the most simple of models. My paper shares some of their perspectives. However, besides the domain and application differences, I argue (1) that the models can be better tested by searching for direct proxies (in lieu of the reliance on inverted first-order conditions in many current tests), and (2) that structural models and quasi-experimental approaches are not substitutes but complements — the predictions of quantitative and structural models can and should be tested in quasi-experimental settings against reasonable alternatives. This means that one approach does not preclude the other. Caballero (2010) also laments similar issues and the general state of affairs in macroeconomics, writing "We are too far from absolute truth to be so specialized and to make the kind of confident quantitative claims that often emerge from the core."

My paper now proceeds as follows. In Section 1, I describe the basic research challenges in corporate finance in general and capital structure in specific. In Section 2, I describe a premier deep-structure model in corporate finance, Hennessy and Whited (2005), and critically reevaluate the empirical evidence in its favor. In Section 3, I do the same for a premier quantitative model, Strebulaev (2007). In Section 4, I contrast their assessments against my own. In Section 5, I explain why some areas of asset-pricing, in which there are strong arbitrage conditions (especially derivatives pricing relative to the underlying equity pricing), are better suited to the approach, while other areas (especially equity pricing) share many of the problems of corporate finance. Section 6 concludes and speculates how research in corporate finance could progress.

1 Corporate Finance

Corporate finance is the study of the behavior of corporations. It faces both common and unique research challenges. This section gives an overview.

It is not uncommon that dozens of economic hypotheses are trying to explain the same phenomenon. The hypotheses are rarely exclusive. Researchers rarely start from the same priors and rarely agree on the first-order effects. The publication process often favors advocacy for a particular economic force rather than an evaluation of relative strengths and

weaknesses. There are few papers that attempt to reconcile the evidence from multiple earlier papers.

The empirical evidence itself is often "murky." Sometimes, the evidence is sensitive with respect to the sample, the controls, or the specification. Frequently, the research explains only a small fraction of the variance of the dependent variable of interest.

In contrast to some areas of asset pricing, there are few real-world arbitrage constraints that would allow third parties to force rational behavior onto non-value optimizing corporations. Staggered boards, poison pills, and other mechanisms can allow managers to isolate themselves effectively from any hostile external forces. Even when not in place, the takeover premium required to unseat management is often greater than the value consequences of poor corporate policies. This suggests that Darwinian selection processes are weak. Nevertheless, some corporate finance theories attribute an unconflicted hyper-rationality to managers that contrasts starkly with managers' generally more heuristic and conflicted decision-making process.

Although there is a large number of firms in the cross-section, the data frequency in corporate finance is also usually fairly low, with annual data being most common. Depending on the topic of the study, data availability can be spotty and induce selection biases. When firms disappear mid-year (and they do so on average after 10 years), financial statement data from their final fiscal year is rarely available. This can lead to survivorship biases. Other common problems that plague empirical research in corporate finance are strong firm-size effects that are not fully understood, residual heterogeneity across firms and industries, multicollinearity, and errors in variables due to proxy measurement problems.

On the positive side, corporations are subject to many observable "natural experiments" that can be viewed as exogenous from firms' perspectives. For example, U.S. firms have frequently been subject to tax changes, regulatory changes, aggregate or idiosyncratic technology shocks, changes in external financing availability (for example, in 2008), personnel deaths, and so on, all of which have been ingredients in some corporate finance models.

To animate these points, my paper now focuses on one concrete subfield, capital structure. However, the reader should remain cognizant that the issues covered readily carry over into other areas of corporate finance (and beyond).

1.1   Capital Structure Theories

Perhaps the most important question in the capital structure literature is how leverage ratios come about.[1,2] Modigliani and Miller (1958) showed that capital structure is not an important choice under fairly strong perfect market conditions. However, these conditions do not hold in the real world. Therefore, many papers have argued that capital structure choice can be value-relevant. Under the additional assumption that corporations or managers optimize, they should choose specific capital structures.

A sampling of forces includes:

Deadweight distress costs favor equity because equity holders cannot force the firm into financial distress. Sometimes, this is made specific. For instance, Roberts and Sufi (2009) identify the propensity of firms to enter financial distress by the distance from bond covenants.

Corporate taxes favor debt because the interest is tax-deductible (for example, Graham, 1996).

First put forth by Robichek and Myers (1966), a "tax vs. distress costs" tradeoff has been the workhorse capital-structure theory. If managers are unconflicted and maximize firm value, the tax-distress cost tradeoff can lead to an optimal leverage ratio. Other proposed explanations include:

Adverse selection favors debt because the negative signal when firms issue lower-priority securities renders them more expensive. (Adverse selection is also the main ingredient in the pecking-order theory in Myers and Majluf, 1984.)

Risk shifting favors equity because creditors may demand too many restrictions on corporate flexibility (Parrino and Weisbach, 1999).

Unmitigated agency concerns favor equity, especially when corporate governance is weak because managers prefer less pressure and the opportunity to build empires.

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[1]   There are more nuanced views of this question. For example, Rauh and Sufi (2010) argue that this is too narrow a question, because firms also have substantial variation in the types of debt they carry. Others, such as DeAngelo *et al.* (2010a) or DeAngelo *et al.* (2010b), have argued that capital structure should be viewed together with project choice. This perspective is also adopted in Welch (2008, Chapter 21), which considers firm-scale and capital structure jointly.

[2]   Explaining capital structure is different from explaining corporate issuing behavior and/or payout policy. For example, Welch (2011) shows that the correlation between equity issuing activity and leverage ratio changes is close to zero.

ESOPs favor equity because they can be used as efficient motivators for employees (Fama and French, 2002).

Credit ratings clientele effects favor equity especially for firms barely or just ranked investment-grade because an investment-grade rating is required to attract some institutional investor clienteles (Kisgen, 2006).

Tangibility favors debt (Rampini and Vishwanathan, 2010) because it reduces the cost of creditors to recover their collateral.

Some of these are forces that can be derived from other forces. Many other economic forces can have situation-specific capital-structure implications. For example:

Frictions (transaction costs to issuing or retiring debt or equity) can induce corporations to be inert most of the time (Fischer *et al.*, 1989).

Stock returns can induce higher or lower market leverage ratios (Welch, 2004). Analogously, operating performance and depreciation rules can induce higher or lower book leverage ratios.

Timing considerations can induce managers to issue debt or equity when they believe that one is cheaper than the other (Baker and Wurgler, 2002).

Consensus concerns can induce managers to issue equity when they are in agreement with their investors and debt otherwise (Dittmar and Thakor, 2007).

M&A capital needs can induce firms to issue securities as needed to facilitate acquisitions (Fama and French, 2002).

Peer effects can influence firms to imitate their industry peers when their capital structures have diverged too much (Welch, 2004; Leary and Roberts, 2009).

Project considerations, as described in DeAngelo *et al.* (2010a) (cash needs), DeAngelo *et al.* (2010b) (lumpy investment), Hennessy and Whited (2005) (productivity), or Shivdasani and Stefanescu (2009) (pension liabilities), can make it relatively more advantageous to issue short-term securities or long-term securities (of which equity is one).

Crowding out of limited bond buyer demand when government issues debt can reduce corporate borrowing (Greenwood *et al.*, 2010).

Precommitments (sinking funds) can favor equity if external financing conditions make it difficult to find equity funding at the time when debt comes due.

Hubris may induce firms to take on too much leverage (Roll, 1986; Malmendier and Tate 2005).

Managerial preferences, measurable as the identity of the manager, may create an idiosyncratic preference for either debt or equity (Bertrand and Schoar, 2003).

Investment bankers may convince CFOs to shift around their capital structures in order to generate fees (and allow the CFOs to look active).

Non-optimal behavior could lead firms to follow arbitrary rules, in which they may not pay attention to bankruptcy costs, tax benefits, transaction costs, and project funding needs, but to something else that may or may not be reasonable and/or observable. For example, they may target a bogie, such as book leverage ratios, even if or when these are not meaningful for measuring tangible assets or assessing future distress costs. Managers could be "asleep at the switch" and readjust whenever they wake up (or when a heuristic band is violated). If the objective function is flat, optimal behavior is difficult to distinguish from non-optimal behavior.

Many of these economic hypotheses have been published with accompanying favorable empirical evidence. This list of influences is of course not exhaustive, nor are the forces mutually exclusive. They may operate simultaneously. They may influence different firms differently at different times. The influences may be complementary or substitutive. They may be non-linear.

Tests of any of these research theories can be against specific alternative hypotheses or against unspecific null hypotheses. One reasonable null hypothesis is that firms behave as they did in the past. Another reasonable null hypothesis is that firms behave randomly, that is, without regard to their own characteristics. Both null hypotheses are necessary, but not sufficient. Beating them are weak tests that any specific theory should be able to pass.

