Noisy Prices and Inference Regarding Returns*

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Abstract

We assess how noise in prices affects estimates of return premia estimated from CRSP monthly returns, and document statistically significant bias for every stock characteristic considered. The bias can be large in economic terms, e.g. equal to 50% or more of the corrected estimate for firm size, return skewness, and illiquidity. Any explanatory variable that is correlated with the variance of the noise is susceptible to significant bias in associated return premium estimates. We also show that differing inferences obtained on the basis of equal- vs. value-weighted portfolios reflect bias due to noise as well as a true weighting effect.

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Abstract

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I. Introduction

Some of the most frequently studied research questions in the field of Finance invoke comparisons of mean rates of return across securities and portfolios. While such comparisons lie at the heart of the vast empirical asset pricing literature that assesses whether mean returns are related to measures of risk or variables that might proxy for risk, they are used in many other applications as well. For example, researchers have assessed relations between mean returns and a diverse array of firm attributes, including the quality of corporate governance (Gompers, Ishii and Metrick (2003)), aggregate short selling and institutional ownership (e.g. Asquith, Pathak, and Ritter (2005) and Boehmer, Huszar, and Jordan (2010)), media coverage (Fang and Peress, 2009), success of customer firms (Cohen and Frazzini, 2008), recent asset growth (Cooper, Gulen, and Schill, 2008), and credit ratings (Avramov, Chordia, Jostova, and Philipov (2007)), among others. Comparisons of mean returns are also required in studies that designate a "benchmark" portfolio to assess whether returns to certain securities are abnormal, as in Barber and Lyon (1997) and Lakonishok and Lee (2001).

However, the price data that is used to compute security returns contains noise attributable to an array of market frictions. While the effects of noisy prices on estimates of return volatility have been studied extensively, e.g., by Aït-Sahalia, Mykland, and Zhang (2005), Bandi and Russell (2006), Engle and Sun (2007), and Andersen, Bollerslev, and Meddahi (2010), among others, the effects for estimation of mean returns and for inference in asset pricing tests have received much less attention.

That microstructure noise can indeed affect inference regarding returns was demonstrated by Blume and Stambaugh (1983), who showed that zero-mean noise in prices leads to upward bias in returns computed from those prices, with the magnitude of the bias in each security's return approximately equal to the variance of the noise in the security's prices.[1] Blume and Stambaugh also showed that portfolio returns obtained by equal weighting remain upward biased. In an empirical application using daily data, the authors show that microstructure noise accounts for

[1]The bias is simply due to Jensen's Inequality. To illustrate, suppose that the true security value is constant at $10, but that microstructure frictions lead to transactions occurring at either $9.90 or $10.10, with equal probability. Observed returns can be -1.98% (25% probability), 2.02% (25% probability), or zero (50% probability). In a large sample the mean observed return will be 0.01%, even though value is constant. If the example were repeated with prices of $9.80 or $10.20, the mean observed return would increase to 0.04%.

approximately half of the observed small firm premium (measured as the differential in mean daily returns for portfolios of small versus large firms), as of their 1963 to 1980 sample period.

To avoid potential adverse effects of noise on inferences, a number of authors, including Conrad and Kaul (1993), Rouwenhorst (1998), and Canina, Michaely, Thaler and Womack (1998), implement the Blume and Stambaugh (1983) recommendation to compute portfolio returns on a buy-and-hold basis, either by computing value-weighted returns or by setting weights to be equal at the end of each calendar year and keeping share positions constant through the subsequent year. Other authors, including Jegadeesh and Titman (2001), Amihud (2002), and Pástor and Stambaugh (2003), proceed by excluding relatively illiquid securities (in particular those with low share prices) from their analyses. While the share-price filter likely mitigates the adverse effects of microstructure noise, a potential cost of this correction is the loss of valuable information from the excluded stocks.

In contrast, many researchers who study security returns make no allowance in their empirical work for the potential effects of noisy prices, making inferences based on equal-weighted portfolio returns and/or ordinary least square (OLS) regressions using returns as the dependent variable.[2] The implicit assumption in these studies is that the any effects of noise in prices are small enough to be safely ignored. This perception is likely attributable at least in part to (i) the evidence that bid-ask spreads, which comprise a highly visible source of microstructure noise, are quite narrow in the recent U.S. data, and (ii) the fact that many studies rely on returns measured at a monthly horizon, in contrast to the daily horizon studied by Blume and Stambaugh (1983).

However, bid-ask spreads are not the only reason prices are noisy. The most important source of microstructure noise in markets characterized by relatively narrow spreads is likely "price pressure". That is, larger buy and sell orders or sequences of orders can move prices beyond their longer-term equilibrium, giving rise to "transitory volatility," if liquidity supply is not perfectly elastic.[3] Harris (2003, page 414) observes that "Large orders and cumulative order imbalances created by uninformed traders also cause prices to move from their fundamental

[2]Examining papers published in only two premier outlets, *The Journal of Finance* and *The Journal of Financial Economics*, over a recent five year (2005-2009) interval, we were able to identify twenty four papers that report uncorrected equal-weighted portfolio returns. A listing is available from the authors on request. Literally dozens of published studies report results of OLS regressions using security or portfolio returns as the dependent variable.

[3]Noise also arises due to the rounding of prices to a discrete pricing grid, and due to non-synchronous trading of securities.

values....transitory volatility includes both the price changes that impatient uninformed traders cause and the subsequent reversals of those price changes." Hendershott, Li, Menkveld, and Seasholes (2010) provide evidence of substantial transitory volatility in recent data on monthly returns to New York Stock Exchange securities. Specifically, the authors estimate that price pressure related to customer order imbalances and market-maker inventory accounts for transitory volatility that is 28% as large as variation in monthly changes to underlying security values.

Whether noise-induced biases are indeed small enough to safely ignore when returns are measured at the monthly interval is ultimately an empirical question. The central aim of this paper is to quantify the impact of these biases in empirical tests representative of those that appear in the literature. We do so by contrasting raw and corrected empirical results obtained when estimating OLS return regressions and when comparing returns across attribute-sorted stock portfolios. To correct for noise-induced biases we implement the methodology introduced by Asparouhova, Bessembinder and Kalcheva (2010; hereafter ABK).[4]

ABK extends the Blume and Stambaugh (1983) analysis to OLS regressions with rates of return as the dependent variable, showing that noise in prices leads to biased and inconsistent coefficient estimates. The authors provide an expression for the magnitude of the bias and propose a correction that leads to consistent parameter estimates. Applied to cross-sectional analyses in the style of Fama and MacBeth (1973), the ABK correction simply calls for the cross-sectional return regressions to be estimated by weighted-least-squares, with the prior-period gross return used as the weighting variable. When applied in a regression setting we refer to the ABK correction as "return-weighted least squares (RWLS)." Similarly, when applied to portfolio returns, the ABK correction is to compute the weighted mean, again using the gross prior return as the weighting variable.

The array of explanatory variables used in the empirical literature is too extensive to allow

[4]We adopt the ABK correction rather than the buy-and-hold method in part due to ease of implementation. Also, the ABK correction provides bias-corrected estimates of equal-weighted outcomes, while buy-and-hold methods provide estimates of position-weighted outcomes. For example, price-weighted and value-weighted portfolios are both buy-and-hold in the sense that no trading is required to maintain portfolio weights as time passes. However, true price- and value-weighted returns may differ from true equal-weighted returns. Blume and Stambaugh's empirical implementation focuses on returns to portfolios that are equal-weighted at year end, with positions held constant through the subsequent year. A disadvantage is that this method does not eliminate bias for the first observation of each year.

for reassessment of all prior work. With the aim of selecting variables that are reasonably representative of those that have appeared in the literature, we focus on the set considered by Brennan, Chordia, and Subrahmanyam (1998), supplemented by measures of idiosyncratic volatility and skewness, as well as a measure of illiquidity.

The results reported here reveal that noise in prices leads to statistically and/or economically significant biases in typical specifications implemented in monthly return data from CRSP. ABK show that the direction of the bias depends on the cross-sectional correlation between the variance of the noise in prices and the variables used to explain returns. Empirically, we find the bias to be positive for inverse share price, idiosyncratic volatility, idiosyncratic skewness, and illiquidity, and negative for firm size, dividend yield, trading volume, and lagged returns. In some cases (e.g. firm size or idiosyncratic volatility) the bias reinforces the true relation between mean returns and the explanatory variable, implying that uncorrected estimates overstate the true relation, while in other cases (e.g. firm specific skewness and momentum) the bias partially offsets the true relation, implying that uncorrected estimates under estimate the true relation. Further, in some cases, e.g., idiosyncratic volatility, the bias can be highly significant in statistical terms, but relatively small in economic terms. In other cases, e.g., firm size, idiosyncratic skewness, and illiquidity, the bias is large in economic terms, equal to 50% or more of the corrected estimate. Of the firm-level variables studied here, only the estimated relation between returns and the market-to-book ratio is largely free of noise-induced bias.

When we estimate results by subperiod, we find that even during the most recent (2001-2007) subsample microstructure noise continues to impart significant bias into estimates of return premiums associated with firm size, share price, firm-specific volatility, firm-specific skewness, momentum, and stock illiquidity. That biases attributable to microstructure noise remain significant even though bid-ask spreads have narrowed significantly in the post-decimalization period provides indirect but strong evidence that bid-ask spreads are not the sole, or even the most important, source of noise in prices. Our results therefore provide indirect confirmation of the Hendershott, Li, Menkveld, and Seasholes (2010) findings of large transitory volatility attributable to microstructure frictions in recent monthly return data from the NYSE.

As ABK note, assuming serial independence of noise realizations, the absolute magnitude of the bias attributable to microstructure noise is invariant to the time horizon over which returns are measured. The relative importance of the bias therefore declines as measured returns grow larger over longer time horizons. The findings here imply that the bias remains empirically important even in returns measured at the monthly horizon. One possible contribution to the explanation is that the month-end prices used to compute calendar-month returns contain more noise than other days of the month. Such a phenomenon could arise, for example, from trading used to move month-end prices strategically, along the lines documented by Carhart, Kaniel, Musto, and Reed (2002).

We also present some results relevant to the choice of weighting variable when constructing portfolio returns. As noted, comparisons of mean returns across portfolios is frequently implemented in asset pricing tests. The issue also arises when defining a benchmark portfolio for purposes of assessing whether returns to certain stocks are unusual. If portfolios are equal-weighted then returns are upward biased. Also, if portfolios are formed after sorting stocks by size or illiquidity or other attributes correlated with these variables, then some portfolios will tend to be dominated by stocks that are difficult or costly to trade. Researchers often study returns to value-weighted portfolios to mitigate this concern. However, as Fama and French (2008) emphasize, value-weighted mean returns can be dominated by a few large stocks. Further, it is not entirely obvious that value-weighting is optimal for purposes of statistical inference. For example, when estimating the parameters of asset pricing models, the information contained in small-firm returns is potentially as relevant as the information contained in large-firm returns.

As we show here, value-weighted mean returns are not subject to the noise-induced upward bias that is contained in equal-weighted mean returns, implying that the differential in mean returns across value- versus equal-weighted portfolios is attributable not just to differential weighting, but also to microstructure noise. The ABK correction provides consistent estimates of equally-weighted portfolio returns. It can therefore be used to construct an unbiased, equal-weighted, benchmark return. Also, the ABK correction allows for decomposition of the observed differential in mean equal-weighted versus value-weighted returns into a component that reflects the effects of microstructure noise and a component that reflects the true effects weighting by market capitalization rather than equally.

