



# Cliff's Perspective

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## It's Time for a Venial Value-Timing Sin

November 7, 2019

Early in the classic movie *Young Frankenstein*, when the villagers' suspicions have begun to rise, inspector Kemp [cautions them against](#) going too far and starting a dangerous riot. Later in the movie he relents and announces that this is now, indeed, [the time to riot](#). I will not be recommending any pitch forks and torches. However, like the good inspector, but in a different arena, we have generally cautioned against too much "factor timing" (and market timing too). In fact, we have taken the other side of the debate versus those saying, starting at least [three years ago](#), that it was time to seriously up the bet on the value factor. We disagreed for two reasons. One, we simply think factor timing, while not impossible, is a [lot harder](#) than some others believe.<sup>1</sup> Second, and much more mundane but kind of important, we simply didn't think the value factor looked particularly cheap back then.<sup>2</sup> Well, things have changed. There's an old economics joke that goes something like, "a recession is when your neighbor loses his job, a depression is when you lose yours." To us a recession is when the value factor performs poorly but we still do well, a depression is when we suffer with it. For most of the last 10 years of the value factor's drawdown it's been a recession. For the last almost two years it's been a depression.

We've also taken the circumspect side of the value-based market timing debate.<sup>3,4</sup> Our circumspection comes from the fact that market timing is a really hard way to make a living. So hard that we've called market timing an investing "sin." However, our recommendation has always been not to live sin free, but instead to [sin a little](#).<sup>5</sup> Essentially that means relatively modest timing and only at reasonably extreme events. We've written many similar things about factor (or style) timing often making a direct analogy between the two.<sup>6</sup>

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<sup>1</sup> This includes the fact that timing a factor with its valuation (more on how to do that later) may partially up the passive weight on value, and since [value on average has delivered positive returns](#) this somewhat inflates the apparent performance of the timing. Past performance is not a guarantee of future performance.

<sup>2</sup> Some did show statistics, based on techniques we developed in 1999, showing the value factor looked quite cheap – but we generally thought they were cherry-picked or poorly constructed (or such opinions were based only on poor prior factor performance, not current valuations, when such poor performance doesn't always lead to a cheaper factor – more on this later).

<sup>3</sup> "Value-based" means simply owning more of the market when it looks cheaper and less when it looks more expensive. What measures to use and how to size that bet, of course, varies.

<sup>4</sup> Of course, the market is itself a factor (vs. cash). We're distinguishing it here from the other factors we discuss that are all long and short baskets of stocks reflecting relative, not vs. cash, return.

<sup>5</sup> Obviously, we don't really think it's a sin!

<sup>6</sup> One way I like to think about it, for factor or style timing of long-short factors, is, of course, nobody knows the precise right ex ante weights to put on different factors. If one just historically optimizes (not our recommendation but the example works) you find it's generally a "flat surface," a term for an optimization where the optimal solution is not much more optimal than other relatively nearby solutions (i.e., factor weights that differ only modestly from the optimizer's recommendation). As an example, imagine everyone agreed that the value and momentum factors were great. It's still very hard to tell if 60/40 value/momentum or 40/60 value/momentum is the right long-term allocation. That's not a bad thing, both are pretty good and if that's true both allocations should produce long-term returns. But, it does mean we don't get a lot of guidance once we get in the vicinity of optimal. I think of "sinning a little" as making sure any tactical changes to weights are on this "flat surface." That is, a timing position attempting to get the short term right, but that seriously hurts the forecast long-term results if held long term, would be a cardinal not a venial sin.

Below we discuss how to measure whether a factor, in this case the value factor, is itself rich or cheap versus history. The answer, regardless of the approach taken in measuring cheapness, is that value is currently quite cheap compared to history. In other words, value does not look like a factor with too many people chasing it today (as we are often asked), rather it looks like a shunned out-of-favor factor. At best that creates opportunity, at worst it means the last almost two, or even the last 10, years don't show a permanently broken value factor. Instead they show one that hasn't worked for the same reason it has had tough times in the past: it's on the outs with investors.<sup>7</sup> We end up with a view that, unlike a few years ago, it is indeed time to "sin a little" (up the value weight somewhat). Factor timing is an ugly thing... But I think it is about time we did some!<sup>8,9,10</sup>

## Measuring the "Value of Value"

In late 1999 we were in the midst of a different famously bad period for value strategies. I don't want to over stress the analogy to today. That period rhymes with the last few years but they aren't the same. Still there are few truer aphorisms, certainly in investing, than *plus ça change*. That event, starting virtually to the day we made our first investment, led us to our first ever round of "this is a really tough period, let's check everything" (we're up to round three now – not so bad over 25 years from Goldman Sachs to AQR, but still the opposite of fun when it happens...). One of the tools we created then is something that has become known as the "value spread" and is in widespread use today at AQR and many other shops (and in [academia](#)). It is indeed a measure of the "value of value," at least as compared to history.

The academic work until that point had largely, perhaps even exclusively as I can't recall an exception, focused on sorting stocks on value characteristics, and then studying the return difference between the "cheap" and "expensive" stocks. There was little to no discussion of magnitude. That is, what if sometimes the expensive stocks were way more expensive than the cheap stocks, and what if sometimes they were only narrowly so? To answer this question [we built](#) a measure of this gap. It's really simple (though as we'll soon see it can be done in many different ways). Let's suppose you have a way to form a systematic value long-short portfolio, Fama and French's [HML](#) being the most famous example where they form a long and short portfolio on just price-to-book (P/B). Instead of just focusing on the return spread between the cheap and expensive sides, as was the norm back then, we looked at the ex ante magnitude of the valuation differences. For example, if the expensive portfolio is trading at a P/B of 10, and the cheap portfolio at a P/B of 2, the ratio is 5. For that particular portfolio, as can be seen in the first figure below, this ratio is almost always between about 4 and 10. But, during the tech bubble it hit 16, more than double the average, and more than 50% larger than the prior or subsequent maximum. Along with

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<sup>7</sup> Later we'll dissect the story a bit more for the last almost two years vs. the prior eight years of the value drawdown.

<sup>8</sup> Note, there are potentially other ways to time factors, of course, beyond looking at whether a factor is cheap or expensive. In various studies, [we](#), and [others](#), have studied factor timing using the past performance of a factor and found, similarly, that there is some mild evidence to allow for some small amount of "sinning" at times based on factor trend. Further, using macro-economic conditions, such as business cycles or level of interest rates, to time factors is often attempted as well, but our studies have shown there is very little support for these forms of factor timing (we think they are generally exercises in data mining driven story telling – though we remain wide open to someone, us or others, discovering a macro-based forecasting model for factors we could believe in).

<sup>9</sup> The evidence on pure value timing of value is only so-so. We've written some of those papers (largely in response to others who think it's a slam dunk). Generally, the empirical predictability has the right sign, but many studies ignore transaction costs. Also, these empirical studies of timing strategies don't have many data points, certainly not many of the relevant extreme ones. When data is weaker, such that you can't definitively rule something in or out based on the data, one's priors must matter more. If value works for behavioral finance reasons, i.e., people make errors, it's hard for me to imagine people are perfectly rational about the size of those errors. For example, if the difference in P/E between stocks cross-sectionally predicts returns because people overdo it, that is the high P/E stocks deserve to be high but not as high as they're priced and vice versa, it's hard for me to imagine when that difference across stocks is larger the "larger" part is suddenly perfectly rational (the same argument applies for risk-based explanations). Also, it's important to think of value timing in the context of the "flat surface" of optimal portfolio construction. If we had a strong view of the ex ante "right" long-term weights, then deviations from that would have to have much stronger priors and evidence. But nobody has any idea within 5% (probably even much wider) of what the long-term optimal is. I think that gives us more honest latitude (in a Hippocratic "do no harm" concept we've employed since our group formed at Goldman Sachs) for small tilts and thus more room to indulge our strong prior belief about the strength of value-based timing decisions. Again, all for small moves. Sin a little!

