Perspectives

A Fanatic is One Who Can't Change his Mind and Won't Change the Subject[1]

July 16, 2017

In his latest whitepaper, Rob Arnott is still repeating things like this: **"We point out that some of these factors owe much (or all) of their past efficacy to rising relative valuations."** It's not a minor assertion but one of his central themes. It was the main topic of the first major paper in his recent wave of "most of the factors are overpriced, data mined, and doomed, save only for the value factor which I still occasionally rename and claim as my own" series, and he returns to it here.

I have lots of disagreements with Rob on a variety of topics connected to these issues. But, most of the disagreements are of degree and not of kind, and there are even some important agreements. For instance, I think he overstates the power of pure price-based contrarian factor timing and understates how correlated such timing is to the simple value factor itself. Though, as a related yet separate issue, I have often noted that even if factors are not expensively priced versus history (and most are not right now) simply being more well-known than in the past increases their short-horizon left-tail. I believe I'm a realist and not a cheerleader and Rob and I do share some of these worries. I also agree with much of his firm's recent work arguing that value investing is far from dead. I would, in particular, also agree with his consistently reminding us that many investors, to their harm, tend to chase 3-5 year performance (what I've called momentum timing done at a value time horizon in peeve #3). When Rob tries to talk investors out of chasing 3-5 year factor performance he's quite simply doing the Lord's work. Now, where we differ here is I don't think you should do too much of the opposite either. But, even if I disagree on the power of contrarian factor timing, at least here I think he is getting the sign right.[2] Frankly, all considered, on most issues we agree on more than we disagree (though admittedly I don't always make that clear enough!).

Which brings us to this important exception: On his assertion that factors **"owe much (or all) of their past efficacy to rising relative valuations,"** there isn't, as yet, any such ground for agreement or even reasonable disagreement. Let's step back. Many worry about too much data mining among academics and practitioners to find factors. Rob worries about it and so do I. If that is all he was saying here we'd be copacetic. But he's going much further than such general worries about data mining. With this and many other similar statements, he is saying that he has identified and quantified the specific thing misguided factor researchers data mine over — richening valuations — and that this is such a huge effect that much of the quantitative investing community is misled (and thus actually at least a tad incompetent). This is not minor stuff. He says it over and over throughout recent work. He said it in the first and now in this latest paper in this series. He says it in conference talks and webinars 24/6 (on Sunday he rests). Yet, he's quite clearly wrong, has been shown this, and doesn't moderate what he's saying or respond to said evidence. If I seem frustrated by all this I readily admit it! In fact, as I discuss in my "Philippic" the idea that rising (or falling) valuations could influence a backtest can have teeth under the combination of a) a slow turnover factor, b) a relatively short period, or c) an end point where valuation is very extreme. An example is a, say, < 20 year backtest of value investing (one of the slower turnover factors) examined at the end of the 1999-2000 technology bubble. At that point you'd find that the return to the value factor is unimpressive or even lousy for precisely the reason Rob focuses on — it cheapened tremendously over that period. However, Rob's papers are clearly looking at the core set of factors that have backtests going back at least 50-60 years, and he's clearly talking about these factors (e.g., profitability, low beta, momentum) where none of these three conditions apply (ok, maybe, as shown in my Philippic, profitability has low enough turnover, but valuation changes still don't matter over the 60 years or so that we can test this one over). However, let me make a prediction. If he ever does respond on this issue he will focus on much shorter backtests (where there are times that richening or cheapening matters). But remember, respected academics (and thoughtful practitioners) usually don't draw conclusions on the long-run efficacy of a factor based on only a decade or so of data. Most importantly, the main factors being discussed here, and Rob impugns in his various papers, are all indeed testable for much longer periods.

His repeating (and repeating) this specific, very damning, and very broad accusation against most of the factor researching world really surprises me. I thought I put it to bed as "without merit" here by showing that since factor portfolios are dynamic, not static, and part of valuation changes are changes in fundamentals, not price, the effect Rob keeps asserting can only account for a tiny fraction of historical average factor performance, not the near 100% ("or all") that he alleges.[3] There's more to it than this short summary but, if interested, you will have to read the original as to do it all again here would take years and cost millions of lives.

It goes without saying that I may indeed be wrong. But, if so, Rob and team should demonstrate it. Frankly, he should either retract his oft made assertions or support them. But, at this point I've been beaten down and would settle for something much weaker than those two alternatives: just stop repeating the falsehood anew! That has yet to happen.

If I wrote something, and Rob went to great pains to demonstrate I was wrong, I'd either publicly admit it (we all make mistakes) or counter

with where I think he, in turn, went astray. But I would not simply repeat the broadside over and over because I liked the story, ignoring a critic of Rob's stature (I mean this last part honestly and without sarcasm).

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[1] That quote is attributable to Churchill. As you might guess after reading this, I am not sure if I'm referring to Rob, myself, or both of us. Ok, certainly I should be included. Though, while you could be forgiven for thinking otherwise, I promise I'd truly love to stop iterating on this one and have it addressed and resolved one way or the other.

[2] That is, if you have to time this way, sell expensive and buy cheap.

[3] In fact, as I discuss in my "Philippic" the idea that rising (or falling) valuations could influence a backtest can have teeth under the combination of a) a slow turnover factor, b) a relatively short period, or c) an end point where valuation is very extreme. An example is a, say, < 20 year backtest of value investing (one of the slower turnover factors) examined at the end of the 1999-2000 technology bubble. At that point you'd find that the return to the value factor is unimpressive or even lousy for precisely the reason Rob focuses on — it cheapened tremendously over that period. However, Rob's papers are clearly looking at the core set of factors that have backtests going back at least 50-60 years, and he's clearly talking about these factors (e.g., profitability, low beta, momentum) where none of these three conditions apply (ok, maybe, as shown in my Philippic, profitability has low enough turnover, but valuation changes still don't matter over the 60 years or so that we can test this one over). However, let me make a prediction. If he ever does respond on this issue he will focus on much shorter backtests (where there are times that richening or cheapening matters). But remember, respected academics (and thoughtful practitioners) usually don't draw conclusions on the long-run efficacy of a factor based on only a decade or so of data. Most importantly, the main factors being discussed here, and Rob impugns in his various papers, are all indeed testable for much longer periods.

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