Why I do not understand Capital Structure Research

USC Presentation

Ivo Welch

October 2010

(Publicly posted at http://www.ivo-welch.info/academics/ .)

The following paper has been rated

Viewer Discretion Advised

Various Issues

- How should we measure leverage? Does it matter?
  - *Is it "Financial Debt / Assets"?*
- What happens under a null hypothesis of random behavior?
  - *How do leverage ratios behave?*
- What seems to be the single biggest question in Capital Structure?
  - *Do firms readjust (tradeoff)? If not, why not? If some firms adjust, which firms do and when?*
  - *What are the properties of the standard econometric estimators (e.g., OLS) in predicting leverage ratio (changes)?*
- Should we write down structural models to explain the known empirical evidence?

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Quo Vadis

- I will use some award-winning influential papers as examples. (I did not choose them because they are bad, but because they are influential.)
- I predict the points in this presentation will be interesting, maybe entertaining, possibly even memorable, but...
- I predict that most will not be published.
  - If I have time, I will mention what referees tell me is wrong.
  - Three universal points: "I already knew this," "I don't find it interesting," or "points 7, 15, and 20 are wrong."
  - Referees may well be correct on all three!
- Yet...
  - Many current papers continue with the same approaches;
  - If it is not in peer-reviewed print, it is *not* in the literature, and the published paper stands.
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measure

How should we (not) measure corporate leverage?

Could the definition matter?

What is the right measure of Leverage?

- Which firm is more levered?

	Financial Debt	Non-Financial Liabilities	Equity
A	\$30 million	\$30 million	\$40 million
B	\$30 million	-	\$40 million

A is average firm on Compustat. Variance in NFL ≈ variance in other components.

- More than half the papers in the literature define leverage as

$$\text{Leverage} = \frac{\text{Financial Debt}}{\text{Total Assets}}$$

Watch out: Some authors correctly use capital=(debt + equity) as denominator, but write "debt over assets," not "debt over capital." Other authors use "liabilities" correctly, but incorrectly call it "debt."

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	Leverage Ratio
A	30%
B	43%

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What is the right measure of Leverage?

The basic problem is that FD/TA treats NFL as if it is Equity.

- My problem is *not* about whether you want to treat NFL (e.g., A/P) like debt.

  This question is whether you want to use FD/CP or TL/TA

$$\frac{FD}{CP} = \frac{FD}{FD+EQ} \qquad \frac{TL}{TA} = \frac{FD+NFL}{FD+NFL+EQ}$$

- My problem is about whether you want to treat NFL like *equity*.

$$1 - \frac{FD}{TA} = \frac{EQ+NFL}{TA}$$

Can It Matter?

Try four common variables: • Firm Size; • Accounts Payables; • Book To Market Ratio; • Tangible Assets; (PS: Could add more, such as taxes and implied-debt-ratio.)

		const	Log(AT)	BEQ/MEQ	NI/AT	PPEGT/AT	AP/AT	$\overline{R}^2$
BV FD/AT	Std Coef	0.000	0.256	−0.119	−0.129	0.088	−0.158	
	T_{NW}	(+7.16)	(+8.50)	(−4.72)	(−1.29)	(+3.71)	(−7.72)	(+7.5%)
BV FD/BCP	Std Coef	0.000	0.354	−0.114	−0.129	0.035	0.108	
	T_{NW}	(+3.93)	(+11.11)	(−2.24)	(−1.32)	(+1.48)	(+7.01)	(+15.7%)
BV LT/AT	Std Coef	0.000	0.408	−0.170	−0.143	−0.056	0.207	
	T_{NW}	(+16.49)	(+18.65)	(−5.01)	(−1.26)	(−2.49)	(+15.64)	(+29.6%)
MVFD/MAT	Std Coef	0.000	0.336	0.098	−0.122	0.056	−0.123	
	T_{NW}	(−0.08)	(+10.76)	(+3.87)	(−1.35)	(+2.53)	(−5.69)	(+15.2%)
MVFD/MCP	Std Coef	0.000	0.447	0.152	−0.144	0.022	0.110	
	T_{NW}	(−5.61)	(+18.27)	(+5.20)	(−1.47)	(+1.13)	(+6.33)	(+32.9%)
MVLT/MAT	Std Coef	0.000	0.454	0.226	−0.150	−0.054	0.182	
	T_{NW}	(+0.09)	(+23.00)	(+7.84)	(−1.40)	(−2.76)	(+14.26)	(+45.8%)

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So What?

- Don't practitioners use FD/TA in bond covenants?

  Yes, but covenants also use dividend payouts, which does not make it a leverage measure.

- Can you find cases where the coefficients do not change??

  Yes. Many. (I hope most.)

  Capital structure regressions are often subset-sensitive, where subsets are implicitly defined by data availability restrictions. also, some researchers like book-values; others like market-value.

- Which studies findings would change?

  I do not know.

- Which studies findings would *not* change?

  I do not know. How do I know what to trust? Do you?

- Does it make sense to show how one specific study changes?
  - *Maybe, but I did not want to critique one paper.*
  - *I would love to see someone else research this.*

- What is the cost of improving the leverage ratio measure??

  Zero! *Note—different leverage measures have different meaning. Also, Rampini-Vish (2010) suggest including leases.*

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Why This Will Never Be Published

in a top journal (agreed to International Review of Finance, Capital Structure Issue)

Referee Objections:

- No one knows what the correct leverage measure is. Even the author offers two definitions. I can pick on them, too.
- By mentioning specific papers that use this measure (but not showing that they change), this paper is impuning them. This makes this paper offensive, to say the least.
- I can show you that for some regressions that I just ran, the results did not change. Here, look, ...
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...but I really don't want to see any more papers explaining FD/TA.

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nullprocess

$$\Rightarrow$$

How Would Random Leverage Evolve?

Intent: PLACEBO

Uses:

- I want a "placebo" data set, in which there is no firm-specific behavior (and thus by definition no intentional behavior).
- If the suggested model suggests firm-specific choice, and if a researcher gets the same findings in the placebo, then this positive empirical finding is not in support of the model.
  Random behavior can explain it.

...but placebo isn't so simple. it isn't $L_{i,t} = L_{i,t-1} + \epsilon_{i,t}$ with $\epsilon_{i,t} \sim N(0, \sigma^2)$.

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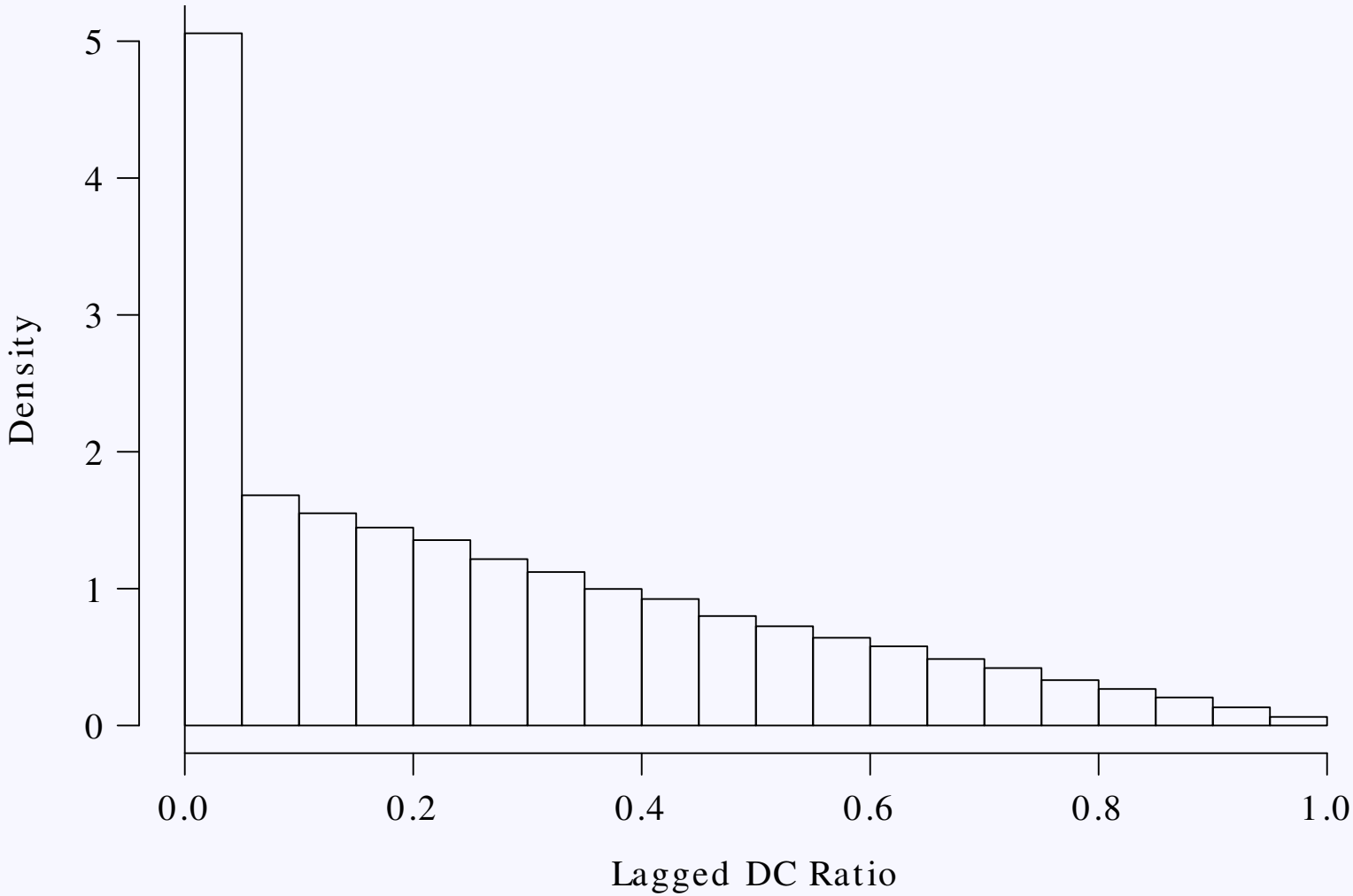
Actual Distribution of Leverage Ratios



Placebo Process Properties

- Cannot simulate $L_{i,t} = L_{i,t-1} + \epsilon_{i,t}$ with $\epsilon_{i,t} \sim N(0, \sigma^2)$.
- Cannot just redraw $L_{i,t}$—after all, leverage is stationary.
- $L_{i,t}$ should not exceed [0,1] bounds.
- $L_{i,t}$ should reflect masspoint at $L = 0$—Logit/Probit fail, too.
- Process should be general, reasonable, believable—hard to argue with random nature.
- A "Unit-Root" Process
- Should be easy to use.

