Critique of Quantitative and Structural Modeling (Capital Structure)

MIT Lunch Presentation

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- Too much for one presentation. Slides posted on http://research.ivo-welch.info/.
- Always present my rawest research at MIT. No exception.

The following paper has been rated

Viewer Discretion Advised

History of Economic Models

A "History of Economic Modeling"

- Non-algebraic textual economic models ($\approx$ pre WW-II)
- Algebraic Models (Samuelson 1947, $\approx$ post WW-II). Econometrics.
- Lucas Critique (1976): Econometric reduced-form models failed for policy evaluation—we need "structural models" (SMs) to predict counterfactuals out of sample.

  Counterfactuals here are policy changes.

- Mehra-Prescott (1985): Quantitative aspects are important.

A flippant "Recent History of Economic Modeling"

- Labor, IO, Trade, Asset Pricing, and now CorpFin got "envious" of what feels more like "real science," and started building "real models" instead of "toy models," too.

Economic Model Criteria and Success Metrics

- Desirable Model Features

  (i.e., should make sense, and aid insight (Understanding))

- Ability to Explain Economic Phenomenon

- Large subjective aspects in viewing models, especially with respect to choice of prior model. Yes, even "science" is "art."
- Some but less subjectivity in viewing explanatory power.

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Now Model Features (Classifications).

Then Model Testing

Then Corporate Finance

Then Specific Corp Fin Models — Strebulaev and Hennessy-Whited

Model Attributes

To talk about differences in model, I first define some model attributes.

- Simple vs. Complex Models
- Qualitative vs. Quantitative Models
- Derived-Relationship vs Structural Models

To be explained in more detail soon.

Conceptual Question:

- Is scientific progress towards more complex, quantitative, structural models inevitable?
- Or is there an optimal interior choice?

Simple vs. Complex

- Complexity is *not* mathematical sophistication in finding solutions. Complexity is tough to define. Maybe

  intuitiveness of "input $\longrightarrow$ output" relations

- Simpler is *not* always better. No complexity $\Rightarrow$ no model.
- Real life may not be simple. (DNA coding.) It may take (more) model complexity to explain an economic phenomenon.
- Still, Occam's razor (and Hannibal Lechter) suggests the simplest model for the economic phenomenon to be explained.
- But there is a cost to a reader to comprehend complex models.

  Complexity limits model appeal to general readers.
  (Ironically, complexity may help referee appeal.)

- Simplicity is related to model esthetics, but they are not the same.

  (Mechanism design can be beautiful, but it is rarely simple.)

(Referee considerations: Is simple "obvious"? Is complex "deep and insightful"? Also, does "difficult" signal author commitment and capabilities?)

Qualitative vs. Quantitative

Qualitative Model	⇔	Quantitative Model
Often only Full-Domain Unambiguous **Qualitative** Statics		Often (calibrated) parameter-specific **Quantitative** Effects
common when comparative statics can be solved and signed.		common when comparative statics are non-algebraic or ambiguous.
real life statics may not be unambiguous.		requires great faith in model. strong lens to view data.
ignoring ambiguous comparative statics ignores potentially valuable model predictions. some models have qualitative statics that depend on other qualitative statics.		uses model (insights) more efficiently
less sensitive to unmodeled factors (sensitive only if they reverse comparative statics)		more sensitive to unmodelled factors. (omitted factors typically shift locations/effects.)
commonly "partial" (marginal) complementary view of model predictions.		commonly (or naturally) requires "exclusive" view of world
"Easy" to add controls (e.g., firm-size)		Difficult to add controls

Qual vs. Quant: Testing Consequences

Common (not universal) Quantitative Testing Approach:

- The model is the null hypothesis.
- Unmodelled specific factors are assumed absent. When not, joint power often attributed to model (residuals are tested on alternative factors).
- Tests of all moments (MLE) or tests of some important moments (SMM) between in-model variables.

  Note: The MLE vs. SMM is not a choice about what to consider a good fit. Of course, MLE will reject. GMM can diagnose why. But MLE is the model's true test. SMM is also much easier to do.

- Occasionally, assessment or test of fit of model.

Common (not universal) Qualitative Testing Approach:

- Less ambitious. Variable need not explain everything.
- Simultaneous competitive test. Joint power often not *a priori* attributed to model, but "fairly" apportioned via econometrics.
- Easier simultaneous control (e.g., Fixed Effects).
- Even small marginal explanatory power is often viewed as success.
- Occasionally, also cross-comparative statics.

Derived-Form vs. Structural Models

- Observed variables vs. unobserved "structural variables."
- Definition: structural model relies on structural variables.
- Every economic model has structure beneath it. Most interesting empirical theoretical constructs are not directly observable.

  Only religion has an ultimate cause.

- The question is how deep the structure is.
  - Shallow: The empirical variable is a measured proxy for an unobserved theoretical construct. (Errors-In-Variables)
  - Deep: A completely different variable, where only complex economic forces (e.g., optimizations) determine the map from observable to unobservable.
- Model tests relate map from one observable back down to an unobservable, then back up to a different observables again.
- If the world is structural, building a structural model (i.e., based on unobservables) may be necessary for (predictive) stability.
  BUT IT MUST BE THE RIGHT STRUCTURAL MODEL. STRUCTURE PER SE DOES NOT GUARANTEE STABILITY.

Cost of Structure

- ...but understand what structure costs you and buys you.
- **The most important conceptual difference:**
  - Model inference in observed-variable models is robust with respect to **uncorrelated** omitted forces, but not robust with respect to correlated omitted forces.
  - Model inference with structural variables is not robust also with respect to **uncorrelated** omitted forces.
    - strong assumption (pretense) of knowledge
    - it may be logically impossible to really ever test unobservables—they are not observed.

Structural vs. Non-Structural Models (Variables)

afterwards, you should say—of course, this was obvious and I knew this. you do. you may even know some technical names. "it's just an example of misidentification." let's just highlight again how big a problem structure is for inference.

- y is dep var. Two indep vars, r (right) and w (wrong).
- True Model:

$$y = r$$

- Orthogonality: Right and wrong forces are orthogonal

$$r \perp w$$

- Simplest Example: Some third variable is sum of the two:

$$Q = r + w$$

Non-Structural Variables

- Assume r and w are observable.
- Researcher believes the wrong model.

$$w \to y$$

- Researcher estimates

$$y \quad = \quad \hat{a} + \hat{b} \cdot w + \text{noise}$$

- Expected coefficient estimate is $\hat{b} \to 0$

  because $w \perp r$

Conclusion:

- Researcher correctly rejects the false model.
- Omitted **orthogonal** forces do not influence inference.
- Inference can only be wrong if r correlates with w.