The next obvious question is how research can choose among forces. Ideally, empirical evidence would allow us to assess the relative importance of all these economic hypotheses, and how they correlate. Subsequent research could then make informed choices about which economic forces a useful model of capital structure should include and which it

can (more or less) ignore. Unfortunately, embedding more than a few hypotheses within the same modeling framework has proven to be difficult. It is rare.[3]

The more common research approach is to rely on priors to identify the subset of forces that are assumed to matter. The choice can be based on economic intuition, informed by surveys of the individuals that actually decide on the capital structures (Graham and Harvey, 2001), or justified by results from earlier studies.The maintained assumption in tests with ex-ante variable selection is that omitted economic forces are orthogonal to included *measured* variables. Therefore, *ceteris paribus*, when a variable is omitted that has only a weak influence on capital structure, this variable is also less likely to affect inference with respect to the force that research is exploring. In the extreme, if a variable is uncorrelated with capital structure and its changes, and if it is uncorrelated with the independent measured variables, then it has no distortive influence on inference about other forces.

1.2   Other Specification Issues

Empirical capital structure tests also have to struggle with other common problems in this domain, such as:

- Empirical proxies are rarely exact theoretical constructs. For example, empiricists can often measure only average values, even though the theory calls for marginal values. (Tobin's Q is a good example.)
- Firm size, for reasons not fully understood, can be an important determinant.
- Industry, for reasons not fully understood, can be an important determinant.
- Time, for reasons not fully understood, can modulate the strengths of different relations.

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[3] An early piece that explicitly tested multiple theories together was Titman and Wessels (1988). Shyam-Sunder and Myers (1999) have entertained the pecking order theory as an embedded competitor — and promptly found it to perform as well as the tax-distres-friction model. 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. Other papers have handled competing explanations implicitly (for example, with fixed effects), usually in order to control for the omission of variables that the researchers are not interested in.

- Many firms lack the data that is necessary to investigate the target hypothesis. The missing data may not be random.
- Public firms last only ten years on average, and final fiscal year data is rarely available. Stock-return data and/or a code that describes the reason for disappearance between the last reported financial statement and the day of disappearance is available but unfortunately rarely used. Attributing a 100% leverage ratio to firm-years in which stock returns were dismal during the year but the firm never reported a final financial statement would be a reasonable check.
- Many "independent" corporate finance variables seem themselves endogenous.
- Variables may require scaling, though it is not clear whether this should be done by dividing variables by book or market assets, tangible assets, sales, or even net income.
- Many independent variables, especially financial statement metrics, are often highly correlated, even after firm-size normalization.
- Because leverage ratios are bounded variables with unusual dynamics, a resampling approach (as in Chang and Dasgupta, 2009 and Iliev and Welch, 2010) is necessary to establish the properties of common linear estimators whose properties are often familiar only in the context of normally distributed residuals.

Empirical papers can employ tests to assess whether these issues are important, and, if they are, attempt to correct for them. For example, they can perform residual diagnostics. This can lead to the inclusion of further control variables, such as firm-size, time measures, industry controls, or fixed effects. Related to fixed-effect econometric models, some research strategies focus principally on estimating differences, not levels.

1.2.1 Isolating managerial capital structure changes

Capital structure theories typically focus on the actions of firms, but leverage ratio changes (dct, "debt-to-capital, total") are also influenced by corporate performance. A good approach is therefore to take out the part of leverage changes that is due to corporate performance (which is named dcp, "debt-to-capital passive"), and focus on predicting the remainder (which is named dca, "debt-to-capital active").

$$\text{dct}_{t-1,t} = \frac{D_t}{D_t + E_t} - \frac{D_{t-1}}{D_{t-1} + E_{t-1}},$$

$$\text{dcp}_{t-1,t} = \frac{D_{t-1}}{D_{t-1} + E_{t-1} \cdot (1 + x_{t-1,t})} - \frac{D_{t-1}}{D_{t-1} + E_{t-1}}, \qquad (1)$$

$$\text{dca}_{t-1,t} = \frac{D_t}{D_t + E_t} - \frac{D_{t-1}}{D_{t-1} + E_{t-1} \cdot (1 + x_{t-1,t})},$$

where D is debt, E is equity, and x is the capital gain of equity over the year. dca is the net effect of all managerial debt and equity issuing and repurchasing activity, including dividends and coupon payments, during the year. A theory of corporate behavior should primarily explain dca, not dct. (Managerial actions [dca] can of course respond to performance, too.) Moreover, empirical tests with dca instead of dct eliminate not only stock-market induced noise but also biases arising from stock-market return regularities (such as the book-to-market effect).[4,5]

1.3 Empirical Test Approaches

1.3.1 Levels, differences, and quasi-experimental tests

In-Sample Evidence: Early tests in the capital structure literature, such as Titman and Wessels (1988), focused on empirical moments in levels. They established such regularities as the negative correlation between profitability and leverage. If the model is correct, an in-sample test uses the data most efficiently. Moreover, in cases in which the dependent variable is conditionally normally distributed (although this is unfortunately not the case for leverage ratios), there is a well-established econometric toolbox for in-sample tests under linearity assumptions. Of course, if the model does not contain the correct variables and/or if the specification assumptions are violated, then the inference can be incorrect.

In-Difference Evidence: Level correlations can easily lead to misidentification — in the Titman and Wessels example, profitability and leverage may

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[4] Note that a theory could seek to explain on even smaller set of changes, such as equity created by ESOPs or equity not created by ESOPs. This would argue for taking out additional components from dca. However, the procedure of taking out the stock-return induced capital structure change when evaluating managerial actions is conservative and thus should be uncontroversial.

[5] The evidence of poor predictability of dca (reported below) is the same if the rate of return (instead of the capital gain) is used as the performance measure, and/or if debt-due-within-one-year is removed. Not surprisingly, the noise is usually even stronger if dct instead of dca is predicted.

be codetermined by another variable. In particular, simultaneous trending of variables can suggest spurious correlations that do not exist. To improve level tests of capital structure theories, and to reject the null hypothesis that firms behave like they have always behaved, formulating the model in changes is more robust. This means that if a stable panel model holds in levels, it should also hold in differences ($y = a + b \cdot x \Leftrightarrow \Delta y = b \cdot \Delta x$). Suitably adjusted, a test in differences does not present a higher hurdle than a test in levels for a stable model. Of course, estimation in differences ignores some information that estimation in levels retains, and, with a different dependent variable, one cannot expect as high an R^2 for a model in differences as one can expect for a model in levels.

Quasi-Experimental Evidence: However, even regular changes in the dependent and independent variables may be codetermined by an omitted variable, and/or the direction of causality may still be the reverse of what was hypothesized. Thus, many recent tests have shifted to quasi-experimental (QE) methods.[6] These methods focus on explaining empirical moments in specific difference situations only — unusual circumstances in which the economics of the situation makes it clear that some input variable has experienced a quasi-exogenous shock. Good candidates for quasi-experimental studies are sudden discrete changes in an input variable of interest, especially if it affects different firms differently.[7]

Before the advent of such quasi-experimental focus, it was often considered impossible to obtain causality inference empirically, leaving researchers with only one method to establish causality: strong faith in the conjectured theory. However, if a quasi-experiment can come close to the ideal of a randomized double-blind real experiment, it can be almost a direct causality test with respect to the intervention and even without a strong deep underlying model. Of course, the credibility of the empirical test depends on the quality of the experiment — and, of course, if the ideal

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[6]   Angrist and Pischke (2010) list the main econometric tools as instrumental variables techniques, regression discontinuities, and diff-in-diff. (Out-of-sample prediction, tests of models in differences, and a focus on natural experiments can be viewed as earlier attempts to deal with the same problems.) Their paper appears in the Journal of Economic Perspectives with comments by Leamer, Sims, Stock, Nevo, and Keane and with a de-facto response by Einav and Levin (2009).

[7]   Event-studies (Fama *et al.*, 1969) were an early form of quasi-exogenous identification, in which research exploited sharp, unique events. It is usually reasonable to assume that the stock price on a given day is endogenous to an (exogenous) news release, and not the other way around.

is not met, then the research still needs to adjust for the influences of other simultaneous events and other misspecification concerns.

Although the quasi-experimental approach has made empirical analyses with even verbal theoretical underpinnings more believable, it has also made empirical work more difficult and constrained. 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 rather than by the search for answers to good questions, or the search for first-order economic forces.

Fortunately, a shortage of good theory-relevant available quasi-experiments is not (yet) a problem in corporate finance. There have been many identifiable exogenous shocks to the inputs of many corporate finance models over the last fifty years. For example[8]:

Taxes changed repeatedly. These changes also affected different firms differently.[9]

Transaction costs changed repeatedly.[10]

Financial distress costs fell in 1986, when prepackaged bankruptcies began to appear. In 1994, the bankruptcy code was overhauled.

Productivity changed dramatically in different industries at different times.[11]

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[8] Slower changes in many of these variables can allow for in-differences estimations, even though these are more prone to misidentification. For example, the costs of issuing have slowly decreased over the decades and are higher in bear markets than in bull markets. Productivity shocks may have diffused slowly through some industries, and research can exploit different diffusion speeds in different industries in different years.