Placebo leverage data is posted with gvkey and year on

http://www.ivo-welch.info/academics/leverage.placebo/

Download and run it. No effort involved.

Standard Econometrics

On leverage data, yes, you can use standard econometric estimators

...but their standard properties (such as unbiasedness, efficiency, probability distribution, etc.), as derived under common econometric processes, will not apply. Don't think that your econometrics textbook can tell you the properties.

Not just mildly—a lot (even for censored normal, logit, etc.)

Having a placebo, you can learn the estimate for the placebo, and then compare how much bigger or smaller the estimate in the actual leverage data is. This gives you the "net effect" managerial or firm activity.

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On leverage data, yes, you can use standard econometric estimators

...but their standard properties (such as unbiasedness, efficiency, probability distribution, etc.), as derived under common econometric processes, will not apply. Don't think that your econometrics textbook can tell you the properties.

Not just mildly—a lot (even for censored normal, logit, etc.)

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Chang-Dasgupta

What about Chang-Dasgupta?

- Great paper. (Complementary [and independent].)
- Focuses on book-values.
  (No papers have used CD approach and/or process since.)
- In most similar form, CD do the following:
  - draw funding need from Norm(0.16,0.5) in terms of book assets,
  - then assume either D or E financing (50%).

(Our papers have very little overlap in applications and findings.)

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SKIP

Other CD processes are funding-deficit contingent process. Firm deficit is ΔA - ΔRE.

- In its most similar form, CD do the following:
  - draw funding need from Norm(0.16,0.5) in terms of book assets,
  - then assume either D or E financing (50%).
  - This will cause strong placebo reversion of low D_{t-1} firms. In real life, 0-leverage ratio firms tend to continue with 0-leverage. Should we call this reversion?
- Slightly odd Chang-Dasgupta experiment:
  - Firm has \$100 debt and \$100 equity.
  - Looses \$50 in value (factory blows up). (no dRE)
  - Actual: presumably equity declines. L=\$100/\$150=2/3.
  - Placebo: Assign \$50 reduction to D or E with x% probability. 2/3 or 1/3.
  - Net L = actual - placebo > 0.

  Value changes tend to be associated with particular kinds of changes in debt and changes in equity. CD focus on first modeling value changes, but have unconditional debt and equity changes.
- Deficit can be endogenous. What if firms pay out more (dRE) if they raise equity?
- What if firms with low deficits have certain types of capital structures?
  Deficit itself can be related to other variables.
- A deficit-orthogonal process will be less disputable.
- Relies on a specific accounting identity, which is not the same in market values as in book values.

Don't misunderstand me—I love this paper. Still, our process is better, it can apply with market value based leverage ratios, and it is used to look at different estimates. (Besides, our paper also offers a reconciliation estimator, not described in this presentation.)

A Novel Placebo Process

- Our Placebo Process:
  - Start with each firm's own lagged leverage: $L_{i,t-1}$.
  - Draw random firm-year from the sample (R, r). Totally unconditional of firm i. Don't use anything with i (or even t) to draw R, r.

    Compute $\%\Delta E_{R,r}$ and $\%\Delta D_{R,r}$.

  - $E_{i,t} = E_{i,t-1} \cdot (1 + \%\Delta E_{R,r})$
  - $D_{i,t} = D_{i,t-1} \cdot (1 + \%\Delta D_{R,r})$
  - $L_{i,t} = E_{i,t} / (D_{i,t} + E_{i,t})$
- Special Case: $D_{i,t-1} = L_{i,t-1} = 0$
  - Choice 1: Draw from other zero firms → firm often exits zero. (WARNING)
  - Choice 2: Draw from all firm-years → usually continues zero.

  Advice: Be conservative for question on hand. Try both.
- (Could be done additive (in terms of ratio) instead of multiplicative.)
- We can break out stock returns in E as performance. Stock-return performance is first-order *not* an intentional capital-structure adjustment mechanism. We call it managerial capital structure choice only if it involves active issuing or repurchasing. (This also avoids attributing B/M or other stock return anomalies to managerial actions, and reduces noise.)

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Inference — What is this Useful for?

- I will not try to distinguish an exact placebo process against a process that is near-placebo.
- The question is simply
  1. "does capital structure roughly follow something close to a placebo process"; vs.
  2. "is there strong evidence of meaningful large managerial intervention."
- The question is one of rough magnitudes, not about exact magnitudes, and/or what the process is when $t \to \infty$.
- It is a question about a specific population moment, average SOA.
- We do not use the process to ask whether not a single firm reverts. (The answer to this is that some managers do revert.)
- Placebo process should also help us assess third variables' influence on leverage ratio.

beginnings

$$\Rightarrow$$

Back To The Beginnings (LRZ)

How Permanent Are Leverage Ratios ?

Brattle Distinguished Paper, 2008.

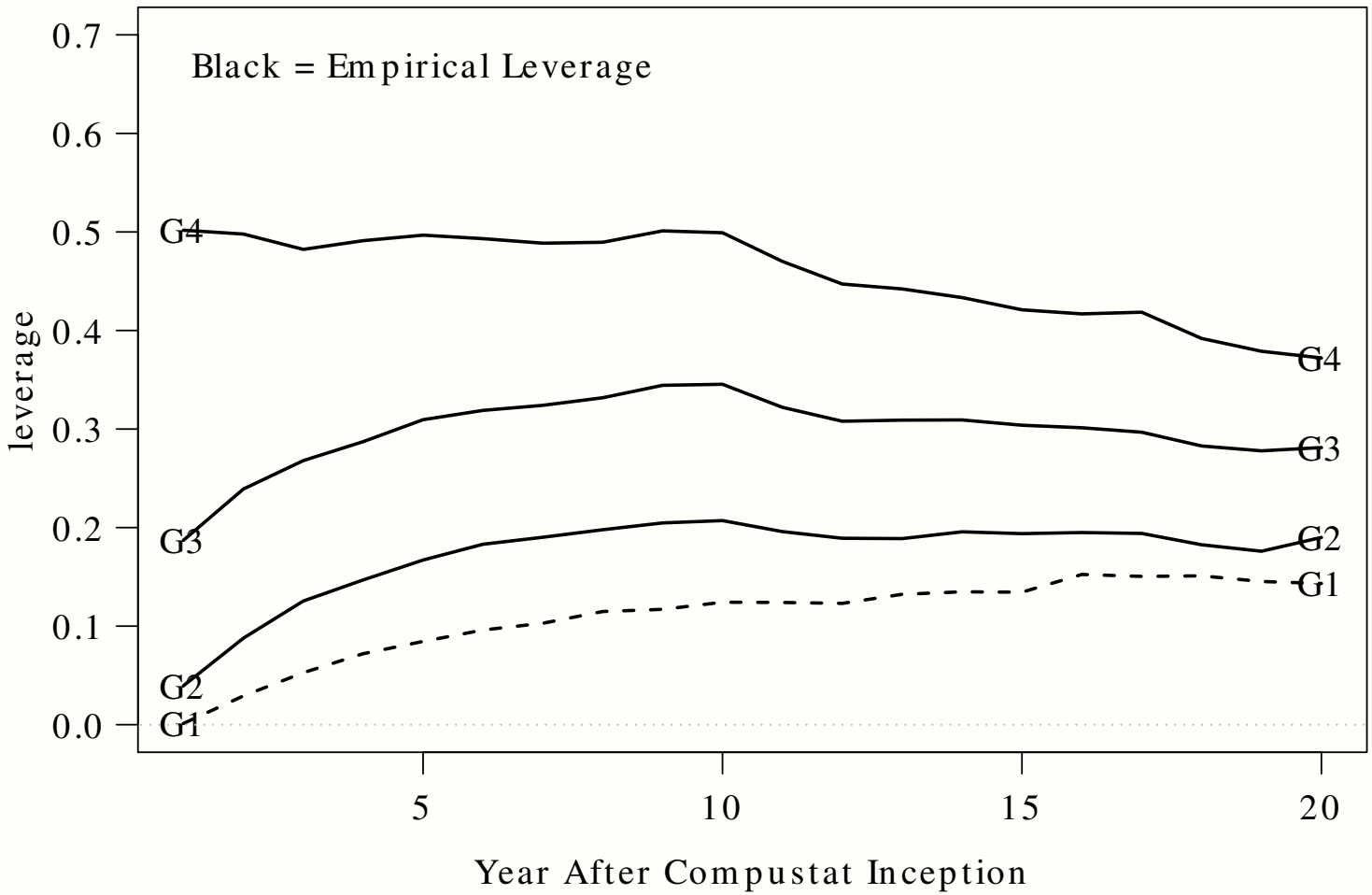
Permanence

- Main point of LRZ:
  slow adjustment ⇒ permanence ⇒ L_0 matters at t+...
- Let's rerun LRZ tests on random leverage placebo.

