The Siren Song of Factor Timing

Aka "Smart Beta Timing, aka "Style Timing"

Clifford S. Asness
AQR Capital Management, LLC
Greenwich, CT

Invited Editorial published in *Journal of Portfolio Management,*
Quantitative Equity Strategies Special Issue, 2016

Published article can be viewed at:
https://www.aqr.com/library/journal-articles/the-siren-song-of-factor-timing

Abstract

Everyone seems to want to time factors. Often the first question after an initial discussion of factors is "ok, what's the current outlook?" And the common answer, "the same as usual," is often unsatisfying. There is powerful incentive to oversell timing ability. Factor investing is often done at fees in between active management and cap-weighted indexing and these fees have been falling over time. Factor timing has the potential of reintroducing a type of skill-based "active management" (as timing is generally thought of this way) back into the equation. I think that siren song should be resisted, even if that verdict is disappointing to some. At least when using the simple "value" of the factors themselves, I find such timing strategies to be very weak historically, and some tests of their long-term power to be exaggerated and/or inapplicable.

The Siren Song of Factor Timing

Aka "Smart Beta Timing aka "Style Timing"

Clifford S. Asness[1]

What follows is the working version posted: April 14, 2016. Final, published article can be viewed at: **https://www.aqr.com/library/journal-articles/the-siren-song-of-factor-timing**

While consensus might be too strong a word, modern financial researchers have mostly coalesced behind a set of "factors" that both explain security returns and deliver a positive return premium (not necessarily the same things). A "factor" is the spread between the return on one set of securities, systematically and clearly defined, versus another. Perhaps the most famed and basic one is the market factor or the spread of the capitalization weighted stock market over the risk free rate. Other factors, and the ones I'll discuss here, compare some stocks to other stocks.[2] These include such well-known examples as: the spread between the return of small vs. large stocks ("size"), cheap vs. expensive stocks ("value"), recent winners vs. losers ("momentum"), higher vs. lower yielding securities ("carry")[3] and low risk and more profitable companies vs. high risk and less profitable companies ("quality").[4]

"Smart beta" as a term is a relatively recent relabeling of factor investing. It usually comes with a focus on the simplest versions of known factors implemented in a long-only (i.e., performance vs. a benchmark) fashion and mostly, to date, in individual stocks. It's also common to call groups of factors that are driven by a common theme (e.g., value or momentum) "styles." While this essay will stick with discussing "factors" the semantic wars rage on and unless otherwise noted these comments should be considered applicable to factors, smart beta, styles, and probably other labels.

Research has mostly focused on the average returns to these factors (size and statistical significance). How strong are they? How robust are they? "Robustness" meaning do they pass a series of tests of reasonableness including working out-of-sample and fitting a sensible economic story.[5] If successful,

---

[1] Clifford Asness is co-founder of AQR Capital Management in Greenwich, CT.

[2] Although we'll focus on the most commonly studied area of individual stock factors for this essay, if applicable, most robust factors also have efficacy for comparing one country's stock or bond market to another, comparing currencies, commodities, etc. The factors I discuss here have, to the extent possible, survived these important out-of-sample tests. See, for example, Asness, Frazzini, and Pedersen (2013) and Asness, Ilmanen, Israel, and Moskowitz (2015).

[3] In stocks, the way carry is typically defined (as dividend-to-price) is not substantially different from the value factor, but for many non-equity markets, carry captures something separate from value.

[4] Risk can be measured by things like realized market beta, volatility, or leverage.

[5] As always, we use the word "works" as statisticians and economists, meaning delivering extra return on average with, in our view, acceptable risk and reasonable periods of underperformance. Nothing "works" all the time in investing!

robustness tests like these may increase our belief that the average results are real and not random fluctuations found by computers too powerful and databases too vast for our own good. Furthermore, research has also focused on what particular combination of these factors one should hold in an optimal portfolio. There is some broad agreement on the set of candidate factors and some major overlap in recommended combinations but rarely do independent researchers agree precisely. This story will continue.

Factor Timing

Finding a factor with high average returns is not the only way to make money. Another possibility is to "time" the factor. To own more of it when its conditional expected return is higher than normal, and less when lower than normal (even short it if its conditional expected return is negative).[6] An extreme form of factor timing is to declare a previously useful factor now forever gone. For instance, if a factor worked in the past because it exploited inefficiencies and either those making the exploited error wised up or far too many try to exploit the error (factor crowding) one could imagine the good times are over and possibly not coming back. I think of these as the "supply and demand" for investor error![7] Factor efficacy could go away either because supply went away or demand became too great.