[9] For example, Hennessy and Whited (2005) mention one possible experiment: they begin their sample in 1993 because Graham (2000) showed that different tax parameters applied earlier. Thus, the Hennessy-Whited model can be tested based on how its inference changed between pre- and post-1993 data. Givoly *et al.* (1992) looked at the 1986 TRA, which is another natural laboratory.

[10] For example, on December 1, 2005, the SEC made it easier to do shelf-registrations for firms with more than $700 million in equity, or firms that have issued more than $1 billion in registered non-convertibles. (This practically invites a discontinuity test approach.) In 2008, the disappearance of many investment banks and the differential capital constraints on the surviving investment banks raised the costs of issuing, especially for firms that did not have precommitted credit lines.

[11] For example, in the transportation sector, costs fell with the wider introduction of GPS, pagers, mobile email, satellite navigation, and containers. (Again, geographic, firm-specific, and industry-specific heterogeneity further strengthen such tests.) SOX imposed new record-keeping costs on many firms, reducing productivity for firms just above the threshold (Iliev, 2010). DNA tests and cures (such as antibiotics) appeared at specific times and lowered the costs in particular sectors of the healthcare sector. Note also that even when the variables of interest (here productivity) are not easily measurable, exogenous shocks to them may be much easier.

Financial constraints during the 2008 financial crisis affected firms differently based on their contractual background (Almeida *et al.*, 2009) and/or based on their existing banking relationships.

There have also been many idiosyncratic exogenous shocks, such as cases in which key personnel suddenly died or became incapacitated.

Quasi-experimental tests also provide an appealing unconditional null conjecture: Any theory should predict variables better if the known quasi-exogenous shock to the presumed-exogenous variable is *not* ignored. This test is powerful because the model can now be rejected even if "firms behave as they always have." A quasi-experimental test is also stronger than a simple in-differences test, because the research should detect changes only in a subset of clearly identified cases (years and firms), and not in others.

1.3.2 In-sample and out-of-sample tests

In-sample prediction is often more susceptible to overfitting than out-of-sample prediction. This makes an out-of-sample test a good complement to an in-sample test. Out-of-sample tests lean less on the assumption that the model is correct, and make it easier to assess how stable and predictively useful a model is. Suitably adjusted for sampling variation and estimation uncertainty, an out-of-sample test does not present a higher hurdle than an in-sample test for a stable model.

Like in-sample tests, out-of-sample tests also make it easy to contrast competing models, whether they are nested or competitive. Models can be compared based on a neutral criterion, such as mean-squared-error prediction or the proportion of the variance that a set of independent variables can explain. (The latter also allows ascertaining which variables are more important and first-order and which are not.)

Note that level, differences, and quasi-experimental tests can all be performed either in-sample or out-sample. For the latter, the model parameters would be fit up to the point of the change, and the model would then be used to predict subsequent changes.

2 Structural Modeling

Most of the rest of this paper evaluates two specific models in light of the formidable research challenges in corporate finance that were described in

the previous section. Hennessy and Whited (2005) is principally a structural model. It leans strongly on its first-order conditions to identify the influence of an unobservable "structural" variable (productivity) on observable variables (liquidity and capital structure). Hennessy and Whited highlight mostly qualitative implications of the model. Strebulaev (2007), discussed in the next section, is somewhat less structurally and more quantitatively oriented. Both papers may well be the most prominent ones in their respective genres. Both won Brattle prizes for the best paper in corporate finance in the Journal of Finance in their respective years. This makes them excellent targets for this critique.

   This section discusses structural modeling.[12] The main critique here revolves around the gap between the unobservable theoretical variables and the observable tested relationships (variables). When variation in an underlying unobservable variable is used to infer the variation in empirical variables which are very different and potentially subject to other forces — tied together by hypothetical assumed-correct model first-order conditions — the gap can be huge.[13]

2.1 Unobservable Variables and Test Inference

The main problem when the gap is wide is that multiple underlying models can map into the same empirical reduced-form model. The wider the gap between the structure and reduced-form, the more the research must lean not only on the structural model itself when interpreting the reduced-form model findings but also on the belief that there are no or few plausible alternative explanations for the same evidence.

2.1.1 Orthogonality and inference

It is worth explain why the well-known irrelevance of orthogonal forces holds only in the empirical reduced form. For example, consider a scenario in which there are two possible variables, r (right) and w (wrong), and one dependent variable y. Assume that r and w are orthogonal, $r \perp w$ and that the true model is

$$\textbf{True Model} \quad y = r.$$

w has no influence on y ($w \perp y$).

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[12] There are a number of possible definitions of structure (see the appendix), but for the purposes here, my paper does not need to take a stance.

[13] Of course, large gaps could also be caused by excessive noise in simple empirical proxies.

Now assume that the researcher *incorrectly* believes that w could determine y and wants to put this hypothesis to the test. If w is observable, then the model is its own empirical reduced-form. The test regression

$$\textbf{Estimated Model} \quad y = \hat{a} + \hat{b} \cdot w$$

is well known to yield an expected value for $\hat{b}$ of 0 because $w \perp r$. The researcher would correctly reject the wrong model.

Now, assume that there is another observable measure $M = f(\overset{+}{r}, \overset{+}{w})$, specifically $M = w + r$, but the researcher's variable w is not observable (and, of course, the researcher has not thought of r so it is an omitted variable). With w unobservable, the researcher's model is structural in the sense that it relies on an assumed model relation. (The same inference applies if M is tied to w and r through a first-order condition.) To favor the researcher, assume that she knows for sure (and does not merely hypothesize) that $M = w + \epsilon$, where ϵ is uncorrelated with w. This is of course correct in the example (because $M = w + r$ and $r \perp w$). It is easy to verify that the test regression

$$\textbf{Estimated Model} \quad y = \hat{a} + \hat{b} \cdot M$$

is expected to yield a positive $\hat{b}$. The researcher would incorrectly conclude that w influences y and thus accept her model.[14] This is the case even though $w \perp r$ and $w \perp y$. Orthogonality between the true force r and the incorrect force w is not enough to guarantee the correct inference that $w \perp y$. Instead, for the inference to be correct, that is to yield the inference that $w \perp y$, it must be the case that $w \perp M$. Consequently, "holding other forces constant" (the boiler-plate *ceteris paribus* qualifier) means something quite different in the structural model than it does in the reduced-form model. It means that there is uncorrelatedness of other forces with the measured M, not with the conjectured force of interest r.

This problem is more significant when the gap between the measured variable M and the unobserved structural variable coming out of the model is wide. Call x the variable that the researcher conjectures influences y. In the extreme, where the gap is zero, $M = x$ and we are back to the case where the researcher observes the model variable. Hence, if the model is

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[14]   This is a Type-1 error. It is easy to construct other examples in which other unobservable uncorrelated variables induce Type-2 error instead.

wrong and $y \perp M$, then $E(\hat{b}) = 0$, and the inference is correct. But if the gap is wide, $M = x + z$, where z is a large wedge between the model proxy and the researcher's variable x, even with $r \perp z$, any non-zero correlation between z and y distorts the inference.

2.1.2 Remedies

By definition, if the research does not measure the underlying structural variables, then it is difficult to test empirically (establish based on data ex-post) whether the model was based on the correct but unobserved structure. In the $M = r + w$ example, no econometrics can distinguish whether it was r or w that determined y. Instead, models that hypothesize about the unobservable variable (r) must lean hard on the assumptions of the model *and* on the assumption that there are no other influences on the reduced form specifications. A test of a structural model is more likely to provide correct inference if it is a priori all but certain that the structure is correct, and that there are few alternative forces that could drive a wedge between M and r. In some areas of finance, such as in the pricing of derivative securities, this is a fairly credible conjecture.

Unfortunately, the correct structure is rarely a priori certain in corporate finance. In this literature, the structure is often derived from a behavioral theory's first-order condition (and often over a fairly flat surface), and corporate behavior is not subject to third-party arbitrage. If the strength of the relations between observed variables in corporate finance provide any guidance to the strength of relations between unobservable and observable variables, these correlations may well often be in the single digits. (Low explanatory power of variables will also be apparent in the figures below.) If variables are uncorrelated, and the correlation between r and M is 10% and the correlation between r and y is 10%, then the expected correlation between observables M and y would be only 1% *if the theory is correct*. An optimistic way of interpreting such empirical evidence is that the 1% implies much stronger effects between the unobserved variable and the proxies. This will be the inference from inverting conditions — the model forces would seem quite powerful because of the model's expected low fit. A pessimistic way to interpret the same evidence is that the same 1% correlation has left a 99% gap for other variables (w) to slide in and distort the evidence.