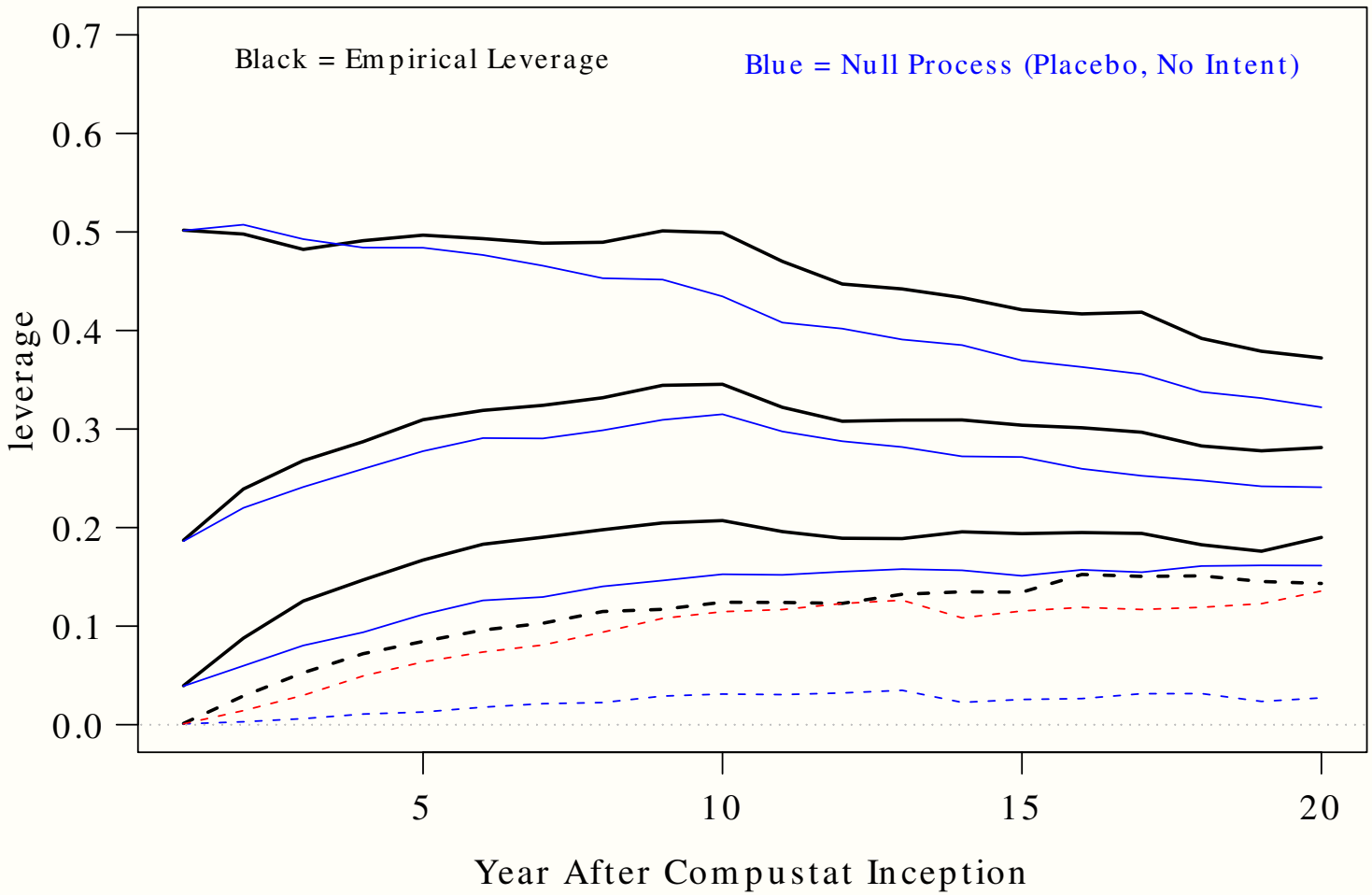
  How important are intentional/managerial/firm-specific leverage changes?

- I have not done an exact replications, because each observation appears only once in our sample (to avoid overlap). LRZ have many observations many times in the figures. (results will change a little.)
- Like LRZ, we ignore survivorship bias. (1 in 10 firms disappear every year. There is evidence that disappearance is leverage-ratio related and performance (stock return) related. Focusing on ex-post survivors, as LRZ do, does not ameliorate bias.) Results are probably somewhat sensitive, but I will be conservative and follow them.
- A generic question is "What is the tradeoff theory?" Does death due to high leverage ratio (natural selection) qualify, or is the tradeoff theory about intentional ex-ante changes to balance against death? This matters to how one treats firms that perform pooly, thus go to 100% leverage, and drop out.

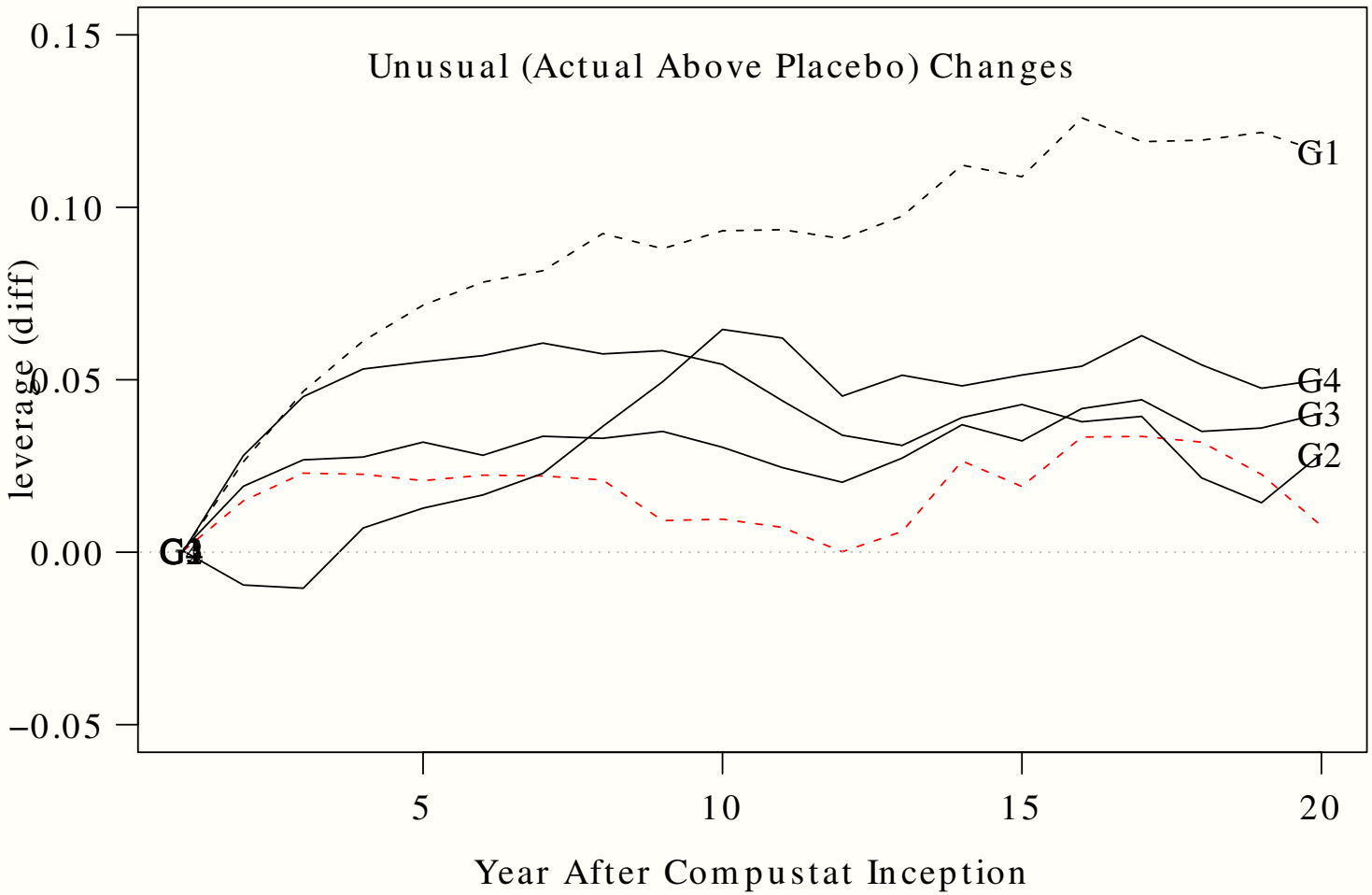
Most Memorable Graph — Empirical



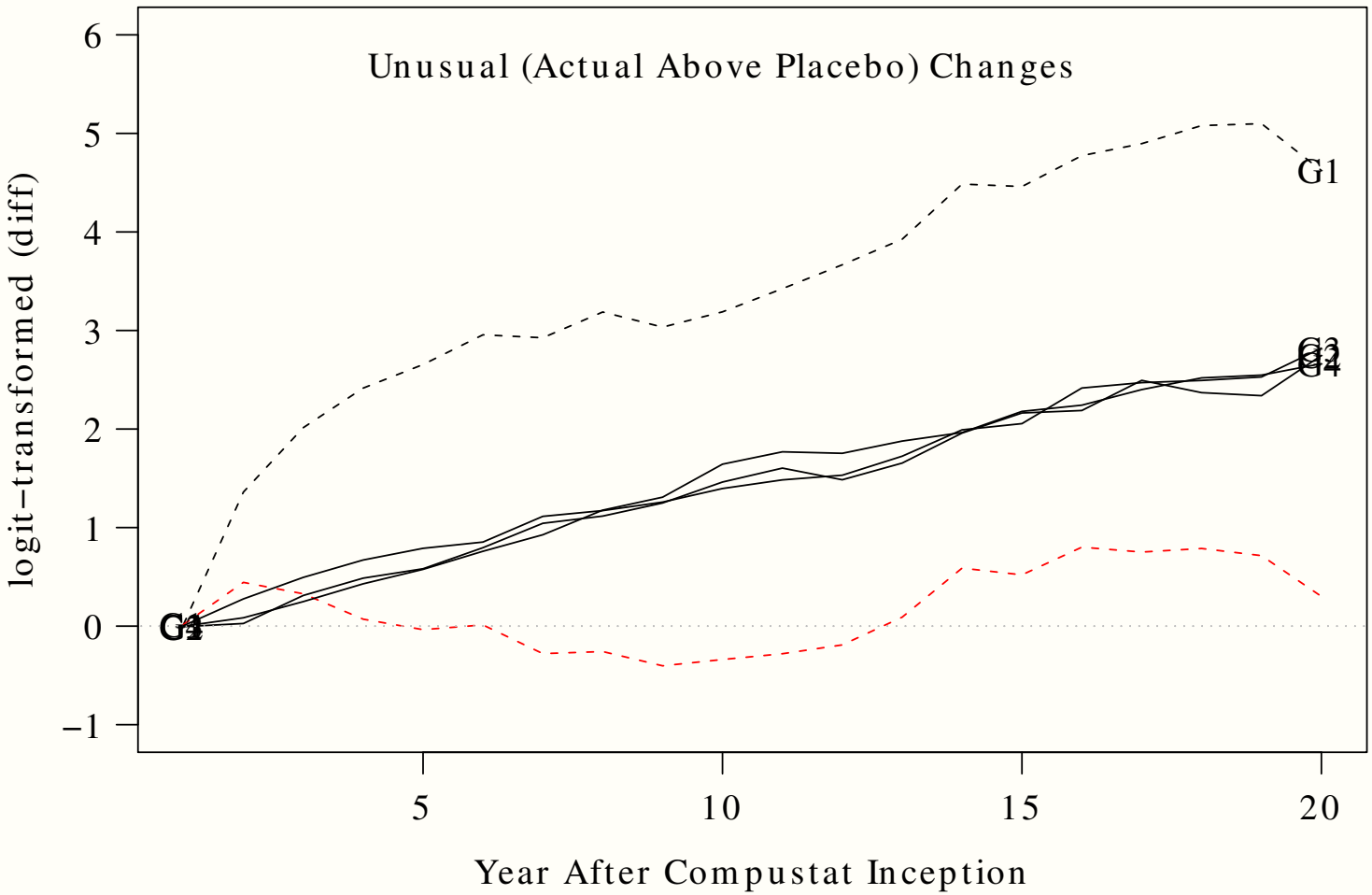
Most Memorable Graph — Empirical and Placebo



Most Memorable Graph — (Empirical - Placebo)



Back To The Beginnings: $\text{Logit}_{i,t}$ Instead



The Power of Fixed Effects

Think of fixed-effect R^2 as a descriptive statistic, not a test statistic. Leverage ratios are not the standard scenario.

