MAPPING OUT MOMENTUM

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Abstract

Momentum effects, whereby prior stock returns predict future returns, have been documented in a wide variety of contexts ranging from single-stock effects to the performance of industry, sector, and factor indices. However, it remains challenging to reconcile and unify these disparate findings due to differences in methodology, calibration, data universe, and the granularity of tests. Our goal is to attribute stock return predictability to a variety of distinct momentum (and reversal) components within a single coherent framework. We focus on S&P 500 stocks and implement consistent data transformations, nested sets of excess returns, and panel regressions to facilitate this attribution. We find that sector and factor momentum coexist, but they often operate on different horizons, and sector momentum is more prone to crash during volatile markets. Collectively, sector and factor momentum explain away most of the security-specific 12-month momentum effect, with factors explaining the greater share. Traditional 12-month momentum is more prevalent for past "loser" stocks whereas crashes and reversals are found mostly among past "winners." Lastly, we show that in the decade after the 2008-2009 financial crisis compared to the decade prior, sector and industry momentum disappeared at the 12-month horizon but intensified in terms of 1-month reversals.

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A large literature has documented momentum and reversal effects in stock markets. Despite the apparent difficulty of reconciling these findings with the Efficient Markets Hypothesis, studies have repeatedly shown robust relationships between trailing stock returns and future performance. Momentum has been observed for individual securities and for aggregations of securities, raising interesting questions about which effect is more elemental and whether one causes or subsumes the other. Jegadeesh and Titman (1993) documented cross-sectional momentum in individual stocks. Moskowitz and Grinblatt (1999) found momentum in the returns of industry indexes. Recently, multiple papers such as Gupta and Kelly (2019), have analyzed momentum in equity portfolios formed on factor attributes (such as size, value, quality, and others). Arnott et al. (2019) argue that factor momentum causes industry momentum. Ehsani and Linnainmaa (2020) suggest that factor momentum causes individual stock momentum.

These findings can be difficult to reconcile and unify for four reasons. First, though the returns of industry and factor portfolios clearly relate to the returns of the stocks that compose them, the composition of the portfolios changes through time. Second, the studies cited earlier generally find the strongest single-stock momentum at a 12-month horizon with significant single-stock reversal (*negative* effects) at 1-month, but the *positive* momentum for industries and factors is often shown to be strongest at 1-month. Third, portfolio-level performance tests are sensitive to decisions such as whether to weight stocks equally or by market capitalization, and whether or not to control for sector exposures – as documented by Scowcroft and Sefton (2005). Fourth, many momentum strategies have experienced occasional crashes as shown by Daniel and Moscowitz (2016) and others. Thus, some genuine momentum effects could be obscured by rare but dramatic reversals. We propose a unified solution to these measurement issues.

Our goal is to disentangle the overlapping effects of momentum and reversal using panel regressions at the stock level. By regressing stock returns on various nested sets of lagged returns that pertain to them, we decompose trailing price effects into distinct industry, factor, and idiosyncratic components. We consider 12-month and 1-month effects simultaneously. To address the risk that some important linkages might go unnoticed, we include momentum signals that are derived from statistically identified groups of "nearest neighbor" stocks. We account for reversals during adverse market conditions by separating our sample into high versus low volatility periods. We also attribute each effect to positive signals (winners) versus negative signals (losers). Finally, we test for structural changes over time by separating our sample into periods before and after December 2009. We conclude with a summary.

Data and Design

We analyze stocks in the S&P 500 universe between 1995 and 2020. We choose this large capitalization and highly liquid market to remove concerns that pricing anomalies are mere artifacts of market frictions, or that the predictive relationships we identify would not have been implementable in practice, or accessible at scale.

We run pooled panel regressions on individual stock returns. Our dependent variable is the total return of stock *i* for the following month, normalized as a cross-sectional percent rank and centered to have zero mean. After this normalization, it is nearly impossible for a few outliers to have an undue influence on results – a concern that would otherwise loom large for stock returns. It also mitigates heteroskedasticity in errors, leading to more robust statistical inference. And, since there are months in which stocks mostly rise (bull markets) or mostly fall (bear markets), the cross-sectional ranking keeps our focus on relative "winners" and "losers." We compute returns from trading day t+4 to trading day t+23 to reflect a subsequent one-month period plus a 3-day implementation lag.

As explanatory variables, we include a variety of trailing one year (day t-250 to t-20) and one month (day t-20 to t) price returns that are related to stock *i*. We consider four categories of price signals.

First, we include the returns of the hierarchical industry portfolios that contain stock *i*, according to the Global Industry Classification System (GICS). We measure industry returns in a nested fashion, taking the excess return of each more granular segment above and beyond the return of the broader class to which it belongs. We compute a stock's level 1 sector return in excess of the broad market, its level 2 industry group return in excess of its level 1 sector return, and its level 3 industry return in excess of its level 2 industry group return. In each case, we percent rank the excess return against the relevant set of cross-sectional comparisons. This approach avoids multicollinearity and provides a clean attribution across levels.

Second, we include the return of stock *i* in excess of its level 3 industry return. These stock-specific excess returns are ranked across all stocks in the universe at time *t*.

Third, we include size, value, investment and profitability factors as proposed by Fama and French (2015). We treat these factors based on observable attributes the same way we treat industry classifications. Specifically, we form decile portfolios by ranking stocks cross-sectionally on a given attribute (such as book-to-market for the value factor) and identify the portfolio that contains stock *i*. For that stock, we record the cross-sectional rank of that decile portfolio's trailing returns compared to the nine other decile portfolios. We re-rank for each attribute, so these groups are not nested like the industry classifications. Mathematically, it must be the case that if factor momentum occurs for groups of stocks, the stocks that compose the factor portfolio at any point in time "inherit" the performance trends of the composite factor as a component of their own trailing total return.