Cautious corporate finance researchers can consider the following:

Narrowing the gap:[15] Ideally, the research would identify as close a
   reduced-form proxy as possible for the structural variable. In many
   cases, even a bad direct-measure proxy is not only better than none
   but better than an inverted first-order-condition proxy from a behav-
   ioral hypothesis. In the example, this would suggest searching for a
   direct measure $w + \epsilon$, in which the error epsilon is not correlated
   with r.[16] In some cases, it may not be possible to find a good proxy for
   a structural variable, but it may be possible to identify a good proxy
   for measurable changes in structural variables.
   For the specific example of Hennessy and Whited (2005), while cor-
   porate productivity is difficult (but not impossible) to measure, the
   introduction of new technologies as positive shocks to productivity
   in particular industries is almost surely measurable. If a researcher
   believes the theory to be true, the additional data work should be
   worth the effort. My paper argues that a test of a productivity-based
   theory with productivity data is more credible than a test of the theory
   based on a correlation between liquidity and capital structure.

Increasing test stringency: Research can view evidence in favor of deep-
   structure models with wide gaps more skeptically. Sometimes, more
   stringent empirical tests or a higher bar can help. Although no empir-
   ical test could help distinguish between w and r in my example, in
   other situations there may well be further functional relationships or
   more empirical moments that can reduce the set of alternative theo-
   ries that can explain the same reduced-form findings.[17]

Enumerating alternative interpretations: Papers can enumerate plausi-
   ble alternative explanations for the observed reduced-form relation-
   ships. If there are too many plausible influences to enumerate, the

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[15] In econometrics, instrument misspecification is an analogous problem. In a deep-structure models —
and especially in models without closed-form solutions, in which the economic intuition is complex
and fit is low — such misspecification can be severe. The question of finding a better instrument is
analogous to narrowing the gap between the observed and structural formulation.

[16] An even better approach may be to extract one proxy from the first-order conditions and another
from direct measurement. The researcher could then report their correlations.

[17] This conflicts with the popular view that not all implications of a theory need to hold. Note that this
is also a stricter view than that in Friedman (1966, p. 8) that a theory need perform only in the arena
for which it was designed. I interpret Friedman's view to mean that a model does not have to offer
implications on every economic phenomenon, not that a model's strong basic implications need not
hold.

empirical evidence in favor of the deep-structure model is weak (and should move one's prior very little), even if the estimated moments all have the correct sign.

2.2 The In-Sample Performance of the Hennessy and Whited (2005) Model Revisited

The Hennessy-Whited project funding hypothesis is principally a deep structure model. It is not principally a quantitative model in the sense that its authors highlighted the qualitative implications more than the quantitative implications. The theory is based on (unobserved) productivity shocks, which have to be backed out through the model first-order conditions from variables that are economically quite different. Both capital structure and observable liquidity invert back into the unobservable productivity that the model is built on. Although the model's forces are intuitive and although the model provides a wealth of moment predictions, it is not easy to understand. Important tax assumptions (Lewellen and Lewellen 2006) work in concert with productivity shocks. The model is not solved in closed-form.

The model is too elaborate to allow a brief characterization here. To reassess its empirical meaningfulness, my paper follows Hennessy and Whited (2005) itself. On page 1131, they state

> 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).

(The model's broader conceptual point was to help the profession understand that a negative relation between liquidity and capital structure can arise even in the absence of transaction costs and asymmetric information.) In my earlier notation, the unobserved structural productivity variable (r) drives the empirically observable reduced-form on two observable variables: liquidity (M), and capital structure (y).

This section will now present empirical evidence that the empirical link between liquidity and capital structure changes is weak. This is not so much a direct test of the model, as it is evidence to assess whether liquidity or changes therein are a first-order influence explaining

managerial capital-structure activity, dca (Equation 1). This is the empirical regularity which Hennessy and Whited (2005) suggest should help shift the reader's posterior in favor of their model.

The first question is whether managers actively delever when their operating cash flows are high. Of course, an empirical "cash-flow vs. active leverage changes" relation would hardly qualify as strong evidence in favor of an optimal-behavior-to-productivity-shocks theory of capital structure. Any theory in which firms use operating cash flows partly to pay back (or assume less) debt could predict this. Such evidence would be in line with their productivity theory prediction. The left panel in Figure 1 shows that the cross-sectional relation between *active* managerial changes (dca) and operating cash flows is strongly negative (with a T of -35), as predicted, but with an R^2 of only 1.2%. 98.8% of the variation in managerial capital structure activity remains unexplained.

Although Hennessy and Whited (2005) use cash flows in their own tests (presumably more related to exogenous productivity shocks experienced by firms than the more endogenous cash holdings), Bolton *et al.* (2010) suggest that cash balances are more suitable. A firm that already has a lot of cash on hand can reduce its leverage ratio, even if it has no particularly positive cash flow. Thus, the second question is whether managers actively delever when their cash holdings are high. Of course, an empirical "cash-holdings vs. active leverage changes" relation would again hardly qualify as strong evidence in favor of an optimal-behavior productivity shock theory of capital structure. Any theory in which firms pay more interest on loans than what they can earn on cash would suggest that managers should pay off some of their own debt — they would lend money to themselves instead of to others. Similarly, any theory in which well-performing firms both pay down some debt and accumulate some cash can predict this. The middle panel in Figure 1 shows that there is no relation between *active* managerial changes (dca) and cash holdings normalized by capital — the R^2 is practically zero.[18]

The third question is whether managers actively delever when their cash holdings increase. Omitting the obvious alternative explanations, the right

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[18] In other specifications in earlier Compustat samples, I obtained a T statistic of –11 and an R^2 of 0.0008. The point of my test is simply to show that the relation is very weak and noisy, not that it is not negative.
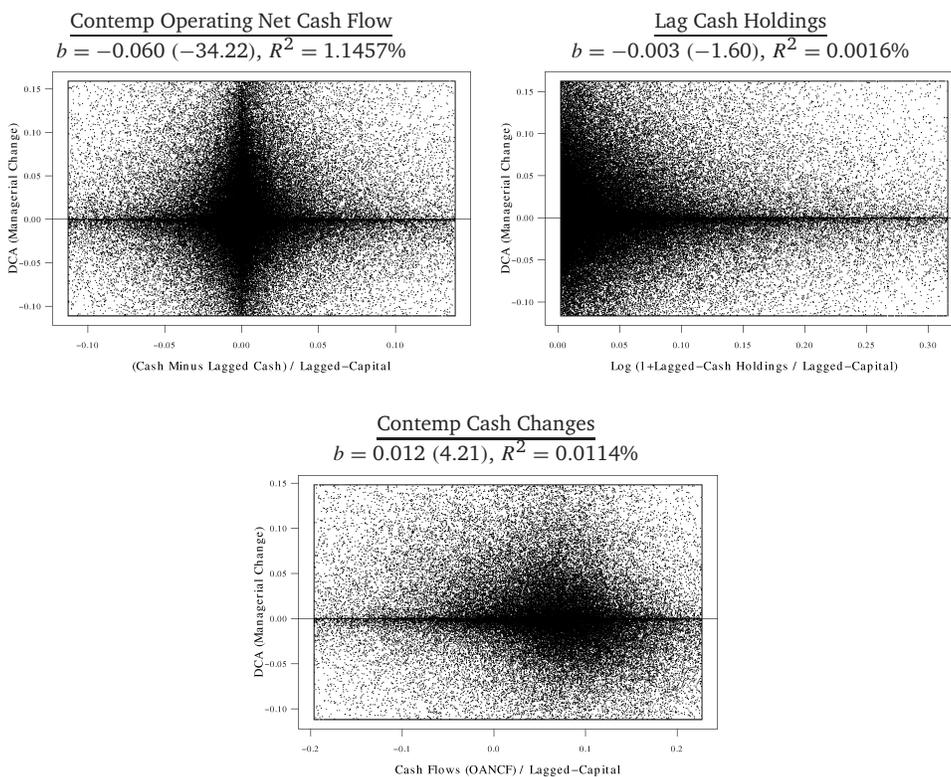
Figure 1. Managerial changes of debt-to-capital ratios (dca, in %) vs. liquidity.

Description: Sample and variables are described in Table 1. In brief, the independent variables are measures of liquidity. The dependent variable (dca) is the active managerial capital structure change. T-statistics are in parentheses following the coefficient.

Interpretation: The plots suggest that knowledge of how "cash rich" firms are does not explain much of managerial capital structure actions. Even the best of the three variables, *contemporaneous operating net cash flow*, leaves 99% of managerial capital-structure activity unexplained.

panel shows that there is no relation here either, with an R^2 that is practically zero, too.[19]

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[19] When the dependent variable is dct instead of dca, the R^2 increase to 0.42%, mostly explained by the fact that firms that have a good year experience increases in both their cash holdings and their stock prices.

It is important for the reader to realize that, even if the explanatory power had been superior, for such empirical evidence to move beliefs greatly in favor of Hennessy and Whited (2005), would also have required a belief by the reader that other variables — orthogonal to productivity or not — would not have influenced liquidity (M). This is simply not plausible.