If Variable is Structural

- Assume r and w are unobservable, but Q is observable.
- The true model is still $r \to y$ and $r \perp w$.
- Researcher believes (wrongly) that underlying structural model identifies

$$(w \to) \qquad Q \to y$$

$$\text{or} \qquad y \leftarrow w \qquad \text{or} \qquad Q = w + \text{Noise}$$

with noise $\perp$ to w. (The noise is r and $r \perp w$.)

- The researcher estimates

$$y = \hat{a} + \hat{b} \cdot Q + \text{noise} = \hat{a} + \hat{b} \cdot (w + r) + \text{noise}$$

- Estimate is $\hat{b} > 0$. Conclusion: researcher accepts false model.

Structural-Form Model on Observables — Inference

- In fact, in this specific case, the inference is the same regardless of whether the researcher's model is right ($y \to r$) or wrong ($y \to w$). It is irrelevant that $w \perp r$.
- Omitted forces orthogonal to w influence inference.

  Any variable that correlates with Q (empirical proxy), not with the model's underlying force w, changes inference.

- Not enough to state that "if other forces are uncorrelated..."
- Stating "if other forces are held constant" literally means requiring zero variation in r (or control for r).
- To conclude that unobs struct force w matters requires **bigger** leap of faith (compared with obsvble variable w).

How to Distinguish Between w and r?

- No empirical test can help distinguish between w and $\perp r$.
- When accepting your model, you need good priors that w was the correct mechanism (or measures of either w or r).
- Ultimately, unless you *know* a priori that Q (not r or w) is orthogonal to f_1, f_2, ..., and/or that w is the right variable, you will accept the model too often.
  - In other examples, more moment tests can help distinguish.
  - You should lean harder on tests—more moment conditions should hold.
    - Even if your model is not rejected by the data, accept structural interpretation with greater caution.
    - If your model is rejected by the data, you can be *really* confident that it is not a good model.
- **Even a bad proxy is better than none.**

“Just an IV issue”?

- If you want, you can call this a misspecified model or a misspecified instrument.

  You can call a nuclear bomb just a weapon, too.

  Naming a problem does not fix it.

- The main problem is ultimately simple:

  Forces that are entirely orthogonal to the structural model’s forces can screw up the inference if they are not orthogonal to observable variables (derived proxies).

- The effect of orthogonal forces on the SM’s inference is very hard to figure out, especially in black-box models, that have to be numerically simulated.

Summary Warning: Inference is More Fragile

Repeat:
Statement: "As long as there are no omitted variables that do not correlate with your force of interest, your regressions will give the correct inference."

- True for observable variables.
- Not true for unobservable variables.

  The bigger the gap between observable and unobservable, the more potential other forces can drive the relation. Pure Proxy = mild. Theory = ?

⇒ universe of problems is **much** larger.

⇒ structural models should be last resort if no proxy exists.

⇒ tests should be *more* skeptical and more stringent.

⇒ tests should hold constant as many forces as possible.

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Done Model Classifications.

Now Model Testing

Then Corporate Finance

Then Specific Corp Fin Models — Strebulaev and Hennessy-Whited

Empirical Testing

- Lowest standard: In-sample (IS) moment correlations.
  - "Wrong Model" includes time-varying model.
  - Judge how good model is. (All models are false.)
  - Does "same behavior as last year" favor the model?
  - Should be heavy on diagnostics.
- In-Sample vs. Out-of-Sample Tests
  - Plenty of model tests in economics look great in-sample, but not out-of-sample.
  - OOS test needs only a criterion, like MSE.
  - Can be used to compare nested or unnested models.
  - We have good alternatives in quantitative models. Models should predict better the actual firm than a randomly drawn firm.
- Standard vs. Quasi-Experimental Tests
  - QE: Regression discontinuities. Natural Experiments. (Weaker: IV). QE contrasts with correlation tests from earlier decades. (Spurious correlations are plenty in corporate finance.)
  - Plenty of models in economics look great in unconditional moments, but have completely misidentified the source of variation [what is exogenous and endogenous] and fail in QE contexts.
  - Interesting extension of QE Tests—researcher may not know levels but shocks.
  - QE Tests are almost Causality Tests, even in simple reduced-form models.
  - QE is "realized counterfactual," in the spirit of Lucas' critique.
  - Problem: Limited availability of quasi-experiments. (But, we have great QEs in corp fin.)
  - Model hypothesis is no longer "same behavior": Model should predict better *with* than *without* shock.

Context

- There is nothing that prevents simple, qualitative, and non-structural models from being tested in all four cases.
- There is nothing that prevents complex, quantitative, and structural models from being tested in all four cases.

Relative Modeling

- Summary of Covered Model Aspects:
  - Complexity
  - Quantitativeness
  - Structure (Non-Measurability)
  - (In-Sample Moment Correlations)
  - Out-of-Sample Moment Correlations
  - In-Sample QE Moment Changes
  - Out-of-Sample QE Moment Changes

- All criteria are relative to substitute alternative theories.
- A theory may not be great, but it may be better than all alternatives.
- Consistent standards.

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Now Corporate Finance

Then Specific Corp Fin Models — Strebulaev and Hennessy-Whited

Corporate Finance = The Behavior of Corporations

Standard Issues of Concern in CorpFin:

- Few realistic arbitrage constraints.
- Often many competing hypotheses. Often modest empirial support for many.
- Rarely agreed-upon first-order effects.
- Rarely universally agreed-upon null hypothesis (priors).
- Theories rarely want exact measurable proxies. (Error in variables.) Many unobservable constructs.
- Often low(er)-frequency observations (annual).
- Common Firm-Size Effects?! Common residual heterogeneity across firms and industries. Residual autocorrelation. (Fixed effects? Differenced specifications?)
- Common selection biases (average lifetime: 10 years)

+ Many natural experiments — exogenous (from firm's perspective) tax or technology changes

Capital Structure = Explain Leverage Ratios of Firms

Related but *very* different: Corporate issuing behavior.

Some Explanations for Leverage:

- Adverse Selection = Pecking-order (Myers)
- Taxes and Transaction Costs (Robicheck-Myers 1966, Fischer-Heinkel-Zechner 1989, Strebulaev 2007)
- Productivity (Hennessy-Whited 2005)
- Inertia (Frictions) vs. High-Activity (Welch, 2004)
- Firm-scale link vs. itself (DeAngelo+)
- Market Timing (Baker-Wurgler 2002)
- ESOPs and acquisitions (Fama-French 2002)
- Industry peers (Welch 2004, Roberts-Leary 2009)
- Pension liabilities and industry (Shivdasani-Stefanescu 2010)
- Credit Ratings (Kisgen 2006)
- Hubris (Roll 1986)
- Covenant Violations (Roberts Sufi 2009)
- Precommitments (sinking funds)
- Risk Shifting? (Parrino-Weisbach 1999)
- Extended: Managerial identity (Bertrand-Schoar 2003). Unmitigated agency concerns (ala Bebcuk)
- Non-optimal behavior? (when heuristic band is violated? or just asleep at switch? or random? or I-bank influence)
- ...