- LRZ report FE explain $R^2 = 67\%$ ($\overline{R} = 63\%$).
- Our data: FE explain $R^2 = 64\%$ ($\overline{R} = 60\%$).
- Placebo: FE explain $R^2 = 68\%$ ($\overline{R} = 65\%$).

Slightly higher R^2 may be due to more deaths of poorly performing high-leverage firms.

- PS: for testing fixed effects vs. known variables, LRZ show that typical variables in LRZ explain little on the margin—but they did not use the variables that would have been strong. if you want to use historical performance as an explanatory variable, please use it in the best functional form, $D/(D + E \cdot (1 + r))$. Yes, $r = r(\text{performance})$ will allow performance to matter linearly, but only very weakly. (Also, not that FE's subtract an overall sample average even from early observations, where ex-post average is not yet known.)

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Evidence

- We agree: Strong permanent component

  Think panel of stock prices (RW), not of stock returns (iid).

  When leverage just wanders, historical leverage matters.

  $\Rightarrow$ *Back to the Beginnings*

  View this evidence as a reinterpretation of LRZ.

- We may disagree: I cannot see much evidence of convergence.

  May or may not have been a point of LRZ, anyway.

Warning—Common Error

- Not in LRZ, but easy to read into the paper.
- Issuing Activity $\neq$ Net Issuing Activity
- Net Issuing Activity $\neq$ Changes in Leverage

SKIP

- Just because you know that x induces issuing debt activity, does not mean it increases leverage!!
- A firm with 90% debt that issues 50% debt and 50% equity is delevering, not uplevering.
- The ratio is non-linear.

  If you run OLS of changes in the debt ratio on changes in debt, the coefficient is either weak or even perversely negative. Explaining changes in debt (or explaining issuing activity) is not explaining changes in leverage ratios.

readjustment

$\Rightarrow$

Do Firms Readjust *On Average*?

- Question about a population summary statistic.
- Not the same question as "what is the best model of capital structure evolution of *i* at time *t*?"

(With Peter Iliev.)

Readjustment Question

- Conventional True Process Specification:

$$\begin{aligned} L_{i,t} &= (1 - \text{soa}) \cdot L_{i,t-1} + (\text{soa}) \cdot T_i + \text{Noise} \\ \Leftrightarrow \quad (L_{i,t} - L_{i,t-1}) &= \text{soa} \cdot (T_i - L_{i,t-1}) + \text{Noise} \end{aligned}$$

  (Targets are firm-specific. Could be time-varying, but some econometric techniques consider them fixed-per-firm. We will substitute a known true target. Could be either.)

- For null, I will use placebo process, not normal iid.
- 10-20% of all leverage-ratio firm-years are $L_{i,t} = 0$.

Existing Estimates of Rho

Paper	SOA	Half-Life
Fama-French 2002	10%	7y
Welch 2004	0%	Never
Flannery-Rangan 2006	34%	2y
Lemmon-Roberts-Zender 2008	25%	3y
Huang-Ritter 2009	22%	3y
Elsas-Florysiak* 2010	26%	2.5y
(Avg Lifetime of Firms on CRSP/Compustat):		≈10y

* Sorting out differences with EF 2010 right now.

Processes and Statistics: =SMM =MC

Think Simulated Methods of Moments or Monte-Carlo.

- $\rho = 1$: Placebo Process
- $\rho = 0$: Immediate Reversion

- A statistic is a function of data. (Population statistic. Sample statistic.)
- An underlying process is assumed. Can be tested with diagnostics.
- An estimation statistic has certain attributes under an assumed process. A statistic that is unbiased under the SDPP is not necessarily unbiased under a different process.

  BB, LD are only (asymptotically) unbiased under SDPP.

- For a given process, I can figure out the properties of any sample estimator; and, if this statistic is informative, "de-bias" it.

  This is just simulated methods of moments.
  PS: A debiased estimator is a new estimator.

- Let's presume the true process is

$$L_{i,t} \quad = \quad \rho \cdot L_{i,t-1} \; + \; (1-\rho) \cdot \text{constant}_i$$

  on placebo process, and see how different estimators do. Will show with and without perfect knowledge of constant_i.

Existing Estimates of Rho

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(we have theories that predict $\rho > 1$, possible for $T \approx 30$ in x-section.)

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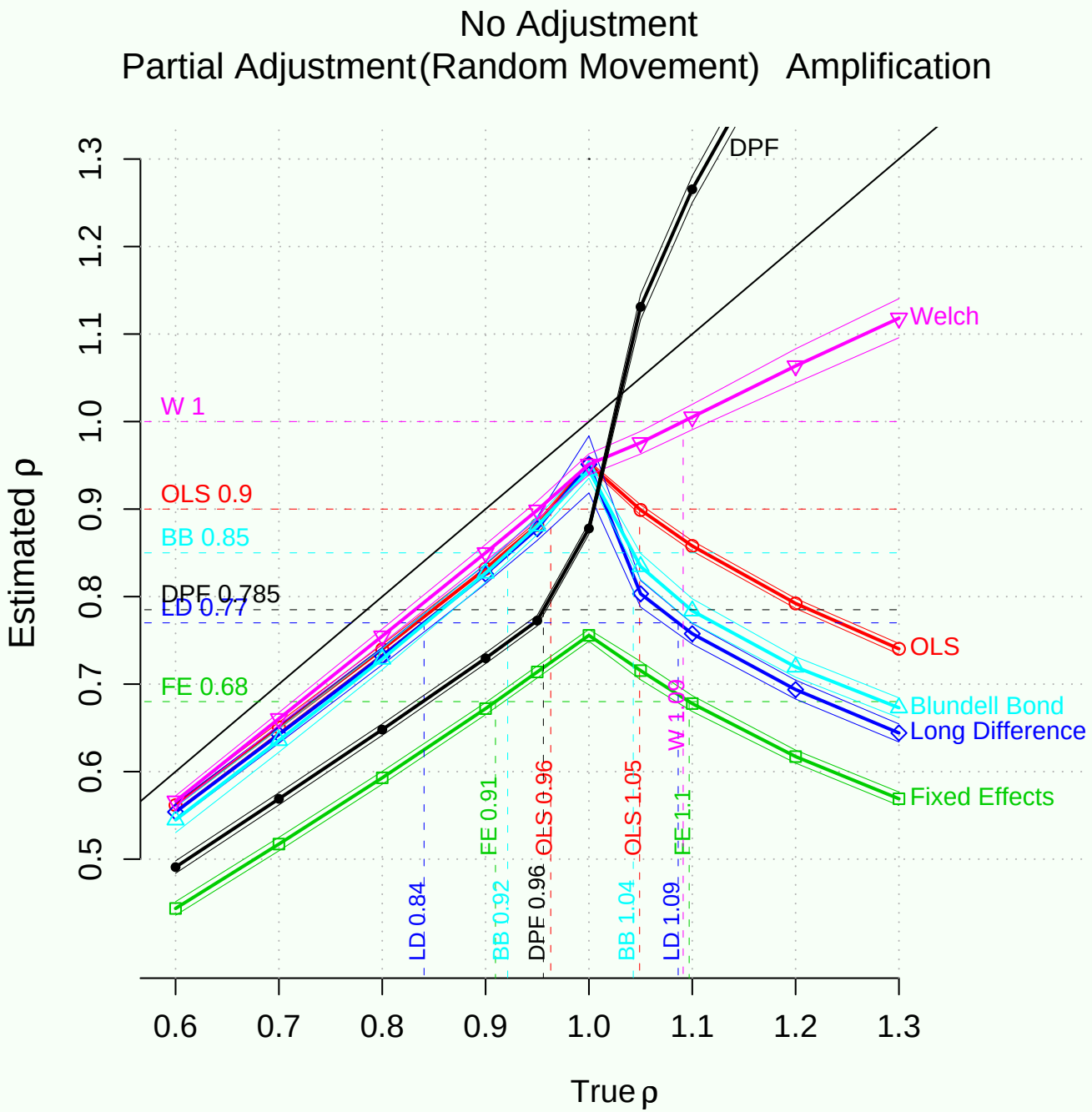
With Known Target, SOA=1 $-\rho$



