Macroeconomic Risk and Momentum Profits*

Susan Xiuling Ji  
*College of Business and Public Administration  
*Governors State University  
*University Park, IL 60484, U.S.A.

Spencer Martin  
*Department of Finance  
*The University of Melbourne  
*VIC, 3010, AUSTRALIA

Chelsea Yaqiong Yao  
*Department of Finance  
*The University of Melbourne  
*VIC, 3010, AUSTRALIA

This version: September 2012

Abstract

Macroeconomic risk continues to be proposed as a source of stock price momentum. For instance, Liu and Zhang (2008) claim that the growth rate of industrial production “plays an important role in driving momentum profits”. This paper shows that the growth rate of industrial production is not the source of momentum profits. Because recent winners and recent losers have nearly identical loadings on the growth rate of industrial production outside of January, there is essentially a net zero factor loading in the 11 months a year when momentum does exist—and a difference only in January, when losers massively outperform winners. We also document the fact that the growth rate of industrial production is not a priced risk factor outside of January. Moreover, application of the same methodology to all factors reveals no evidence that an explanation for momentum profits lies in macroeconomic risk.

* We appreciate comments from Bruce Grundy, Neal Galpin and seminar participants at New York University. Chelsea Yaqiong Yao wrote parts of the paper while visiting NYU, and so she would like to thank the people there for their hospitality.
PREFACE

THESIS COMMITTEE

Professor Bruce Grundy
Professor Spencer Martin

RESEARCH SUMMARY

Chief among my main research interests are currently in investments, empirical asset pricing and behavioral finance. Focusing on the area of momentum investing, my PhD thesis investigates the source of momentum. My paper “Momentum, Contrarian and the January Seasonality” highlights the fact that long-term contrarian is driven entirely by the January effect, rather than by (as argued by De Bondt and Thaler (JF, 1985)) investors’ overreaction. Further, the paper overturns Novy-Marx’s (JFE, 2012) findings by showing that short- and intermediate-term prior returns contribute equally to momentum once January influences are controlled for. Susan Xiuqing Ji, Spencer Martin and I have continued the agenda on testing the driving force of momentum, with the working paper “Macroeconomic Risk and Momentum Profits”. The paper shows that macroeconomic risk is not the source of momentum outside of January when momentum is present, which is in stark contrast with Liu and Zhang (RFS, 2008). I am currently pursuing this research further with recent work in progress, “Price Momentum and Earnings Momentum”, in which Stephen Brown and I investigate the impacts of earnings announcements on momentum.
1. Introduction

A momentum trading strategy, which buys recent winners and takes a short position in recent losers, stands to generate considerable profits (Jegadeesh and Titman, 1993). This cross-sectional predictability of recent past returns has prevailed geographically and temporally. Rouwenhorst (1998), Griffin, Ji and Martin (2005) and Asness, Moskowitz and Pedersen (2012) document the popularity of the momentum phenomenon in the US, the UK, many European and some Asian equity markets. The success of momentum trading strategies largely eliminates the possibility that momentum is due to data mining. An extensive body of recent literature has attempted to address potential explanations for this return continuation phenomenon.

As a way to understand the momentum effect, behavioral patterns have been put forward. Barberis, Shleifer and Vishny (1998), and Daniel, Hirshleifer and Subrahmanyam (1998) build behavioral models to provide a unified account of short- and intermediate-term momentum, and long-term reversal. Grinblatt and Moskowitz (2004) and Yao (2012) argue that those two phenomena evolve independently—since long-term reversal exists only in January and momentum appears outside of January. Hong, Lim and Stein (2000) find that momentum is more pronounced for small firms than large firms, as predicted by Hong and Stein’s (1999) gradual-information-diffusion model. Standing in marked contrast, Israel and Moskowitz (2012) suggest that momentum profits exhibit no reliable relation with size, and attribute Hong, Lim and Stein’s results to sample specificity.

Another main strand of literature focuses on risk-based explanations—attempting to identify the kinds of risk for which momentum profits seek to compensate. Neither the capital asset pricing model nor the Fama-French three factors model can account for momentum profits (Jegadeesh and Titman, 1993; Fama and French, 1996; Grundy and Martin, 2001). Macroeconomic risk, which affects firm investment cycles and growth rates, continues to be proposed as the source of momentum profits. Chordia and Shivakumar (2002) argue that the conditional macroeconomic risk-factor model including a set of lagged macroeconomic variables can capture momentum phenomenon. And momentum profits disappear once stock returns are adjusted for their predictability based on these macroeconomic variables. In sharp contrast, Griffin, Ji and Martin (2003, GJM) document that neither the unconditional nor the conditional macroeconomic risk-factor model can explain momentum profits. More importantly, they show that Chen, Roll and Ross’s (1986, CRR) five-factor model cannot subsume momentum. Liu and
Zhang (2008), however, claim that macroeconomic risk factors play an important role in driving momentum profits, asserting that the growth rate of industrial production (MP) can account for more than half of momentum profits. They argue that it is due to the fact that winners have higher return sensivities to MP than losers do.

The empirical test that seems to be the most natural way to investigate the source of momentum profits involves looking into the 11 months of a year when momentum does exist. Surprisingly, only a few previous studies have pursued this path. The literature notes that the use of momentum strategies results consistently in monetary losses in January (Jegadeesh and Titman, 1993; Grundy and Martin, 2001). Grundy and Martin demonstrate that the massive January loss is due to betting against the classic January size effect through the short sell of losers—which tend to be extremely small firms. These findings suggest that return continuation exists only outside of January. Our analysis of NYSE, AMEX and Nasdaq stocks shows that, from March 1947 to November 2009, momentum experiences a significant loss of 5.69% per month in January with an associated $t$-statistics of 4.59.¹ All of the evidence confirms the substantial January contamination on return continuation phenomenon. Because our economic question concerns the main driving force of momentum profits, it is essential to concentrate on the 11 months of a year (i.e., February to December) when the momentum phenomenon is indeed present.

This article explores whether macroeconomic risk is the underlying risk for momentum profits outside of January. Most relevant to our study, Liu and Zhang (2008, LZ) claim that the MP loadings for momentum ten deciles rise from the loser portfolio to the winner portfolio, which can be attributed to the momentum effect. We use an empirical framework similar to LZ except that we estimate momentum portfolio’s sensitivity to MP outside of January, when winners do outperform losers. In calendar-based time-series univariate regressions, we find that outside of January the MP loadings for momentum ten deciles tend to be U-shaped: the MP loadings for the winner and loser portfolios are 0.59 and 0.62, respectively, in the sample of March 1947 to November 2009. In pooled time-series univariate regressions, the MP loadings for winners and losers in the first month of a holding period are 0.83 and 0.74, respectively. In stark contrast with LZ’s findings, the results from both methods point to the fact that winners and

¹ Section 2 describes in greater detail the construction of momentum strategies. In addition, our analysis of NYSE and AMEX stocks reports a heavy January loss (4.68% per month with an associated $t$-statistics of 4.63) for the same sample period.
losers have nearly identical return sensitivities to MP outside of January, while prior winners outperform prior losers. As a result, winner–loser portfolios have essentially a net zero MP loading outside of January, and show a difference only in January when winners underperform losers substantially.

Another important avenue to explore the relation between macroeconomic risk and the momentum phenomenon is to investigate whether MP explains the cross-sectional variations in non-January stock returns. To that end, we use Fama–MacBeth (1973) cross-sectional regressions for four different factor-model specifications: the one-factor MP model, the Fama–French (1996) three-factor model augmented by MP, the CRR model without default premium and the full-fledged CRR model. We use thirty test portfolios based on one-way sorts on size, book-to-market and momentum. Depending on empirical specifications, the MP risk-premium estimates range from −0.34% per month to 0.17% per month, all of which with one exception are insignificant outside of January. The results indicate that there may not be a significant cross-sectional relation between non-January stock returns and the MP. This finding is not limited to a specific set of test portfolios. For robustness, we expand our analysis to include industry-sorted portfolios. Such extension builds on the growing literature documenting that industry is considered as one of the most important components of cross-sectional returns and serves as a main characteristic in benchmark portfolios (King, 1966; Wermers, 2004; Hou and Robinson, 2006). Previous studies also suggest that different industries bear different cyclical tendencies with macroeconomic conditions, and that industry returns are directly related to economic fundamentals (Boudoukh, Richardson and Whitelaw, 1994). Adding industry portfolios to the test portfolios, we find that the MP risk-premium estimates vary from -0.29% per month to 0.53% per month, and are mostly insignificant outside of January. This further confirms that the growth rate of industrial production cannot account for cross-sectional dispersions in non-January stock returns, which challenges the claims of LZ.

Our study thus extends earlier research on the role of macroeconomic risk on momentum profit by showing two things: (a) almost identical MP loadings for the winner and loser portfolios, (b) no positive and significant link between MP and non-January returns. More importantly, we provide direct evidence of momentum not being a reward for priced macroeconomic risk. Depending on empirical specifications, the CRR five-factor model predicts that expected momentum return are 33% to 68% of the observed momentum return year round,
with all of the differences between the observed and expected returns being significant. Similarly, outside of January, the complete CRR five-factor model suggests that expected momentum returns are 10% to 73% of the observed returns—with the differences between the returns all being significant. The findings provide no evidence that the CRR five-factor model can capture momentum. What follows is our attempt to discuss the role of MP on the momentum effect. The incremental contributions of MP are predicted to be 36% to 72% of the observed momentum return all year round. Concentrating on non-January months, when momentum is present, leads to MP predicting 0% to 7% of the observed momentum return. This indicates that momentum profits cannot be attributed to macroeconomic risk—the MP factor, in particular.