Therefore, as already mentioned, my constructive suggestion for future tests is to search proxies for empirical productivity shocks. For example, the introduction of GPS in the shipping industry (Hubbard, 2000) is less likely to be correlated with other forces listed in Section 1.1.1 than liquidity is (the indirect "first-order-condition inversion approach"). Such tests would seem to speak more to whether productivity influences capital structure than tests that relate liquidity and capital structure. Note also that a productivity proxy could be used to test not only the hypothesized influence of productivity on capital structure, but also the influence of productivity on liquidity.

Hennessy-Whited continue

> 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.

In its most stringent interpretation, this is the hypothesis that capital structure has a persistent component — that leverage ratios are not independent draws. It is difficult to think of any theory in which this would not be the case, and such tests would be uninformative.

A more relaxed and thus more interesting interpretation is the question of how levels of capital structure correlate with changes of capital structure. The prediction then is the exact opposite of readjustment, although it may apply only to high-debt firms. The left panel in Figure 2 shows that the empirical relationship is negative.[20] Still, it leaves 96% of managerial capital structure actions unexplained. Another interesting question is whether there is much hysteresis in dca itself. The right panel shows

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[20] The reader should not draw a strong inference from the negative sign. Iliev and Welch (2010) document that the relation between lagged leverage and current leverage is very weak. The negative pattern in the right panel here may well have come about only [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 the past leverage ratio.
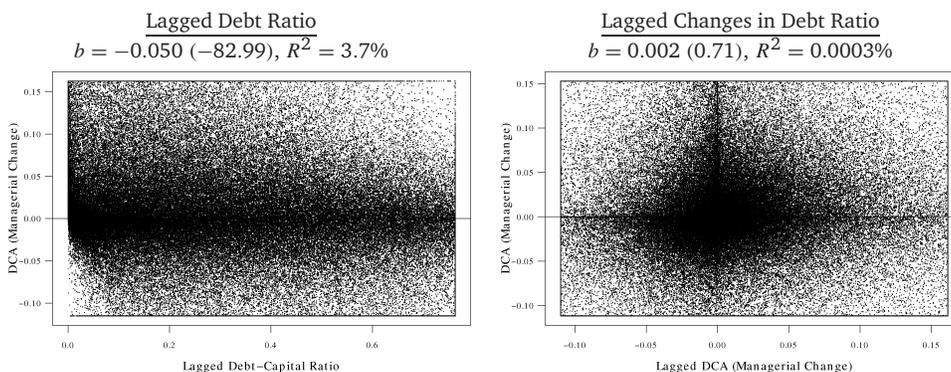
Lagged Debt Ratio
$b = -0.050\ (-82.99),\ R^2 = 3.7\%$

Lagged Changes in Debt Ratio
$b = 0.002\ (0.71),\ R^2 = 0.0003\%$

Figure 2. Hysteresis and managerial changes of debt-to-capital ratios (dca, in %).

Description: Sample and variables are described in Table 1. In brief, the independent variables are lagged measures of capital structure (left) and managerial capital structure changes (right). The dependent variable (dca) is the active managerial capital structure change. T-statistics are in parentheses following the coefficient.

Interpretation: The left plot suggest that knowledge of firm's lagged capital ratios has marginal explanatory power for managerial capital-structure activity, but it leaves 96% unexplained. Note that this is optimistic, because firms that failed to reduce their leverage so may have gone to a 100% debt-ratio (bankruptcy) and dropped out. The right plot shows that there is no hysteresis in managerial capital-structure activities — to predict managerial leverage-related actions, past managerial leverage-related actions are not very informative.

that this is not the case. Managers' actions from the previous year are largely uninformative about their current actions.

2.3 Assessment and Further Challenges

The empirical evidence suggests that even if liquidity has a marginal influence and even if there is some hysteresis, simple measures of either leave almost all capital-structure relevant managerial activities unexplained. This does not mean that Hennessy and Whited (2005) can be rejected. If the productivity link to liquidity and lagged leverage is noisy, the theory may even predict weak relationships among reduced-form observables. My point is therefore not that the data reject the model. Instead, my points are (1) that the correlations between the empirical variables are not so strong as to give the reader great confidence that the model has captured the first-order determinants of managerial capital-structure decisions; and (2) that, even if the correlations had been strong, they could have easily been driven by very

different forces that are orthogonal to the productivity forces assumed to be important. To the extent that the burden is on a theory — in this case that unmeasurable productivity differences systematically drive the observed correlations — the existing empirical evidence in favor of Hennessy and Whited (2005) seems almost non-existent. In the absence of better empirical support, the Hennessy and Whited (2005) model should not be a primary lens through which capital structure should be viewed.

Hennessy and Whited (2005) also did not consider other challenges enumerated in the previous section. Their paper focused on specific forces (productivity, funding needs, and taxes) *a priori,* and thus ignored many other potentially important forces. Liquidity and leverage may well be related to other economic forces that were described in Section 1.1.1. Hennessy and Whited (2005) offered few controls or residual diagnostics. It did not also consider the effects of selection and survivorship biases. It omitted firms with low assets, capital stocks, or sales, leaving only about 20% of the total Compustat population in its tests. It did not offer an estimation in differences. It did not attempt to reduce the depth of its structure. As explained, although it is true that productivity is difficult to measure, even a bad direct measure could have helped pin down productivity as the most likely influence (compared to what is, in effect, no direct empirical measure). It offered no in-differences evidence — when *changes* in leverage ratios would be expected. It offered no quasi-experimental tests. As mentioned in footnote 11, many sudden, researchers could work with large changes in productivity — varied across different firms and industries in different years — and changes in tax rates. Such evidence could greatly aid the reader in assessing the empirical validity of the theory. It offered no out-of-sample evidence. It has not offered convincing specific evidence in the model's favor to distinguish it from other models and there are no papers on the horizon that are likely to provide such specific empirical evidence.

3   Quantitative Modeling

Strebulaev (2007) is a quantitative model that puts forth a dynamic version of the tax-distress-friction tradeoff theory in Fischer *et al.* (1989) in which corporate taxes favor debt, distress costs favor equity, and frictions favor inaction. In addition to its quantitative aspects, Strebulaev (2007)

points out that the tax-distress-friction framework predicts that when firms experience positive profitability shocks, more profitable firms can have relatively more equity in their capital structures because it is optimal that they do not adjust their capital structure (due to transaction costs). This correlation was widely and incorrectly viewed as inconsistent with the tradeoff theory. Although this is an important insight in itself, because this intuition can also be obtained in a one-page sketch model (or using Fischer *et al.*, 1989), it is usually the quantitative aspect of Strebulaev (2007) that is viewed as its hallmark.

3.1   A Brief Sketch of the Strebulaev (2007) Model

In Strebulaev (2007), firms can optimize their capital structures dynamically. The complexity of the model is an order of magnitude higher than that of its predecessor (Fischer *et al.*, 1989). It has 23 parameters and 9 (non-linear) equations, many with unreducible integrals and embedded optimizations. The economic intuition of the model is easy to follow. However, its algebraic intuition is not, because the model has no closed-form solutions or comparative statics. Although the Strebulaev model conveys some intuition through numerical solutions of reduced-form variable, in terms of economic intuition, it is a step backwards from its predecessors. Yet, in exchange, its quantitative formulation also offers great advantages in allow a more efficient use of the data. The model can be tested not only more precisely on specific magnitudes, but even in cases in which the comparative statics are not unambiguous. Qualitative models often simply ignore ambiguous comparative statics in empirical tests.[21] Of course, relative to qualitative directional testing, quantitative tests must lean harder on the model and its empirical proxies in order to identify an exact location in the parameter space.

In sum, in this and in many other quantitative models, it is often the case that at the cost of loss of reader's intuition, a quantitative approach can take the model more seriously, be more ambitious, and if successful, be more empirically useful than a qualitative model.[22]

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[21] On occasion, qualitative tests do multi-moment testing — for example, predicting that one moment has a certain direction only if another moment has a certain direction.

[22] Calibration can serve two purposes: it can help assess whether a model's effects are of the right order of magnitude, and it can help assess whether the model *alone* can explain the empirical evidence. The latter is more objectionable than the former.

3.2 The In-Sample Performance of the Tax-Distress-Friction Model Revisited

This section shows that the basked into which the Strebulaev (2007) theory has put all its eggs (the tax-distress-friction model) has a hole at the bottom. The data suggest that the tax-distress-friction model cannot possibly explain the empirical non-adjustment evidence in corporate leverage ratios that motivated it. My evidence speaks to my first point — that papers in this genre are not built on forces that are known to be the main first-order mechanisms determining capital structures. Although such a critique must be specific to one model being critiqued, it is not difficult to find similar problems in other papers, too.