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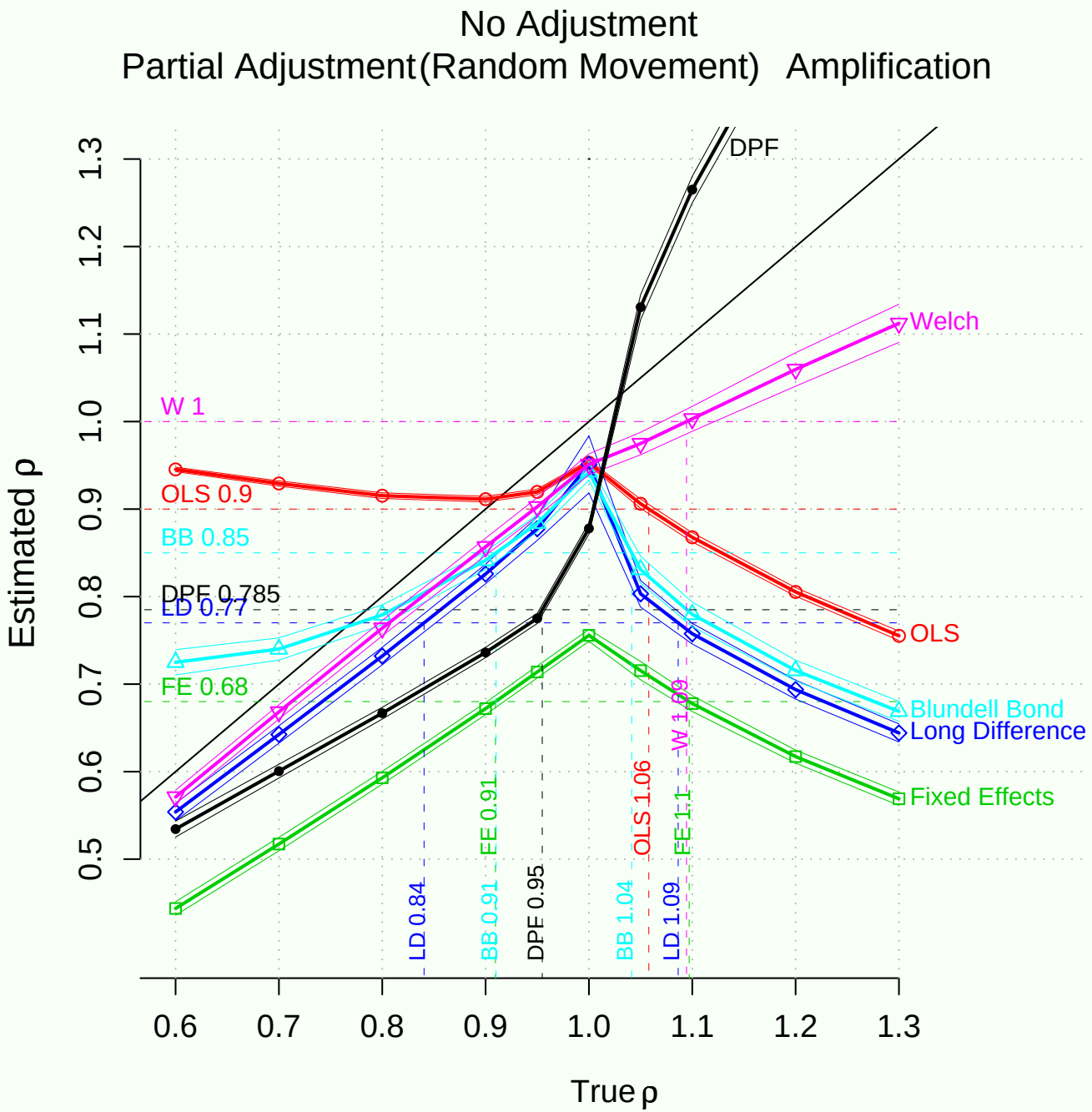

What Else Does Iliev-Welch Paper Do?

- Show earlier attempts to deal with [0,1] leverage limits (e.g., excluding close to zero obs, tobit, etc.) all fail.
- Offers Reconciliation Estimator w/o EF — SOA= -4%

  Hausman Misspecification Error: T= 3.43

  Great for 100,000 firm-years and 5 estimators!!

- With EF, still -4%, but 9% (7y) is a second local, defensible MLE.
- (PS: Note—there is no Cremer-Rao efficient dynamic panel estimator even under much more tractable SDPP.)

Why This Paper May Never Be Published

- I already knew this. I did not believe these papers.

  True for Flannery-Rangan (maybe OLS) in literature. Not for Blundell-Bond, Long-Difference, Welch.

  Footnote 1 in LRZ: Studies by Flannery and Rangan (2006), Hovakimian (2006), Kayhan and Titman (2007), Leary and Roberts (2005), and Liu (2005) find that firms gradually adjust their capital structure in response to various shocks. Please no.

- *Average* readjustment is not an important or interesting empirical regularity.
- The linear adjustment model is misspecified to begin with. We need a different model.
- You could write down a (structural) model in which this evidence would be expected. For example, ... Thus, current paper does not help us sort out what is really important.
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- What is novel here? See Dasgupta-Chang.
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- You could write down a (structural) model in which this evidence would be expected. For example, ... Thus, current paper does not help us sort out what is really important.
- These are not the most important problems in these papers. I could pick on x, y, and z in these papers, too. Why focus on this one?
- What is novel here? See Dasgupta-Chang.
- The paper should have derived an estimator that gets around these problems. =RIP. Even for SDPP, dynamic panel estimators are hellish.
- The paper's tone is too lecturing.

Why This Paper May Never Be Published

- I already knew this. I did not believe these papers.

  True for Flannery-Rangan (maybe OLS) in literature. Not for Blundell-Bond, Long-Difference, Welch.

  Footnote 1 in LRZ: Studies by Flannery and Rangan (2006), Hovakimian (2006), Kayhan and Titman (2007), Leary and Roberts (2005), and Liu (2005) find that firms gradually adjust their capital structure in response to various shocks. Please no.

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Do *Some* Firms Readjust?

(This question is about different population statistics, of course.)

Yes! — SKIP for Time Reasons

$$(L_{i,t} - L_{i,t-1}) = \text{soa} \cdot (T_i - L_{i,t-1}) + \text{Noise}$$

$$\text{soa} = \text{soa}_a + \text{soa}_b \cdot (T_i - L_{i,t-1})^2$$

Empirical			Placebo		
soa_a	=	0.1	soa_a	=	0.05
soa_a	=	0.18	soa_b	=	0.13
soa_a	=	0.05	soa_b	=	0.03

with Peter Iliev, Tarun Mukherjee, Wei Wang

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The Body of Literature

Claims of Much Deliberate Managerial Behavior:

- Leary-Roberts (2005) — Chang-Dasgupta
- Hovakimian (2006) — Chang Dasgupta
- Alti (2006) — Chang Dasgupta
- Flannery-Rangan (2006) — Chang-Dasgupta (BV), Iliev-Welch (MV)
- Kayhan-Titman (2007) — Chang-Dasgupta
- Lemmon-Roberts-Zender (2008) — Iliev-Welch (MV)
- Huang-Ritter (2009) — Iliev-Welch
- Elsas-Florysiak (2010) — Iliev-Welch

Claims of **Lack of** Much Deliberate Managerial Behavior:

- Fama-French (2002) — (CD=BV; IW=MV)
- Welch (2004) — (IW)

Oped:

- (MV) 40% of variation in leverage ratios comes from stock returns
  Other 60% comes from managerial activity, as-yet-unexplained.
- Do not use security issuing activity to argue capital structure changes. The two are not the same; and the correlations are not that high.

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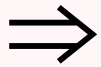
structural



Should We Use Structural Models To Explain Capital Structure Regularities?

- A generic critique—YMMV on specific papers.
- Not everything in the presentation is in the paper.

Historical Background of SMs in Corp Fin

- Lucas Critique: Econometric reduced-form models failed for policy evaluation—we need "structural models" (SMs) to predict counterfactuals out of sample.

  Counterfactuals were policy changes.

- Mehra-Prescott: Quantities are important.
- Other literatures got "envious," and started their own structural models. (Labor, IO, Trade, etc.)
- Quantitative structural models feel more like "real science."

At the risk of Carricature

Structural

- Deep assumptions to model micro foundations.
- Derived reduced forms
- Often not in closed-form
- Quantitative calibration/prediction

Reduced-Form

- Shallow assumptions
- Assumed reduced forms
- Often Qualitative (req. comp. statics)

But, of course,

- All models are structural.
- Models come in shades.
- A general critique is always a caricature for a specific model.

Perhaps it is best to ask: how serious do we take the model? Is it just for illustration and comparative statics (an effect **on the margin**, tested with controls)? Or is it for seeing whether it can explain capital structure dynamics by itself (without controls)?

Criteria for Models

(Sense and) Prediction are Criteria for all types of models:

1. Does a theory make sense and offer good predictions?

   PS: Calibrated models are still just theory, not evidence.

   If theory fails, [almost] full stop.

2. Does a theory fit the data in-sample *better*?

   If theory fails, [almost] full stop.

3. Does a theory predict out-of-sample *better*?

   If theory fails, [almost] full stop.

4. Does a theory predict during quasi-experiments *better*?

   If theory fails, [almost] full stop.

- *these hurdles are relative to substitute alternative theories.*
- *forces can be "on the margin," controlling for other complementary forces.*

Model Sense: Highlighting Inference Differences When Other Forces Are Present

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

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

$$y = r$$

- Orthogonality: Right and wrong forces are orthogonal

$$r \perp w$$

- Some third variable is sum of the two:

$$Q = r + w$$

A Reduced-Form Model On Observables

- 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 $w \perp r$

Reduced-Form Model on Observables — Inference

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

Structural-Form Model With Unobservables

- Assume r and w are unobservable, but Q is observable.
- The underlying structural model identifies

$$w \to Q \to y$$

- Researcher believes the wrong model.

$$\begin{array}{rccc} & y & \leftarrow & w \\ \Rightarrow & Q & \leftrightarrow & w + \text{Noise} \end{array}$$

with noise $\perp$ to w. (This is correct! $r \perp w$)

- Researcher estimates

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

- Estimate is $\hat{b} > 0$.
- In fact, in this case, inference is the same regardless of whether the researcher's model is right ($y \to r$) or wrong ($y \to w$).

Structural-Form Model on Observables — Inference

- Conclusion: researcher accepts the false model.

  *Variables **un**correlated with w or r affect inference.*

- Omitted forces orthogonal to w influence inference.

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

- Inference can **not** only be wrong if r correlates with w.

- To conclude that unobs struct force w matters requires **bigger** leap of faith (compared with obsvble variable w).
- When accepting your model, you need good priors that w was the correct mechanism.
  - *This empirical test could not help you to distinguish between w and $\perp r$.*
  - *In other example, more moment tests can help distinguish.*
  - *But, ultimately, unless you know a priori that Q (not r or w) is orthogonal, you will accept the model too often.*
  - *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.*

Different Names

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

  But how do you get a right instrument??

- The issue is ultimately simple:

  Forces that are entirely orthogonal to the structural model's forces screw up the inference.

- 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:

- For testing derived models on observable variables, you can state that as long as there are no omitted variables that do not correlate with your force of interest, your regressions will give the correct inference.
- For testing structural models on unobservable variables, you **cannot** state that as long as there are no omitted variables that do not correlate with your force of interest, your regressions will give the correct inference.

$\Rightarrow$ tests should be *more* skeptical and more stringent.

$\Rightarrow$ tests should hold constant as many forces as possible.

Capital Structure Research

Dependent variable to be explained: Leverage.