The innovation of adding industry-sorted portfolios to the test portfolios allows for new empirical findings. Interestingly, the incremental contribution of MP generates 6% to 52% of the observed momentum returns all year round, and the differences between the observed and expected momentum returns are mostly significant. Since little evidence shows that industry-sorted portfolios are affected by the classic January size effect embedded in momentum deciles, the change of adding industry-sorted portfolios may cause a dilution effect with regard to the January influence. After further alleviating the January influence of winners underperforming losers considerably, we document that the incremental contribution of MP outside of January decreases dramatically, producing at most 7% of the observed non-January momentum returns. In addition, our analysis demonstrates that the complete CRR five-factor model is unable to account for momentum profit. All of the evidence points to the fact that macroeconomic risk—the growth rate of industrial production, in particular—cannot be the source of momentum profits.

The remainder of this article proceeds as follows. Section 2 describes data and momentum portfolio formation, and also analyzes the seasonal patterns of momentum trading strategies. Section 3 presents the evidence that winners and losers have nearly identical MP loadings outside of January, when momentum does exist. Section 4 shows that the growth rate of industrial production is not a priced risk factor outside of January. It further demonstrates that neither the complete macroeconomic risk-factor models nor the MP risk factor itself can capture momentum profits. Section 5 concludes the paper.
2. Data and Momentum Profits

Our sample is constructed from all stocks traded on the New York Stock Exchange (NYSE), the American Stock Exchange (AMEX) and Nasdaq on the monthly files of the Center for Research in Security Prices (CRSP). Closed-end funds, real estate investment trusts, American depository receipts, and foreign stocks are excluded. We obtain data on stock returns, prices and shares outstanding from the CRSP monthly file. The sample period runs from March 1947 to November 2009 to match the data availability of macroeconomic variables we use in this study.

For macroeconomic variables, the CRR five factors—unexpected inflation (UI), change in expected inflation (DEI), term spread (UTS), default spread (UPR) and changes in industrial production (MP)—are constructed by using monthly data from various sources. Unexpected inflation is defined as \( UI_t = I_t - E[I_t|t-1] \) and change of expected inflation as \( DEI_t = E[I_{t+1}|t] - E[I_t|t-1] \). The inflation rate is designated as \( I_t = \log CPIA_t - \log CPIA_{t-1} \), where \( CPIA_t \) is the seasonally adjusted consumer price index (CUSR0000SA0 series) from Bureau of Labor Statistics. The expected inflation is \( E[I_t|t-1] = r_{ft} - E[RHO_t|t-1] \), where \( r_{ft} \) is the one-month Treasury bill rate from the CRSP monthly file. In line with Fama and Gibbons (1984), we measure the ex ante real rate, \( E[RHO_t|t-1] \). The difference between \( RHO_t \) and \( RHO_{t-1} \) is modeled as \( RHO_t - RHO_{t-1} = u_t + \theta u_{t-1} \). Accordingly it arrives at \( E[RHO_t|t-1] = (r_{ft-1} - I_{t-1}) - \hat{u}_t - \hat{\theta} u_{t-1} \). Term spread (UTS) is defined as the yield difference between 20- and 1-year Treasury bonds, and default spread (UPR) is the yield difference between BAA- and AAA-rated corporate bonds, with data being obtained from the FRED database at Federal Reserve Bank of St. Louis. The growth rate of industrial production for month \( t \) is defined as \( MP_t = \log IP_t - \log IP_{t-1} \), where \( IP_t \) is the industry production index (INDPRO series) in month \( t \) from the FRED database at Federal Reserve Bank of St. Louis. Note that MP is led by one month to match the timing with financial variables since INDPRO series is recorded as of the beginning of a month whereas stock returns are recorded as of the end of a month.

Fama-French three factors from the French data library are: (1) the market factor (MKT), the excess return on market portfolio; (2) the size factor (SMB), the difference in returns between a portfolio of small stocks and a portfolio of large stocks; (3) the value factor (HML), the difference in returns between a portfolio of high-book-to-market stocks and a portfolio of low-book-to-market stocks. Size, and book-to-market portfolios are decile one-sorted portfolios formed on size (market capitalization), value (book-to-market equity), while industry portfolios
are formed accordingly to various industry definitions. All these portfolios are obtained from the French data library.

Following Jegadeesh and Titman (1993) and LZ, we construct momentum portfolios as follows. All of the stocks in the sample are ranked on the basis of cumulative returns in month $t - 7$ to $t - 2$, and accordingly are assigned into ten deciles. Stocks with the highest returns in the preceding two-to-seven months are defined as winners (P10), whereas stocks with the lowest returns during the same period are defined as losers (P1). The momentum strategy buys prior winners (P10) and sells prior losers (P1). Zero-investment winner–loser portfolios (P10–P1) are rebalanced each month, and held for six months from month $t + 1$ to $t + 6$. There is a one-month gap between portfolio formation and portfolio investing, in order to circumvent the mechanical bid–ask bias. The original study on momentum by Jegadeesh and Titman (1993) examines momentum trading strategies by analyzing NYSE and AMEX stocks; they exclude Nasdaq stocks in order to avoid the results being driven by small and illiquid stocks or the mechanical bid–ask bias. Nevertheless, both Jegadeesh and Titman (2001) and Liu and Zhang (2008) add NASDAQ stocks to the sample to construct momentum portfolios. Although they argue that the addition of Nasdaq stocks has very little impact on the profitability of momentum strategies, it may increase the January losses noticeably, which we will discuss in detail shortly. To demonstrate the robustness of our findings, we present the analysis of NYSE and AMEX stocks along with that of NYSE, AMEX and Nasdaq stocks. Furthermore, to avoid our results being driven by small or even tiny stocks of extreme size deciles, we investigate both equal- and value-weighted portfolio returns.

Table 1 shows average monthly returns on sets of ten equal- and value-weighted portfolios (i.e., P1, P2,…P10) and winner–loser portfolios (i.e., P10–P1) formed monthly on the basis of the past two-to-seven months’ returns. The first, fourth, seventh and tenth columns of Table 1 present the overall performance of momentum strategies that are implemented all year round. Panel A shows that, for equal weighting, the top decile of NYSE and AMEX stocks with the highest past returns outperforms the bottom decile of NYSE and AMEX stocks with the lowest past returns by 0.81% per month ($t$-statistic=4.42) in the period of March 1947–November 2009. Similarly, the best performers of NYSE, AMEX and Nasdaq stocks outperform the worst performers by 0.67% per month ($t$-statistic=3.44) in the same period. Panel B reports that, for value weighting, prior winners among NYSE and AMEX stocks have higher returns than prior
losers by 1.02% per month (\(t\)-statistic=5.26)—and the winners among NYSE, AMEX and Nasdaq stocks earn higher returns than the corresponding losers by 1.20% per month (\(t\)-statistic=5.51). Our analysis of NYSE and AMEX stocks suggests that the equal- and value-weighted monthly returns of winner–loser portfolios are similar, but our tests of NYSE, AMEX and Nasdaq stocks show that the value-weighted return is substantially larger than the equal-weighted one. This result is to some extent consistent with the latest finding of Israel and Moskowitz (2012), which documents no reliable relation between momentum profits and size. They suggest that the findings of Hong, Lim and Stein (2000) and Grinblatt and Moskowitz (2004)—that momentum is stronger among small-cap stocks—are sample-specific. To give a fair comparison of equal- and value-weighted momentum returns, the next section of this paper performs a decomposition analysis in order to eliminate the well-documented January influence on momentum profits.

Neither the Capital Asset Pricing Model (CAPM) nor the Fama–French (1993) three-factor model can capture the hedged momentum portfolio returns obtained from long–short positions in the extreme deciles. The CAPM alphas for NYSE–AMEX momentum return are similar with raw returns: 1.10% per month (\(t\)-statistic=4.73) for equal weighting and 1.08% per month (\(t\)-statistic=5.53) for value weighting in March 1947 to November 2009. Consistent with Grundy and Martin (2001), controlling for the three factors of Fama–French deepens the momentum puzzle—since the Fama–French three-factor alphas are higher than the raw returns (being 1.26% per month (\(t\)-statistic=5.32) for equal weighting and 1.23% per month (\(t\)-statistic=6.26) for value weighting).

The second, fifth, eighth and eleventh columns of Table 1 document that the January momentum strategy, which buys winners of the previous June–November and sells losers of the same period, leads to considerable losses in Januaries. In Panel A, with respect to equal weighting, prior winners among NYSE and AMEX stocks underperform prior losers massively (by 4.68% in Januaries); past winners among NYSE, AMEX and Nasdaq stocks underperform past losers substantially (by 5.69% in Januaries). The findings are consistent with Jegadeesh and Titman (1993), Grundy and Martin (2001) and Asness, Moskowitz and Pedersen (2012), who demonstrate the substantial January losses associated with momentum trading strategies. In particular, Grundy and Martin (2001) state that it is due to betting against the classic January size effect, through buying small firms and selling extremely small firms. With respect to value
weighting, as shown in Panel B, the best performers among NYSE and AMEX stocks create lower returns than their counterparts, the worst performers, by 1.83% per month; those winners of NYSE, AMEX and Nasdaq stocks yield moderately lower returns than their respective ‘loser’ counterparts by 2.30% per month in Januaries. Comparison with the equal-weighted results in Panel A reveals that the value-weighted January losses are considerably smaller—less than half—than those borne by their equal-weighted counterparts. Consequently, the value-weighted momentum strategy is noticeably less susceptible to the classic January size effect than the equal-weighted momentum strategy.