The principal mechanism through which the tax-distress-friction model explains non-adjustment is inertia. Firms that experience shocks are reluctant to adjust their capital structures, because the costs of doing so exceed the benefits. In the theory, firms' inertia causes non-adjustment. Although inertia implies non-adjustment, non-adjustment does not imply inertia. If managers randomly increase or decrease their leverage ratios following shocks, they do not adjust — but they are also not inert. The empirical evidence that Strebulaev (2007) claims in support of the model are about non-adjustment, although the prediction of the theory and thus his interpretation is that there is inertia. Inertia is not a "nuisance moment." On the contrary, it is the primary cause of *all* the other quantitative implications in the Strebulaev (2007) model. This section will now provide evidence that firms are not inert but that they are very active — the variance of managerial leverage changes is very high — and, moreover, their managerial capital structure activity does not push them towards readjusting.

The tax-distress-friction inertia hypothesis predicts the following:

1. dca, which are all changes caused by managerial issuing and repurchasing activity, should be low most of the time. Strebulaev quantifies its magnitude: active capital structure changes (dca) should be 0% approximately 85% of the time, and large (10–50%) the rest of the time.

   Figure 3 shows a histogram of dca. There is indeed a small spike at 0% leverage. (Because leverage is defined on the unit domain and 20% of all firms have zero leverage, with many firms remaining at 0 for a long time, dca can of course not be normally distributed.) However, the spike is nowhere near the 85% predicted by the Strebulaev model. There is lots of managerial activity at about −3% and +3%.
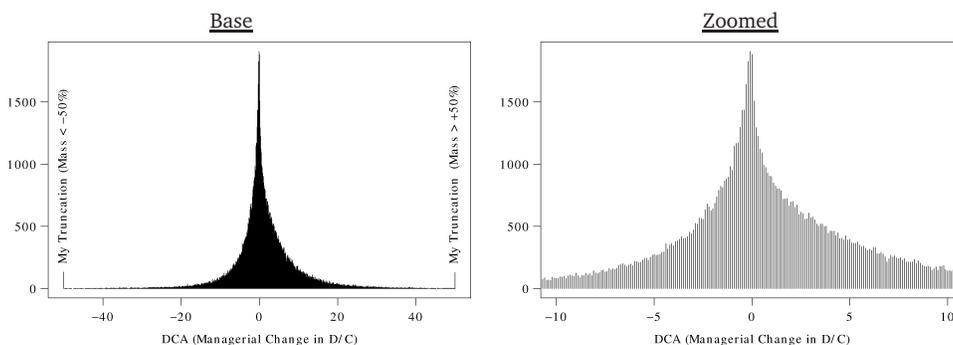
Figure 3. Histogram of managerial changes to debt-to-capital ratios (dca), in %.

Description: Sample and variables are described in Table 1. dca are active managerial changes to capital structure.

Interpretation: Inertia (lack of activity) is not a first-order characteristic of managerial behavior. There is plenty of activity off the zero-center.

Table 2 provides replicated model and actual empirical statistics. The model is a variant of Goldstein *et al.* (2001) and Strebulaev (2007), and generously made available to me by Ilya Strebulaev. His original model used a 5% friction parameter,[23] the second data column in the table. A number of moments in this Goldstein-version are different from the original Strebulaev paper and the current data, because this version of the model has stronger stationarity requirements. For our purposes, the important moments are those on dca. More than 90% of firm-year observations should be exactly zero (without managerial adjustment), with the remaining firm-years seeing very large changes.[24] In contrast, in the data, more than 90% of firm-years have dca's that are *not* zero. The distribution of changes seem quite smooth — nothing like the rare sudden changes prescribed by frictions. This may not be surprising. After all, credit lines and debt and equity repurchases may well make adjustments fairly cheap. However, this is precisely contrary to the friction assumption in the model.

At this point, the friction aspect of the specific model in Strebulaev (2007) can be viewed as inconsistent with the data. dca contains a lot of activity,

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[23] As the Strebulaev and Whited response (Strebulaev and Whited, 2013) will point out, I misinterpreted the number in my eagerness to match the IDR coefficient.

[24] 10% of non-zero firm-years are large enough to produce an unconditional standard deviation of about 10%!

	Unwinsorized					Winsorized at 5% and 95%			
	Min	Median	Max	Mean	Sdv	Range	Mean	Sdv	N
dca (excl. divs)	−99.6	0	98.5	0.9	10.8	−11.5..16.3	0.9	6.3	179,917
Contemp dct	−99.6	0	98.5	1.2	13.7	−18.6..24.1	1.2	10.2	179,954
Contemp dcp	−66.1	0	90.4	0.3	8.8	−12.4..15.5	0.1	6.5	179,954
Contemp log(1+r)	−601.8	2.7	519.1	−2.9	59.7	−106.1..81.8	−2.0	47.8	179,954
Lagged dca	−99.6	0.0	98.5	1.0	10.5	−11.0..16.2	1.0	6.2	162,480
Lagged dc	0.0	21.8	100.0	27.8	25.3	0.0..76.3	27.4	24.3	179,954
Lagged cash	0.0	3.9	274.9	8.2	12.7	0.2..31.6	7.3	8.5	158,928
Contemp cash changes	−965.8	0.1	219,229.4	4.9	637.6	−11.3..13.9	0.5	5.4	154,941
Contemp operating CF	−4,248.8	5.7	30,195.0	5.5	131.2	−19.6..22.7	4.4	10.3	101,032

Table 1. Descriptive Statistics of Capital Structure Changes, in %.

Description: The capital structure change variables dca, dct, and dcp are defined in (1) on page 141 and quoted in percent. In brief, dct are total debt-to-capital ratio changes, dcp are the part due to capital gains, and dca are the part not due to capital gains, that is, caused by managerial actions. The dependent variable is usually dca.

dc is the market-value-based debt-to-capital ratio. Cash is Compustat CH. Operating Cash flows are Compustat OANCF. Cash, cash changes, and operating cash flows are normalized by lagged financial capital (debt plus market value of equity). Lagged cash is also quoted in "one-plus logs." *Contemp* means contemporaneous with dca. *Lagged* means at the beginning of the period for stock variables dc and cash, and one year-lagged for flow variable dca.

The sample is all firm-years from Compustat from 1963 to 2007, excluding firms with lagged financial capital of less than $1,000,000. All reported R^2 in subsequent tables are from regressions on the winsorized variables, and all figures display the winsorized observations at their borders.

which rejects the (tax-distress-) friction perspective. (Welch, 2004, Table 5) further shows that managerial activity occurs in many different claims, too: about half is long-term debt related, about a third is short-term debt related, and about ten percent is equity related.) And the evidence provided here is conservative. Managers could also be very active within the categories themselves. For example, if managers exchanged one form of debt for another (Rauh and Sufi, 2010), or if managers repurchased and reissued equity, or if managers changed debt and equity proportionally, such activity would not even show up in dca as it was defined here.

Again, at this point, one can object that other factors, such as precommitted ESOPs or sinking funds could help explain these year-to-year changes. This is correct, but it is also precisely *not* in the Strebulaev (2007) model. The omission of other forces leave most of the variation in managerial leverage structure activities unexplained.

Although the central friction aspect is already rejected, we can examine the readjustment aspect under the additional assumption that the optimal tax-distress-induced leverage ratio target is moving slowly. This assumption is reasonable for most publicly-traded corporations. Many theories and tests have even assumed this to be perfectly static from year-to-year. Unfortunately, the data also strongly reject the readjustment aspect:

2. The variability of total capital structure changes (dct) should be lower than the variability of capital structure changes in the absence of managerial intervention (dcp). That is, managers should use their capital structure activity to dampen swings in leverage, not worsen them. In Table 2, the calibrated model estimates suggest around -30% as a reasonable correlation between dca and dcp.

    Table 1 shows that $\mathrm{sd(dct)} \not\le \mathrm{sd(dcp)}$. In fact, dca and dcp are approximately orthogonal. Thus, the variance of dct is about $\sqrt{2}$ times the variance of either.

    When stock returns reduce the leverage ratio (dcp $\ll$ 0), managers should work, on average, to increase it again (dca > 0). Conversely, when stock returns increase the leverage ratio (dcp $\gg$ 0), managers should work to decrease it again (dca < 0). These are "on average" predictions, not "in all cases" predictions.

    Table 2 and Figure 4 show that there is no systematic response of managers to stock price movements.

In sum, the empirical evidence strongly suggests not only that managers are very active, but also that they do not use their activity to dampen

	Tax-Distress-Friction Model γ					Current Data
	1%	5%	10%	30%	∞	
Current Debt QMLC						
Mean	59.9%	55.6%	50.5%	38.1%		**28.5%**
Sd	5.9%	9.9%	12.4%	15.3%		**26.4%**
Frac $= 0$	None	None	None	None		**0.120**
Welch Regression Predicting QMLC (with unreported constant)						
QMLL	−0.36	−0.38	−0.45	−0.50	0	**–0.042**
IDR	0.56	0.93	1.147	1.343	1	**0.945**
dca $\equiv$ QMLC–IDR						
Mean	0.034	0.030	0.026	0.020	0.00	**0.010**
Sd	0.074	0.095	0.101	0.099	0.00	**0.108**
Frac $= 0$	83%	91%	94%	96%	100%	**8%**
					Frac $\leq$ 1%	**31%**
					Frac $\leq$ 2%	**45%**
					Frac $\leq$ 3%	**55%**
Correlations						
corr(dca,dcp)	−40.5%	−30.9%	−27.7%	−26.3%		$\geq$ **0**
corr(dca,dcp$_{-1}$)	−39.3%	−25.4%	−18.6%	−10.0%		$\geq$ **0**

Table 2. Model Moments and Data Moments.