For example, focusing on monthly return data to decile portfolios of CRSP common stocks from 1964 to 2007 sorted on the basis of firm size, the equal-weighted size effect (mean return on smallest decile less mean return on largest) is 1.76%, compared to 0.81% in value-weighted returns. Of the total differential between equal- and value-weighted results of 0.96% per month, 0.60% per month (t-statistic = 15.1) is attributable to the upward bias due to microstructure noise contained in the equal-weighted means, while the remaining 0.36% per month (t-statistic = 5.66) is the estimated true effect of value weighting relative to equal weighting. We present a similar decomposition of equal- vs. value-weighted portfolio return differentials for portfolios sorted on the basis of book-to-market ratios, share price, dividend yield, trading volume, firm-specific volume and skewness, prior returns, and the Amihud (2002) illiquidity measure.

This paper is organized as follows. In the next section we review some of the relevant literature. The empirical methodology and explanatory variables used are introduced in Section III. Empirical results obtained when comparing returns across portfolios created after sorting on firm characteristics and when implementing univariate cross-sectional regressions are reported in Section IV. Section V considers the potential curative effect of including a measure of illiquidity as an additional explanatory variable in the cross sectional regressions, and provides results of multivariate regressions of returns on an array of commonly used explanatory variables, with and without corrections for microstructure noise. Section VI concludes.

II. Microstructure Noise And Asset Pricing Tests

We follow Black (1986), Aït-Sahalia, Mykland, and Zhang (2005), Bandi and Russell (2006), Andersen, Bollerslev, and Meddahi (2010), and others in using the term "noise" to refer to deviations between observed prices and those that would obtain in a frictionless market.[5] Asset pricing models generally make predictions regarding the mean returns that would accrue to

[5]In a paper related to our own, Arnott, Hsu, Liu, and Markowitz (2007) consider the possibility that some findings in the empirical asset pricing literature may be attributable to what they refer to as "noise." However, these authors use the term to refer to deviations of price from value that are potentially persistent through time, and that are not necessarily smaller than transaction costs. It might be argued that the label "mispricing" would be more descriptive for such deviations. Indeed, Brennan and Wang (2009) consider a similar framework, and explicitly refer to such persistent deviations as mispricing rather than noise. In contrast, we use the term in the same manner as econometricians who refer to "white noise," i.e., deviations whose expectation is zero given any set of conditioning information.

passive investors. The CAPM, for example, implies higher mean returns to investors passively holding higher-beta stocks. In contrast to such asset-pricing interpretations, the apparent return premia attributable to microstructure-induced noise in prices are mirages that cannot be captured by passive investors.[6] To illustrate the distinction, suppose that underlying values for all securities are constant through time. Bid-ask bounce or other noise leads to positive mean observed returns, and more so for securities with more noise in prices.[7] But, in this scenario, investors who purchased and held securities subject to more noise (e.g. those with wide bid-ask spreads) would not actually earn higher returns, or even a positive return. This insight generalizes. The biases in mean returns, and in return premia estimated by cross-sectional return regressions, that are attributable to noise in prices are illusory, and cannot be captured by passive long term investors.[8]

A. Potential Effects of Noise on Empirical Estimates of Return Premia

The empirical literature measuring mean returns is vast, and we will not try to provide a comprehensive survey. Instead, we highlight some empirical findings that are plausibly affected by microstructure noise in prices. ABK show that the key determinant of the sign and magnitude of biases in slope coefficients obtained in cross-sectional return regressions is the magnitude of the cross-sectional (partial, if a multiple regression) correlation between the variance of the noise in prices and the regression explanatory variables. Similarly, ABK show (Corollary 1 to Proposition 1) that the upward bias in equal-weighted portfolio returns is determined by the average, across securities in the portfolio, variance of noise in prices. If the average noise variance differs across the securities in different portfolios, then the comparison of equal-weighted returns

[6]Note, though, that some investors following active strategies such as the provision of liquidity in response to order imbalances may be able to benefit from the existence of noise in prices.

[7]Individual observed returns can accurately reflect traders' experiences, even while mean returns remain upward biased. Suppose, for example, that an individual purchases at an ask price that exceeds true security value and sells at a bid price that is less than true security value. The return computed from ask to bid accurately reflects this individual's experience on this round trip trade. Nevertheless, the mean return computed from a large number of trades occurring randomly at the bid and the ask is an upward biased estimate of the mean true return and of the mean return to a long term investor, as Blume and Stambaugh (1983) and ABK (2010) demonstrate.

[8]The ABK consistency proof assumes that noise realizations are serially independent. We recognize that microstructure noise will not be completely uncorrelated at short time intervals. For example, a large order broken into smaller trades could lead to dependence in the noise contained in prices over the time horizon during which the large order is executed, and/or until the price movement attracts new liquidity supply. However, most empirical asset pricing studies rely on monthly returns. The assumption that microstructure noise is serially independent is likely to be reasonable when prices are observed at monthly intervals.

across portfolios gives a biased estimate of the true differential in expected returns.

To the extent that microstructure models or economic reasoning allow a researcher to predict the sign of the cross-sectional correlation between the variance of the noise and explanatory or sorting variables, the sign of the bias in asset pricing tests can be predicted. For example, if economic reasoning suggests that the prices of securities with high measured return volatility contain more noise on average than prices of low-volatility securities, then the ABK analysis implies that estimates of the relation between mean returns and volatility obtained by comparisons of equal-weighted portfolio returns or OLS regressions will be upward biased. Alternatively, by comparing asset pricing results with and without the ABK correction, it is possible to infer the sign of this correlation. We next consider the potential effects of microstructure noise or return premium estimates for the set of explanatory variables used in this study.

A.1. Firm Size

Numerous authors, dating at least to Banz (1981), and notably including Fama and French (1992), have documented that average returns to the common stocks of small-capitalization firms exceed those for larger firms, even after allowing for differences in risk. However, it is very likely that the prices of small-capitalization stocks contain more noise than the prices of large capitalization stocks. While noise is not fully observable, it is a well-documented empirical regularity (e.g. Bessembinder, 1999) that one contributor to microstructure noise, bid-ask spreads, are wider for small-capitalization stocks. If small-capitalization stock prices indeed contain more noise, then their returns are more upward biased, and microstructure noise contributes to empirical estimates of the small firm return premium. Indeed, Blume and Stambaugh (1983), focusing on daily returns through 1980, show that the difference between equal-weighted mean size-ranked portfolio returns is substantially greater than when they compute portfolio returns on a "buy-and-hold" basis. We study monthly rather than daily returns, extend the analysis through 2007, and consider both portfolio and regression-based methods.

A.2. Book-to-Market

Fama and French (1992) show, for a sample of NYSE, Amex, and Nasdaq stocks, that common stock in firms with high ratios of equity value on the balance sheet to market equity value earned higher average returns. This finding, often referred to as the "value premium" is also documented by Stattman (1980) and Rosenberg, Reid, and Lanstein (1985). To the extent that prices for "growth stocks" (those with low book-to-market ratios) are subject to more microstructure noise (perhaps reflecting that they are harder to value), their returns are more upward biased, implying that the true "value premium" may be larger than that previously documented.

A.3. Share Price

Brennan, Chordia, and Subrahmanyam (1998) report higher mean returns for stocks with low share prices. They motivate the inclusion of share price when seeking to explain variation in average returns by reference to findings reported by Miller and Scholes (1982). In contrast, Gompers, Ishii and Metrick (2003) estimate an insignificant coefficient when using share price as a control variable in their study of corporate governance and stock returns. Share price is known to be cross-sectionally correlated with numerous firm characteristics, including market capitalization and proportional bid-ask spreads. To the extent that lower share prices are associated with more microstructure noise in prices, the relation between share price and average return is likely to be downward biased (or the relation between inverse share price and returns upward biased) as a result of microstructure noise.[9]

A.4. Dividend Yield

Brennan (1970) hypothesizes that expected returns should be positively related to dividends, to compensate for the higher personal tax burden on dividend as compared to capital-gain income. However, the subsequent empirical work (e.g. Litzenberger and Ramaswamy (1980), and Miller and Scholes (1978, 1982)) has provided inconsistent evidence on the issue.[10]

[9] As in Blume and Stambaugh (1983), noise is defined proportional to the true share price.

[10] Boudoukh, Michaely, Richardson, and Roberts (2007) provide evidence that average returns are more closely linked to total corporate payouts (dividends plus share repurchases) than to dividends, per se.

Microstructure noise may be relevant to these findings. In particular, the more stable, larger, and established firms that tend to pay more generous dividends are plausibly subject to less noise in prices. If so, firms paying higher dividends would have a smaller upward bias in measured returns as compared to firms paying low or no dividends, and microstructure noise would create a bias against finding a return premium associated with dividend yield.

A.5. Trading Activity

A number of authors, including Chordia, Subrahmanyam, and Anshuman (2001), have documented a negative cross-sectional relation between stock returns and trading activity, measured either as share volume or the turnover of outstanding shares. More actively traded stocks generally are more liquid, and have narrower bid-ask spreads (Bessembinder, 1999). This suggests that thinly-traded stocks also tend to be associated with more microstructure noise in prices. If so, the bias attributable to microstructure noise is positive, and contributes to the empirical observation that low trading is associated with higher observed returns.

A.6. Past Returns

Numerous authors, including prominently Jegadeesh and Titman (1993), have documented "momentum effects," whereby stock returns can be forecast to a significant degree by returns to the same stocks over the prior three to twelve months. High observed prior returns may proxy for a number of factors. In particular, Statman, Thorley, and Vorkink (2006) document that high recent returns are associated with increased trading activity. To the extent that this increased volume is associated with improved liquidity and decreased noise, past returns will be cross-sectionally negatively correlated with the variance of the noise in prices, and standard methods will provide downward biased estimates of the true magnitude of momentum effects.

A.7. Idiosyncratic Volatility and Skewness

Several recent papers have considered the possibility that returns are related to stock-specific volatility. In particular, Ang, Hodrick, Xing, and Zhang (2006) report a negative cross-sectional relation between average returns and *recent* idiosyncratic volatility. In contrast, Fu (2009) uses an EGARCH model to estimate conditional stock-specific volatility, and reports a positive relation between this risk measure and average returns.[11] Boyer, Mitton, and Vorkink (2010) present theory and empirical evidence indicating that stocks with high idiosyncratic return skewness earn lower average returns, and that the effect of skewness helps explain the puzzling relation between returns and stock-specific volatility documented by Ang, Hodrick, Xing, and Zhang (2006).

Stocks with more noise in observed prices are likely to also be those with higher measured stock-specific volatility. This reflects in part that noise in prices contributes directly to measured volatility. It also reflects that stocks with greater uncertainty regarding fundamental values are plausibly affected by more noise in prices. If so, the cross sectional covariance between the amount of noise and measured volatility is positive, and standard tests will be biased in favor of finding a positive relation between returns and stock-specific volatility. Also, to the extent that return skewness is greater for stocks (such as small and low-priced firms) that also contain more noise in their prices, the return premium for skewness is likely to also be upward biased, i.e. the true relation is likely more negative than that reported by Boyer, Mitton, and Vorkink (2010).