The CAPM and particularly the Fama–French (1993) three-factor model play a role in explaining why momentum trading strategies suffer such considerable losses in January. Controlling for the market factor results in the CAPM alpha for NYSE-AMEX momentum return being slightly smaller than the raw losses, at 4.42% per month ($t$-statistic=4.30) for equal weighting and 1.64% per month ($t$-statistic=1.96) for value weighting. Controlling for the Fama–French three factors leads to the Fama-French alphas for NYSE–AMEX momentum return being remarkably smaller than the raw losses for equal weighting (2.76% per month, with an associated $t$-statistic of 2.60). More importantly, the value-weighted raw returns of the NYSE–AMEX momentum portfolio in January (−1.83% per month, with an associated $t$-statistic of −2.23) turn out to be positive and insignificant (0.45% per month, with an associated $t$-statistic of 0.05). Our results provide direct support for Grundy and Martin’s (2001) findings that the January momentum losses are due largely to the bet against the size effect.

The third, sixth, ninth and twelfth columns of Table 1 suggest that the momentum strategies generate high returns outside of January relative to all year round—a finding that is particularly pronounced for equal weighting. Panel A shows that the NYSE–AMEX momentum strategy produces an equal-weighted monthly return of 1.30% outside of January. Similarly, the NYSE–AMEX–Nasdaq momentum strategy creates an equal-weighted monthly return of 1.24% outside of January, which is approximately twice as large as the overall momentum return of 0.67%. Note that the findings reflect the dramatic influence of the January losses on the overall returns, which can affect momentum returns materially. In Panel B, with respect to value weighting, outside of January, the NYSE–AMEX momentum return of 1.27% per month is marginally larger than that obtained by the overall counterpart of 1.02% per month; similarly, the NYSE–AMEX–Nasdaq momentum return of 1.51% per month is slightly larger than the return obtained
by the overall counterpart of 1.24% per month. The results show that, for value weighting, the January losses have a very marginal effect on the overall momentum return. The comparison of equal- and value-weighted momentum returns has important implications for testing the source of momentum profits, which will be discussed in detail in the following sections.

Like the findings based on the overall returns, neither the CAPM nor the Fama–French (1993) three-factor model can capture momentum returns. The CAPM alphas for the non-January momentum returns are almost equal (in magnitude) to the raw returns, regardless of equal and value weighting. Controlling for the size and value factors in addition to the market factor leads to the increases in the Fama–French alphas. For instance, the Fama–French alphas for NYSE–AMEX momentum returns are 1.42% and 1.35% per month for equal- and value weighting, respectively—which are both slightly larger than the raw returns. In line with Grundy and Martin (2001), the momentum puzzle seems—to some extent—to be reinforced after controlling for Fama-French three factors.

3. Momentum Loadings on Macroeconomic Risk

Many studies have documented the fact that momentum strategies are profitable outside of January, whereas they suffer substantial losses in January (Jegadeesh and Titman, 1993; Grundy and Martin, 2001). If the momentum phenomenon is really driven by winners having higher MP sensitivities than losers (as argued by LZ), then this prediction should be particularly true outside of January, when momentum profits are really present. If not, then it suggests that LZ’s findings are due entirely to the January influence. This paper utilizes four different factor-model specifications: the one-factor MP model (MP), the FF three-factor model augmented by MP (FF+MP), the CRR model without default premium (CRR4) and the full-fledged CRR model (CRR5). This section provides direct evidence that winners and losers have almost identical loadings on MP in the 11 months of a year when momentum does exist. As a result, there is essentially a net zero MP loading outside of January—and a difference only in January, when losers substantially outperform winners. Section 3.1 shows that MP loadings of winners and losers exhibit unnoticeable differences outside of January. Section 3.2 provides further confirmatory evidence, and it also suggests no converging tendencies of MP loadings for winners and losers one year after portfolio formation. In a marked contrast with LZ, both of the two pieces of evidence demonstrate that winners have similar MP loadings with losers outside of
January, while winners outperform losers considerably. In addition, the findings reflect the fact that LZ’s results are driven entirely by the behavior of the January returns.

3.1. MP Loadings for Momentum Portfolios

Figure 1 presents MP loadings for equal-weighted ten decile momentum portfolios. Table 1 of LZ asserts that using all-month observations, the MP loadings for momentum ten deciles rise gradually from L(Losers), P2, ..., P9 to W(Winners). Panel A presents the MP loadings for momentum portfolios in 1960–2004. The univariate regression produces the wide MP-loading spread between winners and losers (0.08 and 0.52, respectively). Consistent with LZ, for the one-factor model, there is negligible difference in MP loadings from the loser portfolio up to decile six, but from that point the MP loadings rise monotonically from 0.06 to 0.52 for the winner portfolio. Similarly, the wide MP-loading spread between winners and losers remains present if we include the other four macroeconomic factors from the CRR5 model (e.g., UI, DEI, UPR and UTS). Moreover, the loser portfolio has the MP loading of −0.06, and the winner portfolio has the MP loading of 0.47.

Panel B displays the MP loadings for the equal-weighted ten decile momentum portfolios from time-series regressions using non-January observations in 1960–2004. The MP loadings are U-shaped for the ten decile momentum portfolios outside of January. The U-shape implies that extreme deciles load relatively heavily on MP, while the middle deciles load relatively weakly on MP. More importantly, in a sharp contrast with Panel A, we show that the broad spread of MP loadings between winners and losers virtually disappears outside of January, when winners outperform losers. It can be inferred from Panel B that winners and losers have almost identical MP loadings outside of January. In univariate time-series regressions, the MP loading of the loser portfolio is 0.86, which is slightly higher than that of the winner portfolio, 0.75. If we control for the other four macroeconomic variables from the CRR5 model, we see that the U-shaped pattern in MP loadings remains persistent—the loser portfolio with the MP loading of 0.75 and the winner portfolio with the MP loading of 0.71. Comparison of Panel B with Panel A reveals that the MP loading of the loser portfolio experiences dramatic increases, whereas the MP loading for winners does not change noticeably.

Panel C reports the MP loadings for the equal-weighted ten decile momentum portfolios for our entire sample period of March 1947 to November 2009, which covers roughly an additional
eighteen years’ worth of observations. For the one-factor model, the slopes of the MP loadings for the ten momentum deciles become slightly flattened. The MP loading of the loser portfolio is 0.32, and that of the winner portfolio is 0.61, and the monotonically increasing pattern seems to be not as strong as in Panel A. Controlling for the Fama–French three factors or the other four CRR factors does not materially affect these asymmetric patterns. For instance, the CRR5 model yields an MP loading for the loser portfolio (0.37) that is slightly lower than the corresponding loading for the winner portfolio (0.56). The MP loading of the loser portfolio up to decile six varies little, ranging from 0.37 to 0.34. Starting with the sixth decile, we observe portfolio return sensitivities to MP rising slowly from 0.34 up to 0.56 for the winner portfolio—but this increasing pattern is weaker than that obtained for LZ’s 1960–2004 sample period.

Panel D documents the MP loadings of the equal-weighted ten decile momentum portfolios outside of January in our whole sample of March 1947 to November 2009. Consistent with Panel B, the MP loadings of momentum portfolios are U-shaped—with winners and losers having nearly identical MP loadings. Outside of January, the one-factor MP model generates almost equal MP loadings for the loser and winner portfolios, 0.59 and 0.62, respectively. Similarly, controlling for the other three macroeconomic factors from the CRR4 model produces the negligible difference in MP loadings between winners (0.68) and losers (0.70). Controlling for the other four macroeconomic factors from the CRR5 model generates similar results, although winners have slightly higher MP loadings than losers. All of the evidence suggests that extreme decile momentum portfolios have almost identical MP loadings outside of January. It highlights the fact that there is essentially a net zero MP factor-loading in the 11 months a year when momentum does exist—and a difference only in January, when losers massively outperform winners. In contrast, LZ point to the asymmetric pattern in loadings: high MP loadings for the winner portfolio, and low MP loadings for the loser portfolio. Their results are driven by their failure to recognize the fact that momentum trading strategies are profitable outside of January but suffer massive losses in January.

Figure 2 depicts MP loadings for the value-weighted ten decile momentum portfolios. As Section 2 shows, equal- and value-weighted momentum portfolios exhibit very distinct features in terms of the overall and non-January profitability. Specifically speaking, equal weighting produces non-January momentum returns that are noticeably higher than the overall ones, whereas value weighting does not. Panel A presents the MP loadings for value-weighted
momentum portfolios in 1960–2004. Comparing these with the results of their equal-weighted counterparts, we find evidence that MP loadings for ten decile momentum portfolios—particularly for the winner portfolio—decrease dramatically, which indicates that the relation between MP and the winner portfolio tends to be relatively weak. Nonetheless, wide MP-loading variations between the loser and winner portfolios are still present. For example, the one-factor MP model yields MP loadings for winners and losers of $-0.16$ and $0.29$, respectively. Controlling for the other four CRR factors does not decrease the MP-loading gap between winners and losers: the loser portfolio has the MP loading of $-0.11$ and the winner portfolio has the MP loading of $0.28$. Moreover, consistent with the equal-weighted findings, there is little dispersion in MP loadings from the loser portfolio until decile six—but after that, the MP loadings rise monotonically until the winner portfolio.