Ubiquitous EIVs, heterogeneity, selection biases, firm-size effects.

SQM

Recall that quantitative and structural models (SQM) both rely more strongly on *a priori* identification through model:

- Quantitative models lean heavily on model identification to calibrate it to the right spot on the hyper-surface.
- Structural models lean heavily on model identification because they don't have another map $r \to Q$, they see only $Q \to y$, and they want to know whether $r \to y$.

Omitted uncorrelated factors are highly problematic in both.

Model Issues: Which Forces To Include?

SQMs lean more on their own models as **exclusive** explanation of data. In corprate finance:

- Q: Which first-order effects need to be in the model? (Tests?)
  - It would be tougher to argue that it is ok to omit some forces in early SQMs. Omitted effects must be second- and third-order. (SQMs models rarely allow good marginal control.)
- Q: How strong are your priors that you have the right mechanism?
- Q: Does any one explanation seem dominant a priori?
- Q: Which model is the null? (Your own? Random behavior?)

  failing to reject own with 95% confidence is not a high hurdle.

Confirmation-Bias: Li, Livdan, Zhang 2009:

> *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.*

This is based on 1 out of 2 in-sample moments fitting. (PS: if 1 moment rejects, model is rejected!)

Test Issues: How To Deal With...?

In corporate finance:

- How do emp tests deal with selection and survival biases?
- How do emp tests handle size effects?
- Are the emp tests in differences? fixed-effects?
- Are the emp residuals “white”?

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Now Specific Corp Fin Models — Strebulaev and Hennessy-Whited

- Short eval in view of generic (above) classifications
- Show evidence that they fail even on simplest in-sample evidence.
- ...and then I will argue that their utter failure is *not* surprising. It would have been a miracle if they had worked.

Strebulaev:2007

- Chosen because it is a premier paper—a prominent standard bearer for **quantitative** approach.
- Brattle Prize 2007. Job Market Paper → Stanford.
- Unchallenged.

- (A "Tax Distress Friction" Model)

Tax-Distress-Frictions (TDF) Model

The "first" corporate finance model post M&M that we teach.

- Corporate Taxes favor debt.
- Distress costs favor equity.

  (Robicheck-Myers 1966: Taxes vs distress costs).

  Objective function is flat.

- Frictions (costs of changing) favor inertia.

  (Robicheck-Myers 1966: Taxes vs distress costs).

  (Fischer-Heinkel-Zechner 1989: Frictions vs. Tax-Distress.)

⇒ Firms change capital structure rarely (after large shocks).

- Strebulaev adds that frictions can allow high-profit firms to appear to be less levered.

Impressive

TDF: Version 4 (Strebulaev 2007)

- PV of all future net payouts at time 0 (V_0).
- The initial book value of firm assets (A_0),
- The systematic risk of the firm's assets (β),
- The volatility of monthly market returns (σ_E),
- The volatility of monthly 10-year T-bills (σ_D),
- The covariance between equity and debt returns (σ_{ED}),
- the average leverage (L_{av}),
- the volatility of idiosyncratic shocks (σ_I),
- the volatility of the project's net cash flow (σ),
- the proportional costs incurred in selling assets q_A,
- the proportional adjustment costs of issuing/retiring debt q_{RC},
- the proportional direct costs of external equity financing (q_E),
- the proportional restructuring costs (α),
- the fraction of assets that remains after an asset sale (k),
- the partial loss-offset boundary (κ),
- the growth rate of book assets (g),
- a shift parameter in the net payout ratio estimation (a),
- the asset risk premium (RP_A),
- the loss per dollar of full offset in the case of distress (τ_K),
- the marginal corporate tax rate (τ_C),
- the marginal personal tax rate on dividends (τ_d),
- the marginal personal tax rate on interest income (τ_i),
- the instantaneous after-tax riskless rate (r).

TDF: Version 4 (Strebulaev 2007)

The optimization is

$$
\begin{aligned}
c^* &= \underset{c,\, y_U,\, y_{LU} \in R^3_+}{\arg\max} \frac{E^R(\delta_0)+(1-q_R C)\cdot D(\delta_0)}{1-y_U \mathcal{E}_{\delta_0}[e^{-rT_U}|\phi_L(U)=0]-k\, y_{LU}\mathcal{E}[e^{-rT_{LU}}|\phi_B(LU)=0]} \\
D(\delta_0) &= D^R(\delta_0) + \mathcal{E}_{\delta_0}[e^{-rT_U} D_0 | \phi_L^U = 0] + \mathcal{E}_{\delta_0}[e^{-rT_U} w D_0 | \phi_B^{LU} = 0] \\
\frac{\delta}{V_t} &= a + (1-\tau_C)\cdot \frac{c}{V_0} \\
\left.\frac{\partial E(\delta_t)}{\partial \delta_t}\right|_{\delta_t=\delta_B} &= 0 \\
q(x) &= \begin{cases} x & \text{if } k\delta_S > wc \\ (1+q_E)x & q_E > 0, \text{ otherwise} \end{cases} \\
E^D(\delta_0) &= E^R(\delta_0) + \mathcal{E}_{\delta_0}[e^{-rT_U} y_U E^D(\delta_0) | \phi_L^U = 0] + \mathcal{E}_{\delta_0}[e^{-rT_{LU}} y_{LU} k E^D(\delta_0) | \phi_B^{LU} = 0] \\
D^D(\delta_0) &= D(\delta_0) + \mathcal{E}_{\delta_0}[e^{-rT_U} y_U D^D(\delta_0) | \phi_L^U = 0] + \mathcal{E}_{\delta_0}[e^{-rT_{LU}} y_{LU} k D^D(\delta_0) | \phi_B^{LU} = 0] \\
E^R(\delta_0) &= \mathcal{E}_{\delta_0}\left[\int_0^{T'} e^{-rs}(1-\tau)(\delta_s - c)ds\right] \\
&\quad + \mathcal{E}_{\delta_0}\left[\int_{T_L}^{T''} e^{-rs} q\left((1-\tau)(k\delta_s - wc) - \tau_I wc \mathbf{1}_{\delta_s<\delta_t}\right) ds\right] \\
&\quad + \mathcal{E}_{\delta_0}\left[e^{-rT_B} \max\left[(1-\alpha)\int_{T_B}^{\infty} e^{-rs} k(1-\tau)\delta_s ds - wD_0, 0\right] \middle| \phi_{LU}^B = 0\right] \\
D^R(\delta_0) &= \mathcal{E}_{\delta_0}\left[\int_0^{T'} e^{-rs}(1-\tau_i) c ds\right] \\
&\quad + \mathcal{E}_{\delta_0}\left[e^{-rT_L} | \phi_U^L = 0\right](1-w)D_0 + \mathcal{E}_{\delta_0}\left[\int_{T_L}^{T''} e^{-rs}(1-\tau_i) wc ds\right] \\
&\quad + \mathcal{E}_{\delta_0}\left[e^{-rT_B} \min\left[(1-\alpha)\int_{T_B}^{\infty} e^{-rs} k(1-\tau)\delta_s ds, wD_0\right] \middle| \phi_{LU}^B = 0\right]
\end{aligned}
$$