Fourth, we include the returns of an index of "peer" stocks whose price behavior is statistically most similar to that of stock *i* as of time *t*. This statistically defined factor may identify dimensions of similarity beyond the traditional ones we enumerated previously, but which are reflected in prices nonetheless. It might pick up on broad-based factors that we did not include explicitly. Alternatively, it might pick up on narrow similarities in circumstances across firms. Or, it might reflect other effects such as pairs of stocks that tend to be held together by large mutual funds or ETFs or groups of stocks that react similarly to certain news events.

To identify the statistical peers of stock *i*, we measure the Mahalanobis distance of every other stock in our universe to stock *i* and select the 3 with the shortest distance. Originally introduced by Mahalanobis (1927, 1936), this distance measure has recently been applied to financial asset returns in a variety of contexts by Czasonis et al. (2020a, 2020b, 2021a, 2021b), Kinlaw et al. (2021) and Kritzman et al. (2021). Unlike the Euclidean distance between two vectors, the Mahalanobis distance accounts for the covariances of the variables. We use a vector of 24 variables to describe each stock, where each variable is the return for a given month in the past 2 years. Thus, two stocks will be close in Mahalanobis distance if the spread between their respective past returns is small compared to the typical variance across all stocks, and if the spread between their past return patterns are close to the typical pattern of returns that is observed across all stocks. Because the stocks in focus are the most accessible and actively traded securities, it is probable that their historical return patterns embed the market's assessment of which stocks react to common news events or material information. Once we identify the 3 closest peers for stock *i*, we equally weight them as an index and compute their trailing 12 or 1 month returns in the same fashion as for industries and factors. We repeat this process for every stock and compute the cross-sectional percent rank of peer returns.

Exhibit 1 summarizes the data inputs to our analysis. Altogether, we have 18 explanatory variables. Given that our primary goal is to attribute momentum across sources, it is important to emphasize that our variables avoid multi-collinearity. Among all 153 pairwise variable correlations, the maximum is +0.26, the minimum is -0.07, only 2 correlations are above 0.20, and only 17 additional correlations are above 0.10. The average is 0.04.

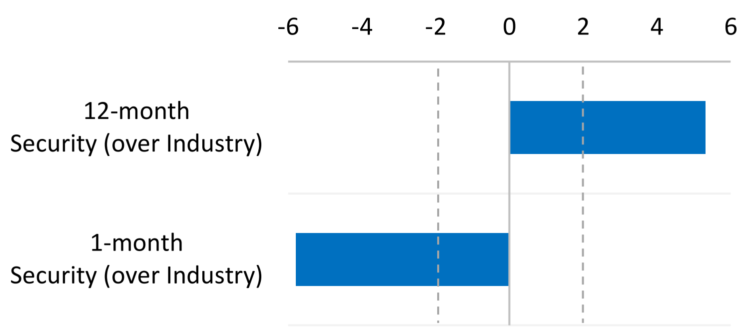
Exhibit 1: Data inputs

Type	Time frame	Category	Variable	Construction
Dependent variable	Future return from trading day t+4 to t+23 (20-day period plus a 3-day lag)	Security	Security total return	Cross-sectional percent rank
Explanatory variables	Long-term: from trading day t-240 to t-20 Short-term: from trading day t-20 to t	Security	Security over Industry	Cross-sectional percent rank of returns
		GICS groups	Industry over Ind. Group Ind. Group over Sector Sector over Market	Cross-sectional percent rank of returns
		Risk factors	Investment Profitability Size Value	(1) Form 10 decile portfolios of stocks according to the underlying definition (2) Compute the cross-sectional rank of each portfolio's return and assign the rank to stocks according to portfolio membership
		Statistical peers	Mahalanobis peer group index	Cross-sectional percent rank of equal-weighted peer stock returns

Decomposing Momentum

We begin with a basic result for single-stock momentum, shown in Exhibit 2, including only the two stock-level predictors. We summarize the coefficient loadings by reporting their *t*-statistics, which retain the same sign as the coefficients but adjust for statistical uncertainty. Given our large sample size, a *t*-statistic of 2 has the natural interpretation that an effect is statistically different from zero with 95% confidence.[1] Though we indicate thresholds of 2 in the exhibit, we acknowledge that the choice of a significance threshold is somewhat arbitrary and subject to debate. Our primary interest is to compare the sign and strength across effects. Throughout this paper, the *t*-statistics we report are based on standard errors that are heteroskedasticity and autocorrelation consistent following the standard method of Newey and West (1987).