In stark contrast with an increasing trend of MP loadings in Panel A, Panel B depicts the U-shaped pattern of MP loadings for equal-weighted ten decile momentum portfolios outside of January. The important implication of the U-shaped results is that extreme decile momentum portfolios returns have nearly identical sensitivities to MP. In univariate time-series regression, outside of January, momentum portfolio return sensitivities to MP decrease monotonically from the loser portfolio (0.40) to decile five/six (0.02), followed by a gradual increase until the winner portfolio (0.42). Controlling for the other four macroeconomic risk factors does not alter this U-shaped pattern outside of January—and also the MP loading of the loser portfolio (0.44) remains almost equal with that of the winner portfolio (0.41). Apart from equal-weighted results, the value-weighted findings provide further confirmatory evidence that winners and losers do not have distinguishable MP loadings outside of January, when winners outperform losers—and thus casts some doubt that MP is the main driving force for the momentum phenomenon.

Panel C presents the MP loadings for ten sets of value-weighted momentum portfolios in the entire sample period of March 1947 to November 2009. In comparison with the results of its equal-weighted counterpart in Panel C of Figure 1, the MP loadings for the ten decile momentum portfolios are relatively small. It implies that value-weighted momentum portfolios bear only a weak link with MP, relative to equal-weighted momentum portfolios. Nevertheless, similar with equal-weighted findings, the slopes of the MP loadings from the one-factor model basically flatten out from the loser portfolio (0.14) until the turning point of decile six (0.13); they then
increase steadily to the winner portfolio (0.47). Controlling for other macroeconomic risk factors creates very similar patterns.

Panel D shows momentum portfolio return sensitivities to MP outside of January, when the momentum phenomenon is present, in the whole sample period from March 1947 to November 2009. Besides the FF three-factor model augmented by MP, all of the other three macroeconomic models produce the U-shape of the MP loadings for the value-weighted ten decile momentum portfolios. In univariate time-series regressions, there is first a decrease in MP loadings from the loser portfolio (0.39) to decile five (0.20), and then an increase from decile six (0.22) to the winner portfolio (0.50). Controlling for the other four CRR macroeconomic variables further enhances the effect, so that winners and losers have basically identical MP loadings outside of January (0.49 and 0.47, respectively). All of the evidence strongly suggests that there is essentially a net zero factor loading in the 11 months a year when momentum does exist, and a difference only in January, when losers massively outperform winners. In contrast, LZ point to the asymmetric pattern in loadings: with high MP loadings for the winner portfolio and low MP loadings for the loser. Their results are driven by their failure to recognize the fact that momentum trading strategies are profitable outside of January but suffer massive losses in January.

3.2. Time-series Evolution of MP Loadings

As an extension of the findings in Section 3.1, this section examines the evolution of MP loadings for winners and losers after portfolio formation due to the following three considerations. First, since ten decile momentum portfolios have a six-month holding period, the MP loadings estimated from calendar-based time-series regressions are in fact averaged over the six months. It is worth examining the evolution of MP loadings month by month after portfolio formation using pooled time-series regressions. Second, the evolution of MP loadings for winners and losers makes it possible to examine whether the wide spread in MP loadings for winners and losers is temporary. In other words, we can investigate whether the wide gap converges gradually after portfolio formation as momentum profits dissipate gradually. Third, if we extend the event window to two years after portfolio formation, the evolution allows of examining whether there is a reversal in MP loadings beyond one year to two years, given the well-documented return reversal (e.g., De Bondt and Thaler, 1986). Consistent with our findings
in Section 3.1, the results confirm that the MP-loading dispersion between winners and losers is economically unimportant outside of January, when momentum does exist—and further, that there is no clear trend of convergence/reversal in the MP loadings between winners and losers.

We estimate the MP loadings from pooled time-series factor regressions (Ball and Kothari, 1989). Our event months \( t + m \) (where \( m=0, 1, \ldots, 24 \)) commence from the month right after portfolio formation to the twenty-fourth month. For each event month, we pool together across calendar month the observations of returns to winners and losers, the FF three factors, and the CRR five factors for each of the event months, \( t + m \). We perform pooled time-series factor regressions to estimate MP factor loadings for winners and losers.

Figure 2 displays the MP loadings of the winner and loser portfolios estimated from pooled time-series factor regressions for each of the event months during the twenty-four-month event-window period after portfolio formation. Panel A reports the MP loadings of winners and losers in 1960–2004. In line with LZ, we find that all standard asset-pricing models produce the MP loadings for winners to be reliably higher than those for losers in the first few months after portfolio formation. The CRR5 model indicates the spread in the MP loadings between winners and losers to be 0.98 in the first month after portfolio formation (i.e., month \( t \)) and 0.58 in the first holding-period month (i.e., month \( t + 1 \)). The wide spreads gradually converge around month seven after portfolio formation. De Bondt and Thaler (1986) document that recent past winners underperform recent past losers beyond the one-year holding period. We do not observe this reversal effect in MP loadings of the winner and loser portfolios.

Panel B presents the MP loadings of the winner and loser portfolios estimated from pooled time-series factor regressions outside of January, when the momentum phenomenon is present. Note that the event months \( t + m \) (where \( m=0, 1, \ldots, 22 \)) commence with the month immediately after portfolio formation up to the twenty-second month, in order to maintain a two-year window. The results are dramatic in that the basic pattern of Panel A changes completely. Comparison of Panel B with Panel A shows that using non-January observations instead of all-month observations reduces substantially the spreads in MP loadings between the winner and loser portfolios. Only the month right after portfolio formation reports winners having slightly higher MP loadings than losers. After that, winners have even lower MP loadings than losers, despite the magnitudes of the MP-loading dispersion between winners and losers being unnoticeably small. The findings provide another piece of direct evidence that the MP-loading variation
between winners and losers is economically unimportant outside of January, when winners outperform losers.

Panels C and D show the evolution of MP loadings in the entire sample of March 1947 to November 2009. Similar to Panel A, Panel C shows that wide spreads in MP loadings between the winner and loser portfolios occur all year round, converging eventually in the seventh month after portfolio formation. The one-factor MP model gives rise to the enormous dispersion in MP loadings between winners and losers: 0.86 in month $t$, 0.58 in month $t+1$ and 0.54 in month $t+2$. Controlling for the other three macroeconomic factors from the CRR4 model also produces broad spreads in MP loadings between winners and losers: 0.89 in month $t$, 0.60 in month $t+1$ and 0.44 in month $t+2$. From the seventh month onward, not only do the spreads between winners and losers reverse, but also the magnitudes of the spreads between extreme deciles become noticeably small.

Panel D documents that the wide gap in MP loadings between the winner and loser portfolios in Panel C has been narrowed down substantially. The one-factor MP model reveals the MP-loading variation between winners and losers to be 0.27 in month $t$, 0.09 in month $t+1$ and 0.05 in month $t+2$, which are only 30%, 15% and 10% of the corresponding MP-loading spreads in Panel C. Similarly, the CRR4 model reports the MP-loading dispersion to be 0.30 in month $t$, 0.11 in month $t+1$ and 0.08 in month $t+2$, which are only 33%, 18% and 18% of the corresponding MP-loading spreads in Panel C. Interestingly, the CRR5 model shows that the MP loadings of the winner portfolios are generally smaller than those of the loser portfolios (in the month subsequent to portfolio formation), although the spreads are inconsiderable. All of the evidence points to the fact that there is no asymmetric pattern of MP loadings between winners and losers; in fact, winners and losers have almost identical loadings outside of January, when momentum does exist.

4. Macroeconomic Risk and Momentum Profits

Thus far, we have shown that winner and loser portfolios have very similar MP loadings outside of January, despite the fact that winners outperform losers considerably. In this section, we address directly the central question of this study: are momentum profits a reward for priced macroeconomic risk (especially the growth rate of industrial production)? Section 4.1 estimates macroeconomic risk premium from two-stage Fama–MacBeth (1973, FM) cross-sectional
regressions. Because our economic question seeks to trace the source of momentum profits that exists only outside of January, we are most interested in examining whether MP can account for the cross-sectional variations in non-January stock returns. Intuitively, if MP plays an important role in driving momentum profits, then the MP risk-premium estimates are very likely to be economically and statistically significant outside of January. Several of our tests confirm the conjecture by showing that MP cannot capture the cross-sectional dispersions of non-January stock returns. Section 4.2 uses these risk-premium estimates to calculate expected momentum return implied by macroeconomic factor models. Our analysis finds evidence that the strong significance of expected momentum return due to the MP contribution is strong outside of January, relative to the observed momentum return. Our findings also document the fact that the expected momentum return implied by the complete macroeconomic factor models is significantly different from the observed momentum return. Thus we conclude that macroeconomic risk cannot be the source of momentum. Finally, Section 4.3 demonstrates the robustness of our conclusion.