Why do I call factor timing a "siren song" in my title? Well, factor timing is very tempting and, unfortunately, very difficult to do well. Nary a presentation about factors, practitioner or academic, does not include some version of "can you time these?" or "is now a good time to invest in the factor?" I believe the accurate answer to the first question is "mostly no." However, my answer is usually met with at least mild disappointment and even disbelief. Tempting indeed.

I argue that factor timing is highly analogous to timing the stock market. Stock market timing is difficult and should be done in very small doses, if at all. For instance, Asness, Ilmanen, and Maloney (2015) call market timing a "sin" and recommend, using basic value and trend indicators, to only "sin a little." The decision of how much average passive stock market exposure to own is far more important than any plausibly reasonable amount of market timing. Given my belief in the main factors described above – that is I do not think they're the result of data mining or will disappear in the future – the implication is to maintain passive exposures to them with small if any variance through time.[8] Good factors and diversification easily, in my view, trump the potential of factor timing.

Asness, Friedman, Krail and Liew (2000), henceforth AFKL, introduced the idea of measuring the "value spread" of the value factor itself as a basic timing tool for the value factor. The "value spread" is the valuation measure (in that paper book-to-price but this can, and has, been extended to any valuation

[6] Note, whether a factor has a long-term positive mean and whether or not one can time it are really somewhat independent questions. A factor could have a zero long-term mean but be "timeable." Alternatively (and I will argue far more relevantly) a factor could have a large and economically intuitive positive average long-term mean but not be timeable at all.

[7] Of course we always have the two possibilities that real factors may be caused by inefficiencies or rational risk-based effects or some combination of the two.

[8] "Passive" in this case is passive exposure to a dynamic long-short factor.

measure) of the long portfolio divided by the same measure for the short portfolio. While the value spread for value factors (if the spread and the factor are constructed using the same measure, like book-to-price, sales-to-price, etc.) will always be over 100%, as that's how the factor is constructed, there is considerable variation through time. The basic idea was that the factor may perform better than normal when it's ex ante cheaper than normal and vice versa. This idea has been used and extended to other factors in work such as Cohen, Polk and Vuolteenaho (2003), Asness (2015), and Arnott, Beck, Kalesnik and West (2016), henceforth ABKW.

I find that in anything like normal times using these value spreads to time factors is not particularly useful. The timing strategies that logically follow (going long the factors when they are cheap versus history and short when they are expensive) aren't exceptionally strong and way too correlated to the basic value strategy itself to make great impact on a portfolio.[9] This is intuitive. If you attempt to time a strategy like, say, the profitability factor, with its own richness or cheapness compared to history, it seems rather obvious that this timing will itself have a strong similarity to basic value investing. I find this is indeed so and nothing special is added from the act of factor timing itself.

While I believe that aggressive factor timing is generally a bad idea[10], there is one possible exception. Perhaps the only thing of interest in these value spreads would be if and when we see things unprecedented in past experience. The 1999-2000 tech bubble episode focused on by AFKL was indeed such a time. If timing were ever to be useful it would be at such extremes. Factors being "arbitraged away" or an extreme version of "factor crowding" would likely entail observing such extremes. In the extreme crowding case we'd see spreads in the opposite direction of what value experienced in 1999-2000 when the value factor looked much cheaper than any time in history. So, an "arbitraging away" would lead to a factor looking much more expensive than any time in history. To date, the evidence that this has already occurred is weak and mixed. For example, if you look at the "value spread" of the factors through time to judge them as cheap or expensive, you get very different answers depending on whether you use, say, book-to-price or sales-to-price.[11] For instance, if you use book-to-price[12] you'd find the value factors currently look cheap[13] versus history (though nowhere near the levels of 1999-

[9] Results are often positive, but we already expect that to occur as we know value strategies themselves are positive over the long haul. The question isn't whether they're positive but whether they're additive to an already diversified factor investor who is already invested in the non-timed value factor itself.