Exhibit 2: Stock-level Momentum
Panel regression *t*-statistics

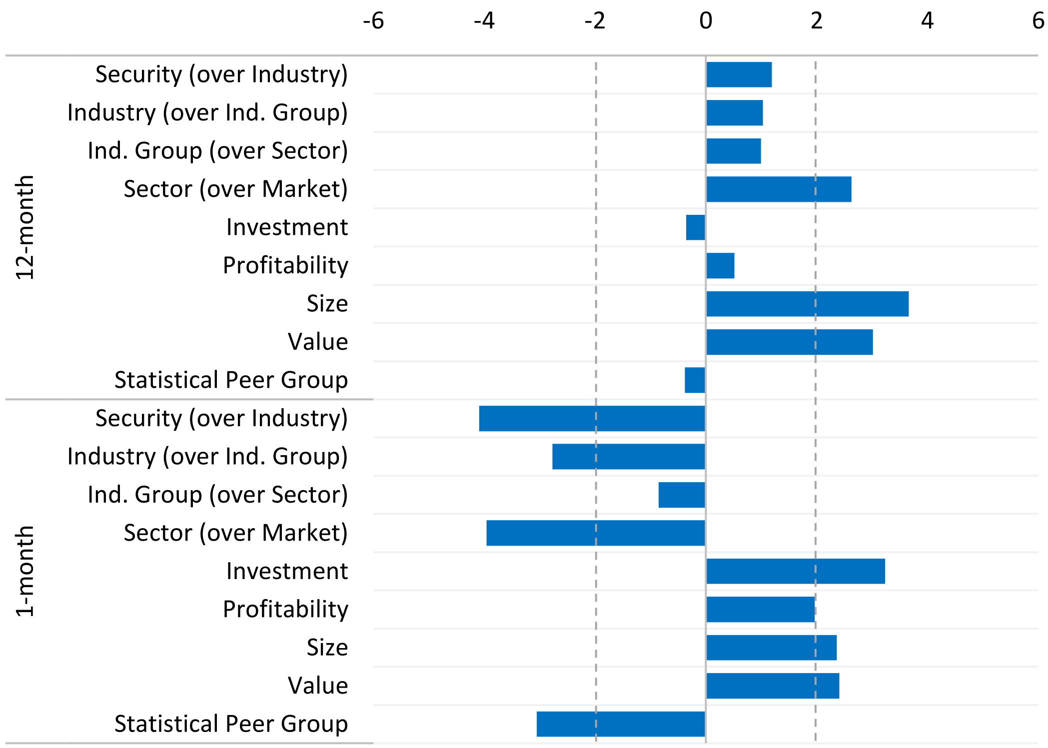


In Exhibit 2, we observe strong positive momentum for the 12-month (up to month *t-1*) signal and reversal for the 1-month signal, consistent with previous literature. Both effects are highly statistically significant.[2]

Next, we run the panel regression including every predictor from Exhibit 1. Exhibit 3 shows the *t*-statistics of the resulting regression coefficients for the full period 1995 to 2020.

Exhibit 3: Momentum Components

Panel regression *t*-statistics



Compared to Exhibit 2, the stock-specific 1-month reversal effect remains highly significant, but the 12-month stock-specific momentum effect diminishes to such an extent that it is no longer statistically significant. In its place, we see significant 12-month momentum associated with the trailing returns of a stock's economic sector, a cohort of similarly sized stocks, and a cohort of stocks with similar valuations. In addition to security-specific reversals, the 1-month reversal effect is also highly significant in terms of the trailing returns of a stock's industry, sector, and close statistical peer group. Interestingly, the 1-month signals for all four factors are positive and generally significant, in contrast to the 1-month effects of specific stocks and industry classifications which are negative (reversals). This finding aligns with prior literature for factors.

These results reveal that the cross-sectional predictability of trailing returns comes from many coincident and distinct sources. Our setup allows us to compare the strength of these effects directly, and in common units. The findings suggest that 12-month single stock momentum is subsumed by other forms of momentum, but single stock 1-month reversal is not. The influence of industry classifications and factors differ. Industries and sectors exhibit strong reversal effects, whereas positive momentum appears to be dominated by factors. It is premature, however, to conclude that factor momentum explains industry-related momentum on this basis. We show in the next section that positive industry and sector momentum is indeed a prevalent force, but it is obscured in the full sample by occasional crashes.

Calm versus Volatile Markets

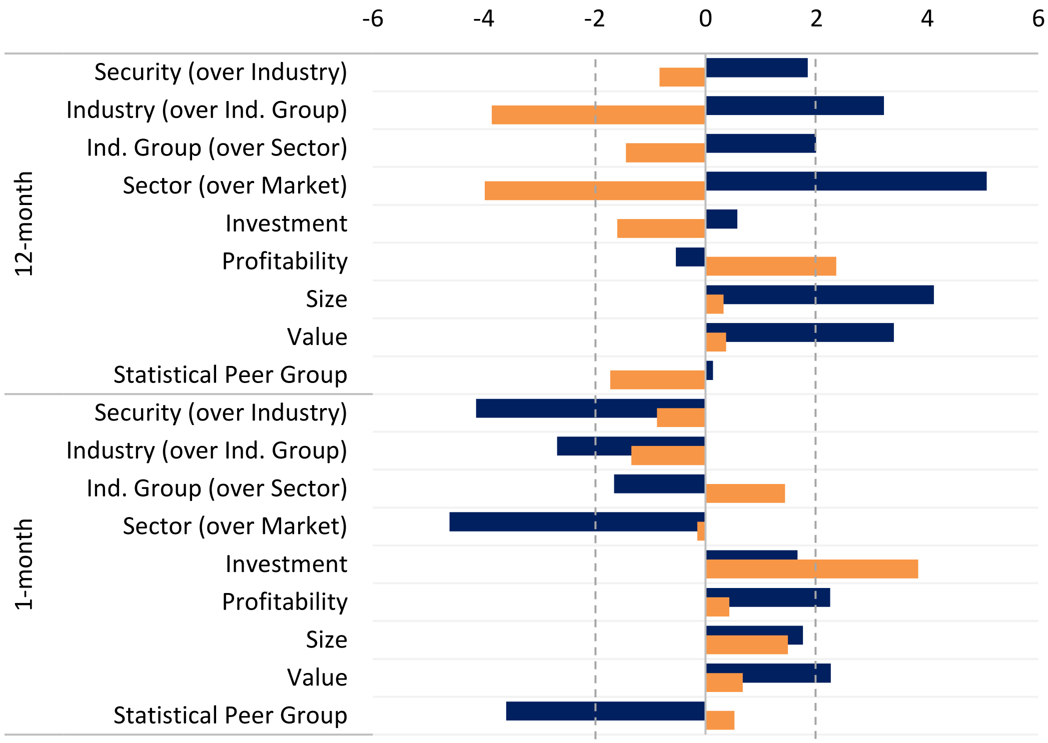
The tests we have presented so far implicitly assume that momentum effects are stable over time. There are good reasons to doubt that assumption. As a strategy that benefits from the continuation of price trends, one might expect positive momentum to work best during stable regimes. Unexpected shocks or increased uncertainty may overwhelm persistent trends, or even reverse them if news causes investors to revise prior beliefs. The general tendency of assets to rise slowly and crash quickly – perhaps driven by behavioral factors – could create long periods of positive momentum together with less common but dramatic moves in the opposite direction.