Strebulaev:

$$\left.\begin{array}{l}\text{Taxes}\\ \text{Transaction costs}\end{array}\right\} \rightarrow \begin{array}{c}-\text{Profitability}\\ \text{Lagged L}\end{array} \rightarrow \text{Leverage}$$

Hennessy-Whited:

$$\left.\begin{array}{l}\text{Productivity Shocks}\\ \text{Taxes}\end{array}\right\} \rightarrow \begin{array}{c}-\text{Liquidity}\\ \text{Lagged D}\end{array} \rightarrow \text{Leverage}$$

Domain of Capital Structure Research

- Taxes, Transaction Costs, Productivity (S, HW)
- Market Timing (Baker–Wurgler 2002)
- ESOPs and acquisitions (Fama-French 2002)
- Industry peers (Roberts-Leary 2009)
- Pension liabilities and industry (Shivdasani-Stefanescu 2010)
- Managerial identity (Bertrand-Schoar 2003)
- Credit Ratings (Kisgen 2006)
- Hubris (Roll 1986)
- Covenant Violations (Roberts Sufi 2009)
- Unmitigated agency concerns
- Precommitments (sinking funds)
- Risk Shifting? (Parrino-Weisbach 1999)
- Adverse selection (pecking order) (Myers ...)
- Non-optimal behavior?
  - when heuristic band is violated?
  - asleep at switch?
  - random? (I-bank influence)?
- ...

Which one?

To consider a structural model as exclusive explanation of data:

- How strong are your priors that you have the right mechanism?
- Does any one explanation seem dominant a priori?
- 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!)

What Can we Do Now?

- Identify the mechanisms separately first:
  - Start with Graham-Harvey style surveys.
  - 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)
- Proceed and model one or two forces, and then test?
  - Higher hurdles (test stringency, more moments, closer proxies).
  - Look for OOS Quasi-experiments. (Taxes, X-costs, etc.)
- Be pragmatic: what model predicts best OOS?
  - Yes, yes, yes...but my hopes for SM in corpfin are low.
  - Compare SMs to strong alternative models.
  - Assess the model quality of fit on many moments.

IS Evidence I

My Claim: Even today's best SMs have no good empirical support.

- Never entertained plausible alternatives.
- In-sample tests focused on weak predictions.
  - Existing 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.
- Will show below if one expands IS perspective slightly,
  - Below: Funding model fails as a first-order explanations.
  - Below: Tax-distress-friction model fails fundamentally.

IS Evidence II

SMs are more fragile, thus diagnostics are all the more necessary.

- In capital structure, we face
  - Errors-in-variables (proxies)
  - Residual heterogeneity across firms and industries
  - Residual auto-correlation
  - Selection biases (average lifetime: 10 years)
  - Overfitting
- How do we deal with this?
  - In reduced form (linear approximation) marginal models, we do:

    Specification tests. Control variables. Fixed effects. Differences.

    (every reduced form empirical paper at least tries.)
  - In structural model form tests, we do:

    uhhm...

    few structural form papers really try.
- Have we attempted to control for alternatives?

  This is very hard to do in SMs, but even more necessary.

Out-of-Sample Evidence

- No SM in corporate finance has ever been tested out-of-sample.
  - Needs only a criterion, like MSE.
  - Can be used to compare nested or unnested models.
  - We have good alternatives in quantitative models. They should
    - predict better actual than a randomly drawn firm.
    - predict better than "how firm has always behaved."

If I am wrong, please let me know asap.

Quasi-Experiments I: Generic

- QE Tests are almost Causality Tests.
  - Example: Regression discontinuities. Natural Experiments. (Weaker: IV)
  - QE can IMHO convincingly demonstrate empirical causality for even simple reduced-form models.
  - They contrast with correlation tests from earlier decades. (Spurious correlations are plenty in corporate finance.)
- Looking for "realized counterfactuals," as do Quasi-E tests, is in the spirit of Lucas' critique about counterfactual prediction.

  QE ≡ Realized Counterfactual

  - We have great alternatives in QE for SM tests:
    - model hypothesis is no longer "same behavior."
    - model predicts better *with* than *without* shock.

Quasi-Experiments II: In Capital Structure SM Models

- No SM in corporate finance has ever been tested in a quasi-experiment.
- Are there suitable identified shocks in capital structure?
  - Tax Law Changes (Many).
  - Transaction Costs (SEC Reg Change in 2005).
  - Bankruptcy law changes. Delaware Rulings? Financial Distress Costs (1986=prepacks. 1994=overhaul).
  - Productivity (GPS, Satnav, Containers. SOX Record Keeping costs).
- QE tests are not alternatives to SM...

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

- **...on the contrary: I want to test SM's on them.**

  For example, if a SM 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 SM is probably not useful.

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

Quasi-Experiments III: Limitations

- Shocks researchers claim to be exogenous may be endogenous to the system.

  Shocks are often exogenous to individual firms! Research can exploit variation to shocks across firms.

- There are natural limits to what quasi-experimental studies can do.

  Yes, but we have not reached them.

What Can We Do?

- I am **not** against SMs.
- I want to be convinced that the mechanism is the most likely one.
- I want models that predict well, preferably 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 "margin."
- 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.

Personal Assessment

- I do not know any successful SMs in corporate finance.

  I will take the most prominent SM papers now and critique them specifically.

- 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.

Causality Inference and Parsimony

SKIP

Parsimony

- Causality evidence is by nature extremely hazardous.
- Via model means "by assertion."

  If I claimed real-world causality evidence and/or looked at counterfactuals with a linear reduced-form model, you would laugh me out of the room.

- Counterfactuals (causality directions): better via quasi-experiments.

Parsimony

- These models are often black-boxy—it is very difficult to understand all the gears in the models. They are often non-linear, and without closed-form solutions.
- Quite forgivable if they predicted well—but not if they do not.

$$\Rightarrow$$

Strebulaev:2007

- Not a bad paper; prominent standard bearer for approach.
- Brattle Prize 2007. Job Market Paper → Stanford.
- Unchallenged.

Tax-Distress-Frictions (TDF) Model

- 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.)

$\Rightarrow$ Firms change capital structure rarely (after large shocks).

Strebulaev 2007

- Strebulaev (Tax-Distress-Frictions)
- TDF models predict inertia ($\Leftrightarrow$ profitability).
- Strebulaev calibrates non-readjustment empirical moments to theoretical model moments.
- Strebulaev is a far more complex paper, but really just a TDF.

Inertia vs. Non-Adjustment

- Inertia: Statement about variance.

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

- Non-adjustment to Shocks: Statement about changes

  $$L_{t=1} = 0.5 \rightarrow Shock \rightarrow L_2 = \begin{cases} 0 & prob\ 50\% \\ 1 & prob\ 50\% \end{cases}$$

- 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.

OOS and Quasi-Experimental TDF Tests

For all TDF models (incl. Strebulaev):

- No OOS tests.
- No quasi-experimental tests.
  - (T) Changes in Tax Law.
  - (D) Changes in Bankruptcy Costs.
  - (F) Changes in Frictions. (UW entry)
  - Changes to variables not in the theory should not matter.
- Tests are IS fits of the model to chosen moments.
- Alternative Hypotheses are ?. Controls are ?

TDF's Principal In-Sample 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 Descriptive Statistics

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

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

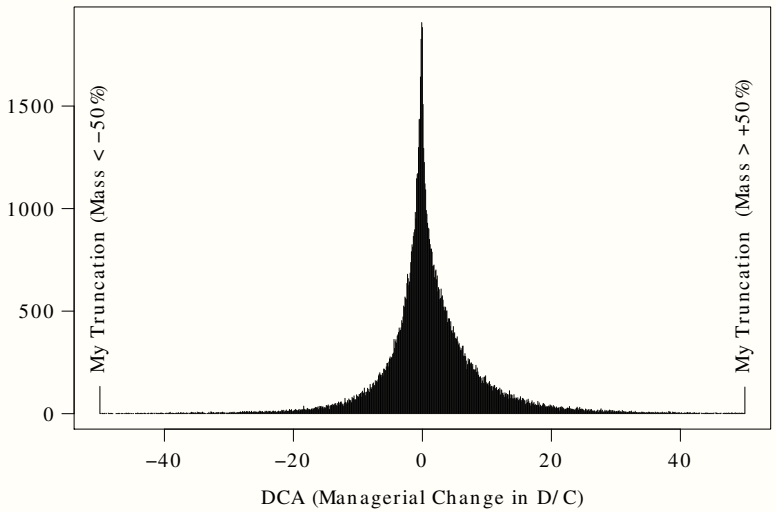
$$\text{Active} \quad \text{dca}_{t-1,t} = \frac{D_t}{D_t+E_t} - \frac{D_{t-1}}{D_{t-1}+E_{t-1}\cdot(1+x_{t-1,t})}$$

- **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

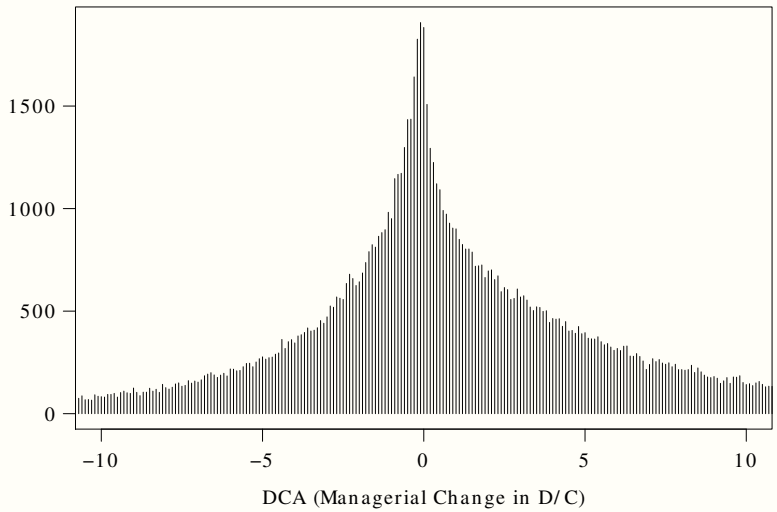
PS: Other empirical research interested in what managers are doing, why not use dca instead of dct? Avoids stock-market caused noise and stock-market anomalies (such as B/M).

Inertia Evidence: dca Histogram

Base



Zoomed



- 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 "in percent per single year" here. 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.)