Description: The simulations are based on a model similar to that in Strebulaev (2007) (based on Goldstein *et al.*, 2001), and kindly made available to me by Ilya Strebulaev. The friction parameter γ closest to the original model is 5%. Simulations are based on 3,000 firms and 140 quarters. The Welch regression is run as a simple pooled regression, which delivers practically the same results as the Fama-Macbeth regressions in the original paper.

Interpretation: dca adjustments are too large (and too smooth) to warrant the friction-induced discrete characterization of capital structure changes. Moreover, the correlations between dca and dcp suggest that managers do not change their capital structures primarily to stabilize their capital structures.

fluctuations in their capital structures. Instead, managers add to capital structure volatility. What is important is that for any future theory that is based on a moving target to explain the evidence, this target must not
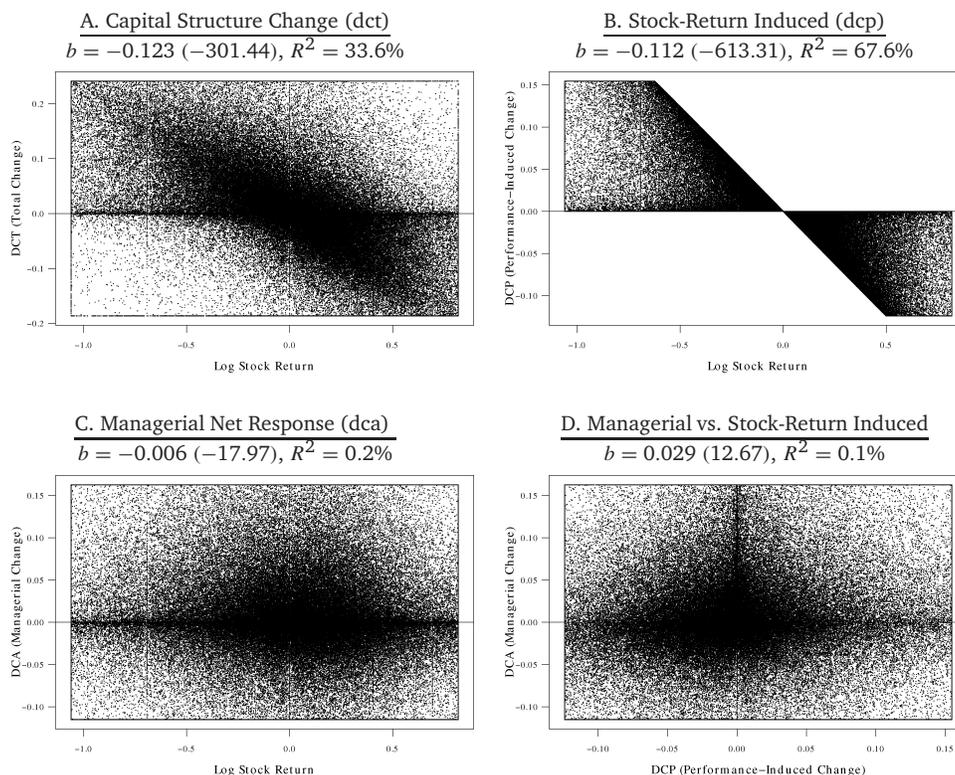
A. Capital Structure Change (dct)
$b = -0.123\ (-301.44),\ R^2 = 33.6\%$

B. Stock-Return Induced (dcp)
$b = -0.112\ (-613.31),\ R^2 = 67.6\%$

C. Managerial Net Response (dca)
$b = -0.006\ (-17.97),\ R^2 = 0.2\%$

D. Managerial vs. Stock-Return Induced
$b = 0.029\ (12.67),\ R^2 = 0.1\%$

Figure 4. Changes of debt-to-capital ratios, in %, and stock returns.

Description: Sample and variables are described in Table 1. In brief, the independent variable in Panels A through C are contemporaneous stock returns. In Panel D, it is the change in capital structure caused by these stock returns. The dependent variable (dca) are active managerial capital structure changes. (Horizontal bands at zero in Panels A, B, and C occur because about 10% of all firms had zero debt, so their leverage ratios cannot be affected by stock returns.) T-statistics are in parentheses.

Interpretation: Panel A illustrates that stock returns explain at least one-third of the year-to-year variation in capital structure. Stock returns are a first-order determinant of capital structure changes. Panel B illustrates stock returns do not explain 100% of stock-return induced leverage variation, because firms' own lagged capital structures modulate the effect of the same stock return differently for different firms. Panels C and D illustrate that, although there is significant managerial capital structure activity (wide spreads in dca), there is no clear directional managerial counteracting response to stock returns.

only show tremendous variability, but the target's movements must also be orthogonal to what stock returns do to firms' leverage structures. This simply does not seem in line with a wide range of existing theories. A different

way to state the issue is that average outcome leverage is quantitatively too eerily close to what stock returns do in the absence of managerial intervention. It is highly unlikely that these revised capital structures also happen to be on the numerically exact target. The evidence suggests that managers are actively maximizing something that we have not yet identified.

In sum, even if managers are less capital-structure active than they would be in the absence of capital-structure transaction costs, inertia is simply not a first-order determinant of capital structure. Managers are active. Whatever their motives, managers show no interest in dampening the volatility of their leverage ratios.

3.3   Assessment and Further Challenges

Like all tax-distress-friction models, the Strebulaev model is fundamentally incapable of explaining high levels of non-adjusting managerial activity. Nevertheless, Strebulaev (2007) declared victory based on two moments — a negative correlation between stock returns and leverage, and a negative correlation between productivity and leverage — even though the theory's causal link from inertia to non-adjustment is absent. Thus, the model fails even the weakest test, in-sample scrutiny.

Strebulaev (2007) also did not consider the other challenges enumerated in Section 1. It focused on the three tax-distress-friction forces *a priori*, and thus ignored a host of other potentially important forces. The forces it chose seem weak. It is not clear that the other forces mentioned in Section 1 are orthogonal to those in the model, nor is it clear how they would affect the inference. Strebulaev (2007) offered few controls or residual diagnostics. It did not consider the effects of selection and survivorship biases. Its reduced-form inference was based on 75 years of simulated quarterly data, which is a marked contrast to the average 10-year life span of publicly traded firms. It did not use mid-year stock returns to predict impending disappearance. It offered no estimation in differences, which in this context would have meant using the model's prediction to test exactly when large changes in capital structure were expected vs when they occurred. It offered no quasi-experimental tests. Good quasi-experimental tests of the tax-distress-friction theory are readily available — all three ingredients (taxes, distress costs, frictions) experienced regulatory changes in recent decades that can be viewed as exogenous from the perspective of individual corporations. If anything, quantitative models are better suited than qualitative models

to tests using such natural experiments. For example, specific changes in corporate income tax rates should have led to specific quantitative changes in leverage ratio. (Givoly *et al.*, 1992) investigated the 1986 Tax Reform Act.) The question then is whether the Strebulaev model can predict the quantitative response better than a more naïve capital-structure DTF model.

4 Contrasts in Perspectives

The following three simple empirical regularities about managerial capital structure activities have been defying explanation:

1. Managers are active in issuing and retiring debt and equity.
2. This managerial (net) capital structure activity is orthogonal to the (non-linear) influence of stock returns on leverage ratios.

Combined with the facts that stock returns have significant volatility and that there is variation in lagged leverage ratios, the first two regularities imply that stock returns can explain about 45% of the variation in year-to-year market-leverage changes. (Long-term debt net issuing can explain about 30%, other debt changes can explain about 20%, and equity net issuing can explain about 5%.)

3. Although there are many statistically significant variables, some with large economic coefficients, no known variables can explain much of the variation (R^2) in managerial capital structure activities.

In my opinion, managerial capital-structure behavior either largely remains a mystery and/or is inconsequential most of the time (similar enough to the extreme form or Modigliani and Miller (1958)).

In contrast, the two models that were described above conclude with much more optimistic perspectives. Hennessy and whited (2005) write

> 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) writes that

> 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.