4.1. Estimating Macroeconomic Risk Premium

In line with CRR, GJM and LZ, we estimate the macroeconomic risk premium by using the two-stage FM cross-sectional regressions. The first-stage time-series regression involves regressing the returns of the test portfolios on the FF three factors and/or CRR five factors in order to estimate factor loadings. We use the full sample, extended and rolling windows in the first-stage time-series regressions.\(^2\) Note that the extended window requires at least two-years of monthly observations to run the first-stage regressions.

\[
r_{pt} = \alpha_p + \beta_{UL,p}UL_t + \beta_{DELP,D}DEI_t + \beta_{UTS,p}UTS_t + \beta_{UPSP,p}UPR_t + \beta_{MP,p,MP} + \epsilon_{p,t} \tag{1}\]

The second-stage cross-sectional regression regresses the returns of the test portfolios excess of the risk-free rate on factor loadings obtained from the first-stage regressions in order to estimate risk premiums. Using the full-sample window, we regress portfolios’ excess returns in

\(^2\) Liu and Zhang (2008, Table 5 and 6) show that the results from both full-sample and extended-window regressions suggest that the MP premium is economically and statistically significant, and also that the growth rate of industrial production can account for momentum profits. Their results from rolling-window regressions, however, provide the opposite findings. They suggest that factor loadings are estimated more precisely from full-sample and extended-window regressions than from rolling regressions. Thus the focus of our discussions is on the result from the full-sample and extended-window regressions—unless it is mentioned in particular.
month \( t \) on factor loadings estimated from the first-stage time-series regression of the full sample. Using the extended window, we regress portfolios’ excess returns in month \( t \) on factor loadings estimated from the first-stage regression in month \( t - t_0 \) to \( t \) \(-x \) (where \( t_0 \) is the first observations and \( x \) ranges from the 24\(^{th}\) observations). Using the sixty-month rolling window, we regress portfolios’ excess returns in month \( t \) on factor loadings estimated from the first-stage regressions in month \( t - 60 \) to \( t - 1 \). The risk premiums—the time-series averages of the estimated slopes—will be used for calculating expected momentum return in order to test its significance relative to the observed momentum return, which will be discussed in detail in Section 4.2.

\[
r_{p,t} - r_{f,t} = \alpha_{p,t} + \gamma_{UL, t} \hat{\beta}_{UL, p} + \gamma_{DEL, t} \hat{\beta}_{DEL, p} + \gamma_{UTS, t} \hat{\beta}_{UTS, p} + \gamma_{UPR, t} \hat{\beta}_{UPR, p} + \gamma_{MP, t} \hat{\beta}_{MP, p} + \epsilon_{p,t} \tag{2}
\]

In line with LZ, we use thirty test portfolios—ten size-, ten book-to-market- and ten momentum portfolios—in two-stage FM cross-sectional regressions. For robustness, our tests also add industry-sorted portfolios into the existing thirty test portfolios to be used for estimating risk premium and calculating expected momentum return, which will be discussed later. Moreover, because Section 2 shows that equal- and value-weighted momentum portfolios have different characteristics, we utilize both sets of ten momentum portfolios. Panel A of Table 2 reports the estimates of risk premiums using ten equal-weighted momentum-, ten size-, and ten book-to-market portfolios. Two sample periods (i.e., 03/1947–11/2009 and 01/1960–12/2004) generate results that are basically similar to those of LZ. Depending on empirical specifications, the MP premium estimates range from 0.76% per month to 1.08% per month—and all are significant. In comparison with the MP risk-premium estimates, the HML premium estimates are small (although mostly significant), varying from 0.17% per month to 0.30% per month. And the MKT premium estimates are all negative (although insignificant). In contrast to LZ, the entire sample period of 03/1947–11/2009 reports the UTS premium as \(-1.22% \) per month (with an associated \( t \)-statistic of \(-2.57 \)) and the UPR premium as 0.25% per month (with an associated \( t \)-statistic of 1.93). This finding implies that the UTS and UPR premium estimates change with the sample period and are not persistently strong.

---

3 In the literature, there is mixed practice for two weighting methods, equal and value weighting, respectively. Many momentum studies examine equal-weighted returns (e.g., Jegadeesh and Timan, 1993, 2001; Hong, Lim and Stein, 2000; Chordia and Shivakumar, 2002; Liu and Zhang, 2008). Notwithstanding, value weighting has gained ground recently (Fama and French, 2008; Heston and Sadka, 2008; Moskowitz and Israel, 2012). In particular, Bhattacharya and Neal (2011) confirm that the popularity of value weighting is strong, in particular for NYSE stocks.
Panel B of Table 2 reports risk-premium estimates using value-weighted momentum-, size- and book-to-market portfolios. Using value-weighted momentum portfolios instead of equal-weighted momentum portfolios leads to slight increases in the MP premium estimates, from 0.80% per month to 1.41% per month, depending on model specifications and sample periods. Interestingly, the MKT premium estimates are all economically and statistically significant (from −0.62% per month to −1.60% per month). The UTS premium estimates are −0.79% per month in the sample of 1960–2004 and −1.83% per month in the sample of 1947–2009. The results suggest that the market and UTS risk-premium factors appear to be able to capture the cross-sectional variations of value-weighted instead of equal-weighted momentum portfolio returns.

Table 3 presents the risk-premium estimates from two-stage FM cross-sectional regressions outside of January. With the objective to rationalize momentum profits, it is important to understand whether stock returns are cross-sectionally related to macroeconomic risk in those months when momentum does exist. Many previous studies have pointed out that the momentum strategy consistently loses money in January (Jegadeesh and Titman, 1993; Grundy and Martin, 2001; Asness, Moskowitz and Pedersen, 2012). Accordingly, we estimate risk premiums for the CRR and FF’s risk factors by using non-January observations. If momentum is a reward for the priced MP risk factor, then MP may play a role in accounting for the cross-sectional dispersions of non-January stock returns, and vice versa.

Panel A of Table 3 presents the risk-premium estimates using non-January observations of ten equal-weighted momentum-, ten size- and ten book-to-market portfolios. Use of non-January observations instead of all-month observations results in very different inferences about the quantitative role of the MP risk factor in cross-sectional returns. Depending on empirical specifications, the MP risk-premium estimates vary from −0.64% per month ($t$-statistic=−3.29) to 0.37% per month ($t$-statistic=1.65). Panel B replaces equal-weighted momentum portfolios with value-weighted momentum portfolios in the thirty test portfolios. It presents broadly similar findings. The MP risk-premium estimates range from −0.19% per month ($t$-statistic= −0.46) to 1.19% per month ($t$-statistic=1.58), which are all insignificant and even negative at times. All of the evidence demonstrates that the growth rate of industrial production is not a priced risk factor. Consequently, it refutes the claims of CRR and LZ, which are driven completely by the January influence. More importantly, Subsection 4.2 highlights the fact that the growth rate of industrial
production cannot explain momentum profits outside of January, which we will discuss in detail later.

In contrast with the dramatic changes of the MP risk-premium estimates, Table 3 shows that using non-January returns does not alter the significance of the UTS, UPR and MKT risk-premium estimates. Depending on empirical specifications and sample periods, the MKT premium estimates range from \(-1.89\%\) per month to \(-0.22\%\) per month, which are largely consistent with the estimates of their counterparts in Table 2. The UTS premium estimates range from \(-2.34\%\) per month to \(-1.15\%\) per month, which is largely the same, although being modestly larger than the corresponding estimates in Table 2. The UPR premium starts from \(0.16\%\) per month up to \(0.53\%\) per month, which is slightly stronger than the corresponding estimates in Table 2. Nonetheless, the MP risk factor is the only one yielding estimates that are economically and statistically altered outside of January.

For robustness, we also estimate macroeconomic risk premiums by utilizing forty test portfolios—ten size-, ten book-to-market-, ten momentum- and ten industry portfolios. This change of adding ten industry portfolios builds on the growing literature which points to the fact that industry is considered to be an important component of cross-sectional returns and is used as a main characteristic in benchmark portfolios (King, 1966; Wermers, 2004; Hou and Robinson, 2006). Considering the significance of the industry dimension, we estimate risk premiums for CRR and FF factors by adding ten industry portfolios to the existing thirty test portfolios. If the growth rate of industrial production is really a priced risk factor, then this change should not quantitatively affect the MP premium estimates. If the growth rate of industrial production is not a priced risk factor, then this change of research design might materially affect the MP premium estimates.

Table 4 shows that adding ten industry portfolios into the test portfolios does quantitatively weaken the MP risk-premium estimates—although a majority of cases still produce statistically significant MP risk-premium estimates. Depending on empirical specifications, the MP risk-premium estimates range from \(0.25\%\) per month to \(0.79\%\) per month, and are approximately half of the corresponding estimates in Table 2. Panel A presents the risk-premium estimates of the forty test portfolios—ten equal-weighted momentum-, ten size-, ten book-to-market- and ten industry portfolios. The one-factor MP model reports the MP risk-premium estimate to be \(0.49\%\) per month (\(t\)-statistic=2.31) in the sample of March 1943 to November 2009. It is less than half
of its corresponding estimates from the thirty test portfolios in Table 2, 1.09% per month (t-statistic=3.67). Similarly, the CRR5 model generates the MP risk-premium estimate of 0.42% per month (t-statistic=1.86), which is relatively small and insignificant, whereas its corresponding MP risk premium, estimated from the thirty test portfolios, is 0.76% per month (t-statistic=2.68). Despite the marked change of the MP risk-premium estimates, adding industry portfolios to the existing test portfolios does not alter other risk-premium estimates substantially. Panel B shows the risk-premium estimates from the forty test portfolios that replace equal- with value-weighted momentum portfolios. The MP risk-premium estimates from the forty test portfolios continue to be roughly half of the value of their counterparts from the thirty test portfolios. For instance, the one-factor MP model indicates that the MP risk premium estimated from the forty test portfolios is 0.50% per month (t-statistic=2.72), whereas its corresponding estimate of the thirty test portfolios is 1.24% per month (t-statistic=4.73). By contrast, adding ten industry portfolios affects marginally the UPS, UPR and MKT risk-premium estimates.