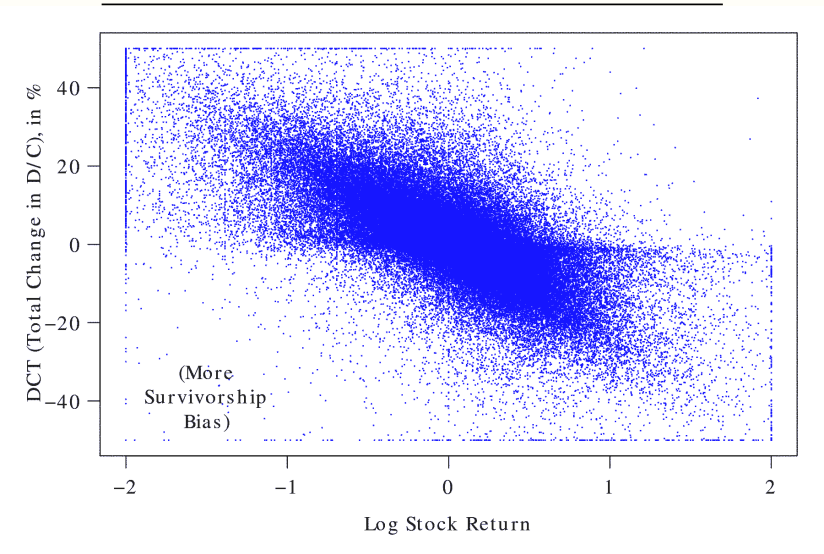
Readjusting Evidence: Stock Return Shocks

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

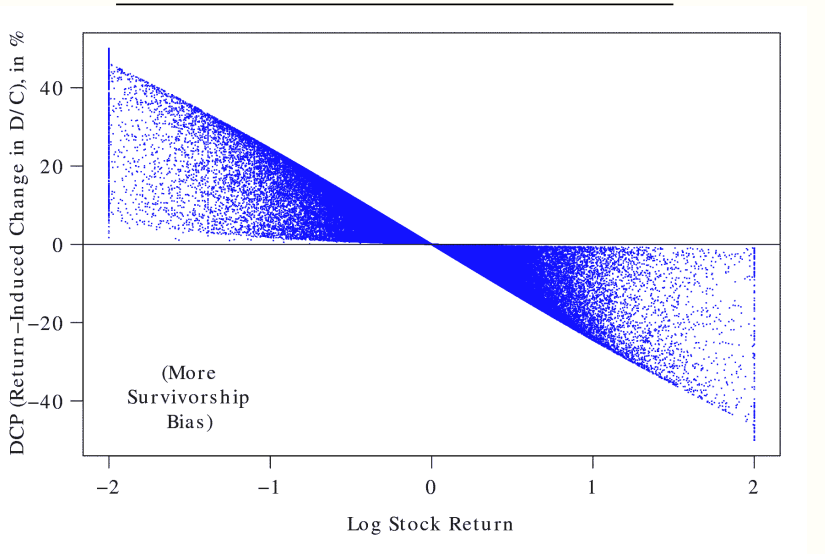
(Welch, JPE 2004)

Readjusting Evidence: Stock Return Shocks

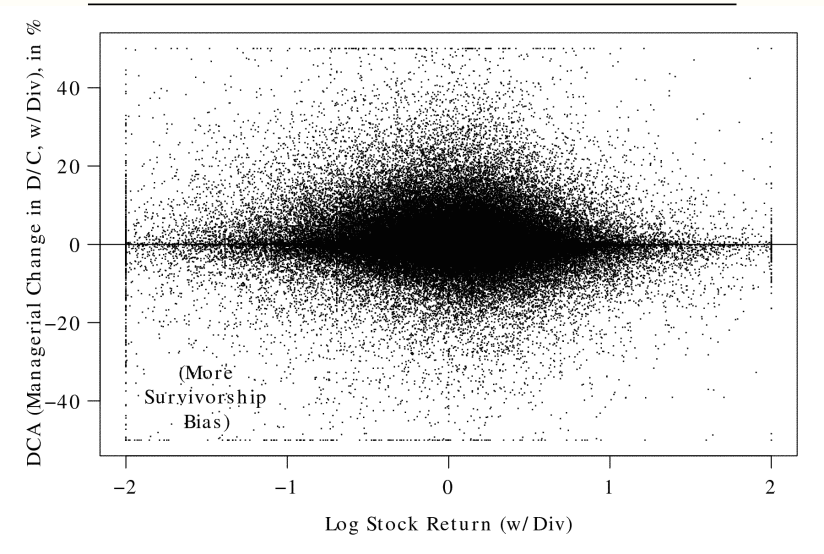
Capital Structure Change (dct)



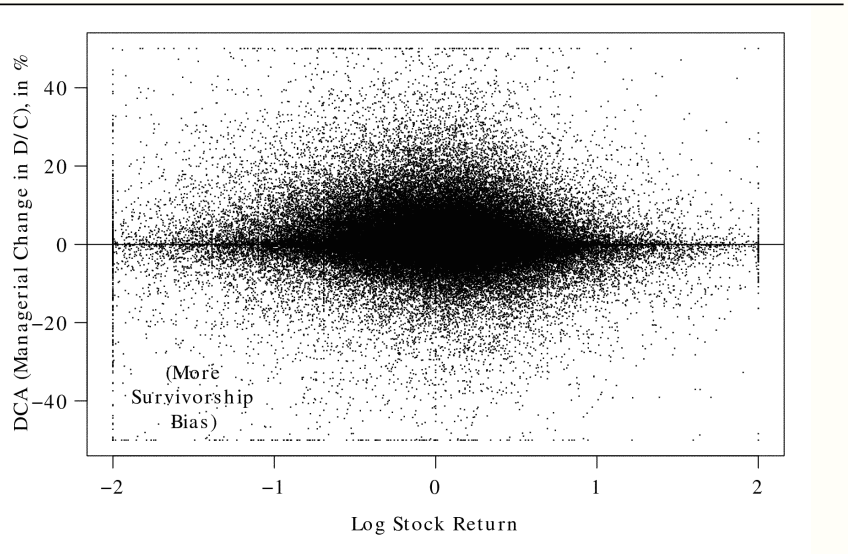
Stock-Return Induced (dcp)



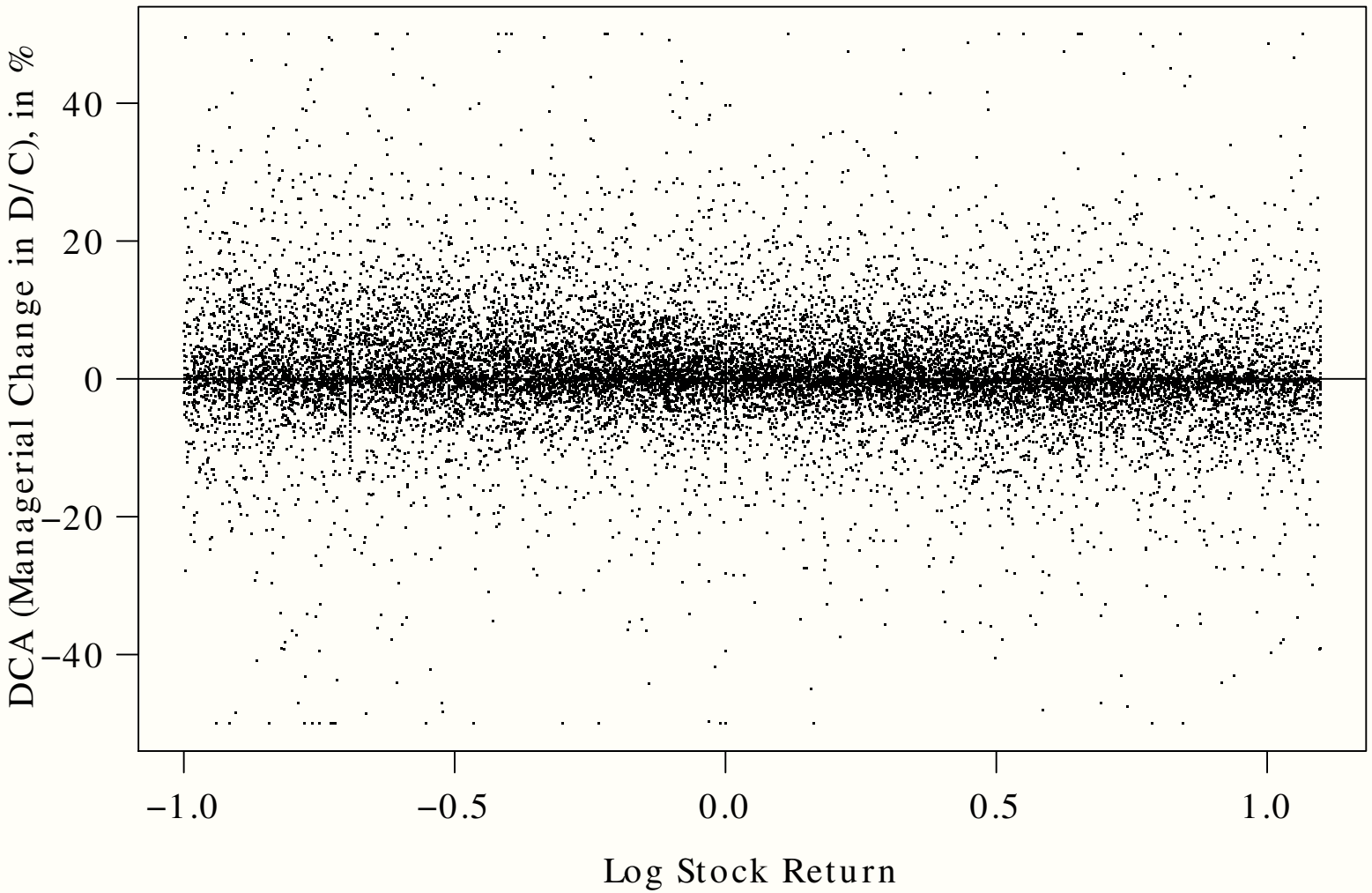
Managerial Net Response (dca)



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 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 first-order of capstruct.

Managers

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

Why This May Not Be Published

- I already knew this. Everyone already knows this.
- The original Strebulaev paper was not that interesting. It lacked x, y, and z.
- Why attack a paper from which we have learned a lot?
- The author should have reproduced and fit the model.