Similar confident sentiment can be found in many other structural corporate finance papers. For example, Li *et al.* (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.[25]

My assessment is more pessimistic. In practice, deep-structure theories in corporate finance, with their wide gaps between structure and reduced form, have only passed empirical tests that would be analogous to judging qualitative reduced-form theories by the t-statistic of a selected variable in an *in-sample* regression, without controls for competitive explanations and confounding variables, and without diagnostics and corrections for a whole range of possible misspecification errors. Moreover, some of these theories were designed to explain a particular correlation observed in earlier papers. In such cases, viewed from a global perspective, the research approach was analogous to searching for a good t-statistic first, followed by finding support for a specific model constructed to deliver this statistic. The models did not consider alternatives more powerful than the simplest of strawmen. In sum, all existing tests of deep-structure quantitative theories that

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[25] LLZ reach this conclusion based on the fact that their model can be calibrated in-sample to some of the moments in the data, although it fails on others. The 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" and "not C." The correct conclusion is "not A." In a deep-structure theory, such a skeptical approach is all the more important. 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. Moreover, LLZ 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.

I am aware of have been perfunctory. And none of these models have ever been tested quantitatively or structurally (rather than in reduced-form) by a paper following up. Abusing the common acronym WYSIWYG, what you see may well be all what you will ever get.

In my opinion, it is no surprise that there have been no quantitative and no deep-structure models that have performed well in explaining empirical corporate finance data. Given the challenges, it would have been a miracle if they had. For now, much managerial capital-structure activity and many other corporate actions largely remain mysteries. We still need to learn what the first-order effects are — because the noise is deafening. The problems discussed in my paper suggest to me that instead of building quantitative and/or deep structure models now, greater rewards can be reaped by making progress on two more basic issues first:

1. Empirically, we need to learn what the first-order associations are. This is mostly an exploratory and descriptive exercise, though it should be theoretically informed. The initial goal can be to learn which variables have a (possibly linearized) strong association with the dependent variable. This is a question of the economic magnitude (and the statistical standard error) of the coefficient. The more ambitious goal is to learn which variables can explain much of the variation. This is a question about the R^2 in explaining the dependent variable.
2. Empirically, we need to learn whether there is good evidence that changes in such variables associate with changes in the dependent variables (usually a managerial action). This applies both to variables that seem economically significant in levels, and to variables that seem to explain much of the variation (R^2) in changes.
3. Empirically, we need to determine which of the first-order variables exert some influence in simple quasi-experimental tests.

In the capital structure arena, the appropriate dependent variable to understand managerial behavior is the managerially-induced change in capital structure, not the stock-market (or accounting) induced total change in capital structure. Surveys of the actual decision-makers, such as those in Graham and Harvey (2001), can help inform this research.[26] Once we have

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[26] Unfortunately, Graham and Harvey neither correlate survey answers with the actual capital structure actions of these firms, nor do they debrief their CFOs when their actions have obviously deviated from their stated motives.

a better idea, it will be more productive than it is today to build more elaborate theories, including quantitative and deep-structure theories, and to put them to the test, taking advantage of the many quasi-experimental tests that the corporate finance context offers.

5 Beyond Corporate Finance

Quantitative and structural modeling has also been popular outside of corporate finance. As mentioned in the introduction, Angrist and Pischke (2010) and Caballero (2010) point out related issues in industrial organization and macroeconomics (though with different perspectives and conclusions). It is a natural question to ask whether asset pricing is better suited to quantitative and deep-structure modeling than corporate finance. There are some differences:

- The data frequency in asset-pricing is often much higher. This can make it easier to uncover the correct mechanism.
- Testing approaches have been different. It has not been uncommon in asset pricing to conduct out-of-sample testing. However, there have been few or no attempts at conducting good and more stringent quasi-experimental tests. On the one hand, this may be because, in contrast to corporate finance, there are few natural experiments in asset pricing that come to mind. For example, it is difficult to think of a clearly identifiable exogenous shock to factor exposures. On the other hand, it seems ironic that micro foundations are as popular in structural theory modeling, as they are unpopular in the empirical tests of these models. For example, habit formation has been a popular modeling primitive, but using individual trading data to measure these habit preferences within the same data set that is used to test the expected return predictions of habit formation has not been equally popular. Of course, the proper conclusion from the lack of a tight link between unobservables and observables (the large gap) and from the lack of quasi-experimental tests is not that we can adopt such models even in the absence of such evidence, but that we should view the empirical evidence as very weak. We need to remain even more skeptical of them.
- Market forces in asset pricing are stronger than they are in corporate finance. When agents make mistakes, unusually good investment opportunities can push prices back toward their correct values. Arbitrage can

in effect convert deep mechanisms into shallow ones. If the mechanism is known, the first point of my critique is no longer applicable. Not surprisingly, structural models in asset pricing have generally performed well where arbitrage bounds are fairly strong, such as in derivatives pricing and possibly fixed-income pricing with a strictly timed final convergence condition. (In the extreme, this is the domain that Summers (1985) termed "ketchup economics.")

Near-arbitrage bounds are much weaker in equities. Quantitative structural models (usually based on risk and market imperfections) have thus also had a poor track-record explaining even first-order empirical regularities, such as the superior performance of value and momentum stocks. Without strong arbitrage-bounds, further structural "enhancements" to the equity pricing models run the risk of overfitting the data — creating more flexible reduced forms that can be attributed to many models beyond the original structural model itself. At some point, we, as a profession, need to ask the question more strongly, more pointedly, and more insistently, whether our consumption-asset pricing paradigm has become too much like the Ptolemaic geocentric paradigm — a theory that is based on our strong priors of how agents *should* behave and that survives only because of our efforts to make the theory more realistic through the addition of ever-more complex epicycles. The alternative may well be simple: agents may just not choose which stocks they purchase based on their covariances with their own future wealth shock contingencies, even if they should.

6   Conclusion

It is tempting to infer from the increasing number of award-winning quantitative and deep-structure corporate finance papers that we have largely settled on theories and modern modeling approaches, that there is good supporting empirical evidence, and that we only need to develop these models further if we want to understand corporate behavior better. Eventually, with more bells and whistles and enough degrees of freedom, the models should fit.

My paper has disputed this. It has argued that no quantitative and (deep) structural model has been subjected to as much empirical scrutiny as the best alternatives. None have succeeded in empirical tests that were more

than perfunctory. Few if any were tested on their own more sophisticated levels by follow-up papers. Follow-up empirical tests have only been either more generic and reduced-form or absent altogether. In the future, quantitative and deep-structure models deserve at least as much scrutiny as simpler models. The modern standard for testing economic theories is quasi-experimental, and quasi-experimental tests are often just as feasible in structural and quantitative contexts as they are in alternative approaches. Moreover, direct empirical proxies for underlying constructs can make tests more credible than correlations among variables that are very different from the hypothesized force and tied to them only by assumed behavioral first-order conditions. In effect, theories which do not provide tests based on better proxies and based on quasi-experiments will remain forever in the realm of conjecture. They remain empirically meaningless.

To be clear, my paper has not argued in favor of theory-free empiricism. Models help inform empiricists where to look and where interpretation pitfalls loom. However, my paper has argued that compared to the more ambitious "comprehensive" models, given our current stage of knowledge in corporate finance, comparative statics from simple "marginal" sketch models with quasi-experimental tests seem better suited to learning what the most important forces. My paper has also not argued against the presence of first-order conditions in models, but it has argued against relying on their inverted forms to infer (identify) causal effects without more direct empirical proxies — when it is likely that these first-order conditions leave a wide gap between an unobserved variable and the reduced-form variable. Finally, my paper has not argued that quantitative and deep-structure models are intrinsically inferior. On the contrary, if such models can explain the data better than their simpler counterparts (and my paper has made some specific suggestions on how to improve on this aspect), then these models can be more useful despite the additional cost to the reader to understand their economic intuition. My paper has however argued that quantitative and deep-structure models are more likely to succeed in simpler settings, in which the first-order mechanisms are well understood. In financial economics, this is really only the case in situations in which there is strong arbitrage. Therefore, I have little hope that structural and quantitative approaches can succeed in corporate finance. However, reasonable people can disagree with my pessimistic assessment. I would be thrilled to be proven wrong.

A.1 Defining Structure

From the perspective of my critique, it is not essential how structure is defined. My arguments are about the lack of a strong link between unobservable constructs and the tested measured constructs. To clarify some ambiguity in different authors' interpretations, this appendix briefly describes three definitions of "structure."

One common definition of a structural model is that it is one whose estimated equations are the result of a first-order condition. The standard example of a reduced-form model to contrast with a structural model is Fama and French (1992). This is not a useful definition for my paper. It is not impossible that some as-yet-unknown model exists whose first-order conditions imply exactly the Fama-French specification. Thus, the model could be reduced-form in the minds of some researchers, but structural in the minds of others. The same published model with the same specifications and data could be reduced-form today and structural tomorrow. There is not even a guarantee that *any* reduced-form models exist in this definition.

Another definition of structure, due to Lucas himself, is a self-fulfilling rational-expectations model. Here the Fama-French model is not structural if it is the case that the relation would change if investors used the model itself. In the strict Lucas sense, a model is structural if only preferences and technology are specified. And in this very strict sense, there are *no* structural models in corporate finance.

Yet, another definition of structure might emphasize the functional form of the relationship. However, functional identification leans so strongly on the model functional input assumptions that it has fallen out of favor.

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