A.8. Illiquidity

ABK note that empirical measures of illiquidity are likely to be positively correlated with the variance of noise in prices, and document significant upward bias in return premia for illiquidity estimated in Fama-MacBeth regressions. Here, we study the Amihud (2002) measure of illiquidity, mainly to assess the sensitivity of inferences to its inclusion in multivariate tests.

[11] Fu (2009) interprets his conditional volatility estimates as the expectation, based on time t-1 information, of idiosyncratic volatility during period t. However, Guo, Ferguson and Kassa (2010) assert that Fu's estimates contain a look-ahead bias. As our intent is to simply assess whether empirical estimates that have appeared in the literature are sensitive to the ABK correction for microstructure noise, we do not investigate this issue.

III. Data and Methodology

A. Methodology and the ABK (2010) Correction

We estimate relations between average monthly stock returns and stock-level explanatory variables based both on cross-sectional return regressions in the style of Fama-MacBeth (1973), and based on raw and factor-adjusted returns to attribute-sorted portfolios. Each method has been widely used in the literature. The methods can potentially lead to different inferences, as cross-sectional regressions require the researcher to specify a functional form, while portfolio return comparisons do not. On the other hand, cross-sectional regressions can be used to evaluate the partial effects of numerous explanatory variables in the same specification, while portfolio sorts are limited for practical purposes to sorting on a relatively small number of attributes.

Applied to Fama-MacBeth analyses, the ABK (2010) correction simply calls for the cross-sectional return regressions to be estimated by weighted-least-squares, with the prior-period gross (one plus) return used as the weighting variable. Similarly, when applied to portfolio returns, the ABK (2010) correction is to compute the weighted mean, again using the gross prior return as the weighting variable. As noted, we refer to the ABK (2010) correction applied in the regression setting as "return-weighted least squares (RWLS)". When computing mean returns we simply refer to the correction as "return-weighting."

ABK (2010) show formally that this weighting procedure provides consistent parameter estimates, even in the presence of noise in prices. The intuition for the method's effectiveness is the same as that provided by Blume and Stambaugh (1983) for the effectiveness of "buy-and-hold" methods in eliminating the bias from mean portfolio returns.[12] In particular, if the prior-period price exceeds asset value, then on average the observed prior-period return is increased and the current period return is decreased relative to true returns, and vice versa if the prior-period price is less than asset value. This introduces a negative covariance between observed returns and the weighting variable, which just offsets the original bias in observed returns.

The consistency proof in ABK (2010) assumes that the random components of true security

[12]RWLS differs from the computation of buy-and-hold returns, in that it generally does not imply a fixed number of shares for each stock in the portfolio.

returns are independently distributed through time. The substantial literature on momentum effects in stock returns indicates that the independence assumption may be violated in actual data. Further, in light of this evidence, the empirical analysis provided here includes past returns as explanatory variables. In Appendix A we report the results of a simulation analysis intended to assess whether the ABK (2010) correction remains effective given these complications. We conclude that the RWLS correction remains effective even when true returns are autocorrelated and lagged returns are included as regressors.[13]

A.1. Cross-Sectional Regression Tests

Within the Fama-MacBeth framework, we estimate each cross-sectional return regression on a monthly basis, by OLS and RWLS, and record the monthly OLS coefficient estimates, RWLS coefficient estimates, and differences between the OLS and RWLS estimates. The final coefficient estimates are the time-series means of the corresponding monthly estimates, and standard errors of the final estimates are based on the variability of the time series estimates. It is noteworthy that, in many cases the t-statistic for the mean difference between the OLS and RWLS estimates, which assesses the significance of the bias in the mean OLS coefficient attributable to microstructure noise, is substantially larger than the t-statistics for either the mean OLS or RWLS estimates themselves. This indicates that the difference between the OLS and RWLS estimates is much less variable across months as compared to the estimates themselves. It is therefore possible to detect a bias even in coefficients that are imprecisely estimated, reflecting that the month to month variability in the part of the OLS estimate attributable to the bias is relatively small.

We estimate the cross-sectional regressions using individual firm returns rather than portfolio returns as the dependent variable. This reflects in part that Ang, Liu, and Schwarz (2008) show theoretically and empirically that individual-stock regressions have better large-sample statistical properties than portfolio-based regressions. It also reflects that the ABK (2010) correction for microstructure noise in cross-sectional regressions is implemented at the individual security level.

[13] An exception is that the ABK (2010) correction should not be implemented if the time *t-1* return is included as a regressor. The prior return cannot function both as the weighting variable and as an explanatory variable.

A.2. Portfolio-Based Tests

ABK show that weighting observed returns by prior period gross returns provides consistent estimates of true equal-weighted returns. We show formally in Appendix B that weighting by prior period market capitalization provides consistent estimates of true value-weighted returns. Thus, when implementing the portfolio approach, we compute the monthly return to each portfolio by three methods: equal weighting, return weighting (based on the prior month gross return) and value weighting (based on the prior month market capitalization) for decile portfolios sorted on a number of firm attributes. The comparison of equal-weighted to return-weighted means provides an estimate of the upward bias in the equal-weighted mean attributable to microstructure noise.

We are also interested in estimating the effect of weighting portfolio returns by market capitalization rather than equally. As noted, a comparison of equal-weighted to value-weighted mean returns is affected both by differential weighting and by the bias in the equal-weighted mean. Comparison of the return-weighted mean to the value-weighted mean provides an estimate of the true "value-weighting effect," exclusive of the effect of removing the bias in the equal-weighted mean. Since we are primarily interested in the asset pricing implications of microstructure noise, our discussion focuses mainly on the "hedge portfolio" that is long the tenth and short the first decile portfolios, and on the differential in hedge portfolio returns across the three possible weighting methods.

Also, we assess the effect of microstructure noise on estimates of expected returns after allowance for variation in the Fama and French (1993) factors. To do so, we report intercepts ("alphas") obtained in time series regressions of individual and hedge portfolio returns on the Fama and French (1993) factors. An advantage of studying adjusted returns is that it allows assessment of whether various explanatory variables contribute to an explanation of return variation through channels other than their effects on premia associated with outcomes on the three Fama-French factors.

B. Data Sources and Explanatory Variables

To assess the potential effect of microstructure-induced biases on empirical asset pricing regularities, we study monthly returns in excess of the treasury interest rate for U.S. equities using data from the Center for Research in Security Prices (CRSP) and the Compustat Industrial North America monthly files. The sample spans the period July 1963 through December 2007 and consists of common stock (CRSP shrcd=10, 11 and 12) of NYSE, Amex and Nasdaq-listed companies (CRSP exchcd = 1, 2 and 3). We include in the sample for a given month those stocks that satisfy the following criteria: (1) return data for the current month and each of the prior 12 months is available (due to this requirement the cross-sectional analysis begins with July, 1964), and (2) data is available to calculate the market capitalization, share price, dollar volume, and dividend yield as of the previous month. Following Fama and French (1992), we exclude financial firms from our sample. Nasdaq stocks generally enter the sample in 1983, due to the requirement that trading volume data be available.

The firm-level explanatory variables used in the study are generally representative of those examined in the empirical asset pricing literature. The specific construction of several of the variables follows Brennan, Chordia, and Subrahmanyam (1998). For each stock and month, the following variables are constructed:

- *Size* - the natural logarithm of the market value of the equity of the firm as of the end of the second to last month.
- *log(BM)* - the natural logarithm of the ratio of the book value of equity plus deferred taxes to the market value of equity[14], using the end of the previous year market and book values. As in Fama and French (1992), the value of BM for July of year t to June of year $t + 1$ was computed using accounting data at the end of year $t - 1$, and book-to-market ratio values greater than the 0.995 fractile or less than the 0.005 fractile were set equal to the 0.995 and 0.005 fractile values, respectively.
- *Dvol* - the natural logarithm of the dollar volume of trading in the security in the

[14] The book-to-market ratio is defined as the sum of fiscal year-end book equity (Compustat item #60) and balance sheet deferred taxes (Compustat item #74), divided by the CRSP market capitalization in December of the corresponding year. The book value of common equity (Compustat data 60) is not generally available prior to 1962, see Fama and French(1992), p.429.

second to last month. Given that the interpretation of trading volume potentially differs across dealer and auction markets, we use indicator variables to allow for separate slope coefficients for NYSE/AMEX and Nasdaq-listed stocks in our regression analysis.

- *InvPrice* - the natural logarithm of the reciprocal of the share price as reported at the end of the second to last month.
- *Yld* - the dividend yield as measured by the sum of all dividends paid over the previous 12 months, divided by the share price at the end of the second to last month.
- *Ret23* - the cumulative return in percent over the two months ending at the beginning of the previous month.
- *Ret46* - the cumulative return in percent over the three months ending three months previously.
- *Ret712* - the cumulative return in percent over the 6 months ending 6 months previously.

We supplement these with the Amihud (2002) measure of illiquidity, denoted *Illiq*, computed as the ratio of daily absolute return to daily dollar volume multiplied by 1,000,000, and averaged over all days with nonzero volume in the previous year.[15] Also, we use data on conditional firm-specific volatility, *CIVOL*, from Fu (2009). Finally, we examine measures of expected idiosyncratic skewness, *Eiskew*, for the period January 1988 to December 2005, as defined by expression (4) in Boyer, Mitton, and Vorkink (2010).[16] Following Brennan, Chordia, and Subrahmanyam (1998), firm-level explanatory variables are expressed as deviations from their monthly cross-sectional mean.

When sorting in portfolios, month t returns are computed for firms assigned to portfolios based on attributes measured at month t-2 (thus skipping one month between the date attributes are measured and the date of the initial price used to compute the return), with three exceptions. For the book-to-market ratio and the Amihud illiquidity measure (attributes that are computed annually), we sort firms based on attributes as of end of the prior July. We create idiosyncratic-volatility portfolios by sorting on the conditional idiosyncratic volatility estimates from Fu

[15] *Illiq* and *Dvol* are standardized as per Eq. 3 and Eq. 4 in Amihud (2002).

[16] We thank Keith Vorkink for sharing the idiosyncratic skewness estimates used in Boyer, Mitton, and Vorkink (2010), and Fangjian Fu for sharing the idiosyncratic volatility data used in Fu (2009). The latter dataset is available from Fangjian Fu's website http://www.mysmu.edu/faculty/fjfu/default_files/Page422.htm.

(2009). When assessing momentum portfolios the sorting variable is the six-month return from month *t-12* to *t-7*. This reflects that the evidence in Jegadeesh and Titman (1993) indicates the strongest momentum effects at a six-month lag.[17]

C. Descriptive Statistics

Panel A of Table I reports the time-series averages of the cross-sectional means, medians and standard deviations for a number of key empirical variables, before log transformations, for the full sample. The mean monthly return is 0.875%. The mean market capitalization for sample stocks is $1.003 Billion. The average idiosyncratic volatility is 11.90% and the average idiosyncratic skewness is 1.012. A number of variables, including firm size, share price, and trading volume exhibit positive skewness, as evidenced by means that substantially exceed medians.