No closed-form solution or comp statics. Only numerical. Often unstable. Model Assessment is based on moments from simulated 75 (!) years of quarterly data.

Model Assessment for Strebulaev

- Puts all eggs into TDF Model.
- Not a simple model (see above).
- Principally **quantitative**, although also conceptual: if firms don't respond due to costs of response, then higher-income firms can have less leverage.
+ Great to have specific quantitative predictions.
- Not principally structural

Tests:

- All evidence in Strebulaev is IS and on non-QE moments.
- Ignores selection biases (see 75 years)
- No control for forces outside model (although S does have many important ones)
- No residual (model) diagnostics.
- No assessment of main model moments (= complete failure)
- No OOS tests
- No QE tests (Shocks to Taxes, Bankruptcy Costs, Frictions [UW Entry]; no other effects should matter.)
- No Assessment of Model Fit

PLUS

- I will conduct the IS model assessment to show that TDF is a complete empirical failure.

I will now show that the TDF model fails as a first-order explanation

(not just Strebulaev).

Inertia vs. Non-Adjustment

- Inertia: Statement about variance.

$$L_{t=1} = 0.5 \rightarrow Shock \rightarrow L_2 = 0.5 \qquad TDF$$

- Non-adjustment to Shocks: Statement about changes

$$L_{t=1} = 0.5 \rightarrow Shock \rightarrow L_2 = \begin{cases} 0 & prob\ 50\% \\ 1 & prob\ 50\% \end{cases} \qquad not\ TDF$$

- Inertia $\Rightarrow$ Non-Adjustment
- Non-Adjustment $\not\Rightarrow$Inertia
- In TDF, non-adjustment is due to inertia.
- In TDF, it is impossible to have non-adjustment without inertia,
- ...but all Strebulaev confirming tests are about non-adjustment, and the inference is about inertia.

TDF's Main (Non-QE) Prediction

- **Inertia:** Firms are inactive most of the time (and/or when they have not been hit by a large shock).
  - Strebulaev's calibration: Histogram has 85% mass at zero.
- **Readjustment:** Although rare, when firms change their capital structure, they undo deviations from their TD optimal ratio.
- When firms are active, they make large **RE**adjustments.
  - say, $|\Delta L| > 10\%$. All theory calibrations suggest more like 30-50%.
  - Total capital structure change volatility should be a little *less* than it would have been in the absence of managers. (No activity = same vol)
  - If shock is identified as stock return on x-axis, then I should see low activity at shock$\approx$0; high counteracting activity in corners undo shocks.
- Predictions apply to all TDF models. (mentioned calibrations are from strebulaev.)

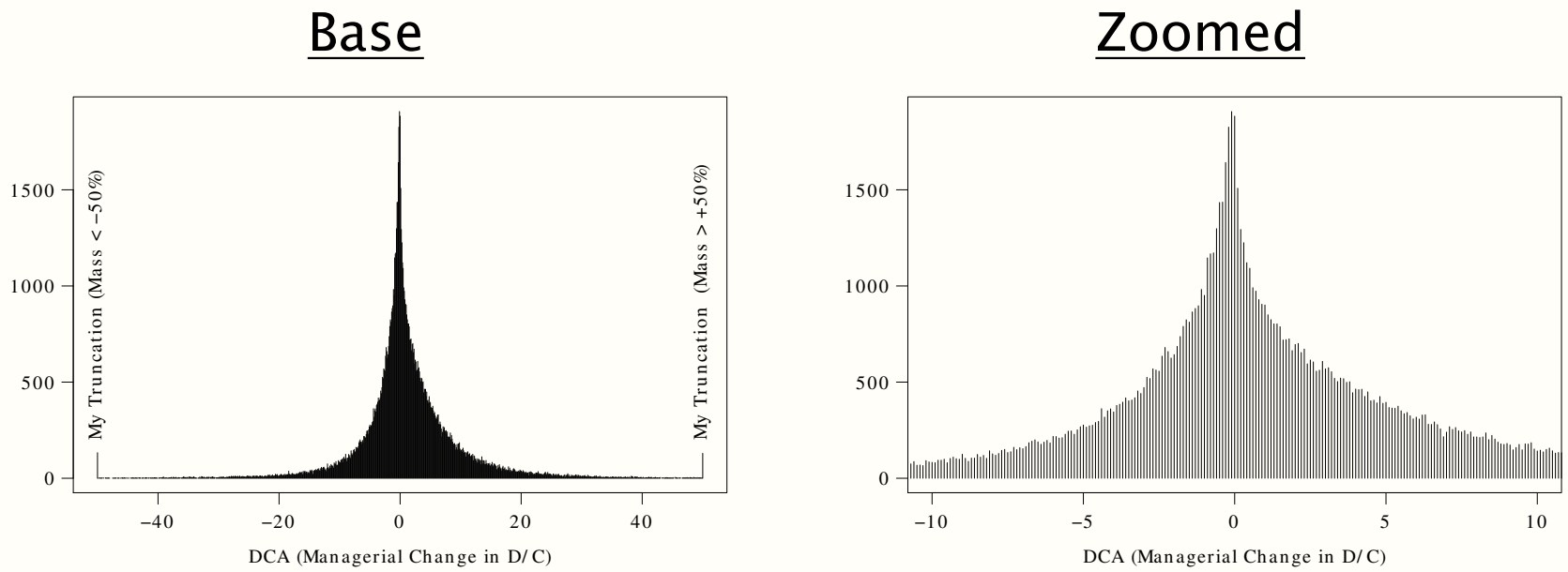
S-II: Evidence: Basic Implications

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

- **Inertia:** TDF says |dca| should be low most of the time, but sometimes large. Think 0% about 85% of the time. 10-50% about 15% of the time.
- **Readjusting:** Var(dct) $<$ Var(dcp)
- **Readjusting:** dca $>$ 0 when dcp $<<$ 0. dca $<$ 0 when dcp $>>$ 0

PS: Other empirical research interested in what managers are doing, why not use dca instead of dct? Avoids stock-market caused noise and stock-market anomalies (such as B/M).