<sup>10</sup> When trading many stocks in cross-sectional stock strategies, you just go with the best Sharpe ratio combo of value and momentum or more accurately you use your best guess of the Sharpe ratios based on the empirical support and your prior beliefs. In a timing model you do the same thing, find your best guess of the Sharpe ratio (yes Sharpe ratio is not the right measure for a timing model with varying vol, I'm using the term generally) of a value-based timing model and a momentum-based timing model and based on those create the optimal combination. However, in timing models you have far fewer data points to help you estimate the true Sharpe ratios. So, again, things like prior beliefs have to matter more. Another thing to consider is that in a cross-sectional stock strategy the strategy isn't too sensitive to what happens to any one individual stock. For instance, if a stock is cheap but has poor momentum and as a result of that you don't have much of a position in that stock and it "jumps" back to fair value before it got momentum behind it, then it still doesn't matter much to the strategy overall. However, in a timing strategy, if you get a huge quick move you can miss the once a decade opportunity, making adding trend to value-based factor timing at least a little riskier in this sense. Still, while this initial small move towards value is based on just valuation, further moves (and we have room for more) would come from a combination of the value and trend of the value factor and a more formal model.

evidence that this measure had some efficacy for forecasting future value returns<sup>11</sup> this gave us a lot of support in sticking with value through its turnaround in early 2000 and beyond. We still, in many forms (next section) monitor and use this measure today. We use it for assessing how cheap value looks after it's been beaten up. And, we use it because the value opportunity that has been with us for a century might one day be arbitrated away (in that case we'd expect to see this ratio smash down to levels not seen before and stay very low – quick preview, this ain't what we're seeing today, quite the opposite).

### **There Are Lots of Ways to Measure the “Value of Value”**

Value is not just price-to-book, though following the academic literature we're going to start there. Some combination of the incredible success of Fama and French's (who use P/B<sup>12</sup>) work in academia and the early adoption of P/B among some value index providers (likely not unrelated events) has led to P/B sometimes being thought of as synonymous with systematic value investing. It's not. There are lots of ways to measure value, some we actually think are better than P/B (though P/B does remain in our composites<sup>13</sup>). [Here](#), among a bunch of other things, we discuss our belief, and the evidence for, using a composite of reasonable value measures not just one.<sup>14,15</sup>

Furthermore, value is not just an industry bet. Consider most financial media discussion and [industry](#) analysis of value investing. It's often dominated by something like “tech vs. textiles and banking.” But, in fact, back in the 90's [we showed](#) that value actually works better if you don't (or try not to – you can never do this perfectly) take an industry bet. We call value strategies that don't take an industry bet (going long and short within each industry) “intra-industry value” and value strategies that only take an industry bet (going long and short across industries by trading industry baskets) “inter-industry value.” A third type of strategy simply sorts among stocks in a way that's agnostic to the existence of industries, resulting in a mix of intra-industry and inter-industry value. We call these strategies “raw value” and they are the norm, at least in popular perception (we think many other quants also remove some or all of the industry bet). In general, we are big fans of intra-industry value, not so much of inter-industry value, with raw value not surprisingly in the middle. It turns out that the returns to systematic value are correlated across these ideas. That is, when value performance is strong intra-industry, all-else-equal you'd expect it to be strong inter-industry and vice versa.<sup>16</sup> But this correlation is far from perfect, and we believe the long-run risk-adjusted returns of intra-industry value are considerably better than those of inter-industry value (it's not clear that the latter are even positive). This can occasionally lead AQR's discussion of the characteristics of the value factor to differ from those of one factor value (like P/B) that's allowed to take big industry bets as commonly applied in popular factor indices (e.g., Russell).

So, different systematic value strategies can differ on how they measure value and whether they try to remove the industry bet. But that's not all. Value strategies can also differ by asset universe. Are you just looking at the U.S.A., or going international to cover other developed, or even emerging, markets? All stocks, just small stocks, just large stocks, or some combination? Value strategies can differ by weighting scheme. Is the long and short portfolio cap-weighted, equally weighted, weighted by the value signal, or something else? The strategy can be run at a constant ex ante volatility (using some risk model) or just be long and short \$1 all the time. The strategy can be hedged for its current estimated market beta (trying to maintain beta neutrality all the time, not just over the long term and again using some risk model) or not.

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<sup>11</sup> Even though this note is about a recommended venial sin, we've probably gotten a little less excited about value-based factor timing than when we wrote this paper back in 1999 (back then we might've said “sin a bit more than a little”) even though the prediction in the paper bore true with value strategies raging back starting in early 2000. I believe this is the rare case of something working in real life and a modeler still deciding the evidence isn't quite as good as they first thought. Usually the bias runs the other way. So we have that going for us, [which is nice](#).

<sup>12</sup> I think they would agree other measures would serve well too (they've written about this). Also, famously, they actually use B/P not P/B just to keep everyone on their toes!

<sup>13</sup> By “composite” I mean the composite of different measures of value that we utilize in our models (more on this below).

<sup>14</sup> E.g., not just P/B but price-to-sales, price-to-cash flow, price-to-earnings both trailing and forecast, the list can get quite long.

<sup>15</sup> Recently, some have specifically argued that using book value is an outdated measure given the growth of intangible assets among companies today that are not reflected in book values. While there is more to this debate which is beyond the scope of this piece, the use of multiple measures as we would advocate mitigates the fact that there is no perfect measure of value given, among other things, the noisiness of accounting measures and the differences among companies and industries. Ironically, while this argument has been made to explain why value no longer works particularly in response to the recent drawdown, P/B measures have been some of the relatively better performing (i.e., have underperformed less) value metrics compared to other measures (e.g., based on sales or earnings) that are immune to these arguments. Go figure.

<sup>16</sup> Perhaps this is because there's just a natural thematic event or correlated sentiment among investors going on where when cheap industries outperform so do cheap companies within industries – but also perhaps because our industry classifications can never be perfect so the bets can't be absolutely perfectly separated.

It's total chaos! OK, not really. Value is a concept. If it works over the long-term, and we believe it does, it's either because a portfolio of value stocks is riskier than a portfolio of growth stocks and thus rationally rewarded with more expected return, or because the market isn't perfect and behavioral finance kicks in, making the cheap stocks somewhat too cheap and vice versa.<sup>17</sup> Note, neither of these explanations comes with a road map of how to measure value, how to weight portfolios, what universe to test or trade over, etc. It shouldn't upset us that there are lots of ways to measure and implement systematic value investing. Most reasonable ones are quite correlated to each other over the long-run but can also differ substantially over short periods. So, while not a problem, it does mean we need to be very clear what we're doing, and in the best case examine multiple methodologies across these different dimensions in trying to prove or disprove anything about value investing.<sup>18</sup>

OK, but we're not done yet. Once you've set on a systematic value strategy there are yet more choices in how to measure the value spread (the "value of value"). In general, you're going to be comparing the valuation ratio (or ratios) of an expensive portfolio to that of a cheap portfolio. But, do you use the median value in each portfolio, the cap-weight average, the sum of the earnings or book or sales of the portfolio divided by the market cap of the portfolio (which is not quite the same thing as the cap-weight average)? Do you compare them using a ratio,<sup>19</sup> a difference, or something else? The list goes on.

