

Introduction

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This volume is the first in a set of planned special volumes, in which the *Critical Finance Review* (CFR) is soliciting teams to reanalyze seminal studies in finance (suggestions for future issues are welcome). The reanalyses are peer-refereed. However, the CFR expects to accept them, given due and appropriate competence and care. This editorial process is similar to the processes used for symposium issues at other peer-reviewed journals. Critically, the papers will explicitly not be rejected if they report that all aspects of the original studies were perfect.

These special volumes exist because they make it possible to debate alternative viewpoints on the empirical findings of greatest importance to our profession. They will make it easier to assess both p-hacking and reverse p-hacking. Despite our call emphasizing that the editorial decision to publish reanalyses does not depend on their findings, the authors of reanalyses still have some conflict of interest. They would likely prefer to find some weaknesses in the original studies. This is similar to the conflict of interest that the original authors had. They likely preferred to find few weaknesses. Both conflicts of interest are intrinsic.

Reexaminations also have the benefit of hindsight. They can use better techniques and more recent data. Who would not want to know whether the subsequent financial crisis of 2008 has altered the inference in earlier liquidity papers?

1 Reproduction

Our inaugural CFR special volume revisits three seminal papers on liquidity in stock markets: Acharya and Pedersen (2005), Amihud (2002), and Pástor and Stambaugh (2003).

- **Acharya and Pedersen (2005 Table 4)** was reproduced by Holden and Nam (2019, Table 3) and in Kazumori *et al.* (2019, Table A4). Acharya and Pedersen (2005) explain that their variables are highly collinear. This sensitivity probably explains why the two reproducing papers fail to agree with the original paper on both coefficient estimates and test inference. Even the overall explanatory power (R^2) of the regressions are not always identical.
- **Amihud (2002 Table 2)** was reproduced by Harris and Amato (2019, Table 2) and Drienko *et al.* (2019, Table 2). The coefficient estimate of the

key variable (IlliqMA) has a lower alpha estimate, but both the sign and the statistical significance remain.

- **Pástor and Stambaugh (2003 Table 8A)** report alpha spreads that are about 20% higher than those reported in Li *et al.* (2019, Table A6). Pontiff and Singla (2019, Table 1) reproduced(!) the estimate on the current Pastor–Stambaugh website, which is only about 6% higher than that in Pástor and Stambaugh (2003, Table 8A).¹

None of the papers' original results were reproducible in the strict sense (Welch, 2019). It is impossible to reproduce all but the most trivial results without archived code and data, and this was not practice in the early 2000s. Pástor and Stambaugh (2019) point out that even CRSP has corrected data in the interim. Neutral or beneficial improvements in the code may also have altered results. Even the two concurrent reproducing papers—working off the same descriptions (and with the benefit of an original paper's numbers to consult)—could not agree on the exact same numbers!

For the three original liquidity papers, the reproduced results seem close enough to suggest that they were carefully analyzed at the time.

2 Further Reanalysis

Given that original data and code for the three liquidity papers are no longer available, it is not possible to cleanly classify the remaining types of reanalysis (should the baseline be the original or the reproduced numbers?). The reanalyses and responses contain elements of reproductions, extensions, updates, replications, reexaminations, and reconciliation.

My own (current) perspectives are as follows:

- Kazumori *et al.* (2019) and Holden and Nam (2019) find that many of the original LCAPM results in Acharya and Pedersen (2005) do not survive reproduction. For the most part, the results in the extended sample results are similar, with some results holding and others not. Kazumori *et al.* (2019) further extend the analysis to Japan, where all tests fail. Arguably, Japan is and always has been the source of populations different from those in the US. Yet, it leaves the question of why the theory would hold in the US but not in Japan.

In response, Acharya and Pedersen (2019) point out that these issues are not unlike those plaguing the CAPM. Many but not all of the coefficients even

¹Pástor and Stambaugh (2003) had inadvertently omitted describing a data-treatment detail. Such imperfect textual descriptions are almost inevitable. With some emails to and fro, our papers in this volume have now cleared this up. Future authors should no longer have any difficulties extending Pástor and Stambaugh (2003).

have the correct theoretical sign—something that cannot be said of many CAPM tests. As for me, although I find the ideas and theory in Acharya and Pedersen (2005) beautiful, economically sound, even convincing, it seems that the empirical data does not favor their hypotheses.

- Both Drienko *et al.* (2019) and Harris and Amato (2019) first confirm the Amihud (2002) results and then find that the relations have since become much weaker or have outright disappeared. Both also find that other dollar-trading volume ratios can perform about as well.

In response, Amihud (2019) suggests an improved liquidity factor. It is the purpose of academic research to build on existing research, learn from shortcomings, and improve on it. I hope that this new model will become a leading contender in explaining liquidity in the future.

- Li *et al.* (2019) and Pontiff and Singla (2019) both find that the Pástor and Stambaugh (2003) factor performs even stronger in the extended sample. However, both also find that the factor performance is sensitive to its construction details. For example, there is a discontinuity in the treatment of zero vs. one share of trading.

In response, Pástor and Stambaugh (2019) point out that this reflects a discontinuity in how prices are quoted: with one share of trading volume, a CRSP price is either a bid or an ask; with zero shares, it is a mid-point. This is important when it comes to measuring price reversals (it would be interesting to research whether excluding these observations contains a selection effect and not only a treatment effect). They also mention an interesting “smell test”: Their factor (and some other factors that their response discusses) pick up the 2008 liquidity crisis, whereas other factors suggested by Li *et al.* (2019) and Pontiff and Singla (2019) do not.

3 Conclusion

Intuitively, liquidity seems as, or more important than, factor risk. However, our profession still does not fully understand it. We need a more comprehensive and parsimonious understanding of its patterns, both in stock markets and beyond.² The three original papers have shaped our understanding of liquidity in the stock market. Our six reanalyses and three responses should help us understand it further, and with the hindsight and perspective of 2019. This is scientific progress.

Pástor and Stambaugh (2019) explain why progress has been so difficult: like rare events, liquidity is a sleeper agent that raises its head primarily in severe

²Spiegel (2008) raises even deeper questions about cross-market liquidity effects reaching beyond stock markets.

episodes. As in Welch (2016) (pardon thy self-citation), it may be possible to use the pricing of other financial contracts to measure the associated liquidity-premium in times in which a liquidity crisis may have loomed but ultimately did not occur.

The CFR and the broader profession owe a debt of gratitude to all participating authors.

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