Inertia Evidence: dca Histogram



- Cannot be normally distributed, of course.
- Yes—spiky at zero.
- No—mode is zero, but lots of activity at, say, -3% and +3%.
- Compare to typical *theoretical* magnitudes (> 10% or > 30%)
- (Next draft—use just long-term debt for predetermined.)
- (Stock-return induced change dcp adds to weight left of zero.)

Readjusting Evidence: Unidentified Shocks

	Mean	Sdv
$\text{dct}_{t-1,t} = \frac{D_t}{D_t+E_t} - \frac{D_{t-1}}{D_{t-1}+E_{t-1}}$	1.15	12.9
$\text{dcp}_{t-1,t} = \frac{D_{t-1}}{D_{t-1}+E_{t-1}\cdot(1+x_{t-1,t})} - \frac{D_{t-1}}{D_{t-1}+E_{t-1}}$	0.17	9.3
$\text{dca}_{t-1,t} = \frac{D_t}{D_t+E_t} - \frac{D_{t-1}}{D_{t-1}+E_{t-1}\cdot(1+x_{t-1,t})}$	0.97	8.7
dca^+ (with divs)	1.26	8.7

Units are %/year. x is pct capital gain. (r instead of x gives similar results.) Winsorized at $|0.5|$ gives identical results.

- Activity Is Not Readjusting: sd(dct) $\not\leq$ sd(dcp) !
- dca (active) $\perp$ dcp (passive).
  var(dct) $\approx$ var(dca) + var(dcp) $\quad (\sqrt{2}\cdot 9 \approx 12.7)$
- What are managers thinking?? I do not know
- Note: no misspecification tests. mea culpa. (my focus is the non-inertia variance.)

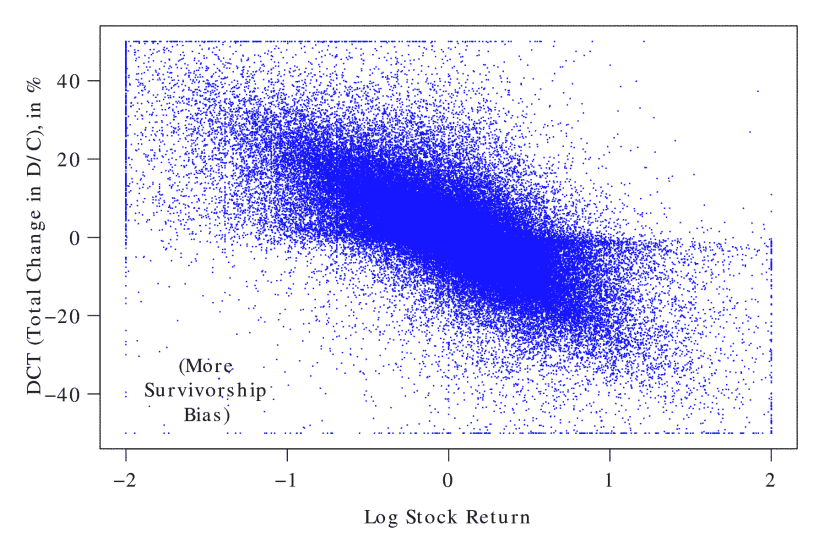
Readjusting Evidence: Stock Return Shocks

Let's identify the shocks as stock-return caused changes in leverage.

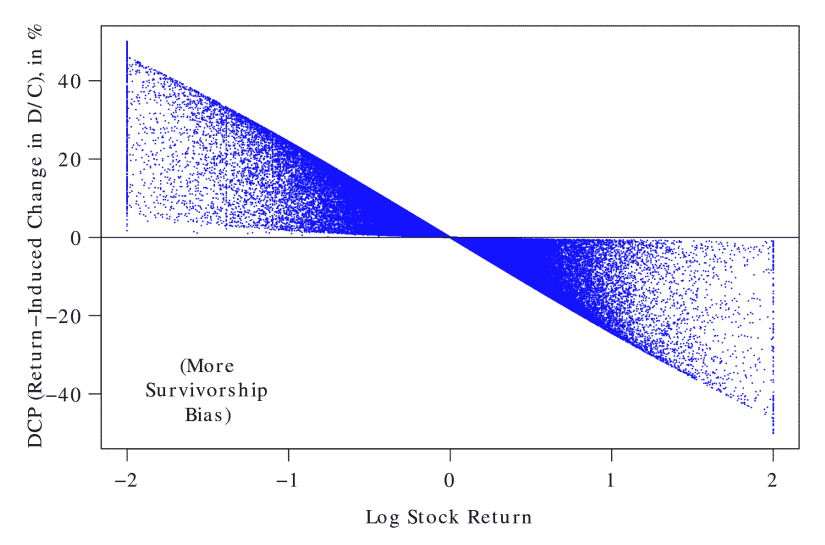
(Welch, JPE 2004)

Readjusting Evidence: Stock Return Shocks

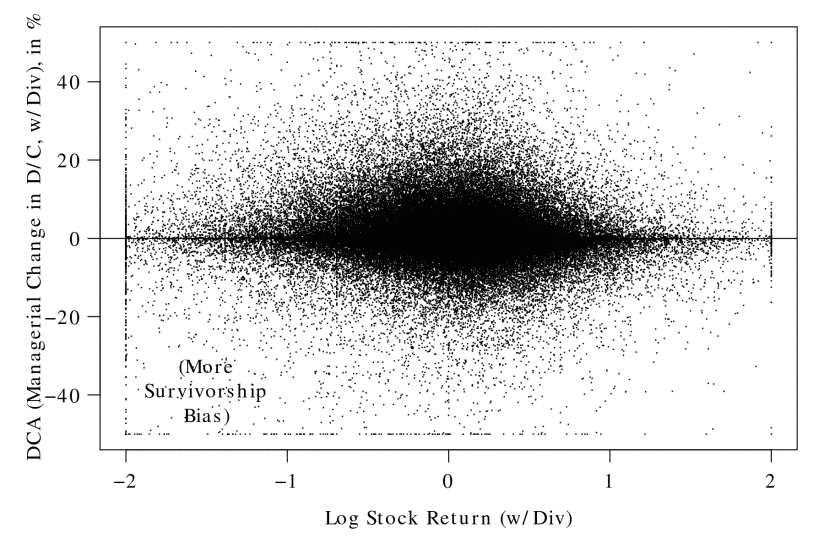
Capital Structure Change (dct)