Obviously, I can't do every combination of design choice (not even every reasonable combination). The dimensionality is just too large and too continuous. I will focus on three designs that start out as very generic and then get more "AQR specific." We have, of course, looked at many other permutations and found similar results to what I show here. I will leave a more specific description of each methodology to the appendix, but in broad terms the three can be described as follows:

1. An "academic" approach corresponding reasonably closely to what Fama and French do in their famous HML construct built over a very large universe of U.S. stocks including small cap stocks. This approach uses only P/B to sort stocks and only P/B to measure the time-varying value of value.<sup>20</sup> I focus here on the "raw" (no industry adjustment) results that are most common in academia but also demonstrate how intra- and inter- sometimes really look different.<sup>21</sup>
2. Here we use a mix of four widely-known valuation factors (P/B, P/E where earnings are trailing, P/E based on forecasted future earnings, and price-to-sales (P/S)<sup>22</sup>) and attempt to completely remove the industry bet, as well as we can, built over a universe of large to mid-cap stocks so we can more realistically use a weighting scheme that isn't cap weighted and is closer to what we think many real world quants use. We also use a combination of all four measures to calculate the value of value historically (remember, what you use to form a portfolio, and how you measure the value of value, don't have to be the same – in fact they are not the same in (3) below).
3. Now we use AQR's full model for value-based stock selection (approximately 25 value factors) that contain both value measures we consider "styles" and also some we think are closer to "alpha." This is all done while attempting to remove the industry bet built over a universe of large to mid-cap stocks. Even though we use 25 value factors to form the long and short (cheap and expensive) portfolios we will

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<sup>17</sup> I have never been a pure efficient marketer (in reality nobody is – Gene Fama shocked our class in 1988 by saying early on "markets are almost surely not perfectly efficient" – this is what passes for breathtaking at the University of Chicago!). I wrote my dissertation (data ending 1990!) on the success of the price momentum strategy, one very difficult to reconcile with risk-based stories (though some still try). Over my career, and perhaps time away from University, I admit to drifting more towards behavioral rather than risk-based stories, though not entirely.

<sup>18</sup> In fact, one would argue this is a virtue. The fact that multiple measures are correlated and yield similarly good long-term performance gives us more confidence that this is a robust and persistent source of return. We should be more concerned if it only worked one specific way.

<sup>19</sup> We think of the ratio, say the P/B of the expensive divided by the P/B of the cheap portfolio, as the measure of the "value of value" we want to study, and the difference as more akin to a "carry" strategy, itself interesting and perhaps worth a blog unto itself, but not what we'll study here. One property we've always liked about the ratio is that it's unaffected by the valuation level of the overall market – only the relative valuations of expensive and cheap stocks. In other words, if all stocks rally or fall by the same amount the ratio is unchanged (holding fundamentals constant). Carry, on the other hand, is different when the market itself is more expensive or cheap.

<sup>20</sup> The valuation ratio you use to construct a systematic value portfolio does not have to be the same one you use to measure its current cheapness, though we find this usually the intuitive choice (except in our third design choice as I'll explain).

<sup>21</sup> As you'll see below, I also show historical value of value for the same methodology but where industry bets are not allowed (you have to be balanced long and short in each industry) and also, really for curiosity as we don't trade this, the value of value to a strategy that only takes industry bets.

<sup>22</sup> As the appendix discusses, we give somewhat less weight to the two earnings measures than to P/B or P/S as they are the most highly correlated measures (and thus P/E is getting more effective weight than it looks like). Also, as the appendix discusses, our fundamental anchor for sales is actually enterprise value, a modified form of "price."

measure their value spread using the same four measures as in (2) above as some of the more proprietary factors don't lend themselves to ratios (zeros and sign flips can mess up ratios).

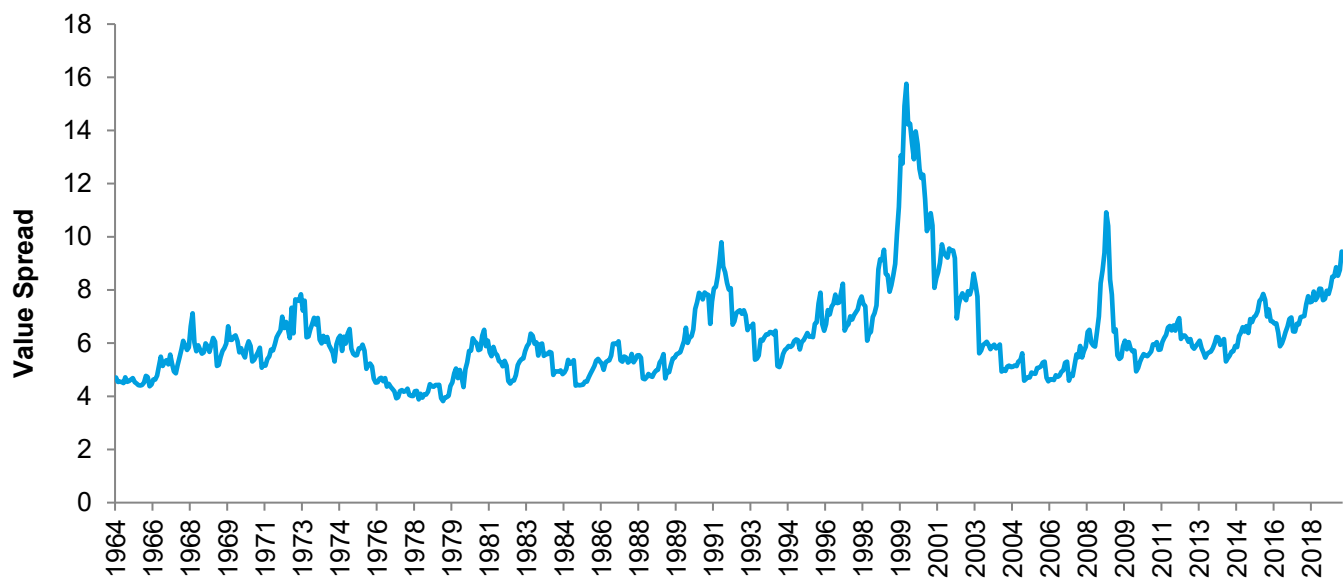
Another difference between the three approaches, briefly mentioned above, is that in (1) we use cap-weights to create the long-short portfolios,<sup>23</sup> but in (2) and (3) we use signal-weights (the cheaper you are the bigger the weight). This mix and methodology is meant to come closer to what many applied quants actually implement. Again, more details are in the appendix.

OK, let's check out the historical and current value spreads (the "value of value").

### Historical and Current Spreads

Method (1), the "academic" method, just sorts stocks on P/B (B/P if you're Fama-French fans!).<sup>24</sup> The graph below is the P/B of the expensive 30% of stocks divided by the P/B of the cheap 30% of stocks.

#### Academic HML (B/P) Spread December 1964 – August 2019



Source: AQR, CRSP, XPressFeed. Please see Appendix for more detail on data and assumptions. For illustrative purposes only and not representative of any portfolio that AQR currently manages. Hypothetical data has inherent limitations, some of which are disclosed in the Appendix.

The value of value for this methodology has been higher than today three times in the past: the early 1990s, the tech bubble (way higher), and very briefly during the global financial crisis (GFC). Since the tech bubble was so extreme, I think it's helpful to look at statistics that both include and exclude that period. You can't exclude it from your thinking as it happened, and thus, even if very low probability, you have to be able to survive it happening again. However, you can look at statistics both including it and separately over what you think are only normal and "normally abnormal" times, as both can provide perspective.

|                                  | Full Period      | Ex-Tech Bubble   |
|----------------------------------|------------------|------------------|
| <b>Percentile</b>                | 96 <sup>th</sup> | 99 <sup>th</sup> |
| <b>Z-Score</b>                   | +1.92            | +3.01            |
| <b>Percent of Peak Deviation</b> | 36%              | 71%              |

Source: AQR, CRSP, XPressFeed. Please see Appendix for more detail on data and assumptions. For illustrative purposes only and not representative of any portfolio that AQR currently manages. Hypothetical data has inherent limitations, some of which are disclosed in the Appendix.