  Actually, we did try. The Matlab optimization from Ilya's code is often quite unstable and aborts. Besides, the graphical readjustment intuitive evidence is much clearer, even to "non-inductee" consumers of this research.

- The author should or could have reproduced another structural model, in which ...
- This is the kind of work I would expect a good referee report to contain, not a paper. It's too simple.

hennessy-whited

$$\Rightarrow$$

Hennessy-Whited 2005

- Not a bad paper; prominent standard bearer for approach.
- Brattle Prize 2005.
- Unchallenged (except Lewellen-Lewellen, *unpublished*)

OOS and Quasi-Experimental Tests

- No OOS tests.
- No quasi-experimental tests:
  - Changes in Firm Patents. (Own Productivity Shock)
  - Changes in Technology. (Systematic GPS, etc.)
  - Changes in Tax Law.
  - Changes to variables not in the theory should not matter.
- Tests are IS fits of the model to chosen moments.
- Alternative Hypotheses are ?. Controls are ?

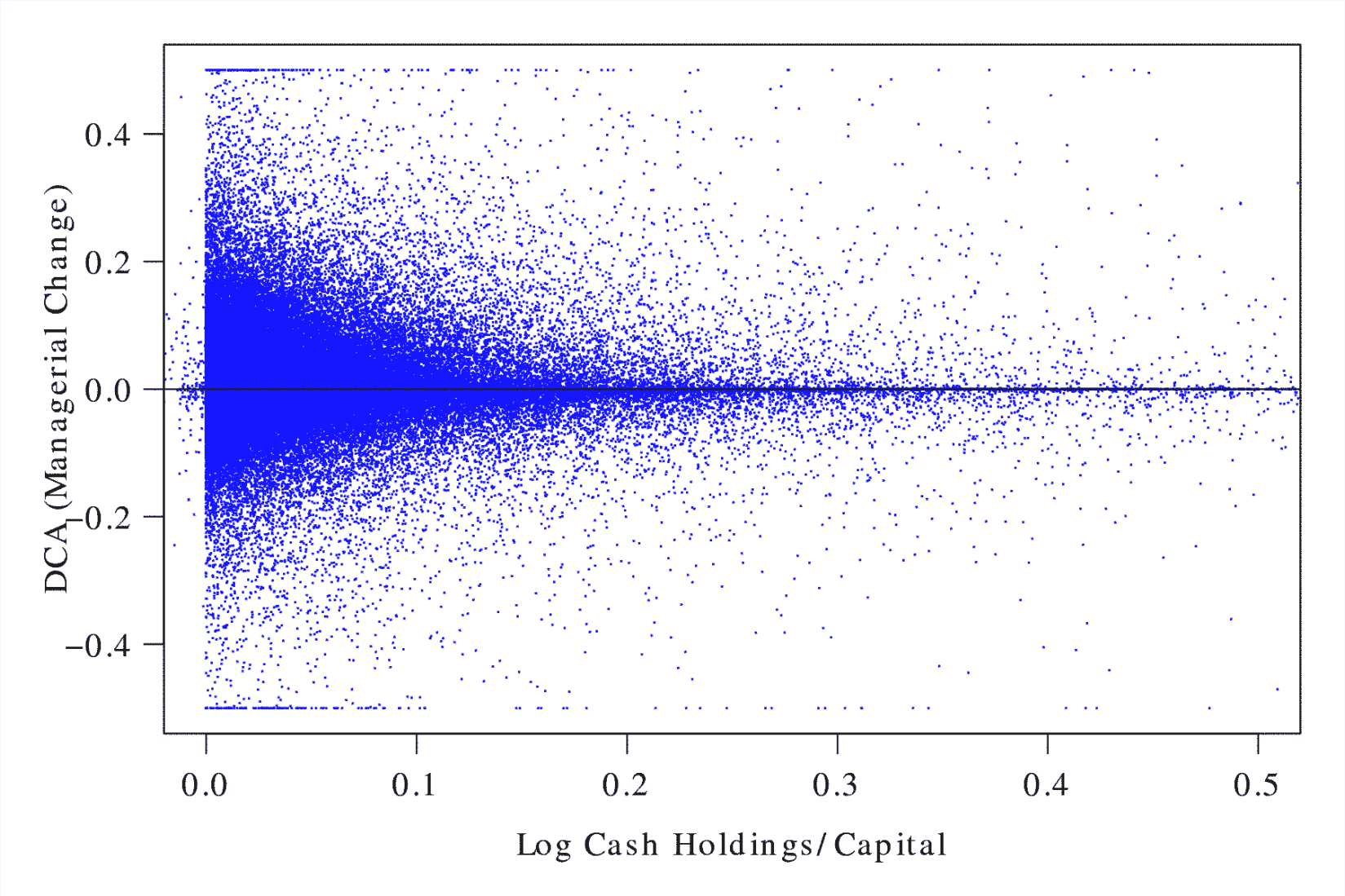
Hennessy-Whited (2005) Predictions

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).*

One referee maintains that this HW model has different implications, and that I have not fully understood its meaning.

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



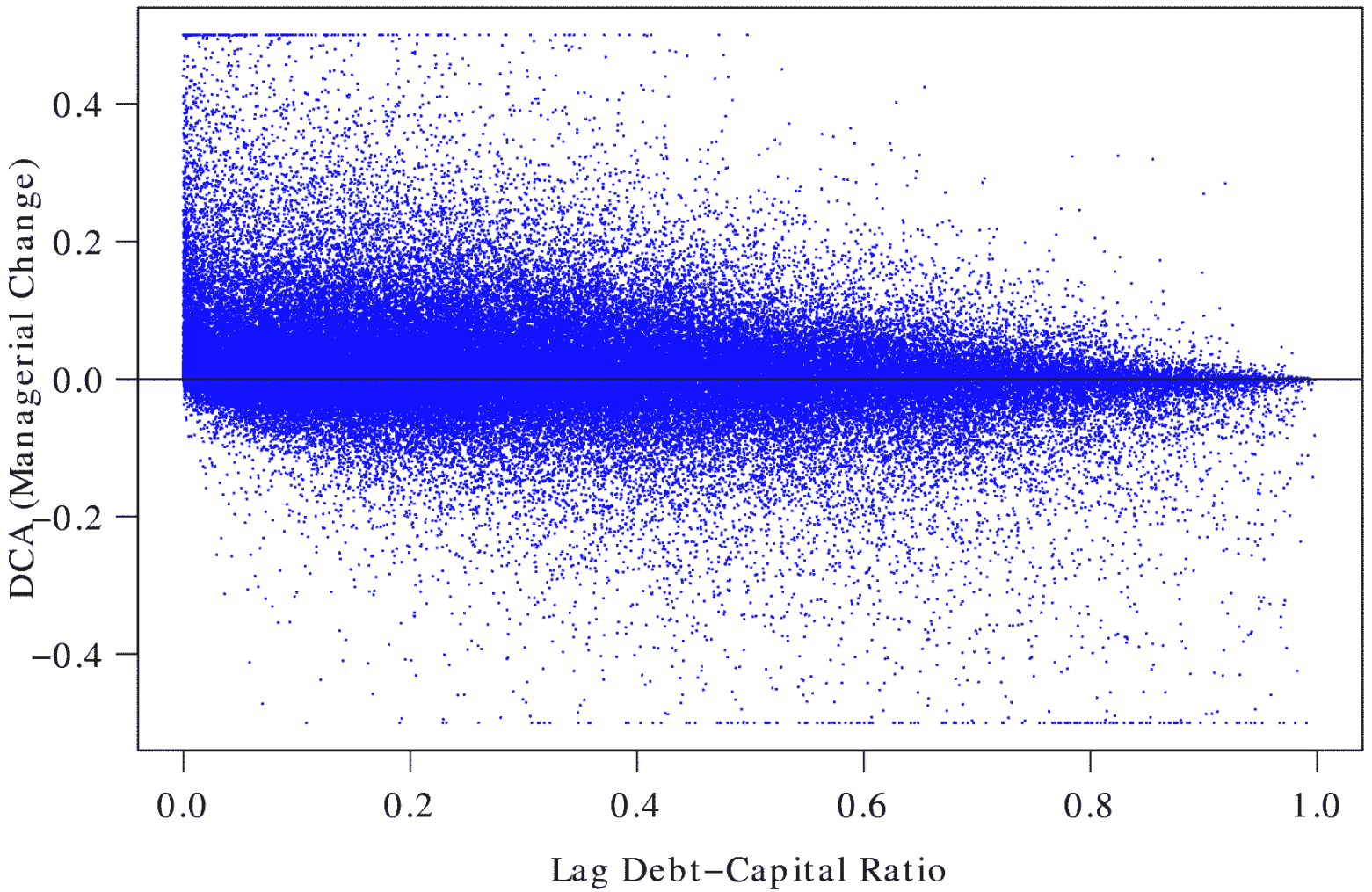
- Yes, the coefficient is -0.025 (T= -11), but R^2 is 0.0008.

⇒ 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 this 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.?

Why This Will Never Be Published

- I already knew this. Everyone already knows this.
- The original HW paper was not that interesting. It lacked x, y, and z.
- Why attack a paper from which we have learned a lot?
- The author should have reproduced and fit the model.
- The author should or could have reproduced another structural model, in which ...
- This is the kind of work I would expect a good referee report to contain, not a paper. It's too simple.

structural-conclude

S-IV: Conclusion: Are We Here?

- 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.

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, SM often explain well for derivatives and FI.
- ⊖ Empirically, most SMs 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.

My Future Plans

My career plan:

I plan to run for the title of

"Mr. Popular"

among capital structure researchers.

Quo Vadis?

What did I conclude?

- Most referee points are correct and/or ultimately indisputable.

  There is a "taste" aspect in how to assess problems.

- Capital structure is a small literature. There are 10-20 authors, many coauthors. I will likely draw similar referees again and again.
- Academic journals are biased towards the creative and novel. This is good—***unless*** *it is tough later to correct issues.*
  - Referees are not fond of seeing existing work (possibly, friends or (ex-)colleagues) critiqued. Possibly related to their own work.
  - Again, in all comments, **the referees may well be correct.**
  - Journals are not thrilled to see own published papers torn down.
- Many papers still cite as evidence the classic Titman-Wessels (data ended in 1982—30 years ago). Great paper—but does it still hold?
- We need a journal for publishing critiques.

  http://www.critical-finance-review.org/

  Of course, I cannot publish my own papers in my own journal. I hope others will write critiques and submit them to the CFR.