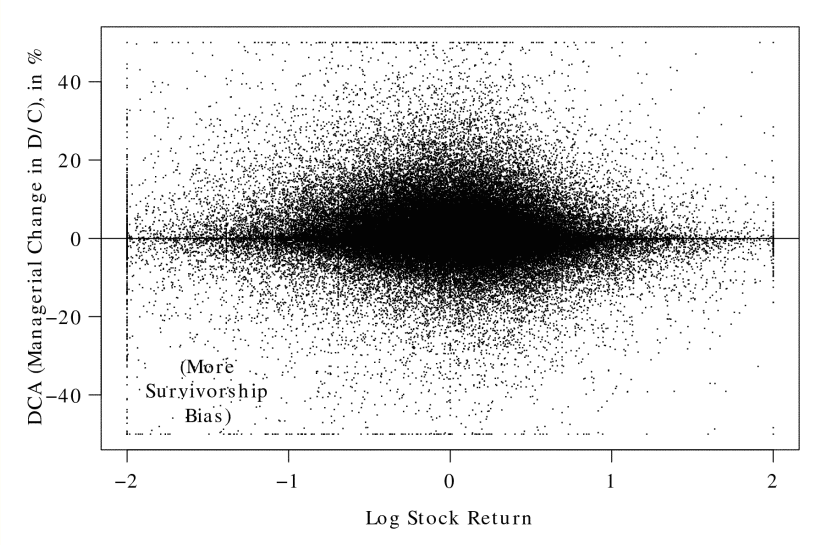
Stock-Return Induced (dcp)



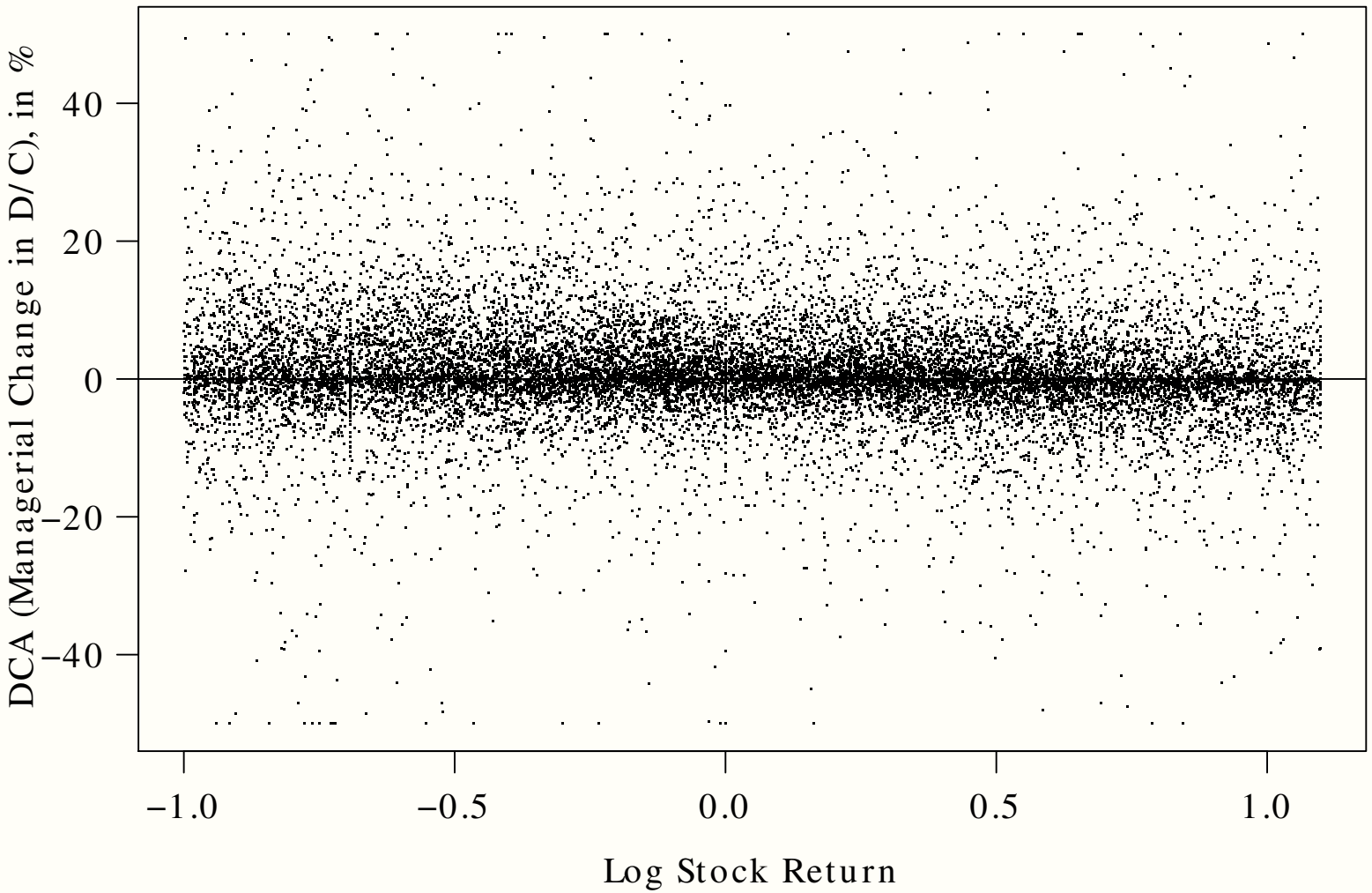
Managerial Net Response (dca)



Managerial Net Response (dca$^+$ [with divs])



S-II: Pruned Managerial Net Response (dca^+);



- Managers do not undo shocks. ("consistent" with TDF)
- No evidence of inertia. (totally inconsistent with TDF)

S-II: Evidence: Inertia?

- My evidence against inertia was conservative.
  - Debt for debt change does not show up. (Rauh-Sufi 2010)
  - Equity for equity change does not show up.
  - Proportional changes in D and E do not show up.

  Firms could be very active, but net lev changes could be zero.
- **Firms are just not inert most of the time.**
- The TDF theory fails. Not just the structural one—*any.*
- The Pecking Order theory fails, too. Firms are not so (equity)-inert.
- **The TDF-DTT cannot explain lack of readjustment, because the causal inertia link is broken.**

⇒ Let's agree: even if "frictions lower activity" is a marginal effect, inertia cannot possibly be a first-order determinant of capstruct. Firms are way too active.

Managers

- The high activity levels suggests that managers are doing something intentionally...
- ...but I do not know what they are trading off just yet.