<sup>23</sup> Though we don't follow Fama and French who give equal weight to the differences among small stocks as to those among large stocks. Rather we include a broad universe and cap weight giving small stocks some impact, but far less than in the Fama-French construct.

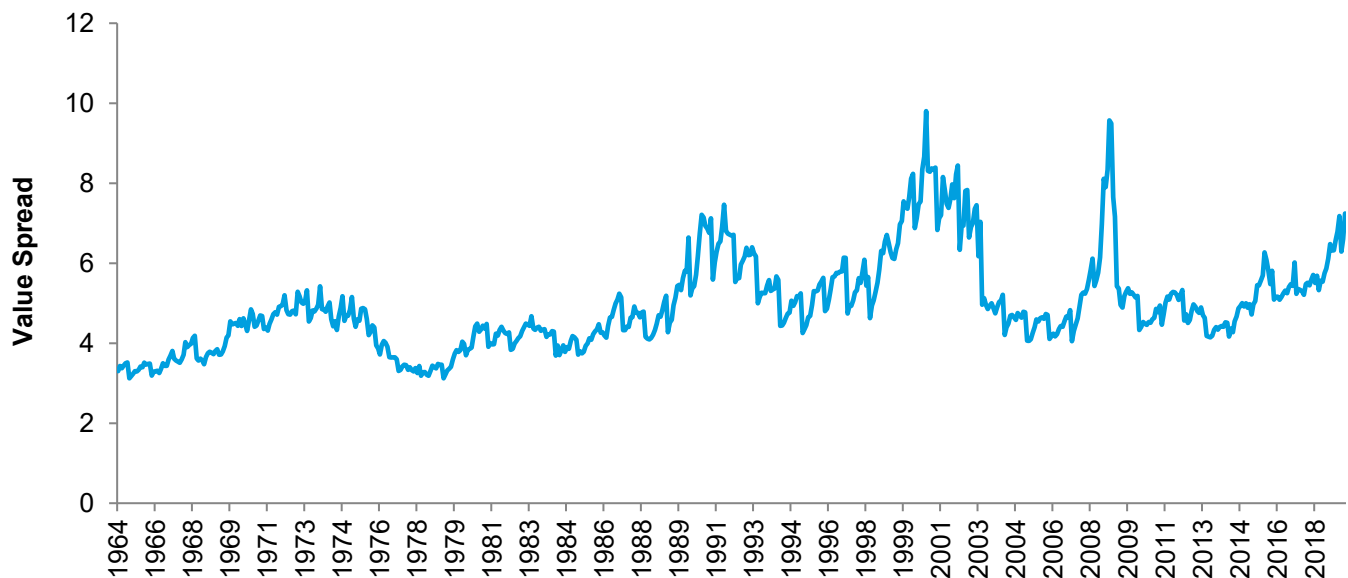
<sup>24</sup> While this method (1) is very motivated by Fama-French it still uses our monthly "devil" version of B/P where we use the latest market price together with a lagged book price.

The stats above show that today (8/31/19<sup>25</sup>) we're at the 96th percentile versus history (99th if you exclude the tech bubble period<sup>26</sup>). That's a +1.92 standard deviation event (+3.01 if you exclude the tech bubble which, as the bubble makes the series very right-skewed also makes standard deviation the wrong measure for the full period but more reasonable ex-tech bubble). It's worth noting that 96th or 99th percentile is not to be confused with "96% or 99% of the maximum we might see." To get a sense of this, I also include "percent of peak deviation" in the last line of the table. This measure takes the final value (8/31/19) and subtracts the series median from it, and divides this by the tech bubble peak minus the same median. This measures the magnitude the current value of value is above the median as a fraction of how much above the median it was at the peak of the tech bubble (or the maximum value excluding the tech bubble in the right column). While currently quite cheap versus most of history, again 96th percentile, this measure is still only 36% of the way to how far the tech bubble got over median (71% of the maximum excluding the tech bubble). That is, it looks quite cheap, and cheaper than almost all other months in history, but occasionally has been far cheaper.

Now let's dig a bit deeper into this "academic" version of the value of value. To start, we'll do the same thing as above but without, to the best of our ability, allowing an industry bet (so you use P/B to go long and short cheap vs. expensive in a balanced way within each industry with the long portfolio being the 30% of stocks cheapest as compared to their industry and vice versa). Here is that historical spread:

### Academic Intra-Industry HML (B/P) Spread

December 1964 – August 2019



|                                  | Full Period      | Ex-Tech Bubble   |
|----------------------------------|------------------|------------------|
| <b>Percentile</b>                | 95 <sup>th</sup> | 97 <sup>th</sup> |
| <b>Z-Score</b>                   | +1.96            | +2.40            |
| <b>Percent of Peak Deviation</b> | 50%              | 53%              |

Source: AQR, CRSP, XPressFeed. Please see Appendix for more detail on data and assumptions. For illustrative purposes only and not representative of any portfolio that AQR currently manages. Hypothetical data has inherent limitations, some of which are disclosed in the Appendix.

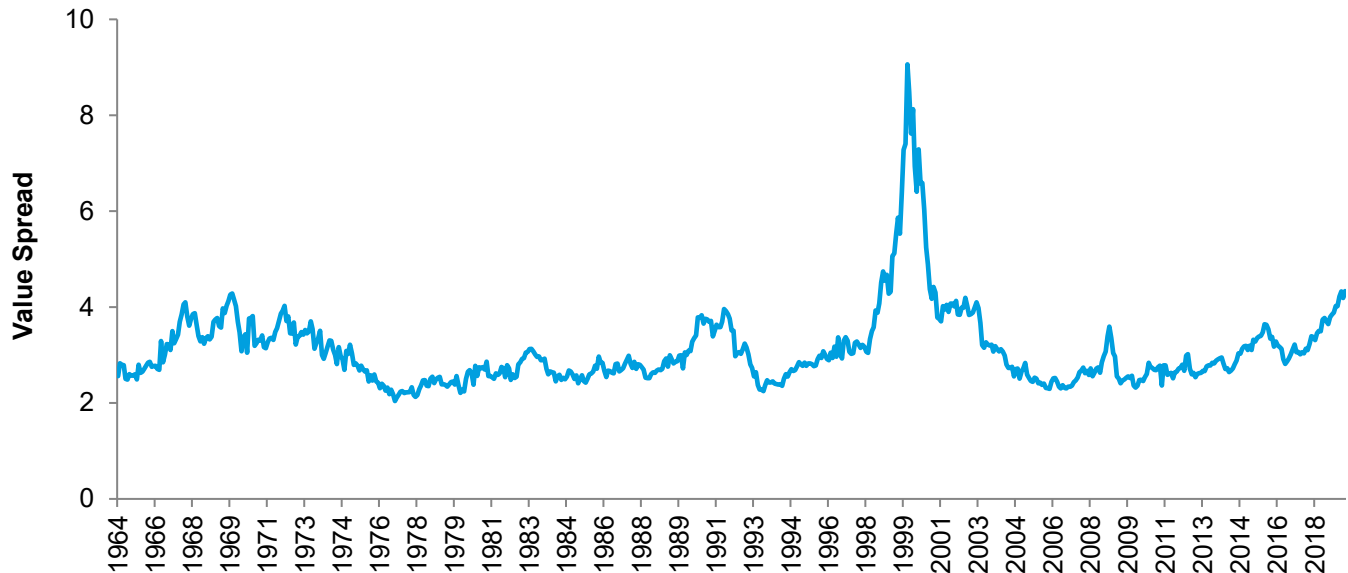
Fairly similar final results, but a somewhat different time pattern. If you don't allow an industry bet, then the peak "value of value" in the GFC was actually about the same as the peak of the tech bubble. This makes sense (at least directionally). Although, as the above graph shows, value got very cheap even within industries, there's a reason we call the tech bubble the tech bubble not the "value within each industry" bubble. When you take out the "tech versus everything else" bet, as intra-industry does, the tech bubble looks extreme, but less so.