So far, we have shown that the addition of industry portfolios into the group of thirty test portfolios weakens markedly the MP risk-premium estimates, despite the fact that the MP risk-premium estimates continue to be significant. Our analysis now focuses on the 11 months of a year when momentum does exist, because we are most interested in investigating the cross-sectional relation between macroeconomic risk and expected returns outside of January. Table 5 reports the risk-premium estimates for the CRR’s risk factors using non-January observations of the forty test portfolios. Panel A estimates the risk premiums from the forty test portfolios, including equal-weighted momentum portfolios. It shows that, depending on model specifications and sample periods, the MP risk premium ranges from −0.29% per month (t-statistic=−2.48) to 0.53% per month (t-statistic=2.35). Note that apart from those two estimates, all other MP risk-premium estimates are economically and statistically insignificant. This finding is consistent with the estimates from using non-January observations of the thirty test portfolios in Table 3. It further confirms the fact that the growth rate of industrial production cannot capture the cross-section dispersions of non-January stock returns. All the estimates of other risk-factor premiums are very similar with the corresponding estimates from using non-January observations of the thirty test portfolios in Table 3 (with one exception, the MKT risk premium).

Panel B estimates the macroeconomic risk premiums from the forty test portfolios, including the value-weighted momentum portfolios. The results show that the MP risk-premium estimates
span from $-0.13\%$ per month ($t$-statistic$=-0.87$) to $0.44\%$ per month ($t$-statistic$=1.60$). Compared with Panel B of Table 4, the MP risk-premium estimates show losses in both economical and statistical significance. Consistent with our findings from the thirty test portfolios, our analysis highlights the fact that the growth rate of industrial production is not a priced risk factor outside of January. In sharp contrast with the dramatic changes of the MP risk-premium estimates, the UTS, UPR and MKT premium estimates remain significant in general—resembling those reported in Table 4. To sum up, all of the evidence in this section refutes LZ’s claim that the growth rate of industrial production is a priced risk factor in standard asset-pricing tests. A variety of our tests confirm that the growth rate of industrial production cannot capture cross-sectional differences of non-January stock returns.

4.2. Expected Momentum Profits

This section uses macroeconomic risk-premium estimates from Tables 2–5 to calculate expected momentum returns predicted by macroeconomic risk-factor models. We analyze the significance of expected momentum returns relative to observed momentum returns. If the complete macroeconomic factor models can capture momentum returns, then expected momentum returns implied from the models should be insignificant from the observed momentum returns, and vice versa. Similarly, we conjecture that if the growth rate of industrial production can account for momentum returns, then the incremental contribution of the MP risk factor should be insignificant, from the observed momentum returns. Our analysis provides no evidence that an explanation for momentum profits lies in macroeconomic risk.

In line with GJM and LZ, we estimate factor loadings of a momentum strategy on CRR five factors (i.e., UI, DEI, UTS, UPS and MP).

$$\text{WML}_t = \alpha + \beta_{UI} UI_t + \beta_{DEI} DEI_t + \beta_{UTS} UTS_t + \beta_{UPS} UPS_t + \beta_{MP} MP_t + \epsilon_t$$

(3)

Expected momentum returns, $E[\text{WML}]$, are estimated as the product of CRR’s factor loading of a momentum strategy (i.e., betas) from Equation (3) and risk-premium estimates from two-stage FM cross-sectional regressions (i.e., gammas) from Equation (2). The incremental contribution of MP, measured by $E[\beta_{MP} \gamma_{MP}]$, is estimated as the product of the MP factor loading ($\hat{\beta}_{MP}$) from Equation (3) and the MP risk-premium estimate ($\hat{\gamma}_{MP}$) from Equation (2).
Note that our discussions will concentrate on the results from the full samples and extended windows, unless otherwise mentioned.  

\[ E[WML] = \beta_{UI} y_{UI} + \beta_{DEI} y_{DEI} + \beta_{UTS} y_{UTS} + \beta_{UPR} y_{UPR} + \beta_{MP} y_{MP} \]  

\[ E[\beta_{MP} y_{MP}] = \beta_{MP} y_{MP} \]  

The left-hand block of Panel A of Table 6 largely replicates LZ (2008, Table 6, Panel B) in their 1960–2004 sample period. The thirty test portfolios—ten equal-weighted momentum-, ten size- and ten book-to-market portfolios—are used to estimate risk premiums. Consistent with LZ’s findings, both the CRR4 and CRR5 models mostly produce significant differences between the observed and expected momentum return, but the incremental contribution of MP, \( E[\beta_{MP} y_{MP}] \), differs little from the observed momentum return. For example, with the extended-window regressions, the CRR5 model produces expected momentum return, \( E[WML] \), to be 0.57% per month or 75% of the observed equal-weighted momentum return. Further it is significantly different from the observed equal-weighted momentum return (\( t \)-statistic=2.25). The incremental contribution of MP, \( E[\beta_{MP} y_{MP}] \), is 0.65% per month or 86% of the observed momentum return. The difference between them, however, is insignificant (\( t \)-statistic=0.45).  

Our analysis of momentum returns all year round appears to suggest that MP can account for momentum profits—but we need to exercise caution in offering any conclusive statements for the time being, since the absence of momentum in January has not been addressed.  

The right-hand block of Panel A reports the results of our entire sample period, 1947/03–2009/11. Recall that we showed, in Table 2, that the MP risk-premium estimates are mainly the same between the 1960/01–2004/12 and 1947/03–2009/11 samples. Similar to the findings for 1960/01–2004/12, the macroeconomic risk-factor models produce mostly expected WML returns, \( E[WML] \), that significant from the observed returns in 1947/03–2009/11. With the extended window, the CRR5 model generates the expected momentum return to be 0.43% per month, or 69% of the observed equal-weighted return, and the difference between the observed and expected returns is significant (\( t \)-statistic=2.25). In marked contrast with the finding for the sample of 1960–2004, the incremental contribution of MP, \( E[\beta_{MP} y_{MP}] \), can be no longer

---

4 LZ show that the MP risk factor cannot capture momentum profits accordingly to the results from the rolling-window regressions, whereas the full-sample and extended-window regressions provide just the opposite results. They suggest that the estimates from full-sample and extended-window regressions are more precise than those obtained from rolling regressions.  

5 Similar to LZ, with rolling-window regressions, the incremental contribution of MP can explain only about 15% of the observed momentum return.
insignificant, according to the observed returns. With the extended windows, the CRR5 model indicates that the MP incremental distribution is 0.03% per month or 5% of the observed equal-weighted return. And the remaining 95% is significant ($t$-statistic=3.10). These findings indicate that the least important source among the CRR five factors is the MP risk factor, since the complete CRR5 model explains 69% of momentum profits, whereas the MP risk factor captures only 5%. Nevertheless, with the full samples, both the one-factor MP model and the FF+MP model determine that the MP incremental contribution is not significantly different from the observed equal-weighted momentum return. For example, the one-factor MP model states that the expected momentum return is 0.31% per month (or 49% of the observed momentum return)—and more importantly, it significantly differs from the observed momentum profits.

Panel B of Table 6 replaces ten equal-weighted momentum portfolios with ten value-weighted momentum portfolios in the thirty test portfolios. Value-weighted momentum strategies have very different return behavior from equal-weighted momentum strategies, particularly in January, since value weighting can effectively alleviate the January size influence on momentum. Using value-weighted momentum portfolios as part of the test portfolios allows us to focus on momentum by distinguishing the January size effect to some degree. Like Panel A, all the standard asset-pricing models produce expected WML returns to be significantly different from the observed value-weighted return, which indicates that none of the models can capture momentum profits. Unlike Panel A, the incremental contributions of MP are significantly different from the observed value-weighted momentum returns, which suggests the MP risk factor cannot account for momentum profits for the most part. In direct contradiction to the corresponding results in Panel A, both the one-factor MP model and the FF+MP model determine that the incremental contribution of MP, $E[\beta_{MP}Y_{MP}]$, is significantly different from the observed momentum returns. With the full-sample regressions, the CRR5 model generates the incremental contribution of MP as 32% of the observed value-weighted momentum return in 1960–2004, with the remaining 68% being significant (with an associated $t$-statistic of 3.74). The question naturally arises as to why replacing equal-weighted momentum portfolios with value-weighted momentum portfolios can lead to such fundamental change. One possible explanation is that value-weighted momentum returns rather than equal-weighted momentum returns mitigate largely the impact of January when momentum doesn’t exist. Table 1 has shown that value-weighted momentum strategies suffer considerably smaller January losses (2.30% per month)
than equal-weighted momentum strategies (5.69% per month). The opposite inference from value-weighted results (in Panel B) relative to equal-weighted results (in Panel A) reflects the influence of January momentum returns to a certain degree, which we will discuss in detail soon.