Regime shifts may operate through other channels, too. Wang and Xu (2014) show that adverse market conditions lead to temporarily low momentum payoffs, perhaps due to excessive pessimism about past losers during market downturns that cause strong positive rebounds in those stocks. It is also possible that other drivers of momentum, such as behavioral biases, underreaction to news, or allocations of capital across investment products, also differ by regime. For any of these reasons, an unconditional full sample regression might fail to identify cross-sectional momentum that occurs in some periods but reverses in others.

To investigate this issue, we segment the time periods in our historical sample according to market volatility, measured as a month's average value of the CBOE's Volatility Index (VIX). We run two separate panel regressions: one for the next month returns of stocks that coincide with low volatility (VIX <= 25) and the other for next month returns that coincide with high volatility (VIX > 25). We choose this simple threshold because it identifies approximately 20 percent of the sample as high volatility. Note that we condition on the volatility that prevails in month *t+1*, which is only observable after the dependent variable returns have occurred. Therefore, this test does not evaluate an implementable timing strategy. Instead, it is an ex-post attribution of momentum effects conditioned on *realized* volatility. We use VIX not because it is forward-looking, but because it provides an observable proxy for investor sentiment about market volatility at any given point in time. The fact that volatility conditions are defined contemporaneously to return outcomes does not impart any bias to our attribution analysis, because the effects we measure are cross-sectional. Exhibit 4 presents these results.

Exhibit 4: Calm versus Volatile Markets

Panel regression *t*-statistics (dark blue = low volatility, orange = high volatility)



The low volatility (dark blue) results in Exhibit 4 are qualitatively similar to the unconditional results in Exhibit 3. The notable exceptions are that 12-month momentum for security-specific, industry, industry group, and especially sector are now much more significant. Each of these effects reverses meaningfully in volatile periods, which explains why they were not identified in the full sample regression. The same dynamic applies to the 12-month investment factor and statistical peer group momentum, but less so. Size and value merely cease to work at the 12-month frequency, while profitability is the only 12-month effect that works better in volatile times. Most of the one month effects, both reversals and positive momentum, simply fail to generate reliable results of any kind when volatility is high. The one exception is the investment factor.

Why do industry and sector momentum signals reverse so strongly during volatile periods while factor-based momentum signals do not? One possible reason is the relative stability of sector and industry groups, compared to the revolving composition of factor portfolios. Persistent momentum in a sector will inflate the value of a stock within it, and since stocks do not change sectors often, it will continue to experience sector-driven momentum until it is overvalued and eventually crashes. Persistent factor momentum likely has a different impact on stocks, because stocks that are included in the factor eventually rotate out and are replaced by others. To the extent some stocks are temporarily overvalued as a result, they may correct more gradually. Therefore, we speculate that perhaps bubble-like behavior in sectors leads to more spectacular crashes than does similar bubble-like behavior in factors.

As an extension to the results shown in Exhibit 4, we perform a similar analysis to determine whether the sector/industry effects or the factor effects explain more of the security-specific 12-month momentum. In particular, we include only the sector/industry variables or only the factor variables along with security-specific momentum, and we report these results in the Appendix. Our main takeaway from these experiments is that sector/industry variables by themselves only explain a modest portion of security-specific momentum, which still retains a highly significant *t*-statistic of 2.85. However, factor-based signals on their own reduce the *t*-statistic of security-specific momentum to 1.27.

To summarize, our full sample results from 1995 to 2020 that account for both calm and volatile regimes reveal that:

- There is some security-specific 12-month momentum that is not explained by broader market segments, but it is only marginally significant at best (*t*-statistic = 1.85).
- Security-specific 1-month reversal is highly significant (*t*-statistic = -4.14).
- Significant and distinct sources of industry/sector momentum and factor momentum coexist.
- Sectors, industry groups, and industries each exhibit significant and distinct sources of momentum and reversal.
- There are meaningful residual effects explained by close peer stocks for 1-month reversals, but not for 12-month momentum.
- Sector/industry momentum crashes more dramatically in volatile markets than does factor momentum.
- Security-specific 12-month momentum is "explained away" by factor momentum to a greater extent than it is explained by sector/industry momentum (see results in the Appendix).