Panel B of Table I reports time-series averages of monthly cross-sectional correlations between a number of key variables, after transformations as described in Section III.B. A few are noteworthy. Firm size and the book-to-market ratio exhibit a substantial negative average correlation (-0.280), implying that firms that are small in absolute market capitalization tend to also be small relative to the book value of their assets. Conditional idiosyncratic volatility and idiosyncratic skewness are both strongly negatively correlated with firm size and strongly positively correlated with log of inverse price, indicating higher firm-specific volatility and skewness at small firms and firms with lower share prices. Expected idiosyncratic volatility and idiosyncratic skewness are themselves substantially correlated (0.399).

IV. The Effects of Microstructure Noise in Univariate Asset Pricing Tests

A. Returns to Attribute-Sorted Portfolios

We begin by assessing the effect of microstructure noise on asset pricing tests implemented by comparisons of returns across attribute-sorted portfolios. Table II reports mean returns to

[17] However, to maintain consistency, we focus on the time series mean of one-month portfolio returns, rather than the three to six-month returns studied by Jegadeesh and Titman (1993).

the first and tenth decile portfolios, and to the "hedge portfolio" that is long the tenth portfolio and short the first portfolio, for the explanatory variables described in Section III.B. We also report "alpha" estimates, obtained as intercepts from time-series regressions of portfolio returns on the Fama-French (1993) factors. We focus on univariate portfolio sorts because it is possible to form reasonably strong conjectures as to the likely correlation, and hence the direction of microstructure-induced bias, between levels of unobservable microstructure noise and individual explanatory variables. We subsequently report univariate and multivariate regression results that includes various combinations of firm characteristics as regressors.

A.1. Equal-Weighted Results

Results obtained when forming portfolio returns on an equal-weighted basis are consistent with the findings of previous studies. Focusing on the column labeled EW_{10-1}, which reports mean returns to hedge-portfolios formed on an equal-weighted basis, we observe the well documented "size effect" (mean hedge portfolio return is 1.76% per month with an associated t-statistic of 5.20), and the "value premium" (mean hedge portfolio return of 1.40% with associated t-statistic of 6.71). Momentum effects appear as well, as the hedge portfolio return is 0.50% per month, with a t-statistic of 2.29. For illiquidity-sorted portfolios the hedge portfolio return is 1.38% per month, with t-statistic of 4.69.

Our results support Boyer, Mitton, and Vorkink's (2010) regression-based results, as the mean return to the equal-weighted portfolio of stocks with high firm-specific return skewness is less than that to the portfolio of low skewness stocks by 1.20% per month, with an associated t-statistic of 2.43. We also obtain results consistent with Fu (2009), as the mean return to the equal-weighted hedge portfolio (high idiosyncratic volatility less low idiosyncratic volatility stocks) is a remarkable 3.40% per month (t-statistic of 8.02), which is the largest hedge portfolio return observed for any of the firm attributes considered in this study.

Consistent with the regression-based results reported by Brennan, Chordia, and Subrahmanyam (1998), we observe a strong share price effect, as returns to the hedge portfolio (low share price decile less high share price decile) are positive and significant (1.43% per month, t-statistic = 3.54). Also consistent with their results, we observe a trading volume effect, as the mean return to the hedge portfolio that is long high-volume stocks and short low-volume stocks is -1.43% per month, with a t-statistic of -5.31. The only insignificant result is with respect to dividend yield, where the mean hedge portfolio return is -0.31%, with an insignificant t-statistic of -1.42. This insignificant result is also consistent with the prior literature.[18]

A.2. Biases Due to Noise in Prices

The main focus of this paper is on the effect of correcting empirical estimates for the effects of microstructure noise in prices. Since hedge portfolio returns provide evidence as to whether a given attribute is associated with cross-sectional variation in mean returns, we mainly discuss the difference in hedge portfolio returns. In particular, for each variable of interest we make two simple comparisons. First, we compare the difference in hedge portfolio returns using equal vs. return-weighting, denoted by the column heading $EW - RW$, to the uncorrected equal-weighted hedge portfolio return (EW_{10-1}). This comparison gives an estimate of the portion of the apparent firm attribute return premium that is illusory, and arises due to microstructure noise.

We also compare the $EW - RW$ hedge portfolio differential to the equal-weighted vs. value-weighed differential, denoted $EW - VW$. As noted, assessment of the "value-weighting effect" by comparison of value- to equal-weighted hedge portfolio returns is affected by the bias contained in the equal-weighted results. The second comparison, therefore, provides insights as to the portion of the apparent "value-weighting effect" that is due to noise as opposed to the true effect of the shift to value-weighting.

The empirical results reported on Table II reveal statistically significant bias attributable to microstructure noise in the mean equal-weighted hedge portfolio return, for every explanatory variable considered. T-statistics for the $EW - RW$ differential range in absolute value from 2.23 (for the book-to-market ratio) to 15.22 (for inverse share price).

However, the economic relevance of the bias varies across explanatory variables. The bias is most relevant for firm size, share price, trading volume, idiosyncratic skewness, and illiquidity.

[18]Since many stocks pay no dividends in a given year, assigning ten percent of all stocks to each portfolio would result in multiple portfolios comprised of stocks paying no dividends. Instead, we assign all non-dividend-paying stocks to the first portfolio, and dividend paying stocks to the remaining nine portfolios.

The bias is least relevant for dividend yield and book-to-market ratio.

Panel A shows that noise in prices explains about one third of the apparent size effect in monthly returns, as the estimated bias is -0.60% per month, (t-statistic = -15.14), compared to an equal-weighted hedge portfolio return of -1.76% per month. This is broadly consistent with Blume and Stambaugh's (1983) result documenting that noise in prices could explain approximately half of the difference in mean daily returns for portfolios of large versus small capitalization stocks for daily data covering the 1973 to 1980 period. We also note that bias attributable to noise in prices accounts for roughly two thirds of the apparent "value-weighting effect" in the size-sorted hedge portfolio, which is estimated to be -0.96% (t-statistic of -11.94) without correction, compared to -0.36% (t-statistic of -5.66) after correction.

In contrast to the notable effects of microstructure noise on the estimated firm size effect, results reported on Panel B indicate only a modest upward bias in the equal-weighted estimate of the "value premium." While the t-statistic for equal-weighted less return-weighted hedge portfolio return is significant, the economic magnitude of the bias estimate, 0.05% per month, is small relative to the estimated premium of 1.40% per month.

Panel C shows that noise in prices is particularly relevant for the apparent return premium associated with share price, as the bias is estimated to be 0.72% per month (t-statistic = 15.22), half as large as the apparent return premium of 1.43% per month. Noise in prices also accounts for approximately half of the apparent "value-weighting effect" in the price-sorted hedge portfolio returns. We also note that the value-weighted hedge portfolio return is not significant (t-statistic = 0.10), indicating that the portion of the return premium associated with share prices that is not due to microstructure bias is mainly attributable to the smaller stocks within the price-sorted portfolios.

Share prices are not randomly distributed across stocks, but are influenced by managers' strategic choices, including IPO offer prices and stock split policy. Brennan and Copeland (1988) present theory and evidence indicating that stock splits are used by managers as costly signals of favorable firm prospects, where the cost arises due to illiquidity associated with lower share prices. The evidence here indicates that low share prices are associated with more noise in prices (likely due to the associated illiquidity) and therefore with more upward bias in measured average returns.

As noted, theories based on differential taxation of dividend versus capital gains income imply a positive relation between returns and dividend yields, but empirical results regarding the issue have not been supportive. Results reported here on Panel D are consistent with the reasoning that the share prices of low-dividend stocks contain more noise and therefore a larger upward bias in returns, leading to a bias against finding the hypothesized positive relation between average returns and dividend yield. However, even after correcting for such bias, the data fail to support the prediction.

The results in Panel E indicate that the apparent relation between returns and trading activity is also partially attributable to noise in prices. The bias is estimated at -0.44% per month, which comprises about a third of the estimated -1.43% premium associated with trading activity. Bias also accounts also for about a third of the apparent "value-weighting effect." However, the estimated true effect of value weighting as opposed to return weighting remains significant (t-statistic = -6.27), and the value-weighted hedge portfolio return is itself insignificant (t-statistic = -0.95). We conclude that the apparent return premium associated with trading activity is partially attributable to noise in prices, and is in any case mainly associated with the smaller stocks within the volume-ranked portfolios.

As anticipated, microstructure noise contributes to the estimated positive effect of firm-specific volatility. The equal-weighted hedge portfolio return reported on Panel F exceeds the return-weighted hedge portfolio return with a highly significant t-statistic of 11.83. However, the estimated magnitude of the bias (0.48% per month in mean returns) is only about one eighth as large as the estimated hedge portfolio return, which is 3.40% per month. The bias comprises about one forth of the apparent "value-weighting effect" on the hedge portfolio return, but the corrected "value-weighting effect" remains large and significant. About half of the firm-specific volatility premium, 1.48% per month, remains when the hedge portfolios are value-weighed.

Panel G shows that microstructure noise also serves to bias significantly the estimated return premium in the case of skewness-sorted portfolios. The bias-corrected estimate of the skewness return premium is -1.59% per month, or almost one third larger in absolute magnitude than the estimate obtained based on the equal-weighted hedge portfolio mean. Since the theoretical and estimated coefficients are negative, the effect of microstructure noise in this case is to partially obscure the true relationship between returns and skewness. The apparent "value-weighting" effect on the hedge portfolio is almost entirely due to the bias, indicating that the negative return premium associated with skewness is robust to the weighting method used.

Turning to momentum effects, the return-weighted hedge portfolio return reported on Panel H exceeds the corresponding equal-weighted return by 0.28% per month (t-statistic = 10.50), implying that microstructure noise also leads to underestimation of momentum effects. ABK show that microstructure noise introduces downward bias into return premium estimates if the covariance between the variance of the microstructure noise and the explanatory variable is negative. Our results therefore imply that stocks with higher past returns are subject to less noise in prices. One possible explanation is that the increased trading typically associated with high past returns, as documented by Statman, Thorley, and Vorkink (2006), is also associated with greater liquidity and/or investor attention, which reduces noise in prices.

Finally, Panel I shows that the magnitude of the upward bias in the estimate of the return premium for *Illiq* is considerable, as return-weighted hedge portfolio returns exceed equal-weighted hedge portfolio returns by 0.39% per month (t-statistic = 13.57), confirming ABK's regression-based results. In addition, we observe that bias attributable to noise in prices accounts for half of the apparent "value-weighting effect" (0.84%, t-statistic of 6.11) in hedge portfolio returns. However, it is important to note that the true "value-weighting effect" is meaningful (t-statistic = 3.78), and the illiquidity premium is only marginally significant (t-statistic = 1.85) when hedge portfolio returns are computed by value-weighting.

A.3. A Robustness Check: The Effect of Adjusting for the Fama-French Factors

In Table II we also report alphas estimated by regressing portfolio returns on the Fama-French factors. The columns labeled "10-1" report alphas when the dependent variables are returns to the hedge portfolios, with weighting of stocks as indicated. Most importantly for our purposes, adjusting portfolio returns for sensitivity to the Fama-French factors has essentially *no* effect on the the magnitude of the various biases attributable to noise in prices. In particular, the bias estimates and associated t-statistics in the Column headed "$EW - RW$" are uniformly little altered when focusing on alphas as compared to mean returns. The only minor exception is that the factor adjustment renders the bias in the book-to-market hedge portfolio return only marginally significant(t-statistic = 1.75). However, as noted, the estimated bias associated with the book-to-market ratio is economically inconsequential in any case.