<sup>25</sup> Value has had some wild days in the couple of months since this ending – but not nearly enough to materially change this write-up.

<sup>26</sup> Defined here as the period from September 1998 to December 2001, where, as shown in the exhibits, value spreads were extremely wide (rising for the first part of that period and falling for the latter part).

The next graph looks at forming a value portfolio that only trades industries, going long the 30% cheapest industries (industry baskets) on P/B, and short the 30% most expensive, and as usual reports the ratio of the P/B of the expensive industries divided by the P/B of the cheap.<sup>27</sup> Anyway, this is the value of value for industry selection (it's high when the expensive industries are much more expensive than the cheap ones, and vice versa):

**Academic Inter-Industry HML (B/P) Spread**  
December 1964 – August 2019



|                                  | Full Period      | Ex-Tech Bubble    |
|----------------------------------|------------------|-------------------|
| <b>Percentile</b>                | 96 <sup>th</sup> | 100 <sup>th</sup> |
| <b>Z-Score</b>                   | +1.49            | +2.83             |
| <b>Percent of Peak Deviation</b> | 24%              | 100%              |

Source: AQR, CRSP, XPressFeed. Please see Appendix for more detail on data and assumptions. For illustrative purposes only and not representative of any portfolio that AQR currently manages. Hypothetical data has inherent limitations, some of which are disclosed in the Appendix.

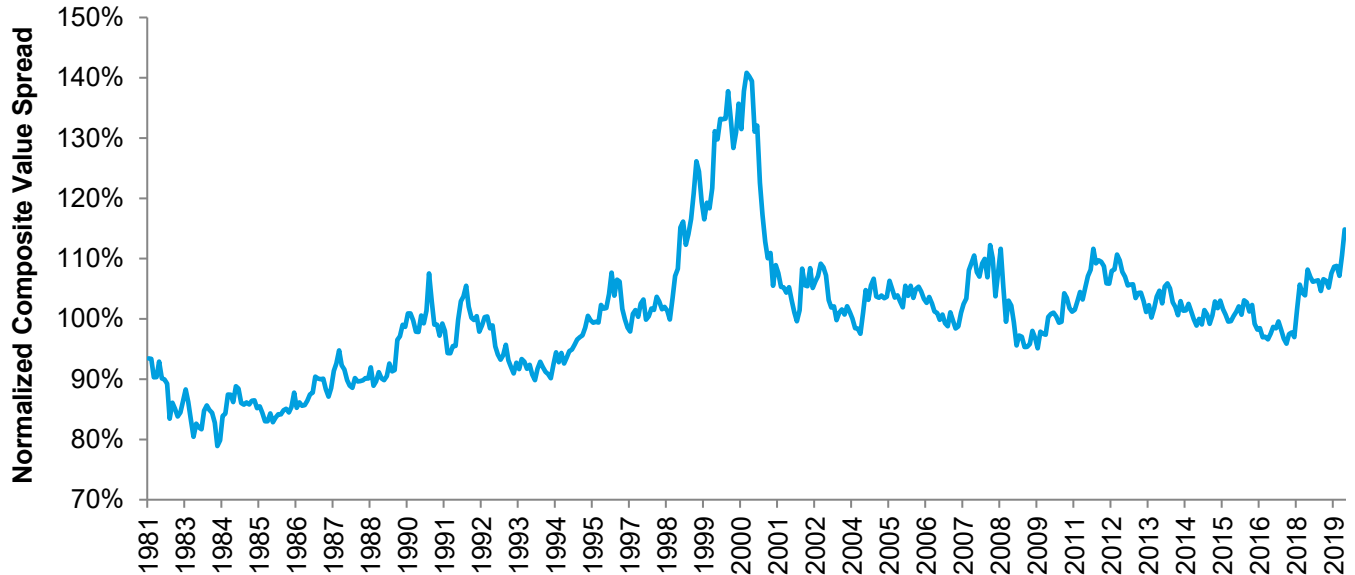
Quite a different picture! It's not a mystery where the tech bubble got its name. You kind of knew this result was coming if you looked at both the raw (no industry adjustment) and intra-industry graphs above together. But it's still kind of wild (at least to me) to see how crazy it got across industries back in 1999-2000. Again, we don't recommend using value or specifically P/B to make industry bets, so this is the value of value for a strategy we don't employ or recommend. Still, as discussed above, industry vs. industry is how much of the world views value investing and, on that measure, value is very cheap today, but not even in the tech bubble ballpark (only 24% of the way!).

<sup>27</sup> Note that we don't recommend trading this portfolio. We believe that value, and maybe specifically P/B as B is perhaps less comparable across industries than some other measures, doesn't do a very good job at choosing industries in the absolute and certainly not compared to how it does choosing stocks within industries.

Now, let's move on to method (2), a considerable step towards how we think most real-world quants implement value but not highly specific to AQR. Recall this method forms cheap and expensive portfolios on four measures of valuation, compares stocks on an intra-industry basis, and measures the value of value using these same four measures.<sup>28</sup>

### Four Measure Construction and Measurement

December 1981 – August 2019



|                                  | Full Period      | Ex-Tech Bubble    |
|----------------------------------|------------------|-------------------|
| <b>Percentile</b>                | 93 <sup>rd</sup> | 100 <sup>th</sup> |
| <b>Z-Score</b>                   | +1.37            | +2.25             |
| <b>Percent of Peak Deviation</b> | 35%              | 100%              |

Source: AQR, XpressFeed, IBES. Please see Appendix for more detail on data and assumptions. For illustrative purposes only and not representative of any portfolio that AQR currently manages. Hypothetical data has inherent limitations, some of which are disclosed in the Appendix.

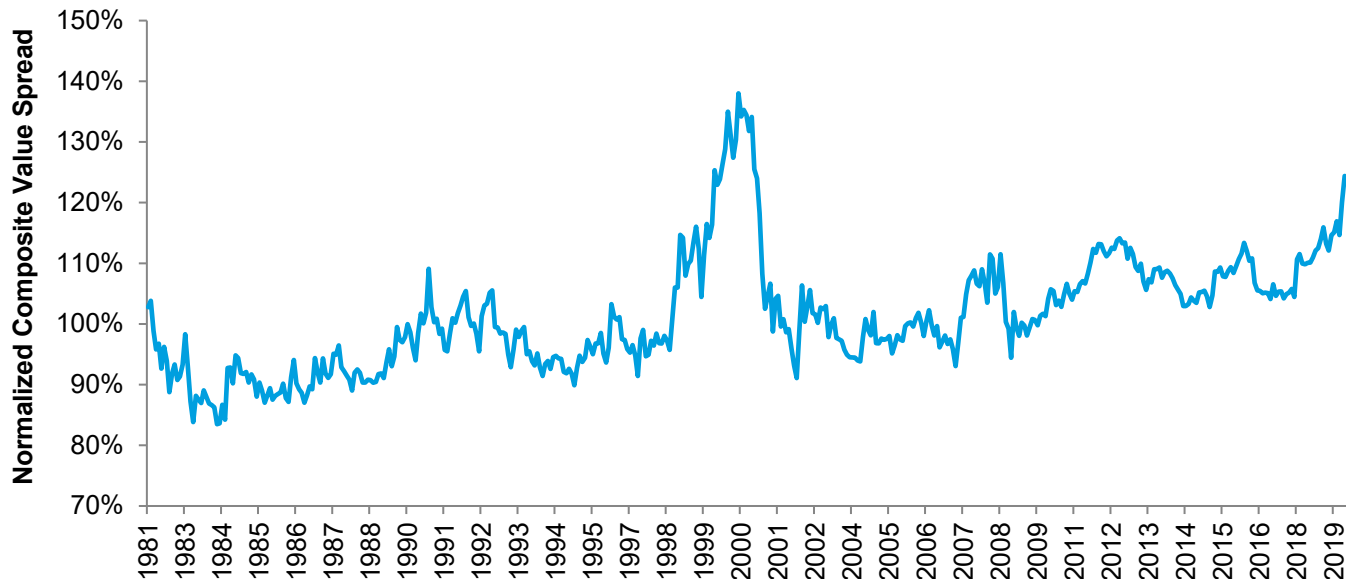
This more realistic methodology (at least, we think it's more realistic for most long-short or long-only active value quants) shows similar results to the simple cap-weighted P/B-only method (method (1) above). Though still only a fraction of the tech bubble, the spread is at the 93rd percentile versus history, and 100th percentile ex the tech bubble. That is, if the tech bubble isn't around the corner again, this is the cheapest it's ever been (obviously an important "if!") and has cheapened a lot in the last year or two.