Winners versus Losers

We further decompose momentum and reversal effects by allowing for the possibility that they differ when a price trend signal comes from a "past winner" versus when it comes from a "past loser." We apply a piecewise linear regression framework that separates each explanatory variable into two new variables. Since the original variable values are centered cross-sectional ranks, for each original explanatory variable, the first new variable is created by including all positive values (in other words, the top half of ranks) to represent past winners, while setting negative values to zero. Likewise, the second new variable representing past losers is created by including all negative values while setting positive values to zero. Thus, the sum of the new positive and negative variables for a given attribute equals the original attribute. This means that the original results from Exhibit 4 are feasible in this expanded regression setup, and would obtain under the null hypothesis that relationships are identical in the domain of positive and negative values. We apply the same VIX segmentation and run a pooled linear regression on these new variables. Exhibits 5 and 6 present the results for past winners and losers. Note that we run this expanded regression including winner and loser variables at the same time, but we present the results in separate exhibits for ease of interpretation.

Exhibit 5: Past Winners

Panel regression *t*-statistics (dark blue = low volatility, orange = high volatility)

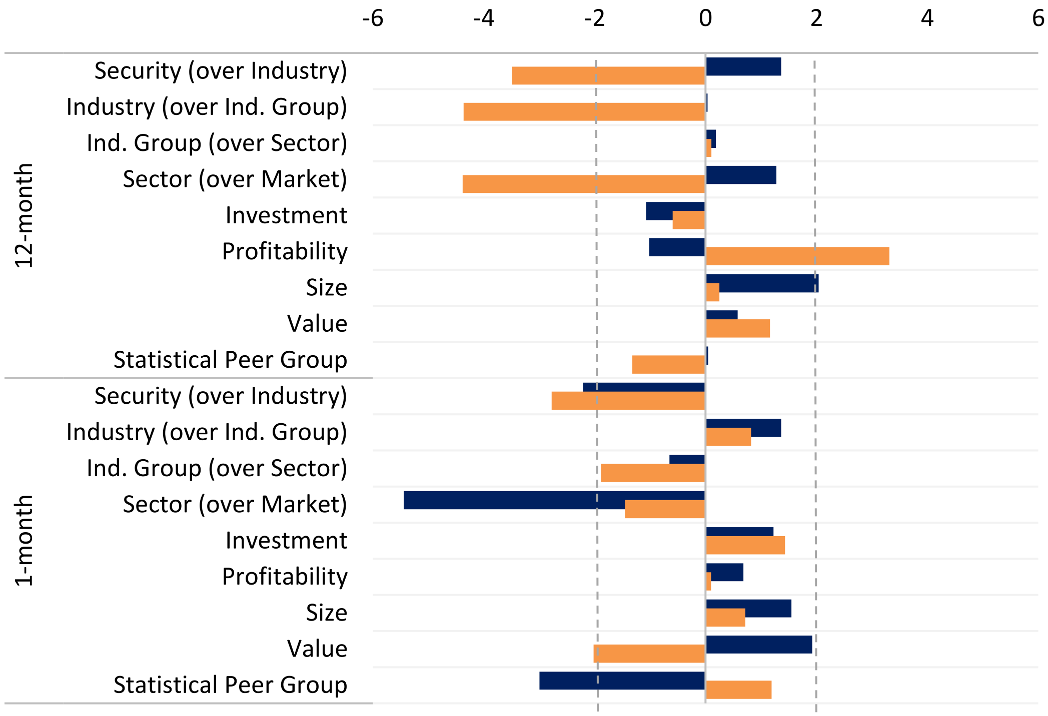
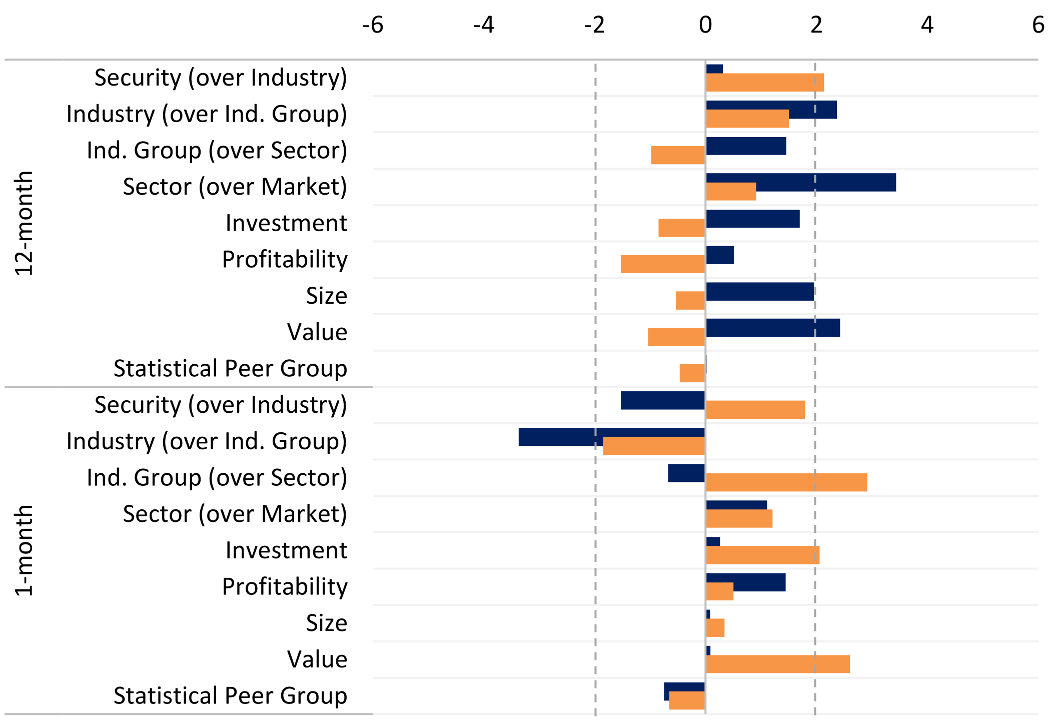


Exhibit 6: Past Losers

Panel regression *t*-statistics (dark blue = low volatility, orange = high volatility)



Overall, the piecewise regression results show that momentum effects manifest disproportionately as negative return outcomes. In particular:

- Momentum in 12-month returns occurs mostly among past losers during calm markets.
- Large negative relationships occur mostly for winners, either as 1-month reversals during calm markets or as crashes of 12-month momentum during volatile markets. This finding is consistent with the intuition that it is easier for overweight positions to build up when the market is quiet, leading to crowded overweight positions and eventually crashes due to market shocks.