- (Leverage activity is not frictions-limited, because firms are too active. Leverage is not moving-optimal-targets (at least alone), because average outcome leverage is too eerily close to average non-adjustment quantitatively.)

Hennessy-Whited 2005

- Chosen because it is a premier paper—a prominent standard bearer for approach.
- Brattle Prize 2005.
- Unchallenged (except Lewellen-Lewellen, *unpublished*)

Hennessy-Whited (2005) Predictions

The model is not easy to understand. No closed-form solution.
Main intuition = main empirical implications:

> *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).*

HW model may have different implications, and I may not fully grasp its depth.

Model Assessment for Hennessy-Whited

- Puts all eggs into Productivity Model.
- Not a simple model. Tough to understand.
- Not principally quantitative.
- Principally structural — productivity is unobservable.

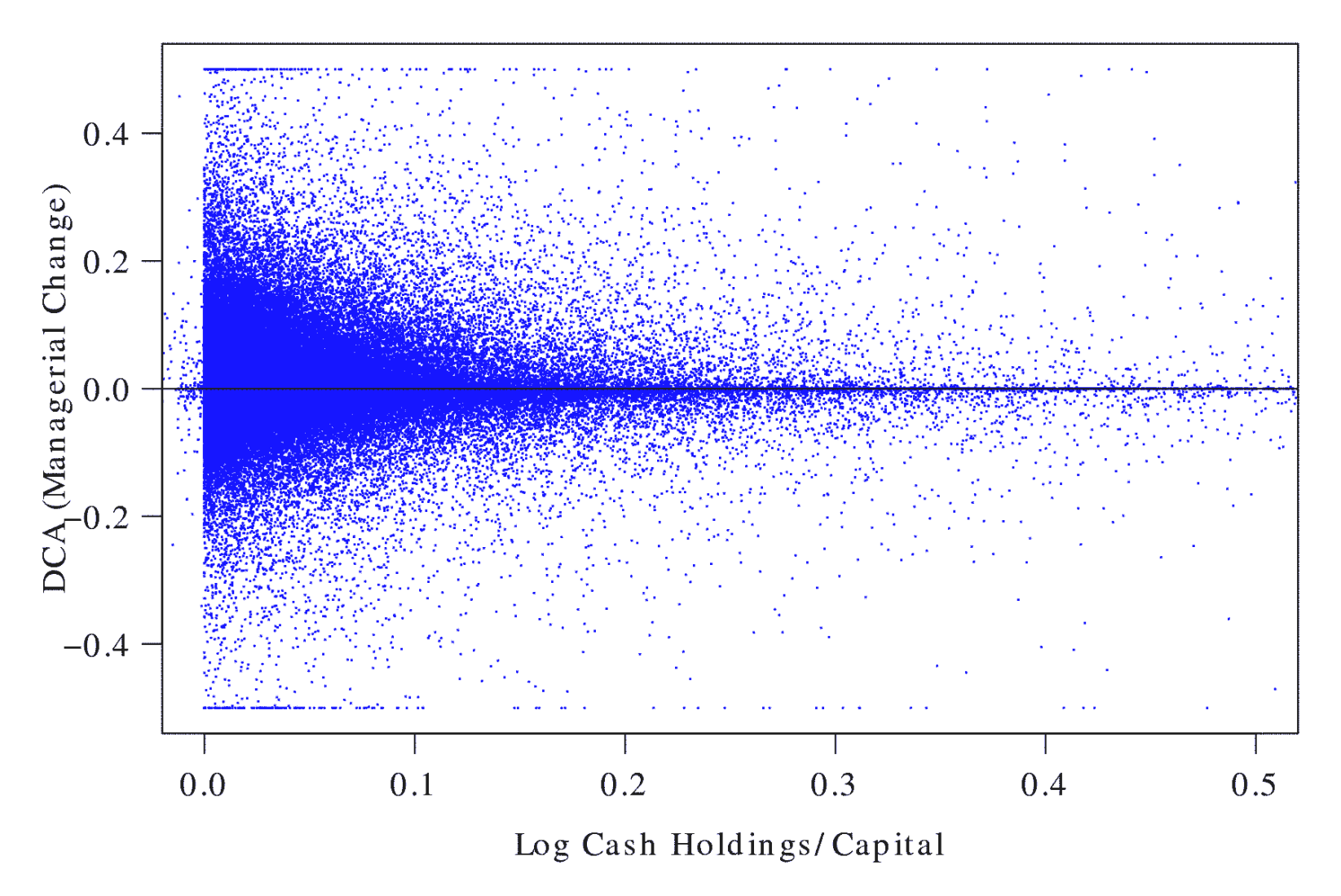
Tests:

- All tests are IS and on non-QE moments.
- No control for forces outside model.
- No model diagnostics.
- No OOS tests
- No QE tests (Shocks to productivity [specific industries [eg GPS, ATMs], specific firms [eg patents], tax laws, etc]).)
- No Assessment of Fit
- Predictions are more vague than Strebulaev.

PLUS

- Show that first-order predictions are third-order important.