<sup>28</sup> We get four separate time series of the value of value for the portfolio formed on the four measures – one for each measure. They aren't the same magnitude so we normalize each by dividing by its historical median, then average these four. Thus, the units on the y-axis are in terms of percent. E.g., 110% means, averaged in what we think is a reasonable way, this portfolio looks 110% of the norm in terms of the "value of value."



Finally let's look at method (3). This uses the full AQR value factor which incorporates both ways of measuring value that we consider "styles" and ways to measure value that we believe qualify as "alpha." Again, all done intra-industry and despite using a broader set of value factors to form portfolios, we still measure the value of value using the same four measures as above.

**AQR Value Using Four Measure Measurement**  
December 1981 – August 2019



|                                  | Full Period      | Ex-Tech Bubble    |
|----------------------------------|------------------|-------------------|
| <b>Percentile</b>                | 97 <sup>th</sup> | 100 <sup>th</sup> |
| <b>Z-Score</b>                   | +2.45            | +3.33             |
| <b>Percent of Peak Deviation</b> | 64%              | 100%              |

Source: AQR, XpressFeed, IBES, Holt. Please see Appendix for more detail on data and assumptions. For illustrative purposes only and not representative of any portfolio that AQR currently manages. Hypothetical data has inherent limitations, some of which are disclosed in the Appendix.

Now we're talking (though I shouldn't be gleeful as getting here certainly stunk)! Even including the tech bubble, today looks pretty cheap at the 97th percentile and almost two-thirds of the way to the tech bubble peak. Excluding the tech bubble the value of value is the cheapest it's ever been by a fairly decent margin.

While each graph above shows the value of value to be attractive now, the AQR specific one is the most extreme. What's driving that? Essentially, looking over the last 10 years we compare the more generic ways to measure value versus the AQR specific way. The AQR way did considerably better over the first eight or so years of the general value drawdown, but worse over the recent almost two years. Couple this with the fact that value spreads tend to be less impacted by long and drawn out losses<sup>29</sup> and more impacted by short quicker losses and you get the wider current AQR value spread. In other words, the eight year relatively good period for AQR's value strategy didn't narrow its value spread too much (versus that of other value measures) as it was long and slow, however the recent almost two-year tough period has caused our relative spread to widen.

<sup>29</sup> Long-term losses don't move value spreads much as over these long periods there is a lot of turnover in the holdings and lots of changes in individual company fundamentals. For an extreme example, if a factor lost for 200 years would you say, "man that factor must be cheap now" or "that's a backwards factor"? The holdings would have turned over many times over that period. If those 200 years of losses were all over one year it might be very cheap as the holdings would most likely be pretty much the same over that period and the fundamentals would change way less.

All things considered, it seems to us that now is indeed the time to sin a little!<sup>30</sup> But before we do that, let's dig a little more into how we got here.

## The Last Decade for Systematic<sup>31</sup> Value – A Tale of Two Periods

Just a little more than a year ago I wrote about the pressure, after bad times, to declare your strategy super-cheap both to encourage client retention and even incentivize tactical additions. Specifically, in a [poorly titled](#)<sup>32</sup> opus last year, I wrote (the first sentence refers to the allure of claiming that value looks super cheap):

Everyone wants me to say that it is. It would indeed be comforting to tell people, “You have to stick with this or add more as it's going to rocket upward very soon!” FOMO can be a powerful inducement. But I just can't do it.

In stock selection, value is still not super cheap (i.e., super cheap would be if the cheap stocks were way cheaper versus the expensive ones than normal). It would be fair to wonder why not, especially given the poor long-term value returns. Well, with any strategy, you can lose because either prices or fundamentals move against you. Unfortunately, more of this current drawdown has been about fundamentals.

Things had started to cheapen back then but weren't yet really, really cheap. To understand what's changed since I wrote this, it's helpful to go back to looking at the value factor since the GFC. Basically, for the first, again say eight years since the GFC, most versions of the value factor underperformed<sup>33</sup> largely because the fundamentals (earnings, cash flow, sales, margins) worked against value. Losing for “real” fundamental reasons tends to lead to two things:

1. Other factors, I'll get more specific below, beyond just the value factor, that are designed to look for quality or recent improvement, i.e., the fundamentals, can really help (and really did!).
2. You don't necessarily get a real cheapening of the value factor despite its losses. If you lose on fundamentals you just lost. That happens. It's fair. Sometimes you win on fundamentals and the strategy doesn't get much more expensive for the same reason (this is a much more enjoyable case). But, essentially, if you bought a low P/E stock, and the P and E both fall 50%, you lost a lot, but the stock didn't get a single drop cheaper (on this one narrow measure). That's at least within hailing distance conceptually of what happened to value in, say, 2010-2017.

We will contrast the last two years to the prior eight on these two dimensions, starting with the second. In the recent period (2018-19), and unlike the prior approximately eight years, value has lost based on price moves without a corresponding major loss on the fundamentals. When value (or any factor) loses in a quick, sharp fashion, it's no fun, but you get a consolation prize. The prize is that the factor generally cheapens a lot. There is very little turnover over a short period, particularly when value loses as the pricing gets more extreme but goes the same direction as before the event,<sup>34</sup> and fundamentals generally don't change fast enough to explain short, sharp movements or induce much turnover in the portfolio (if there's a lot of turnover you can't know if the new portfolio will be cheaper as it's different stocks than those that caused the poor performance). So, the losses are mainly just your longs cheapening against your shorts and the value spread widens sharply. When it's a long slow loss there is more turnover in the factor and more chance for the actual fundamentals to change, greatly weakening the link between factor performance and change in the value of value.

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<sup>30</sup> Finally, this note focuses on the U.S.A. That makes sense given the U.S.A. is the biggest market and has the longest history. Though, of course, we do measure the value of value in all the other regions. Looking around the world all at once (what we call the “developed” region) you get approximately the same percentile (using the AQR full model value portfolio) as you get for the U.S.A. (actually one percentile point less cheap). Japan is the least wide (the lowest value of value) with a percentile in the low 80s. Europe and emerging markets are almost (not quite) as cheap as the U.S.A. In the large cap world that we're examining in this note, the lowest spread (again, using the AQR value portfolio) is Canada, coming in today at the 64th percentile (disappointing, eh?). While not my focus, looking at the value spreads region by region but only in the small cap universe shows very similar results, though a bit less extreme (the value of value is quite attractive but less so versus its own history than in large caps – this is likely the result of poor fundamental performance of small firms which makes the value spread widen less than you might guess from just performance).

<sup>31</sup> I think a lot of these comments probably apply to non-systematic (concentrated active stock picking) value, but I can only speak with any certainty about the systematic versions.