Before 2010 versus After 2010

We now consider whether the dynamics of these effects have changed structurally over time. We split our sample into the period that occurred after the 2008-2009 global financial crisis, and everything that occurred up to that point, using December 2009 as the cutoff. Between these two periods, there was a dramatic shift in monetary policy and regulation, quantitative strategies and factor-based investing became more prevalent, and investors had increased access to information powered by mobile devices, social networking platforms, and various forms of automation such as machine learning.

Exhibits 7 and 8 show the regression results for the earlier and later subsamples, respectively.

Exhibit 7: 1995 to 2009

Panel regression *t*-statistics (dark blue = low volatility, orange = high volatility)

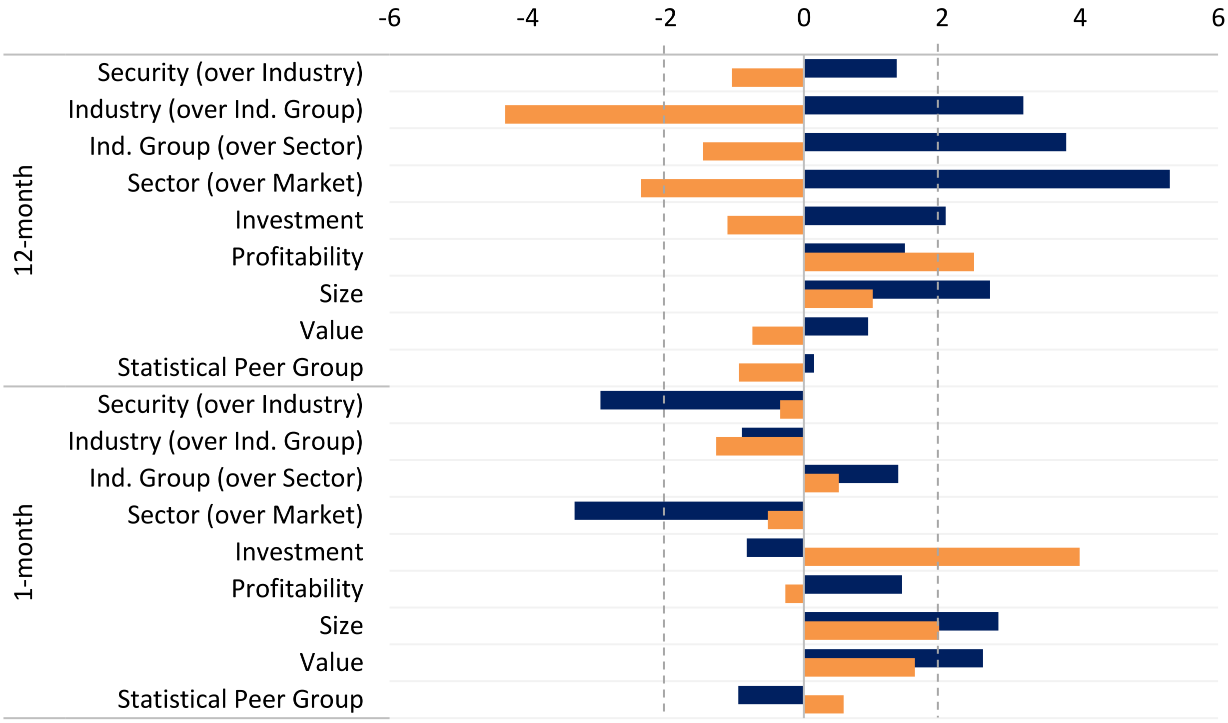
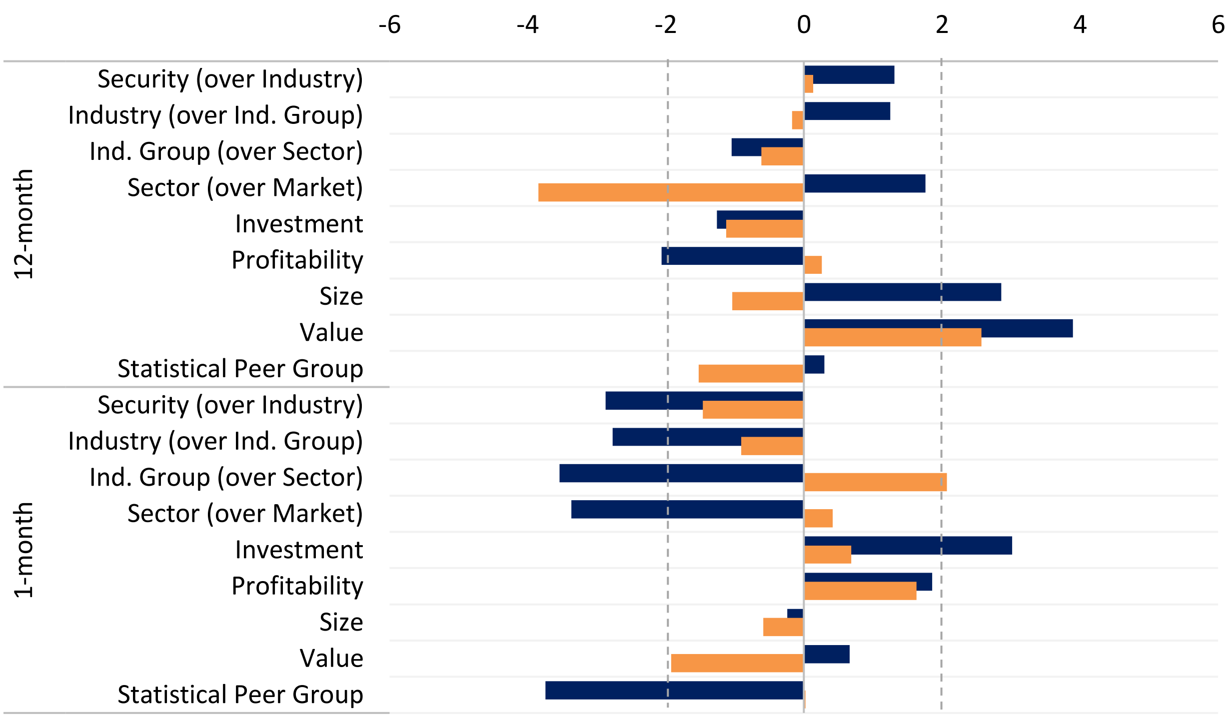