Table 7 addresses directly the concerns about the January influences on the role of macroeconomic risk in capturing momentum profits. Our economic question concerns what drives momentum profits (which exist outside of January rather than in January), so it is only natural to exclude the January observations of all the test portfolios when calculating expected momentum profits. Table 7 shows that outside of January, all complete standard asset-pricing models and the MP risk factor itself produce expected WML returns that are significantly different from observed momentum returns. Panel A shows that outside of January, with the extended windows, the complete CRR5 model generates an expected WML return of 0.72% per month (or 51% of the observed equal-weighted WML return), whereas the incremental contribution of MP is 0.13% per month (or 9% of the observed equal-weighted WML return) in 1960–2004. Similarly, with the full samples, the complete CRR5 model produces 74% of the observed non-January WML profit, whereas the MP factor is virtually unable to generate any non-January WML profit. Moreover, the one-factor MP model mostly predicts the expected WML returns to be approximately 9% of the observed WML returns outside of January. These findings contrast sharply with the results from models based on the information of all year around in Panel A of Table 6, where the MP risk factor can account for considerably large proportions of momentum profits. Panel B presents largely similar findings. With the extended-window regressions, the CRR4 model predicts that the expected WML return is 26% of the observed value-weighted momentum return in 1947/03–2009/11, with the left 74% being significant ($t$-statistic of 7.73). None of the results provide any evidence that momentum profits are a reward for priced macroeconomic risk.

For robustness, using a new set of test assets (i.e., ten size-, ten book-to-market-, ten momentum- and ten industry portfolios), Table 8 reports expected momentum profits estimated on the basis of risk-premium estimates from using the forty test portfolios. Apart from industry portfolios, size-, book-to-market- and momentum portfolios are all affected by the January effect (Grundy and Martin, 2001; Keim, 2008). The addition of industry portfolios serves to dilute the impacts of the behavior of the classic January size effect—which is considered to be the main reason for winners underperforming losers substantially in January. We conjecture that using this
new set of test assets may lead to the significance of expected momentum returns relative to observed momentum returns being stronger in the forty test portfolio sample than was the case without the addition of industry portfolios. The findings in Table 8 confirm this conjecture.

Panel A presents the results from using the forty test portfolios that are comprised of ten equal-weighted momentum-, ten size-, ten book-to-market- and ten industry portfolios. With the full samples, the one-factor MP model reports expected WML return, $E[\beta_{MP}Y_{MP}]$, as 0.14% per month, or 22% of the observed equal-weighted WML return in 1947–2009, and the difference between the observed and expected momentum returns is significant ($t$-statistic=2.21). Panel B reports the findings for replacing equal-weighted momentum portfolios with value-weighted momentum portfolios. With the extended windows, the complete CRR5 model produces an expected momentum profit, $E[WML]$, of 0.44% per month or 37% of the observed value-weighted momentum profit in 1943–2009. And the remaining 63% is significant ($t$-statistic=5.07). The incremental contribution of MP, $E[\beta_{MP}Y_{MP}]$, is 0.13% per month or 11% of the observed value-weighted momentum profits, with the difference between the expected and observed returns being economically and statistically significant. Consistent with previous findings, none of the complete macroeconomic risk-factor models can account for momentum profits. More importantly, we find that the growth rate of industrial production contributes little to the momentum effect—particularly for the value-weighted momentum strategy.

Table 9 concentrates on the 11 months of a year when momentum does exist. Use of non-January observations enhances largely the findings in Table 8 that macroeconomic risk is not the main driving force of momentum profits. Depending on model specifications and sample periods, the incremental contribution of MP, $E[\beta_{MP}Y_{MP}]$, ranges from ‘none’ to the maximum of 7% of the observed WML return. The MP risk factor is generally the least important source of momentum profits in the CRR5 model. Panel A reports that, for example, with the full samples, the complete CRR5 model yields an expected WML return of 0.87% per month, or 67% of the observed equal-weighted WML return in 1947/03–2009/11. The difference between the expected and observed returns is significant at the 1% level ($t$-statistic=3.38). Note that the MP risk factor itself contributes nothing to non-January momentum returns. Panel B, for instance, shows that with the extended windows, the complete CRR5 model explains 19% of the observed return, whereas the incremental contribution of MP accounts for 2% of the observed value-weighted
return in 1947/03–2009/11. Comparing the results in Table 9 with the corresponding results in Table 8 highlights the fact that eliminating January (when losers outperform winners remarkably) does not have a material effect on the existing limited ability of the complete macroeconomic risk-factor models, $E[WML]/WML$, to produce momentum profits. In contrast, this change eliminates basically any existing ability of the MP risk factor to explain momentum profits in Table 8. All of these findings highlight yet again the fact that the growth rate of industrial production cannot account for momentum profits.

So far, we have shown that the momentum effect is not a manifestation of recent winners having temporarily higher loadings than recent losers on the growth rate of industrial production. Our conclusions rest on three pieces of evidence. Firstly, outside of January, there are no significant differences—between either the expected and observed momentum returns or the incremental contribution of MP and the observed momentum return. It is obvious that excluding January observations in a variety of our tests is the most direct way to allow for a focus on the 11 months of a year when momentum is indeed present. Secondly, the MP risk factor plays a negligible role in explaining value-weighted momentum profits relative to equal-weighted momentum profits. Using value-weighted momentum returns instead of equal-weighted returns alleviates moderately the contamination of January, because winners underperform losers to a lesser degree for value-weighted than for equal-weighted strategies in January. Thirdly, including industry portfolios in addition to size-, book-to-market- and momentum portfolios in the test portfolios also undermines LZ’s arguments. Adding industry portfolios has a diluting effect on the possible January influence on our test, due to the fact that all of the other test portfolios (instead of industry portfolios) are associated with the classic January size effect. These three pieces of evidence point to our analysis overturning LZ’s claim by demonstrating that the MP risk factor is not the source of momentum profit outside of January, when momentum does exist.

4.3.Robustness Check

This section performs several robustness checks to enrich the discussions about our main conclusions drawn from the results of the preceding section. Section 4.2 uses ten industry
portfolios—which are one set of industry-sorted portfolios provided on Kenneth French’s website—in addition to the other thirty test portfolios. The French website also provides seventeen industry portfolios, thirty industry portfolios, and so forth, which are formed by different industry specifications. To address the concerns about the robustness of our findings, we replicate the tests by including different sets of industry-sorted portfolios. Further, another potential concern has to do with the extent to which our results may be due to many small and illiquid stocks traded in Nasdaq. To avoid this problem, we use NYSE and AMEX stocks (excluding Nasdaq stocks) to construct ten decile momentum portfolios, which are utilized as test portfolios to replicate all tests performed in Subsection 4.2.

Panel A of Table 10 replaces ten industry-sorted portfolios with seventeen industry-sorted portfolios in the test portfolios. In other words, Panel A utilizes ten size-, ten book-to-market-, ten momentum- and seventeen industry portfolios as test portfolios in two-stage FM cross-sectional regressions. Similar to the findings in Table 9, all standard macroeconomic factor specifications generate expected momentum returns, $E[WML]$, that are significantly different from the observed momentum return. And the incremental contribution of MP, $E[\beta_{MP} \gamma_{MP}]$, is significantly different from observed momentum profits in every case, which is also consistent with Table 9. For example, with the full samples, the complete CRR5 model indicates that the expected WML return is 0.24% per month (or 37% of the observed return) in 1947/03–2009/11. And, more importantly, the remaining 63% is significant at the 1% level. The MP risk factor itself creates an expected WML return of 0.04% per month (or 6% of the observed momentum return). The null hypothesis of no difference between the observed momentum return and the incremental contribution of MP is rejected ($t$-statistic=3.04). The results provide supportive evidence that macroeconomic risk is not the source of momentum profits. Moreover, our main conclusion continues to hold even if we use other sets of Kenneth French industry-sorted portfolios, for example, such as his thirty industry-sorted portfolios.

Panels B, C and D of Table 10 use industry-sorted portfolios alone as the test portfolios in two-stage FM cross-sectional regressions. In the literature, we could find no documented

---

7 Please refer to Kenneth French’s website, http://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html#Research.
8 For the sake of brevity, in this section, we report the results from using equal-weighted momentum portfolios. The basic inferences remain the same if we replace equal- with value-weighted momentum portfolios in the test.
9 The only two insignificant cases appear when the CRR4 and CRR5 models use the rolling windows in the first-stage regressions in 1947–2009. Rolling regressions provide less precise estimates than full-sample and extended-window regressions (Liu and Zhang, 2008).
10 The results are available upon request.
evidence of the classic January size effect on industry-sorted portfolios, whereas many previous studies have shown that the January size effect is embedded in size-, book-to-market- and momentum portfolios (Grundy and Martin, 2001; Keim, 2008). Using industry-sorted portfolios alone prevents our tests from being contaminated by the January size effect (which results in winners underperforming losers massively). Note that the complete macroeconomic factor models not only produce meager expected momentum returns, but even generate negative expected returns occasionally. Use of industry portfolios alone also leads to the incremental contribution of MP being extremely small or nonexistent. Panel B shows the findings for using ten industry portfolios alone as test portfolios. With the full samples, the CRR5 model produces expected momentum return to be 0.06% per month (or 10% of the observed WML return) in 1947/03–2009/11. And the associated t-statistic of 1.66 rejects marginally the null hypothesis of no difference between the observed and expected returns. With the extended windows, the CRR5 model has expected WML return at 0.28% per month (or 45% of the observed returns in 1947/03–2009/11); there is an insignificant difference between the observed and expected WML returns (t-statistic=1.12). In the short sample period of 1960–2004, the CRR5 model indicates hardly any expected WML return. So far, we find mixed evidence about whether the complete macroeconomic risk model can capture momentum profits. With respect to the incremental contribution of MP, the results are a bit striking in that MP contributes little to explain momentum phenomenon, in stark contrast to the corresponding results from using size-, book-to-market- and momentum portfolios as testing portfolios. With the full samples, the one-factor MP model produces expected WML return of 0% per month in LZ’s sample period of 1960–2004 (and 0.01% per month in our entire sample period of 1947/03–2009/11). This result highlights once again the fact that the MP risk factor cannot account for momentum profits. For robustness, Panels C and D utilize seventeen industry-sorted portfolios and thirty industry-sorted portfolios, respectively, as the test portfolios. The basic inferences remain robust. Using only industry-sorted portfolios as test portfolios provides strong confirmatory evidence that macroeconomic risk cannot explain momentum profits. Despite the fact that there is no documented evidence of the January size effect embedded in industry portfolios, we shall still treat the presumption made in the last section with some caution.