[10] Here I discuss only style timing that's purely contrarian based on going longer when the value spread is wide (the "value of value" or the "value of momentum") and shorter when it's tight. Of course, other methods might be reasonable. Factor momentum/trend is an obvious one to try. Still others might be based on economic and behavioral theories of why the factor works (a potentially fruitful path that also has great potential for dangerous data mining). I focus on the narrow question of valuation-based timing here. This is both because I consider it the most basic and intuitive of timing strategies but also as it is most relevant to our biggest worry – that the factor might get arbitraged away (if so we'd expect the value spread to be very low versus history – probably unprecedentedly so).

[11] Data is based on U.S. stocks from 1968-present.

[12] To be clear as in Asness (2015), here we are talking about the ratio spread in book-to-price of long and short portfolios formed on measures like book-to-price and sales-to-price themselves.

[13] Note, Asness (2015) uses data through June 2014 and concludes that value is at normal levels of attractiveness. Extending the same analysis to the present indeed shows that value looks cheap versus history (looking just at the book-to-price value spread of the book-to-price value factor).

2000) and the non-value factors (things like momentum, profitability, low beta) look expensive. However, if instead you use sales-to-price to make this judgment you find current levels are far closer to historical norms.[14]

Importantly, no matter what method we use, current value spreads on the most well-known factors, whether on the high or low side, are within normal historic ranges (we don't see tech bubble like events or spreads extremely tighter than historical ranges). What we see today are not giant unprecedented events but, rather, some factors somewhat historically cheap versus history and others somewhat expensive, and all within historical norms. It seems highly unlikely this is the telltale sign of extreme factor crowding and barring such an extreme I, again, find timing based on these spreads to add little to a portfolio in terms of return or diversification (lost diversification is not overcome by timing benefits).

Of course, just because this arbitraging has not occurred yet does not mean it won't happen going forward. Again, to the extent these factors are based on some market inefficiency (systematic investor error reflected in prices), they are vulnerable to people figuring out, and correcting, their errors en masse, or perhaps more germane to the current debate, too many investors trying to exploit the other's error. However, if these factors have not yet been arbitraged away as I claim, but are in the future, it will be a pleasant path getting there. Being "arbitraged away" is indeed bad on a going forward basis but very enjoyable on the way! Of course this means that continuing to monitor these valuations is important. If we ever see factors far more "expensive" than any time in the past (not merely in one end of the reasonable historical range on some of the measures) it may indeed be time to move on to greener pastures. However, I do not find that this is the case currently.

ABKW make a rather unique argument using value spreads. They argue that using value spreads, the value factor is currently cheap and that other popular non-value factors are expensive – with the clear implication being that this information should be used to bet more on value now and less on everything else. Note, ABKW don't argue the spreads today are unprecedented, merely on the high side (they focus on book-to-price which I also find gives the most extreme results versus other valuation measures). This is not the unique part of their argument. That part comes when they go on to argue these other non-value factors were <u>never</u> real but the result of these factors getting more expensive over time and researchers mistaking this richening for inherent return through the act of data mining. In forthcoming

[14] In fact this is quite a "rabbit hole" to go down. You can look at still more value measures both to form factors or to judge factors cheap or expensive (e.g., cash-flow-to-price, earnings-to-price). You can then do things without regard to industry as we do here or industry-adjust your factors (Moskowitz and Grinblatt (1999), Asness, Porter, and Stevens (2000), Asness, Ilmanen, Israel, and Moskowitz (2015)). Going further, an alternative formulation to the "value spread" in ratios is to take the difference in valuation ratios. Instead of looking at the B/P of the cheap divided by the B/P of the expensive, look at the B/P of the cheap <u>minus</u> the B/P of the expensive. This is interesting and will be the subject of future work. In multiple regressions I find it adds some predictive power. It is better interpreted as a measure of a strategy's "carry" than its "value." Finally, unlike ratios it has a strong natural relation to the market's overall valuation (unlike the ratio where the market's valuation cancels when the market is very expensive the difference is lower and when the market is cheap the difference is higher). Yet more design choices might affect the experiment. Here I discuss just two of the most basic valuation metrics (book-to-price and sales-to-price) as both factors and for measuring value spreads. Future work should venture further down the rabbit hole on all these points!

work I intend to cover this in more depth, but here I'll just say I find this convoluted argument to have little merit. They greatly overstate the impact of factor richening over the very long-term (mostly ignoring the "wedges" found by Ilmanen, Nielsen, and Chandra (2015) or burying them in their own "regression" results which they do not emphasize enough)[15], overstate the impact of richening and cheapening over the shorter term (say the last 10 years) again by emphasizing full value spread changes wrongly assuming these map 1:1 to factor returns[16], use long horizon regressions for portfolios with way too high a turnover for this technique to be useful, and make histrionic comparisons to the tech bubble of 1999-2000 and the "quant crisis" of August 2007. Also, of course, one should ask how and why too much money flowing to "smart beta" has arbitraged away all other factors except the value factor, particularly when value is likely the most popular and well-known factor.