Exhibit 8: 2010 to 2020

Panel regression t-statistics (dark blue = low volatility, orange = high volatility)



The results for 1995-2009 are broadly consistent with our findings in the full sample. The fact that industry, industry group, and sector effects are prevalent during this time aligns with intuition about the dotcom bubble, but in the absence of outliers (due to cross-sectional percent ranking) it is unlikely that such significant statistics would arise without similar dynamics occurring for industries and sectors outside of technology.

The more recent period from 2010-2020 reveals a few interesting trends:

- 12-month momentum in sectors and industries has diminished.
- 1-month reversals in sectors and industries have become more pronounced, and occur at every level of sector/industry stratification, and for close statistical peers.
- There is instability in the 12-month versus 1-month dynamics of factor effects. Between the two time periods, size and value migrated from a 1-month to a 12-month effect, but the opposite occurred for investment and profitability. When interpreting the factor results, it is important to keep in mind that momentum could occur on either side of a trade. For example, the main momentum that occurred for value cohorts during 2010-2020 was one in which the value factor lost money.

Conclusion

Prior literature has documented robust momentum and reversal effects in the stock market. These occur at varying levels of granularity ranging from individual securities to industries, sectors, and factor indices. Whereas recent studies have begun to relate these various types of momentum effects by comparing simulated portfolio strategies in a “top-down” fashion, we take a different, “bottom-up” approach. We use panel regressions on stock returns as a single vantage point to evaluate a multitude of prior return effects in one coherent framework. This approach facilitates comparison because it allows for consistent assumptions, time periods, and relationships between components at the security level. We apply cross-sectional transformations, nested sets of excess returns, and other techniques to facilitate a clean attribution. We restrict our attention to large capitalization US stocks to focus on relationships that are unlikely to arise from issues like extreme illiquidity or inattention.

For stocks in the S&P 500 universe, we decompose the effects of momentum and reversal into distinct components. We find that the capacity of past returns to predict future returns stems not from one source, but from many. Stocks tend to follow the trailing 12-month returns of their industry and sector cohorts, but these trends are prone to crashes and are therefore easy to overlook in a full sample analysis of average effects. In the period since the financial crisis of 2008-2009, 12-month momentum has mostly disappeared for sectors and industries, but it has intensified for 1-month reversals. Factor cohorts also play an important role in predicting the future returns of stocks. Some of these effects occur at a trailing 12-month horizon, and others at a trailing 1-month horizon. Sector and factor effects coexist.

In addition, we find that pro-momentum dynamics exist mostly for past losers in which a stock’s own excess return, or that of its sector or factor cohort, has been negative recently. Meanwhile, the most dramatic crashes tend to occur for stocks (and their cohorts) that are past winners and have performed well recently.

Appendix: Subset Regressions

Exhibits A1 through A3 show results including volatility regimes for subsets of variables. The main takeaway is that security-specific 12-month momentum remains significant in the presence of sector/industry effects but not in the presence of factor effects.

Exhibit A1: Security-specific Variables Only

Panel regression *t*-statistics (dark blue = low volatility, orange = high volatility)

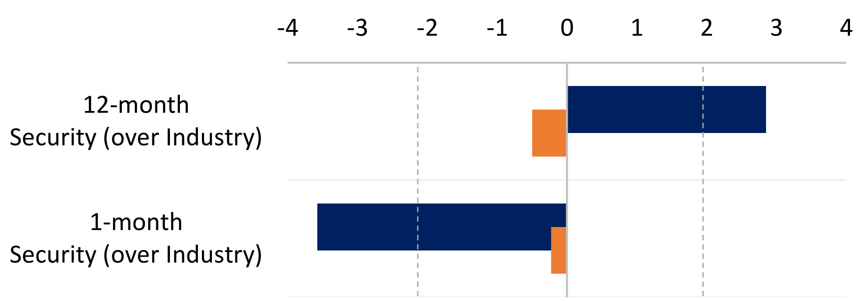


Exhibit A2: Security-specific and Sector/Industry Variables Only

Panel regression *t*-statistics (dark blue = low volatility, orange = high volatility)