Now we concentrate our analysis on the 11 non-January months (when momentum does exist), in order to calculate the expected momentum return and the incremental contribution of
MP. The results are reported in Table 11 which shows no evidence of macroeconomic risk being a reward for momentum profits outside of January. Panel A presents expected momentum returns estimated from the forty seven test portfolios, including ten size-, ten book-to-market-, ten momentum- and seventeen industry portfolios. The complete macroeconomic risk-factor models continue to generate expected WML returns, $E[WML]$, that are significantly different from the observed WML returns. For example, with the extended windows, the CRR5 model provides an expected WML return of 0.40% per month (or 34% of the observed return) in 1947/03–2009/11—and the difference between the observed and expected returns is significant at the 1% significant level (with an associated $t$-statistic of 5.84). Nonetheless, comparing these results with corresponding results in Table 10 shows that controlling for the month of January (when momentum is nonexistent) does have a material impact on our major findings. It can be inferred from Panel A that the MP risk factor itself is barely able to account for any momentum profit. For the best scenario of all the cases examined, the incremental contribution of MP, $E[\beta_{MP_YMP}]$, is 0.05% per month (or 4% of the observed return). Interestingly, the role of the growth rate of industrial production in explaining momentum profits seem to be weakening outside of January, relative to all year round. For instance, the one-factor MP model indicates that the incremental contribution of MP is 0.11% per month (or 17% of the observed return) in 1947/03–2009/11, whereas it indicates that the incremental contribution of MP is $-0.01%$ per month (or 0% of the observed momentum profit) outside of January. Panels B, C and D show expected momentum returns estimated from using industry-sorted portfolios alone outside of January, when momentum does not exist. The basic inference continues to hold.

We now address the concerns about the impact of small and illiquid firms traded on the Nasdaq Stock Exchange. We use momentum portfolios formed in the sample of NYSE and AMEX stocks excluding Nasdaq stocks, and refer to them as NYSE–AMEX momentum portfolios. The previous momentum portfolios formed on NYSE, AMEX and Nasdaq stocks are denoted as NYSE–AMEX–Nasdaq momentum portfolios. Now the test portfolios utilized in two-stage FM cross-sectional regressions consist of ten size-, ten book-to-market- and ten NYSE–AMEX momentum portfolios. This change might result in a particularly pronounced impact on equal-weighted rather than value-weighted portfolio returns. Equal weighting means that portfolio returns can be dominated by some tiny Nasdaq stocks that account for about as large a percentage of the total number of stocks but a very small percentage of the market cap.
Given the fact that the January momentum losses are more pronounced in small firms than in large firms, NYSE–AMEX momentum portfolios should be less exposed to the January influence than NYSE–AMEX–Nasdaq momentum portfolios. In fact, Table 1 provides confirmatory evidence that NYSE–AMEX–Nasdaq momentum strategies (5.69% per month) have experienced more January losses than the corresponding NYSE-AMEX momentum strategies (4.68% per month), but the difference does not seem to be particularly worthy of note. In this case, replacing NYSE–AMEX–Nasdaq momentum portfolios with NYSE–AMEX momentum portfolios as test portfolios might not alter the results to a large degree. In addition, considering the inclusion of Nasdaq stocks into the CRSP monthly file relatively late (since December 29th 1972), this change may have relatively strong influences on the sample period of 1960–2004, relative to the sample period of 1947/03–2009/11.

Table 12 reports the results from replacing NYSE–AMEX–Nasdaq momentum portfolios used in all the previous tests with NYSE–AMEX momentum portfolios. This change does not lead to dramatic differences from the previous results. Panel A includes equal-weighted NYSE–AMEX momentum portfolios in the thirty test portfolios, and Panel B uses value-weighted NYSE–AMEX momentum portfolios. The right-hand block of Panel A shows that the complete macroeconomic factor models cannot account for momentum profits, which is consistent with the corresponding findings for NYSE–AMEX–Nasdaq momentum. With the extended windows, the CRR5 model produces an expected WML return of 0.49% per month (or 62% of the observed WML return) in 1947/03–2009/11. And the difference between the observed and expected WML returns is significant (\(t\)-statistic=4.3). In the left-hand block of Panel B, with the extended windows, the CRR5 model produces expected WML return to be 0.67% per month (or 63% of the observed value-weighted WML return) in 1960–2004. And the difference between the observed and expected WML returns is significant (\(t\)-statistic=3.23). With respect to the economical significance of the incremental contribution of MP, there are very similar though slightly dissimilar findings to the counterpart results from using NYSE–AMEX–Nasdaq momentum portfolios. In the left-hand block of Panel A, with the full samples, the one-factor MP model generates the incremental contribution of MP (0.54% per month, or 58% of the observed return) in 1960–2004, which is significant from the observed momentum return (with an associated \(t\)-statistic of 2.19). We also find that the FF+MP model creates the incremental contribution of MP (0.51% per month, or 54% of the observed equal-
weighted return) in 1960–2004, with the remaining 46% significant (t-statistic=2.90). These findings are inconsistent with the corresponding results from using NYSE–AMEX–Nasdaq portfolios. Nonetheless, neither the right-hand block of Panel A nor the entire Panel B show evidence that the growth rate of industrial production can capture momentum profits. In the right-hand block of Panel B, with the extended windows, the CRR5 model creates the incremental contribution of MP (0.02% per month, or 3% of the observed return), which is significant from the observed momentum return (t-statistic=4.17). In the right-hand block of Panel B, with the full samples, the CRR4 model produces the incremental contribution of MP to be 0.23% per month (or 23% of the observed value-weighted WML return) in 1947/03–2009/11. And the remaining 67% is significant (t-statistic=4.04). Overall, these findings point out the fact that the MP risk factor plays only a minor role in the source of momentum profits.

Consistent with most of the evidence above, Table 13 shows that, outside of January, neither the complete macroeconomic factor models nor the MP risk factor itself can capture momentum profits. Panel A reports the findings for including equal-weighted NYSE–AMEX momentum portfolios in the group of test portfolios. The CRR5 model, for example, with extended windows, produces an expected WML return of 0.74% per month (or 58% of the observed equal-weighted WML return) in 1947/03–2009/11. And the remaining 42% is significant. Moreover, the incremental contribution of MP, $E[\beta_{MP}Y_{MP}]$, is 0.09% per month (or 7% of the equal-weighted observed WML return) in the same sample period, which is also significantly different from expected WML return (t-statistic=7.17). Panel B presents the results from using value-weighted NYSE–AMEX momentum portfolios, which shows that our main conclusions remain robust. Taken together, all of our robustness tests provide further supportive evidence that macroeconomic risk is not the underlying risk for momentum profits.

5. Conclusion

This study examines the role of macroeconomic risk factors in explaining momentum profits in the 11 months of a year when momentum does exist. Our analysis presents three major findings. First, the winner and loser portfolios have almost identical loadings on the MP risk factors outside of January, when momentum does exist. Thus, there is an essential zero MP loading on the winner-loser portfolio, and the difference comes mainly from January, when winners underperform losers massively. Second, the MP risk-premium estimates are statistically
and economically insignificant outside of January, meaning that the growth rate of industrial production is not a priced risk factor, and cannot capture cross-sectional variations of non-January stock returns. Third, macroeconomic risk-premium estimates together with the corresponding factor loadings cannot generate the expected momentum return. In all, we conclude that macroeconomic risk factors are not the main driving forces of momentum profits.

Our empirical analysis has implications for the existing literature on investigating the source of momentum profits. In attempting to examine the source of momentum profits, most of the existing studies do not concentrate on the 11 months of a year when the momentum effect is really present (e.g., Chan, Jegadeesh and Lakonishok, 1996; Chordia and Shivakumar, 2002; Chui, Titman and Wei, 2010). As this may lead to illusory conclusions about what drives momentum, it behoves us to be cautious about the substantial January contamination on investigating the cause of momentum profits.

Our empirical results also provoke some questions for future study. First, would the growth rate of industrial production account for the fact of winners underperforming losers massively in January? Since this question is clearly beyond the scope of our study, it might be worth investigating in future research why winner–loser portfolios lose substantially in January. This would help to understand long-term reversal, which has been demonstrated to be present only in January (Grinblatt and Moskowitz, 2004; Yao, 2012). Second, if macroeconomic risk factors cannot capture non-January momentum profits, why do momentum trading strategies incur considerable monetary losses when economic conditions are poor (Daniel, Jagannathan and Kim, 2012; Daniel and Moskowitz, 2012)? These issues are left for future research.
References:


Tables and Figures

Due to the page limit, all the tables and figures are available on the below website:

https://sites.google.com/site/yaqiongyao/research