The adjustment of returns for sensitivity to the Fama-French factors does provide a partial or complete explanation for some of the return regularities that survive the correction for microstructure noise. Focusing on the bias-corrected results in the column labeled "RW_{10-1}", we observe that alphas are meaningfully closer to zero as compared to mean returns in the case of firm size, book-to-market ratio, trading volume, conditional idiosyncratic volatility, and illiquidity. In the case of inverse share price, the alpha estimate is statistically indistinguishable from zero, indicating that the combination of the bias adjustment and allowance for sensitivity to the Fama-French factors has completely explained the apparent return premium.

In contrast, the adjustment for sensitivity to the Fama-French factors has essentially no effect on the estimated return premium associated with expected idiosyncratic skewness, and actually increases the magnitude of the estimated momentum effect. We find it noteworthy that that correcting for biases attributable to microstructure noise and allowing for variation in the Fama-French factors each have the effect of strengthening, rather than explaining, momentum effects in stock returns.

B. Fama-MacBeth Regression and Subperiod Results

We next report on Table III the results of estimating the return premia associated with firm-specific characteristics by means of univariate Fama-MacBeth regressions. We report these results because such cross-sectional regressions are widely used in the empirical literature. Further, inferences supported by the cross-sectional regressions potentially differ from those obtained when comparing portfolio mean returns, both because of imposition of a specific functional form, and because the analysis is conducted at the level of individual securities rather than portfolios. However, in most instances the cross-sectional regressions support conclusions similar to those obtained on the basis of portfolio return comparisons.

We report empirical results for the full (1964 to 2007) sample, and for three subsamples comprising 1964-1982, 1983-2000, and 2001-2007. The first subperiod is comprised of NYSE-AMEX stocks, while Nasdaq stocks enter for the second subperiod. The final subperiod is mainly comprised of data following the 2001 introduction of decimal pricing, which led to substantial reductions in bid-ask spreads. Comparisons across subperiods allow evaluation of whether asset pricing anomalies have survived their initial discovery. Further, results for the final subperiod allow evaluation of whether biases due to microstructure noise remain relevant after decimalization.

Focusing first on Panels A and B, the results confirm the existence of the firm-size effect (t-statistic = -3.70) and the value premium (t-statistic = 6.84) for the full-sample. Each phenomenon also survives the ABK correction for microstructure noise, as the estimated full-sample coefficients in Column RWLS remain significant (t-statistics = -2.50 and 6.81) respectively.

Notably, the estimated absolute magnitude of the bias in the estimated size effect due to microstructure noise has not decreased across subperiods. Point estimates of the bias are -0.045, -0.086, and -0.059 for the three subperiods, and each is statistically significant. That the bias remains significant in the post-decimalization sample provides indirect but strong evidence that the noise in security prices is attributable to sources in addition to bid-ask spreads, such as temporary price pressure attributable to larger orders.

Horowitz, Loughran, and Savin (2000) document that the empirical relevance of firm size has diminished substantially in the years since papers describing the empirical size effect were first published. Consistent with their findings, we observe that the slope coefficient on firm size estimated by OLS decreases in absolute value from -0.229 during the 1964-82 subperiod to -0.103 in the 1983 to 2000 subperiod. However, the estimated OLS coefficient for the most recent subperiod, 2001 to 2007, has again increased in absolute magnitude, to -0.284. Both the OLS and the corrected (RWLS) estimates of the size effect are largest in absolute magnitude (WLS point estimate = -0.225, t-statistic = -2.35) during the most recent subperiod. Hence, we conclude that reports of the demise of the size effect in returns may be premature.

Univariate regression results for *InvPrc* (Panel C) and *Yld* (Panel D) are also consistent with the portfolio results in the previous subsection. In particular, microstructure noise has an important effect on the apparent relation between share price and stock returns (the difference between the OLS and RWLS coefficients is 0.155, with t-statistic = 15.16 in Panel C). Notably, the results indicate statistically significant upward bias in the OLS estimate of the share price premium for all three subperiods, including the post-decimalization sample (t-statistic = 3.30).

Consistent with the portfolio-based results, the apparent relation between returns and trading activity is partially attributable to noise in prices: in Panel E the mean difference between OLS and RWLS estimates is negative and highly significant, with the t-statistic for Nasdaq stocks equal to -5.18 and that for NYSE/AMEX stocks equal to -7.33.[19] The results reported in Panel I indicate a substantial upward bias in the return premium for illiquidity estimated by the cross-sectional regression, as t-statistics for the difference between OLS and RWLS coefficients are 5.82 for NYSE/AMEX stocks and 5.22 for Nasdaq stocks. The upward bias is large in economic terms, with the OLS estimate fifty four percent larger than the corrected (RWLS) estimate for NYSE/AMEX stocks, and thirty seven percent larger for Nasdaq stocks. The upward bias is significant on both markets for all three subperiods (t-statistics range from 2.09 to 10.49). However, also consistent with ABK, the bias-adjusted (RWLS) estimate remains at least marginally significant (t-statistics range from 1.79 to 3.26) on each market during each subperiod, which supports the conclusion that mean returns are indeed larger for less-liquid stocks.

The relationship between returns and conditional idiosyncratic volatility survives corrections for microstructure noise (Panel F) as the RWLS slope coefficients continue to indicate a strong positive relation between returns and conditional idiosyncratic volatility for all three subperiods (t-statistics range from 2.99 to 4.98). However, results in the row labeled "DIF" indicate a positive and highly significant (t-statistic = 14.28) difference between mean OLS and mean RWLS coefficients, implying that microstructure noise biases upward significantly the estimated relation between returns and firm-specific volatility. The effect of microstructure noise on the results of cross-sectional regressions of individual stock returns on the Boyer, Mitton, and Vorkink (2010) skewness measure (Panel G) is particularly strong in economic terms, as the corrected (RWLS) estimate is approximately 60% larger than the OLS estimate for the full

[19] We allow for distinct slope coefficients here and for the illiquidity measure, as the interpretation of trading volume data potential differs across markets.

sample.

In Panel H, the mean difference between the OLS and RWLS coefficients for the full sample is negative and significant for all three lagged return variables, with t-statistics ranging from -9.20 (for the effect of returns four to six months earlier) to -11.27 (for the effect of returns two to three months prior). The estimated bias is consistent across subperiods, as all nine t-statistics (three lagged return measures and three subperiods) for the difference between OLS and RWLS coefficients are negative and significant, implying that OLS regressions consistently understate the actual effect of momentum in returns.[20]

C. Summary of Univariate Results

This analysis shows that biases attributable to microstructure noise can have substantial effects on asset pricing inferences obtained through simple Fama-MacBeth regressions or by comparisons of mean returns across attribute-sorted portfolios. The direction of the bias can be either positive (as for inverse share price, idiosyncratic volatility, idiosyncratic skewness, and illiquidity) or negative (as for firm size, dividend yield, trading volume, and lagged own returns), can either reinforce or offset the true relation, and can be understood in terms of the sign of the covariance between the variance of noise in prices and the explanatory variable. In some cases, e.g. idiosyncratic volatility, the bias can be highly significant in statistical terms, but relatively small in economic terms. In other cases, e.g. firm size, idiosyncratic skewness, and illiquidity, the bias is large in economic terms, equal to 50% or more of the corrected estimate.

V. Multivariate Evidence

A. Does the inclusion of illiquidity as a regressor serve as an effective correction?

As noted, ABK show that the slope coefficient estimated in a univariate OLS regression is biased in the same direction as the cross-sectional correlation between the variance of the noise

[20]In addition, the simulation analysis reported in Appendix A shows that microstructure noise in the prices used to create the lagged returns used as regressors generates a downward (attenuation) bias in the estimated coefficient on lagged returns, and that this attenuation bias is not mitigated by the ABK correction. This implies that the true effects of momentum may be larger than either the OLS or RWLS estimates reported here.

in prices and the regression explanatory variables. In the case of a multivariate cross-sectional regression, the bias depends on the corresponding partial correlation. This insight implies that the bias can potentially be eliminated by including the variance of the noise in prices as an additional explanatory variable in the cross-sectional regression. In this case the bias in returns should "load" entirely on the variance of the noise, inducing bias on this coefficient, but leaving other coefficient estimates unbiased.

Of course, the variance of the noise in prices cannot be directly observed. The most plausible reasons that noise exists in prices are related to illiquidity, broadly defined. Thus the question arises as to whether the inclusion as regressors of one or more illiquidity measures can eliminate the bias in the estimated coefficients on the remaining explanatory variables.[21]

To shed some light on this question, we repeat the cross-sectional regressions reported on Table III while also including the Amihud (2002) illiquidity measure as an additional regressor. We focus on the Amihud measure because it does not require high-frequency data, and has already been broadly adopted. It can potentially be used as a control variable in virtually all asset pricing studies. Results are reported on Table IV.

To assess whether inclusion of the *Illiq* variable has effectively controlled for microstructure biases, we focus on the "DIF" coefficients on Table IV. If the bias were effectively controlled, OLS estimates would no longer differ from RWLS estimates by a significant margin. In contrast to this reasoning, we observe that mean DIF coefficients and their t-statistics are uniformly little altered as compared to corresponding full sample results reported on Table III. We observe no instances where a DIF coefficient that was statistically significant on Table III was rendered insignificant when the illiquidity measure is included in the regression, or vice versa.

Though not the main focus of our analysis, it is of interest to note that results reported on Panel F of Table IV indicate that the positive return premium associated with the Amihud illiquidity measure is not robust to the inclusion of conditional idiosyncratic volatility, *CIVOL*, in the regression, as the estimated return premium for Nasdaq stocks becomes insignificant (t-statistic = -0.42), while that for NYSE/AMEX stocks actually becomes negative and significant

[21]Note that in a cross-sectional regression of time t returns on illiquidity and other variables, the role of the illiquidity observation for stock i is to proxy for the *variance* of the noise in stock i prices, not the date t (or t-1) outcome on noise in the stock i price.

(t-statistic = -2.49). This result is consistent with that reported by Spiegel and Wang (2006), and indicates that their result is not attributable to microstructure noise.

We conclude from this analysis that the Amihud illiquidity measure is not a sufficiently good proxy for the variance of the noise in prices to accomplish the goal of eliminating the bias in regression slope coefficients attributable to the noise. Apparently the noise in prices reflects aspects of illiquidity other than those captured by the Amihud measure, and/or sources in addition to illiquidity.

B. Sensitivity of Empirical Results to Combinations of Explanatory Variables

A key advantage of the univariate analyses reported in Section IV is that it is possible to form reasonably strong conjectures as to the likely correlation, and hence the direction of microstructure-induced bias, between levels of unobservable microstructure noise and individual explanatory variables. However, empirical asset-pricing studies using the Fama-MacBeth framework typically include several explanatory variables. ABK show that the direction of the bias in the individual OLS slope coefficients estimated in multivariate return regressions depend on the *partial* correlations between the unobservable quantity of noise and the regression explanatory variables. Such partial correlations will likely be quite difficult to anticipate *a priori*.

Table V reports results obtained in multivariate Fama-MacBeth regressions of monthly returns on various combinations of the explanatory variables. In general, conclusions as to which explanatory variables are reliably associated with returns after correcting for microstructure noise are sensitive to the set of explanatory variables included in the regression. Conclusions as to the direction of the bias in regression slope coefficients attributable to microstructure noise are similarly sensitive. Such sensitivity is to be expected given that a number of the explanatory variables are significantly correlated with each other.