Raising the specter of data mining is, of course, fair. Obviously, any factor is susceptible to this potential critique. The cure, imperfect as it is, for this worry is out-of-sample tests (through time, geography, and asset class) and moderation and diversification in any risk we take. We may indeed one day decide some of the factors the literature has uncovered are spurious. But, if so it will be because of plain old data mining not a coincidental 10-50 year richening of the factors that ABKW way over credit.[17]

Is a Crash Coming?

Importantly, both Asness (2015) and ABKW discuss the possibility of crashes in these factors, such as another "August 2007" occurring as crowded factors dramatically sell off (August 2007 saw such a crash with extreme negative results for many stock selection factors for a week or so followed by most of it recovering in the next week or so). Both Asness (2015) and ABKW predict this will likely occur again – Asness rather matter-of-factly, ABKW more as an ominous warning to get out now (of non-value factors only!).[18]

Why is Asness (2015) matter-of-fact about it? It's certainly not because I minimize the potential pain of such an event. Rather, it's an unavoidable consequence of not being the only one to know about a factor. Strategies of all kinds, including traditional long-only markets, are empirically subject to crashes. Crashes are essentially runs on the bank and a strategy being broadly known seems to be a necessary condition to have a run, and perhaps (conjecture here) a sufficient one for it happening at some

[15] For instance, factor richening is not close to being able to explain the long-term (from the 1920s or even 1950s to the present) returns of the momentum or low beta factors.

[16] Again, they do present regression results that should adjust for the "wedges" that make this a far from 1:1 relationship and these regression results are indeed weaker but get far less attention than the top line obviously overstated results.

[17] Even as believers in these factors we've long advised assuming the future will not be as good as the past (advising that backtests should be "discounted"). The fact that data mining sneaks into all results, even largely valid ones, is indeed perhaps the most important reason for such a discount.

[18] ABKW say "we foresee the reasonable probability of a smart beta crash as a consequence of the soaring popularity of factor-tilt strategies." Again, I agree that's possible, and made more possible by these strategies being known and popular (it's very hard to have a run-on-the-bank type crash in the unknown and unpopular). But, what's bizarre in ABKW is that they seem to exempt the value factor from this crash warning even though it's likely the most popular factor powering existing "smart beta" products.

eventuality. The question is not whether known strategies will have such runs/crashes. The relevant questions are whether you can predict these inevitable crashes, whether you can survive them, as the long-term isn't so relevant if you can't, and what your long-term cumulative return is assuming these crashes occur and you do survive them.

My answer to "can you predict them?" is so far no (it's not something to give up on though!). More accurately, I find valuation as studied in Asness (2015) and ABKW is a poor predictor. For instance, examining the main factors (I look at value, momentum, profitability, and low beta for selecting stocks in the USA over 1968-present) you'd be hard pressed to say that these factors looked exceptionally expensive in July of 2007. Yet August of 2007 happened nonetheless. So, valuation didn't help at all in predicting the "quant crisis" and that event reversed itself rather quickly. Unfortunately, I do see such events becoming more common as these factors become yet more well-known. That is, again, a consequence of being "known" more than being expensive (the alternative, possessing unknown strategies widely verified in- and out-of-sample by a great body of research would indeed be better but don't hold your breath!). But, unless we one day see these strategies also richen to unprecedented unsupportable levels, not something we see today, I see this as an unfortunate pain we must bear in exchange for the long-term positives of good factors. Again I make the analogy to knowing that the stock market will one day suffer a short painful "crash" does not mean one doesn't invest in stocks for the long run.