S-II: Evidence: Hennessy-Whited Tests: Cash Holdings



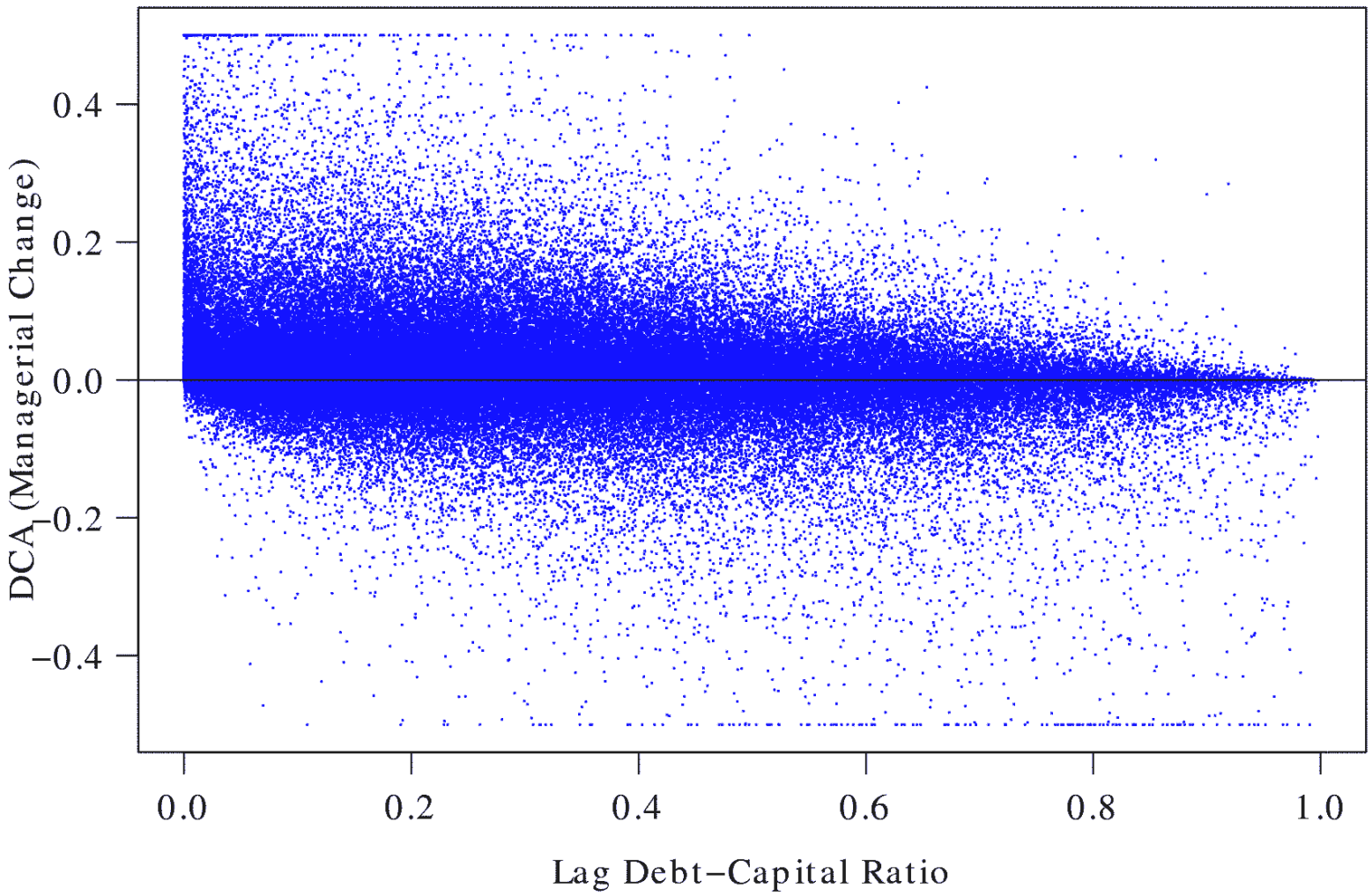
- Yes, the coefficient is -0.025 (T= -11), but R^2 is 0.0008.

⇒ At best, cash is a very marginal influences.

S-II: Evidence: Hennessy-Whited (Continued)

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.*

S-II: Evidence: Hennessy-Whited Tests: Lagged Debt



- HW predict positive slope on right graph for high D/C ratios.
  - Iliev-Welch looks at readjustment in great detail. There are issues in the right graph as regards truncation. IW also explains how to deal with them.

⇒ Not visible. Not a first-order effect.

Evidence

- Should the HW model be the primary lense through which to view capital structure data?
- Or is this a theory "on the margin" that requires controls, OOS evidence, QE studies, etc.?

- Do you firmly believe that measurable liquidity (Q) is primarily determined by productivity (in model) and nothing else?
- Do you firmly believe lagged debt (Q) is primarily determined by productivity and nothing else?
- (These would have to be beliefs that are based on priors, not based on data.)
- If you do not firmly believe this, then you have probably attributed other unknown influences as if it were evidence in favor of the productivity-cause model.

Conclusion: My View of Existing QSM Tests

- Existing QSM models are not the null.
- Existing QSM "tests" were perfunctory.
  (No misspecification tests. Analogy = t-stat of a variable [often theory designed for it] in a reg w/o controls, diagnostics, corrections.)
  A models designed for a test is ok. But hurdles were often analogous to judging a qualitative reduced-form theory by the t-statistic of a 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.
  Spec tests, control vars (FE), in differences,

- No QSM in corporate finance has ever been tested OOS or in a quasi-experiment.
  QE tests are not alternatives to QSM...on the contrary. We want to test QSM in their context.

  QE tests can but need not be be alternatives. That is, without QSMs, the alternative to structural modeling is not mindless large regression models, but disciplined QE identification.

  For example, if a QSM theory based on taxes fails explaining different behavior before and after a tax law change, then it probably means that taxes are not important and the QSM is probably not useful.

(similarly, it's why we typically run regressions in changes to reduce spurious inference.)

Conclusion: QSM View

- Hennessy-Whited 2005:

  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:

  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.

Conclusion

Rest is my own subjective assessment of what should happen.

Conclusion: My View of Theory

- We do not understand what the first-order determinants of managerial capital structure activity are.
  (We do know that stock returns (operating performance) are a major determinant of (non-managerial) market-value based capital structure changes. [Less but similar effect of operating performance with book values])
- I do not know why managers are so active.
- We need to understand the first-order effects.
  - Start with Graham-Harvey style surveys. — esp for structural.
  - Then check against actual behavior. **Debrief CFOs** to understand what they really did.
  - PS: Break out managerial capstruct changes from performance changes (e.g., stock returns, earnings and depreciation)

What Should We Do?

- I am **not** against QSMs per se.
- I want to be convinced that the chosen mechanisms are the most likely one.
- I want models that predict well, not only IS. OOS and/or QE.
- I want to know what the alternative explanations for the same evidence are—what the evidence can and cannot exclude.
- I want to see diagnostics. Does the model seem badly misspecified?
- I want controls for alternative explanations to understand how "marginal" the effect is.
- I want to see how the model predicts when a shock happens.

  "(Level vs. Change regression)"

- I want to know what fraction of the variation is explained.

- I do not know any successful QSMs in corporate finance.
- I do not believe there is much hope that any will come soon in corporate finance. "Early" seems hollow.
- I would be thrilled to be convinced otherwise.

NIP: What About Outside Corporate Finance?

Structural modeling is also popular in asset-pricing.

- ⊕ If arbitrage is possible, it makes complex markets simple. I.e., the mechanism becomes (almost) known!!
- ⊕ Tests are often out-of-sample.
- ⊕ Empirically, QSM often explain well for derivatives and FI.
- ⊖ Empirically, most QSMs seem to fail badly for equities. Will "enhancements" produce overfitting or a better model?
- ⊖ Structural theories' tests without direct proxies identify causes by assumption. (Uncorrelated forces distort inference.) Encompassing assumption is similarly problematic.
- ⊖ Can tests look for quasi-experiments?
  - Sometimes not possible—shock to $\beta_{i,\text{con}}$.
  - Schizophrenic?: Theorists insist on micro-foundations, but then do not trust or ask for micro-empirical tests.
- ⊖ Causality is again by assumption.