Several results are noteworthy. First, the RWLS estimate of the relation between conditional idiosyncratic volatility and returns is reliably positive, as is the upward bias in the OLS estimate of this relation, for all combinations of explanatory variables. Second, the bias-corrected relation between expected idiosyncratic skewness and returns is reliably negative, for all combinations of explanatory variables. Third, the estimated coefficients on dollar trading volume are negative and significant, whether estimated by OLS or RWLS, in the multivariate specifications. Fourth, all coefficients on lagged returns estimated by RWLS are positive and significant, supporting the robustness of momentum effects.

While the univariate evidence indicates that microstructure noise is associated with significant bias for all explanatory variables examined here, the mean difference between the OLS and RWLS estimates in the multivariate specifications is not always significant. With the full set of explanatory variables included (specification 6), we detect significant noise-induced bias in OLS coefficients on inverse share price, idiosyncratic volatility, prior returns, and dollar volume for Nasdaq-listed stocks. In contrast, the estimated effect of noise on OLS estimates of coefficients for firm size, book-to-market ratio, dividend yield, trading volume, and firm-specific skewness are no longer significant.

Further, conclusions as to which explanatory variables are significantly affected by the correction for microstructure biases differs depending on the set of explanatory variables included in the regression. For example, the bias in the estimated coefficient on firm size is highly significant in specifications (2), (3), and (5), but not in specification (6). The main implication of this mixed pattern of significance is that the likely effect of adjusting OLS coefficient estimates obtained in multivariate return regressions for biases attributable to microstructure noise will be very difficult to ascertain *a priori*, and will typically need to be assessed empirically.

Finally, and perhaps most important, we note that inference as to whether particular explanatory variables have a significant effect on mean stock returns is altered by the correction for microstructure noise, in some specifications. For example, in specification (5) the negative coefficient on firm size is statistically significant when estimated by OLS, but the coefficient is overstated by 40% relative to the corresponding bias-corrected (RWLS) estimate, which is only marginally significant (t-statistic = -1.87). In specification (6) OLS estimation indicates only weak momentum effects associated with returns two to three months prior t-statistic = 1.38) while the bias-corrected (RWLS) estimate of the same parameter is more than twice as large and is signficiant (t-statistic = 2.53). Here too, it would be very difficult to anticipate which coefficient estimates will potentially be rendered significant or insignificant by the correction for microstructure bias. In a nutshell, the effect of correcting for noise in prices can be substantial, can alter statistical inference, and must be assessed empirically.

VI. Conclusion

Researchers seek to understand the determinants of variation in mean returns across assets. Most empirical studies either compare returns across portfolios constructed by sorting on attributes of interest, or estimate regressions of returns on attributes or risk factors. Although the research community has been aware at least since Blume and Stambaugh (1983) that noise in security prices can potentially bias the results of return comparison tests, many published papers implement no corrections for potential biases.

The implicit assumption that no correction is necessary may be based on the fact that Blume and Stambaugh (1983) studied daily returns, while most empirical asset pricing studies rely on monthly data. However, prior expectations notwithstanding, the magnitude of the biases is ultimately an empirical question. The main purpose of this paper is to assess the magnitude of such biases, by implementing representative return comparison tests with and without the Asparouhova, Bessembinder, and Kalcheva (2010) correction for the effects of microstructure noise.

Univariate results obtained by regressions of returns on firm attributes or single-dimensional portfolio sorts show that the direction of the bias can be either positive (as for inverse share price, idiosyncratic volatility, idiosyncratic skewness, and illiquidity) or negative (as for firm size, dividend yield, trading volume, and lagged own returns). In some cases the bias is in the same direction as the corrected estimate, implying that the raw estimates lead to overestimation, while in other cases the bias offsets the corrected relation, implying that the uncorrected estimates imply underestimation. Further, in some cases the bias can be highly significant in statistical terms, but relatively small in economic terms. In other cases, notably for firm size, idiosyncratic skewness, and illiquidity, the bias is large in economic terms, equal to 50% or more of the corrected estimate. Of the firm-level variables studied here, only the estimated relation between returns and the market-to-book ratio is largely free of microstructure-induced bias.

The findings reported here indicate that correcting for the effects of noise in prices has significant effects on mean return comparisons conducted with monthly return data. For the corrections to have substantial effects, the variance of the noise in prices must be substantial. Our findings therefore provide indirect support for the Hendershott, Li, Menkveld, and Seasholes (2010) finding that order imbalances and market maker inventory lead to substantial transitory return volatility at the monthly horizon. One possibility is that month-end prices contain more noise than other days of the month. Such a phenomenon could arise, for example, from strategic month-end trading, as documented by Carhart, Kaniel, Musto, and Reed (2002).

We also conclude that the observed relation between mean returns and firm attributes is not entirely attributable to noise-induced biases and small firms in the case of firm size, book-to-market ratios,idiosyncratic volatility, idiosyncratic skewness, lagged returns, and illiquidity, as each of these remains significant when portfolio returns are computed on a value-weighted basis. In contrast, we find insignificant returns to value-weighted hedge portfolios formed on the basis of share price, dividend yields, and trading volume, implying that the apparent return premia associated with these attributes are fully attributable to noise induced bias and to the smaller firms within the portfolios.

Multivariate analysis indicates a more complex pattern, where the direction of the bias attributable to noisy prices depends on the set of explanatory variables are included in the analysis. Further, statistical inference as to whether certain variables (e.g. in this study firm size and lagged returns) affect mean returns can be altered by corrections for microstructure noise, depending on specification. The overall takeaway implication is that the direction of microstructure bias and the effect on statistical inference in multivariate analyses is difficult to envision *a priori*, and will likely need to be assessed empirically.

Researchers may be interested in assessing mean portfolio returns on either an equal- or a value-weighted basis. Microstructure noise induces upward bias in equal-weighted mean returns, but not in value-weighted mean returns. The implication is that researchers who wish to obtain unbiased estimates of equal-weighted mean returns (e.g. to establish an equal-weighted benchmark) and/or to understand the differential between equal- and value-weighted mean returns must implement corrections for the effects of noise in observed prices. The ABK correction used here provides unbiased estimates of equal-weighted mean returns, is easy to implement, and imposes no special data requirements, as the weighting variable is simply the gross prior period return.

Appendix A: The ABK Correction in the Presence of Momentum Effects and Lagged Returns as Regressors

The consistency proof in ABK assumes that the random component of true security returns is independently distributed through time. The substantial literature on momentum effects in stock returns indicates that the independence assumption is violated in actual data. Further, to assess the importance of momentum effects in returns, the empirical analysis provided here includes past returns as explanatory variables. We therefore implement a simulation analysis to assess the effectiveness of the RWLS correction for microstructure noise in a setting where true returns are autocorrelated, and where lagged returns are included as an explanatory variable.

In particular, we create simulated true gross returns for $N = 2500$ securities that depend on both illiquidity (as measured by bid-ask spreads) and on lagged returns. A proportional spread parameter, S_n, is assigned to each security as a random draw from a uniform distribution on the interval 0 to $\bar{S}$, where $\bar{S}$ is either 0.10 or 0.20, and simulated true returns are generated as:

$R_{nt} = \alpha + \beta_{spread} S_n + \beta_{lag2} R_{nt-2} + \epsilon_{nt}$, $n = 1, ..., N$.

We then create simulated observed returns that incorporate microstructure noise. For simplicity, bid-ask spreads are the sole source of noise in the simulated data. In particular, observed returns are:

$R^0_{nt} = R_{nt} \frac{1+\delta_{nt}}{1+\delta_{nt-1}}$,

where δ_{nt} is the noise in the time t price for security n, set randomly to either one half (to simulate trades at the ask) or negative one half (to simulate trades at the bid) the proportional spread parameter for the stock. We simulate returns that depend on the second lag of returns, for three reasons. First, the momentum effects that have been observed in the literature indicate the explanatory power of returns at lags of three to twelve months, not the immediately prior month. Second, it is well-documented (e.g., Roll, 1984) that microstructure noise creates a spurious negative autocorrelation in observed returns at one lag. This effect is well understood, and we wish to focus on different issues. Third, we have verified in simulations that the ABK correction is not effective when prior period returns are included as a regressor. The prior period return cannot serve both as the weighting variable and as an explanatory variable.

The simulations are conducted using parameters of $\alpha = 1.005$, $\beta_{spread} = 0.1$, and $\beta_{lag2} = 0.025$. The residual term ϵ_{nt} is drawn from the $N(0,\ 0.10)$ distribution. Simulated data is created for 50,000 time periods, and cross-sectional Fama-MacBeth regressions are estimated, by both OLS and RWLS, for each period, using simulated observed returns as the dependent variable. We record the mean and standard deviation of each estimated parameter across the 50,000 outcomes, and compute a t-statistic for the hypothesis that each mean parameter is equal to the corresponding true coefficient.

Results are reported in Table BI. Panel A reports results when the cross-sectional average proportional spread is 0.05, while Panel B reports results with a larger average spread of 0.10. Columns (1) and (2) report results obtained when the lagged true return is used as a regressor. These results comprise a benchmark for comparison to those reported in Columns (3) and (4), where the lagged observed return is employed as a regressor.

The results reported in Column (1) of Table BI confirm that microstructure noise leads to upward bias in the OLS estimate of the coefficient on bid-ask spreads, and more so when the price data contains more noise. While the true slope coefficient on spreads is 0.10, the OLS estimate averages 0.127 when the cross-sectional mean spread is 0.05 (Panel A) and 0.153 when the mean spread is 0.10 (Panel B). T-statistics confirm that the bias in OLS estimates of the premium associated with the bid-ask spread is highly significant.

Results reported in Column (2) of Table BI show that the RWLS correction proposed by ABK remains effective even when true returns contain a momentum effect. The average coefficients estimated by RWLS do not differ significantly from the corresponding true parameters, either in the case of the bid-ask spread or the lagged return, in both Panels A and B. We conclude that the existence of a momentum effect in returns, whereby true returns depend on lagged true returns, does not, in and of itself, reduce the effectiveness of the ABK correction for microstructure noise in prices.

However, results reported in Columns (1) and (2) rely on the true lagged return as a regressor. In practice, researchers investigating momentum effects must rely on observed past returns that include the effects of microstructure noise. Columns (3) and (4) of Table BI report results corresponding to those in Columns (1) and (2), except that observed lagged returns replace true returns as regressors. The most notable effect is that the estimated coefficient on lagged returns is decreased. While the true parameter is 0.025, the OLS estimate is reduced to 0.021 when the cross-sectional mean spread is 0.05, and to 0.014 when the cross-sectional mean spread is 0.10. T-statistics confirm that the downward bias is highly significant. The decrease in the mean coefficient on the lagged observed return reflects the standard attenuation bias associated with measurement error in an explanatory variable. The ABK RWLS correction was developed to mitigate the effects of white noise in the prices that define the returns used as *dependent* variables in cross-sectional regressions. Comparing results across Columns (3) and (4) of Table BI, RWLS estimation does not alleviate the attenuation bias associated with measurement errors in past returns.