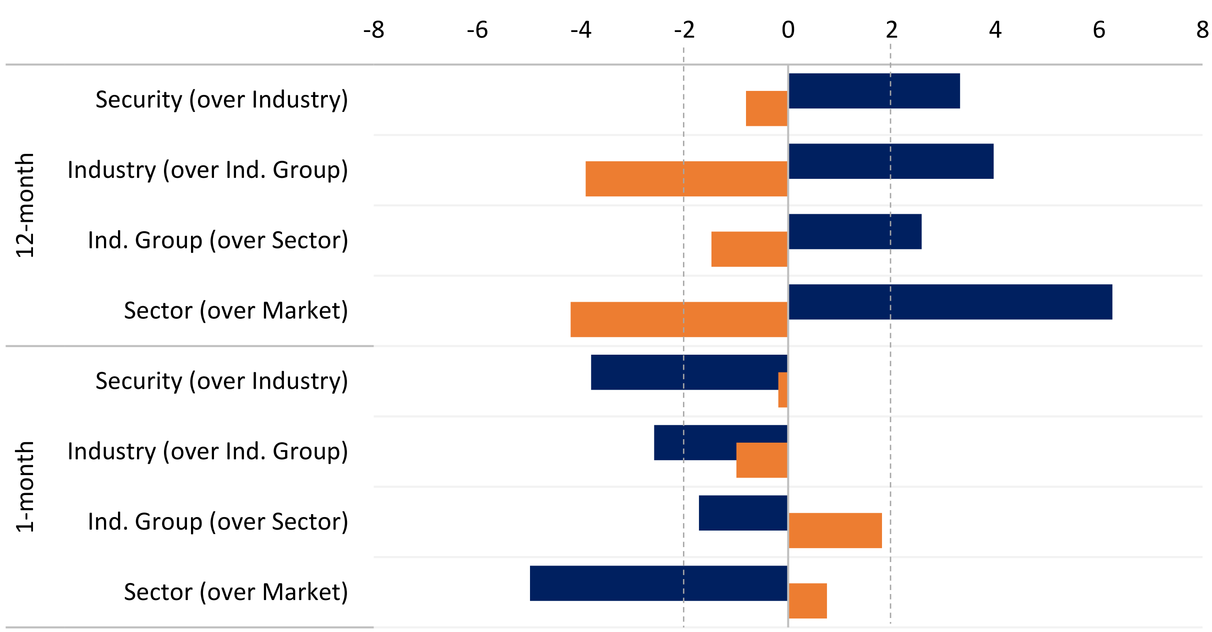
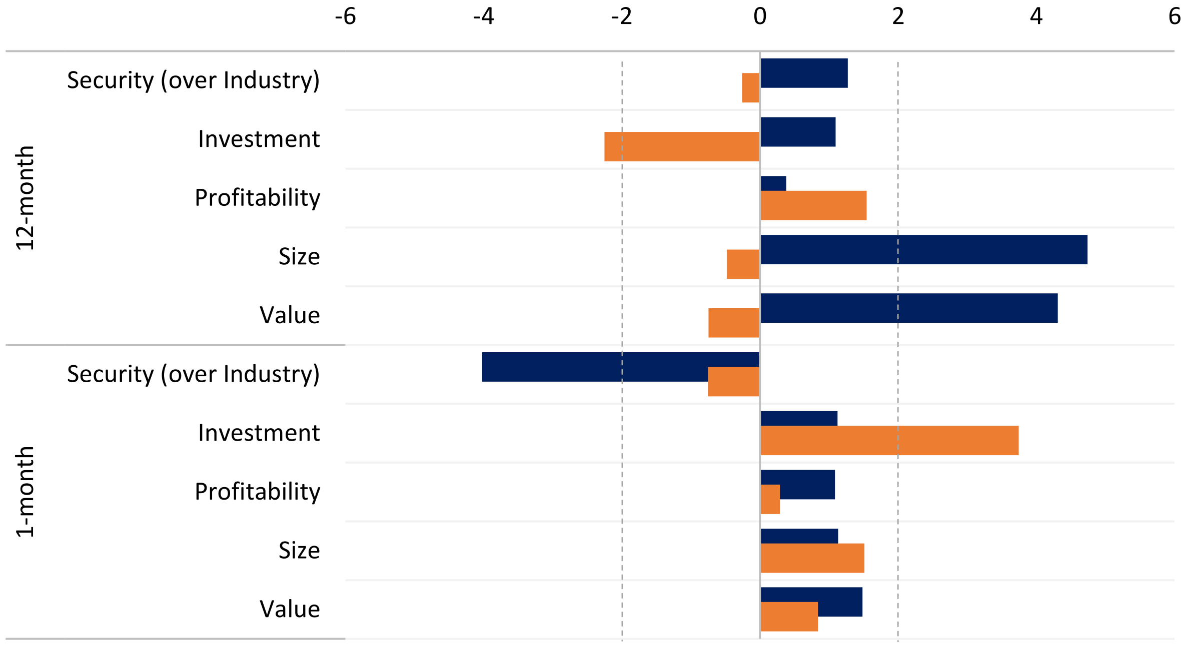


Exhibit A3: Security-specific and Factor Variables Only

Panel regression *t*-statistics (dark blue = low volatility, orange = high volatility)



Notes

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[1] This rule of thumb applies to a one-sided test, in which we hypothesize the direction of an effect. For a two-sided test that does not specify the direction of an effect, the critical threshold would be slightly larger than 2.

[2] To further reinforce the validity of the results obtained from this setup, we ran a version of the same regression that also included a “fake” predictor variable, which was contrived from random cross-sectional percent ranks. The regression correctly ignored the fake variable, which was economically and statistically insignificant in regression outputs, and the actual results from Exhibit 2 did not incur any material change.