<sup>32</sup> I really thought everyone was a comic book fan...

<sup>33</sup> In fact, we'd, again, venture to guess that most traditional stock pickers focused on value also underperformed.

<sup>34</sup> In contrast, when momentum loses it wants to cut and run.

Now let's examine (1), other long-short factors besides value. AQR incorporates a fair number of more fundamental factors designed to pick up companies that are indeed worth or more than worth their valuations, or at least starting to get better (you can loosely think of this as trying to avoid "value traps" though we believe they are positive average return factors on their own).<sup>35</sup> They are generally trying to measure things like gross profitability, the short-term change in fundamental performance, the quality of accounting practices (looking for the dodgy versus upstanding companies), and piggy-backing on the picks of "informed" investors (active managers where you can infer their positions from overall short balances or directly from other sources).

For example, an equally weighted composite of these four fundamental factors<sup>36</sup> using hypothetical AQR backtests<sup>37</sup> (we have to use backtests as we don't directly trade such single themes) has produced a +1.98 gross Sharpe ratio since 1981.<sup>38</sup> In the last 10 years it's produced a 1.52 Sharpe, less than the long-term backtest<sup>39</sup> but still pretty great. But, if you break up the last 10 years,<sup>40</sup> this backtest has produced a Sharpe of +2.23 between the GFC and the drawdown that started in March 2018, and -0.77 during the AQR drawdown (again totaling a 1.52 Sharpe over the two periods combined). That super-strong +2.23 was a large part of why we generally did quite well even as value suffered post-GFC.<sup>41</sup> The -0.77 recently, while value continued to fail since March of 2018 is, in turn, a major part of why we're now suffering.<sup>42</sup>

That is, from basically the end of the GFC through February 2018 (pre-AQR drawdown) a portfolio that went long high gross profitability, long fundamental momentum, long better (in our view) accounting practices, and long those stocks favored by informed active managers, and short the opposites, kicked major butt and really helped pick us up in a period of most value measures losing "for fundamental reasons." After February 2018 (AQR drawdown period) this delivered a modest negative with value still getting creamed.

Essentially, we think the evidence is strong that the first eight+ years of value losing was "rational" (for want of a better word). The expensive companies ex post more than justified their ex ante starting prices based on things like delivered earnings, sales, margins, cash flow, etc. In contrast the last almost two years have seen value lose for "irrational" reasons. Value fundamentals have not come in worse over this recent painful period, it's just prices that have gone the wrong way ("just" seems like the wrong word here...). In such a period, the very bad news is the factors you include to help in a "value trap" don't work nearly as well as the problem is not a value trap but a, perhaps irrational, change in sentiment. The only factor you might expect to help in such an environment is price (not fundamental) momentum, and as much as we've always included price momentum in our process,<sup>43</sup> there's a limit to how much you want. Its long-term Sharpe ratio is good, and its negative correlation to value is great, but not good or great enough to merit far more weight than we use (IMHO) as momentum has a pretty bad [left tail](#) that makes it scary to weight beyond a certain point (if the weight on momentum isn't too large value can do well enough in momentum's crashes to keep things ok<sup>44</sup>), and more mundanely the other non-value non-price-momentum factors are pretty great too (again in our humble opinion).

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<sup>35</sup> Graham and Dodd concentrated stock pickers sometimes get really mad at the academics and quants calling price-to-fundamentals the "value factor" as they correctly look at far more than just price. In reality this is a communication not a philosophical difference as quants and these stock pickers look at some very similar things, G&D stock pickers just combine things together and call the result "value" while academics and quants call price-to-fundamentals value and the other factors other things. Semantic differences can occasionally, of course, lead to ravaging decades-long land wars.

<sup>36</sup> These are four long-short portfolios. For illustrative purposes only and not representative of any portfolio that AQR currently manages. There is no guarantee that these strategies will be successful. There is a potential for loss. Hypothetical data has inherent limitations some of which are disclosed in the Appendix. Please see the Appendix for an explanation of this backtest construction.

<sup>37</sup> These are gross of transaction costs.

<sup>38</sup> Remember – all backtests, even good ones, are overstated! And gross of t-cost backtests for factors with higher turnover are even more overstated than those which trade less (like the value factor). We generally assume significant discounts to backtests going forward.

<sup>39</sup> Most of these measures have been used live over this period so the last 10 years are reasonably close to out-of-sample. We've often used "half of backtest" or even less as a reasonable goal for out of sample so this is comforting.

<sup>40</sup> We break this period of overall suffering for most well-known value measures into the much more enjoyable (to us!) approximately eight years after the GFC but before our own drawdown, and the AQR drawdown.

<sup>41</sup> The other part we already mentioned – that our value measures did better than more generic ones over the first eight years but worse in the last almost two.

<sup>42</sup> This pattern, very strong performance in the first eight-ish years of the value drawdown, but negative during the more recent period, shows up for all four of these long-short strategies (and, obviously as in the text, big time for the combination of the four).

<sup>43</sup> I vaguely recall writing a [dissertation](#) on it a couple of years ago.

<sup>44</sup> In [this paper](#) we examine one such momentum crash (March – May 2009).

So, these last 10 years have been a tale of woe for most well-known versions of the value factor.<sup>45</sup> However, it has only been a tale of woe for AQR value for just over a year and a half (but woe it certainly is!). This corresponds quite well to both the performance (or lack thereof recently) of other factors designed to pick up fundamentals not present in a pure value factor (or, often actually implicitly shorted by the pure value factor), and is also very consistent with value, as we measure it, not getting very cheap until recently, but as of now being very cheap as the recent almost two years of losses have been due to price moves, not fundamentals (and that's when things cheapen).

## A Word About Catalysts

We often get asked “what's the catalyst for value to start to work again?” Frankly, we don't have a great answer to that question (nor do we think anyone else does). For one thing, we think value works (or doesn't) much more “stock-by-stock” than from betting on grand themes (perhaps removing the industry bet is part of why we think that way). It didn't have major catalysts (that we can identify) for why it has lost on price (not fundamentals) these last almost two years, and we admittedly don't know the catalysts going forward.<sup>46</sup> If we did know, we'd wait to see them and then perhaps recommend sinning a lot! All we can do is take bets that we think maximize our chances of, but never guarantee, success. In the absence of great timing power (knowing the catalysts), that's what we think we're doing.<sup>47,48</sup>

## Conclusion – A Modest Tilt

For a long while, even as value suffered, we cautioned against upping the value weight. This was both because timing just ain't easy, and because despite the losses, value simply didn't look exceptionally cheap. Also, as we've discussed here, for a long while as value suffered, we prospered. This was both because our value measures didn't suffer as much, and especially because many other factors, some explicitly designed to capture the idea of a “value trap,” did exceptionally well. However, the last almost two years have been different. Value has continued to suffer, but lately for less fundamental and more just price (investor sentiment) reasons, and lately has suffered more for our mix of value factors than for generic measures.<sup>49</sup>

Having said that, even now we're more circumspect than some. You can find lots of analysis out there that claims this market is crazier than 1999-2000.<sup>50</sup> It's not. Usually I hedge statements like that, both for legal and simple accuracy reasons, with something like “we think it's not.” But this time it's just not, no hedge required. It's currently pretty crazy for value strategies (in a bad way looking backwards in a good way looking forwards) but 1999-2000 was looney tunes.

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<sup>45</sup> And, again, we're guessing most dedicated value stock pickers suffered too.