On a more positive note, RWLS estimation continues to provide average parameter estimates associated with the bid-ask spread that do not differ meaningfully from the true parameter. For example, with a relatively large mean spread of 0.10 (Panel B), the OLS estimate of the return premium associated with the spread (Column 3) is 0.153, while the RWLS estimate (Column 4) is 0.101, which does not differ appreciably from the true parameter of 0.10.[22] We conclude from these simulations that the effectiveness of the RWLS correction proposed by ABK for the effects of microstructure noise in the prices used to compute the returns used as the dependent variable in cross-sectional regressions is not meaningfully diminished by the existence of momentum effects in true returns. However, in those cases where the regression explanatory variables are measured with error, the ABK correction does not mitigate the resulting attenuation bias.

ABK also observe (Corollary 5 to Theorem 2) that the intercept obtained while incorporating their correction is biased if the first-order autocorrelation in true returns is non-zero. By extension, a cross-sectional mean computed while weighting by prior gross returns is then also biased. However, the evidence of momentum effects in returns primarily documents the forecast power of returns three or more months prior (e.g. Jegadeesh and Titman (1993)). The available empirical evidence indicates that the estimated first order autocorrelation in CRSP stock returns

[22] Note, though, that the RWLS estimate does contain an upward bias that can be statistically detected (t-statistic = 5.11) in the very large (50,000 replications) simulated sample. While the bias that arises in this case is too small to be economically meaningful, it may be of interest to note that we have verified through additional, unreported, simulations that the sign of the remaining bias is the same as the sign of the true coefficient on the lagged return.

is close to zero, but slightly negative. Lo and MacKinlay (1990) report autocorrelations of -0.014, -0.034, -0.029 for daily, weekly, and monthly autocorrelations of individual stocks for the period 1962-1987. We compute a similarly small average first order autocorrelation of -0.03 for our sample of monthly returns in the period 1963-2007. Microstructure noise would be anticipated to induce slightly negative first order autocorrelations in observed returns if true returns are uncorrelated at one lag. However, researchers should be aware that using the ABK method to compute portfolio returns is imperfect if implemented in data from markets where true security returns are characterized by substantial first-order autocorrelation.[23]

[23] Essentially the same limitation applies to the Blume and Stambaugh (1983) "buy-and-hold" correction, which gives a biased estimate of portfolio returns if the true return on individual stocks at period t is correlated with the return from time 0 (the portfolio formation period) to time t-1.

Appendix B: Value-weighted Mean Returns

Following Blume and Stambaugh (1983), observed prices differ from true prices due to microstructure noise, including the non-informational component of the bid-ask spread. As a consequence, returns are measured with error. In particular, the observed (gross) return for stock n at time t is:

$$R^0_{nt} = \frac{P^0_{nt}}{P^0_{nt-1}} = \frac{P_{nt}(1+\delta_{nt})}{P_{nt-1}(1+\delta_{nt-1})} = R_{nt}\frac{1+\delta_{nt}}{1+\delta_{nt-1}}, \tag{B-1}$$

where δ_{nt} is zero-mean noise in the time t observation of security n's price. We assume that $\delta_{nt} \perp s_n$, where s_n is the number of outstanding shares of firm n.

The observed return of a value-weighted portfolio is $R^0_{vw,t} = \sum_{n=1}^N w^0_{nt} R^0_{nt}$, where $w^0_{nt} = \frac{s_n P^0_{nt-1}}{\sum_{n=1}^N s_n P^0_{nt-1}}$. The corresponding true value-weighted return is $R_{vw,t} = \sum_{n=1}^N w_{nt} R_{nt}$, where $w_{nt} = \frac{s_n P_{nt-1}}{\sum_{n=1}^N s_n P_{nt-1}}$.

Proposition 1. *If $\sum_{n=1}^{\infty} \frac{Var[s_n \delta_{nt}]}{n^2} < \infty$, then $R^0_{vw,t} - R_{vw,t}$ converges in probability to 0.*

Proof. From the Kolmogorov law of large numbers it follows that $plim \frac{1}{N}\sum_{n=1}^N s_n\delta_{nt} =$

$= plim \frac{1}{N}\sum_{n=1}^N s_n\delta_{nt-1} = 0.$

Multiplying the numerators and the denominators of portfolio weights by $\frac{1}{N}$ we have:

$R^0_{vw,t} = \frac{1}{N}\sum_{n=1}^N \frac{s_n P^0_{nt-1}}{\frac{1}{N}\sum_{n=1}^N s_n P^0_{nt-1}} R^0_{nt} = \frac{1}{N}\sum_{n=1}^N \frac{s_n P^0_{nt-1}}{\frac{1}{N}\sum_{n=1}^N s_n P_{nt-1} + \frac{1}{N}\sum_{n=1}^N s_n \delta_{nt-1}} R^0_{nt}$, and

$R_{vw,t} = \frac{1}{N}\sum_{n=1}^N \frac{s_n P_{nt-1}}{\frac{1}{N}\sum_{n=1}^N s_n P_{nt-1}} R_{nt}.$

$$plim(R^0_{vw,t} - R_{vw,t}) =$$

$$plim\left(\frac{1}{N}\sum_{n=1}^N \frac{s_n P^0_{nt-1}}{\frac{1}{N}\sum_{n=1}^N s_n P_{nt-1} + \frac{1}{N}\sum_{n=1}^N s_n\delta_{nt-1}} R^0_{nt} - \frac{1}{N}\sum_{n=1}^N \frac{s_n P_{nt-1}}{\frac{1}{N}\sum_{n=1}^N s_n P_{nt-1}} R_{nt}\right) =$$

$$plim \frac{\frac{1}{N}\sum_{n=1}^N s_n(P^0_{nt-1}R^0_{nt} - P_{nt-1}R_{nt})}{\frac{1}{N}\sum_{n=1}^N s_n P_{nt-1}} =$$

$$plim \frac{\frac{1}{N}\sum_{n=1}^N s_n(P^0_{nt} - P_{nt})}{\frac{1}{N}\sum_{n=1}^N s_n P_{nt-1}} = plim \frac{\frac{1}{N}\sum_{n=1}^N s_n\delta_{nt}}{\frac{1}{N}\sum_{n=1}^N s_n P_{nt-1}} = 0.$$

□

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Table I. Summary Statistics and Correlations

Panel A represents the time-series averages of monthly cross-sectional means for an average of 3553 stocks over 522 months from July 1964 to December 2007 for the full sample. Returns are monthly and are measured in excess of the treasury interest rate. Firm size is in billions. Book-to-market ratio (BM) is winsorized at the 0.005 and the 0.995 fractiles of the full sample by setting the outlying values to the 0.005 and the 0.995 fractiles respectively. Share price is in dollars. Volume is dollar-trading-volume $ millions per month. Dividend yield and excess return are in percentage. Idiosyncratic volatility (standard deviation in percentage) is the one month-ahead expected idiosyncratic volatility as estimated by Fu (2009). Volume for Nasdaq is available after 1983. Idiosyncratic skewness variable is as estimated by Boyer et al. (2010) and is available from January 1988 to December 2005. *Ret*23, *Ret*46 and *Ret*712 are in cumulative returns in percent as explained in Section III.B. *Illiq* is the Amihud (2002) illiquidity measure. Panel B presents time-series of monthly cross-sectional correlations between the transformed firm characteristics used in the regressions and explained in Section III.B.

Panel A: Summary Statistics

Variable	Mean	Median	St.dev.
Return	0.875	1.062	5.956
Firm size	1.003	0.513	1.040
BM	0.938	0.844	0.381
Share price	24.015	21.276	9.208
Dividend yield	2.245	2.137	0.935
Volume	104.952	26.654	168.159
Idios. vol. (3f)	11.903	12.132	2.075
Idios. skew. (3f)	1.012	0.999	0.239
Ret23	2.684	2.803	9.101
Ret46	4.000	3.673	11.478
Ret712	8.216	7.556	16.643
Illiq	7.597	5.617	6.935

Panel B: Correlation Matrix of Transformed Firm Characteristics

Variable	*Return*	*Size*	*log(BM)*	*InvPrice*	*Yld*	*Dvol*	*CIVOL*	*Eiskew*	*Ret23*	*Ret46*	*Ret712*
Return	1	–	–	–	–	–	–	–	–	–	–
Size	-0.012	1	–	–	–	–	–	–	–	–	–
log(BM)	0.032	-0.280	1	–	–	–	–	–	–	–	–
InvPrice	0.005	-0.790	0.201	1	–	–	–	–	–	–	–
Yld	0.007	0.113	0.081	-0.110	1	–	–	–	–	–	–
Dvol	-0.019	0.882	-0.322	-0.702	0.051	1	–	–	–	–	–
CIVOL	0.058	-0.413	-0.061	0.481	-0.155	-0.265	1	–	–	–	–
Eiskew	-0.034	-0.715	0.132	0.702	-0.046	-0.658	0.399	1	–	–	–
Ret23	0.006	0.066	0.040	-0.131	-0.018	0.123	-0.015	-0.076	1	–	–
Ret46	0.013	0.075	0.047	-0.145	-0.014	0.100	-0.015	-0.102	-0.011	1	–
Ret712	0.017	0.104	-0.033	-0.186	-0.008	0.130	-0.020	-0.157	0.020	0.014	1
Illiq	0.020	-0.324	0.150	0.360	-0.064	-0.340	0.194	0.248	0.024	0.024	-0.002

Table II. Mean Returns and Alphas to Attribute-Sorted Portfolio

The table reports time-series means of monthly returns to the extreme of the ten attribute-sorted portfolios and to the corresponding hedge portfolio. The table also reports alphas for the extreme attribute-sorted portfolios and the corresponding hedge portfolio, estimated as intercepts in time series regressions of monthly portfolio returns on the Fama and French (1993) factors. Portfolio returns for month t are measured on an equal-weighted (EW), return-weighted (RW, weight is period *t-1* gross return), and value-weighted (VW, weight is month *t-1* market capitalization) basis. We also report alphas obtained when the dependent variable is the difference in differences: RW(10-1) less VW(10-1), RW(10-1) less VW(10-1) and EW(10-1) less VW(10-1). Firms are assigned to portfolios based on attributes measured at month *t-2*, with three exceptions. Panels B and I rely on attributes measured annually, and firms are sorted based on attributes as of the prior July. Panel F results rely on sorts of the time *t-1* expectation of period t idiosyncratic volatility. With the exception of Panel D, ten percent of firms are assigned to each portfolio. For Panel D, all firms with dividend yield equal to zero are placed in the first portfolio, and remaining firms are allocated equally to the other nine. For Panel H, the sorting variable is the six-month return from month *t-12* to *t-7*. T-statistics are reported in parentheses.