These factors, like the stock market itself, are now well-known and will indeed possibly "crash" at some point in the future. I think that comes with the territory when investing in the known (and investing in the known can still be a wonderful long-term decision). Invest in these factors if you believe in them for the long-term and be prepared to survive, not miraculously time, turning points. Stick with your long term plan. Frankly, I think ominous sounding references to the August 2007 event when valuations didn't predict that event and are not at bubble extremes today (in either direction) amount to scare tactics essentially shouting fire in a surprisingly uncrowded factor theater.[19,20]

Summary

Everyone seems to want to time factors. Often the first question after an initial discussion of factors is "ok, what's the current outlook?" And the common answer, "the same as usual," is often unsatisfying. There is powerful incentive to oversell timing ability. Factor investing is often done at fees in between

---

Res[19] I say "surprisingly" as given these strategies have become more well-known and popular frankly I was expecting more dramatically expensive factors! One possibility is while direct investing in these factors is more popular, so is cap-weighted indexing, with assets over the last few years moving in that direction from traditional active management. However, if traditional active management implicitly was making some similar factor bets to our explicit factors, and smart beta inflows reflect, to some extent, an implicit factor bet being replaced by an explicit one, then it's not immediately clear what the net effect would be on factor valuation levels.

[20] While difficult to prove with data, anecdotally more factor exposure is obtained these days through long-only "smart beta" type implementations while pre-August 2007 it seems levered long-short implementations were more popular. Intuitively this makes crashes somewhat more unlikely now (though not impossible!) and paradoxically within-reason leveraged implementations somewhat safer (please note "safer" does not equal "safe").

active management and cap-weighted indexing and these fees have been falling over time. Factor timing has the potential for reintroducing a type of skill-based "active management" (as timing is generally thought of this way) into the equation. I think that siren song should be resisted, even if that verdict is disappointing to some. At least when using the simple "value" of the factors themselves, I find such timing strategies to be very weak historically, and some tests of their long-term power to be exaggerated and/or inapplicable.

In sum, here's what I would suggest. Focus most on what factors you believe in over the very long haul based on both evidence (particularly out-of-sample evidence including that in other asset classes) and economic theory.[21] Diversify across these factors and harvest/access them cost-effectively. Realize that these factors, like the stock market itself, are now well-known and will likely "crash" at some point again. So, invest in them if you believe in them for the long-term and be prepared to survive, not miraculously time, these events sticking with your long term plan. If you time the factors, and I don't rule it out completely, make sure you only "sin a little." Continue to monitor such things as the value spreads for signs these strategies have been arbitraged away – like value spreads across a diversified set of value measures being much less attractive and outside the historical reasonable range – signs that, as of now, really don't exist.

[21] For instance, ABKW raise a real issue with the profitability factor noting the absence of a good economic story why you should get paid for owning higher quality assets. That's a fair question, and I'd add that we don't have a great behavioral story either other than "people underestimate how slowly profitability regresses to the mean."

References

Arnott, R., Beck, N., Kalesnik, V., and West, J. “How Can “Smart Beta” Go Horribly Wrong?” Fundamentals, Research Affiliates, 2016.

Asness, C. “How Can a Strategy Still Work If Everyone Knows About It?” AQR Capital Management, 2015.

Asness, C., Frazzini, A. Israel, R., and Moskowitz, T. “Fact, Fiction and Value Investing,” AQR Capital Management, 2015.

Asness C., Frazzini, A., and Pedersen, L. “Quality Minus Junk,” Working Paper, AQR Capital Management, 2013.

Asness, C., Friedman, J., Krail, R., and Liew, J. “Style Timing: Value versus Growth,” The Journal of Portfolio Management, Vol. 26, No. 3, (2000), pp. 50-60.

Asness, C., Ilmanen, A., Israel, R., and Moskowitz, T. “Investing With Style,” Journal of Investment Management, Vol. 13, No.1, (2015), pp. 27-63.

Asness, C., Ilmanen, A., and Maloney, T. “Market Timing Is Back in the Hunt for Investors,” Institutional Investor, November 11, 2015.

Asness, C., Moskowitz, T., and Pedersen, L. “Value and Momentum Everywhere,” Journal of Finance, Vol. 68, No. 3, (2013), pp. 929-985.

Asness, C., Porter, R., and Stevens, R. “Predicting Stock Returns Using Industry-Relative Firm Characteristics,” 2000.

Cohen, R., Polk, C., and Vuolteenaho, T. “The Value Spread,” The Journal of Finance, Vol. 58, No. 2, (2003), pp. 609-642.

Ilmanen, A., Nielsen, L., and Chandra, S. “Are Defensive Stocks Expensive? A Closer Look at Value Spreads,” 2015.

Moskowitz, T., and Grinblatt, M. “Do Industries Explain Momentum?” The Journal of Finance, Vol. 54, No. 4, (1999), pp. 1249-1290.