<sup>46</sup> Even looking at the biggest value event in our history, the 1999-2000 tech bubble, with the benefit of hindsight we still don't know what the catalyst was for the fever to finally break in March of 2000. Some point to the “burn rate of cash” of dot coms (as highlighted in a famous Barron's cover story). Perhaps that was it. But for the prior few years they were able to raise new capital to fund those high burn rates. So, it's not enough to identify that as the big catalyst, you also have to say (and ideally identify how to do this ex ante so you can time the strategy) why, finally, as NASDAQ hit 5000, but not 4000, this refinancing became impossible. If we can't precisely identify catalysts even ex post, we are seriously skeptical about ex ante (though we'll never stop trying to prove ourselves wrong here!).

<sup>47</sup> Perhaps one can think of past performance or price momentum in the value factor as a signal about any potential “catalysts.” This approach waits for some evidence of a change in the conditions that have caused widening value spreads before acting (while remaining agnostic to what actually triggered that change). We do incorporate some of this approach in our process also, though one contrast between value and momentum timing is we find the predictability of the latter to be a bit more binary – we care about the sign but don't necessarily scale up our conviction when magnitudes get larger. With value, when the magnitude gets big enough (as it did in the tech bubble or in the more recent period) we think the opportunity becomes that much more attractive, to the extent where it makes sense to start to act even if the momentum has not yet manifested (you then act more when the momentum hopefully starts!). Also keep in mind that often in these big value spread events as value starts to recover there comes a point where the value spread and the momentum of value both point in the same direction. At times like that a timing model takes its biggest pro value bet.

<sup>48</sup> Some point to low interest rates and/or passive indexing as causes for value's travails (negative catalysts that presumably could one day turn around). The interest rate point may be directionally right, as value is a shorter duration asset than growth, but is quite weak explaining very little of the historical returns to value. The indexing thing I don't even fully get (value spreads are also wide within the major indices). But no matter. These are possible reasons for why value has gotten extremely cheap vs. history. But that it has gotten extremely cheap versus history is the salient fact. Anyway, neither for these, based on their power and their story, seem likely useful candidates for timing value going forward. [If you think rates are going to rise don't use that to bet on value, short a bond.](#)

<sup>49</sup> This is pretty darn self-serving but one theory for this is our value measures are in fact more precise so when value fails for rational reasons we fail less, and when it fails for irrational reasons we suffer more. Anyway, that's my story and I'm (semi) sticking to it.

<sup>50</sup> It's not, as we demonstrated throughout this piece. But there's another way 1999-2000 was different. In 1999-2000 the expensive stocks were worse companies than the cheap stocks on many of our objective measures. That's rare. 1999-2000 was truly special (in good and bad ways!).

But waiting to invest in value, or even to up the weight slightly as we're doing, until we hit March of 2000 levels isn't reasonable. It's like waiting to invest in stocks until you see a Shiller CAPE of 7 like back in 1982. If next month the Shiller CAPE falls to 14 from today's low 30s, which would really stink for the world, we'd say it's time to sin a little (perhaps very little as market timing is really hard), particularly of course for those who can maintain a long horizon. We would not recommend waiting for another 50% drop that probably will not come. The odds for medium- to long-term investors are better at a 14 CAPE than the low 30s and, absent a likely unrealistic ability at precise timing, we go with the odds.

Right now the spread analysis shows that value, all kinds but particularly AQR value, is quite cheap versus history but, obviously, does not tell us precisely when value will work again. Importantly, it does tell us the strategy has not been arbitrated away (as some fear). Instead, it tells us that value is out of favor, somewhat the opposite of being arbitrated away, and that today is the time to sin a little in value's direction.

If you believe, as we do, that value is a good long-term strategy, and an important part (not all) of an investment process, we would recommend a modest<sup>51</sup> extra amount of value than the norm.<sup>52</sup> To the skeptics we would ask "if not now, when?" If the answer is "only when it's as bad as the tech bubble" we just think you're making the wrong call.

Now, it would be nice to turn out to be surprisingly (from dumb luck) good at the timing and have it start to work as soon as you read this piece!

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<sup>51</sup> Again, modest, as we have consistently shown in our writings and said here that one should be cautious when it comes to timing.

<sup>52</sup> The amount will vary by portfolio and each portfolio's objectives, please contact your AQR representative for details where you're interested.

## Appendix

Earlier in the main text, we defined value spreads as the ratio of the weighted average valuations of the long and short sides of a long-short dollar-neutral factor portfolio.<sup>53</sup> We now describe in more detail how each of these long-short value portfolios are constructed and the corresponding valuation measures used for their value spreads.

1. The academic approach uses a book-to-price factor built over a U.S. all-cap universe that combines the NYSE, AMEX and NASDAQ. It is similar to the Fama-French HML factor, except that up-to-date prices are used. The raw version of the factor ranks stocks over the entire universe. The intra-industry version ranks stocks within industries only so as to take no industry bets, whereas the inter-industry version ranks only industries. The industry classification is based on SIC (Standard Industrial Classification) codes before 1986 and MSCI GICS (Global Industry Classification Standard) codes after 1986. The long side of each portfolio includes the best (cheapest) 30%, while the short side includes the worst (richest) 30%. The long and short sides are then market-cap weighted. The value spread uses the book-to-price of these book-to-price portfolios.
2. The “four widely-known measures” approach uses a more investable, U.S. large-cap-only universe from the union of the Russell 1000, MSCI U.S, S&P 500 and S&P 400. The four-factor value portfolio combines four factors, namely book-to-price (BP), trailing-earnings-to-price (EP), forward-earnings-to-price (FEP) and sales-to-enterprise-value (SEV), at weights of a third, a sixth, a sixth and a third respectively (the idea being to assign equal weight to book value, earnings, and sales, and the earnings raw weights are lower as they’re really two correlated versions of the same idea). Each of these four value measures is adjusted for cash and short-term investments, and each factor is built to be industry-neutral, beta-neutral and dollar-neutral by using within-industry value scores adjusted for market beta. The industry classification is based on MSCI GICS industry codes. Stocks are weighted proportionately to these value scores (as opposed to cap weighting in (1) above). The value spreads for the portfolio use the same measures used to build the portfolios (i.e., they are the BP, EP, FEP, SEV ratios of the long and short sides of the combined four-factor value portfolio.)
3. The AQR full model value portfolio is constructed in a similar manner as the AQR four-factor value portfolio but includes around 25 different proprietary factors. As mentioned earlier, the usage of ratios for value spreads precludes the usage of value measures that can be negative. So, the value spreads for the AQR full model value portfolio utilize the same four value measures BP, EP, FEP and SEV, applied to the long and short sides of the combined full model value portfolio.

For both the AQR and the academic approach, we exclude stocks with negative fundamentals (this is known ex ante so involves a strategy choice not cheating) from the portfolios, and windsorize values at 1%.

### *Non-Value Fundamental Factors*

As mentioned in the main text, AQR evaluates stocks on not just value but also the complementary themes of profitability, fundamental momentum, earnings quality, and investor sentiment, in order to weed out “value traps.” Each of these four themes is captured through several proprietary factors e.g., profitability includes margins; fundamental momentum includes earnings revisions and improvement in fundamentals like margins; earnings quality includes accruals; investor sentiment includes the activity of informed investors like hedge funds and mutual funds. Each factor is built to be beta-neutral, dollar-neutral, and target constant ex ante volatility of 7%. Most of these factors are built to be industry-neutral, using industry classification based on MSCI GICS industry codes, but some of them are allowed to take industry bets (where there is economic intuition and empirical evidence for doing so). The factors in each theme are weighted so that 75% of each theme is industry-neutral, and 25% is not.

### *Limitations of Backtested Performance*

The results presented reflect hypothetical performance an investor would have obtained had it invested in the manner shown and does not represent returns that any investor actually attained. The information presented is based upon the above hypothetical assumptions.

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<sup>53</sup> The value spread is constructed to use constant leverage (say a dollar long, a dollar short) at all points in time, so that merely changing leverage (due to the targeting of constant volatility) does not lead to a change in value spreads.

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