	Extreme Deciles and the Hedge Portfolio									Hedge Portfolio Differentials		
	EW_{10}	EW_1	EW_{10-1}	RW_{10}	RW_1	RW_{10-1}	VW_{10}	VW_1	VW_{10-1}	*EW-RW*	*RW-VW*	*EW-VW*
Panel A: Size												
Means	0.508	2.272	-1.764	0.492	1.660	-1.167	0.430	1.235	-0.805	-0.596	-0.362	-0.959
T-stats	(2.57)	(5.77)	(-5.20)	(2.51)	(4.38)	(-3.59)	(2.31)	(3.41)	(-2.54)	(-15.14)	(-5.66)	(-11.94)
Alphas	0.013	1.173	-1.159	0.003	0.595	-0.591	0.050	0.197	- 0.146	-0.568	-0.444	-1.012
T-stats	(0.38)	(4.68)	(-4.44)	(0.11)	(2.55)	(-2.42)	(2.65)	(0.93)	(-0.69)	(-14.10)	(-7.08)	(-12.45)
Panel B: Book-to-Market												
Means	1.583	0.184	1.398	1.387	0.040	1.346	1.053	0.308	0.744	0.052	0.602	0.654
T-stats	(5.29)	(0.57)	(6.71)	(4.71)	(0.13)	(6.50)	(4.10)	(1.33)	(3.24)	(2.23)	(3.26)	(3.55)
Alphas	0.508	-0.342	0.851	0.335	-0.473	0.809	0.035	0.134	-0.099	0.042	0.908	0.950
T-stats	(3.67)	(-2.86)	(6.14)	(2.51)	(-4.21)	(5.83)	(0.30)	(1.63)	(-0.68)	(1.75)	(5.33)	(5.60)
Panel C: Inverse Price												
Means	2.133	0.701	1.432	1.403	0.691	0.712	0.521	0.479	0.042	0.720	0.669	1.389
T-stats	(4.56)	(3.40)	(3.54)	(3.14)	(3.36)	(1.86)	(1.13)	(2.55)	(0.10)	(15.22)	(4.88)	(9.43)
Alphas	0.955	0.237	0.717	0.267	0.235	0.032	-0.602	0.139	-0.742	0.685	0.774	1.459
T-stats	(3.05)	(4.45)	(2.10)	(0.92)	(4.25)	(0.10)	(-1.98)	(3.75)	(-2.36)	(14.26)	(6.03)	(10.24)
Panel D: Dividend Yield												
Means	0.695	1.006	-0.311	0.638	0.790	-0.151	0.524	0.592	-0.067	-0.160	-0.083	-0.243
T-stats	(3.80)	(2.94)	(-1.42)	(3.55)	(2.35)	(-0.70)	(3.08)	(1.87)	(-0.27)	(-8.34)	(-0.59)	(-1.70)
Alphas	-0.046	-0.034	-0.011	-0.092	-0.229	0.136	-0.082	-0.089	0.006	-0.148	0.129	-0.018
T-stats	(-0.55)	(-0.28)	(-0.09)	(-1.15)	(-1.95)	(1.12)	(-0.90)	(-0.98)	(0.05)	(-7.58)	(1.05)	(-0.15)
Panel E: Trading Volume												
Means	0.464	1.896	-1.432	0.457	1.451	-0.994	0.429	0.623	-0.193	-0.437	-0.800	-1.238
T-stats	(1.93)	(6.02)	(-5.31)	(1.92)	(4.75)	(-3.82)	(2.22)	(2.80)	(-0.95)	(-14.12)	(-6.27)	(-9.05)
Alphas	-0.054	0.880	-0.934	-0.053	0.464	-0.517	0.051	-0.155	0.206	-0.416	-0.724	-1.141
T-stats	(-0.96)	(4.56)	(-4.38)	(-0.93)	(2.54)	(-2.53)	(2.06)	(-1.25)	(1.60)	(-13.16)	(-5.56)	(-8.19)
Panel F: Expected Idiosyncratic Volatility												
Means	3.657	0.256	3.400	3.163	0.245	2.918	1.963	0.480	1.482	0.481	1.435	1.917
T-stats	(7.24)	(1.71)	(8.02)	(6.45)	(1.63)	(7.12)	(4.29)	(2.96)	(3.79)	(11.83)	(6.02)	(7.63)
Alphas	2.427	-0.327	2.754	1.972	-0.336	2.308	1.059	0.071	0.988	0.445	1.320	1.766
T-stats	(8.14)	(-7.23)	(8.97)	(7.02)	(-7.43)	(7.92)	(4.52)	(1.50)	(3.98)	(10.82)	(5.42)	(6.88)
Panel G: Expected Idiosyncratic Skewness												
Means	-0.356	0.854	-1.210	-0.739	0.846	-1.585	-0.881	0.809	-1.690	0.375	0.105	0.480
T-stats	(-0.63)	(3.06)	(-2.43)	(-1.36)	(3.03)	(-3.35)	(-1.70)	(2.83)	(-3.69)	(7.19)	(0.33)	(1.41)
Alphas	-1.071	0.117	-1.188	-1.439	0.125	-1.565	-1.835	0.308	-2.143	0.376	0.578	0.955
T-stats	(-2.70)	(0.90)	(-2.58)	(-3.96)	(0.96)	(-3.63)	(-6.55)	(2.09)	(-6.06)	(6.95)	(1.94)	(2.95)
Panel H: Lagged Returns												
Means	1.115	0.618	0.497	1.005	0.230	0.775	1.064	-0.273	1.338	-0.278	-0.562	-0.841
T-stats	(3.47)	(1.60)	(2.29)	(3.14)	(0.62)	(3.69)	(3.45)	(-0.84)	(5.00)	(-10.50)	(-3.20)	(-4.69)
Alphas	0.341	-0.395	0.737	0.247	-0.758	1.006	0.558	-0.992	1.551	-0.268	-0.545	-0.814
T-stats	(2.94)	(-1.82)	(3.40)	(2.15)	(-3.81)	(4.81)	(3.95)	(-5.29)	(5.72)	(-9.97)	(-3.02)	(-4.44)
Panel I: Illiquidity Ratio												
Means	1.874	0.492	1.382	1.472	0.481	0.991	0.973	0.426	0.546	0.391	0.444	0.835
T-stats	(5.24)	(2.24)	(4.69)	(4.21)	(2.21)	(3.43)	(2.85)	(2.26)	(1.85)	(13.57)	(3.48)	(6.11)
Alphas	0.778	-0.072	0.850	0.413	-0.079	0.493	0.058	0.039	0.018	0.357	0.474	0.832
T-stats	(3.88)	(-1.20)	(3.99)	(2.16)	(-1.38)	(2.40)	(0.31)	(1.65)	(0.09)	(12.33)	(3.78)	(6.16)

Table III. Univariate Fama-MacBeth Regressions

Reported are results of implementing cross-sectional Fama-MacBeth regressions of monthly stock returns on NYSE-Amex stocks from 1964 to 2007 and on Nasdaq stocks from 1983 to 2007. We report results when the dependent variable is *Return*. Panel A through I report results for the different firm-specific characteristics. The coefficients reported in Column OLS are the time-series means of the monthly cross-sectional OLS regression estimates, while coefficients reported in Column RWLS are the time-series means of the monthly cross-sectional WLS regression estimates, where the weighting variable is one plus previous month return. The coefficients reported in Column DIF are the time-series means of the difference between the OLS and WLS coefficient. *T*-statistics are reported in parentheses.

		Period	OLS (Mean *t*-stat.)	RWLS (Mean *t*-stat.)	DIF (Mean *t*-stat.)
Panel A	*Size*	*1964–2007*	-0.186 (-3.70)	-0.121 (-2.50)	-0.064 (-13.90)
		1964–1982	-0.229 (-2.60)	-0.184 (-2.13)	-0.045 (-10.38)
		1983–2000	-0.103 (-1.49)	-0.017 (-0.25)	-0.086 (-11.26)
		2001–2007	-0.284 (-2.67)	-0.225 (-2.35)	-0.059 (-3.48)
Panel B	*log(BM)*	*1964–2007*	0.496 (6.84)	0.488 (6.81)	0.009 (1.23)
		1964–1982	0.502 (3.85)	0.466 (3.61)	0.036 (4.69)
		1983–2000	0.475 (5.23)	0.482 (5.30)	-0.008 (-0.65)
		2001–2007	0.535 (3.08)	0.558 (3.34)	-0.023 (-1.02)
Panel C	*InvPrice*	*1964–2007*	0.300 (2.75)	0.146 (1.39)	0.155 (15.16)
		1964–1982	0.377 (1.97)	0.261 (1.40)	0.116 (11.87)
		1983–2000	0.147 (1.07)	-0.056 (-0.42)	0.203 (13.01)
		2001–2007	0.494 (1.73)	0.360 (1.39)	0.135 (3.30)
Panel D	*Yld*	*1964–2007*	0.006 (0.49)	0.012 (0.90)	-0.005 (-6.20)
		1964–1982	0.013 (0.43)	0.023 (0.78)	-0.010 (-5.80)
		1983–2000	0.006 (1.41)	0.007 (1.80)	-0.002 (-2.55)
		2001–2007	-0.009 (-1.74)	-0.008 (-1.64)	-0.001 (-0.87)
Panel E	*Nydvol*	*1964–2007*	-0.098 (-2.05)	-0.069 (-1.47)	-0.029 (-5.18)
		1964–1982	-0.026 (-0.58)	-0.032 (-0.74)	0.006 (1.93)
		1983–2000	-0.070 (-0.83)	-0.024 (-0.28)	-0.046 (-4.93)
		2001–2007	-0.362 (-2.21)	-0.283 (-1.86)	-0.079 (-3.46)
	Nadvol	*1983–2007*	-0.252 (-2.87)	-0.181 (-2.09)	-0.071 (-7.33)
		1983–2000	-0.141 (-1.50)	-0.067 (-0.71)	-0.074 (-6.67)
		2001–2007	-0.537 (-2.72)	-0.474 (-2.48)	-0.062 (-3.21)
Panel F	*CIVOL*	*1965–2007*	0.126 (8.37)	0.106 (7.28)	0.019 (14.28)
		1965–1982	0.147 (5.21)	0.128 (4.67)	0.018 (9.46)
		1983–2000	0.113 (6.10)	0.090 (4.98)	0.023 (10.49)
		2001–2007	0.104 (3.20)	0.093 (2.99)	0.011 (3.50)
Panel G	*Eiskew*	*1988–2005*	-0.627 (-2.28)	-0.994 (-3.86)	0.367 (7.53)
		1988–2000	-0.719 (-2.18)	-1.142 (-3.67)	0.423 (6.91)
		2001–2005	-0.383 (-0.77)	-0.603 (-1.34)	0.220 (3.10)
Panel H	*Ret23*	*1964–2007*	0.003 (0.90)	0.008 (2.55)	-0.005 (-11.17)
		1964–1982	0.007 (1.44)	0.010 (1.99)	-0.003 (-6.45)
		1983–2000	0.002 (0.53)	0.009 (2.74)	-0.007 (-10.54)
		2001–2007	-0.006 (-0.58)	-0.002 (-0.17)	-0.005 (-2.86)
	Ret46	*1964–2007*	0.005 (1.71)	0.008 (2.95)	-0.003 (-9.20)
		1964–1982	0.007 (1.39)	0.009 (1.79)	-0.002 (-4.60)
		1983–2000	0.003 (0.99)	0.007 (2.68)	-0.004 (-7.23)
		2001–2007	0.003 (0.41)	0.006 (0.99)	-0.004 (-3.72)
	Ret712	*1964–2007*	0.007 (4.52)	0.008 (5.72)	-0.002 (-9.67)
		1964–1982	0.010 (3.84)	0.011 (4.37)	-0.001 (-4.98)
		1983–2000	0.006 (3.85)	0.008 (5.25)	-0.002 (-7.84)
		2001–2007	0.0003 (-0.09)	0.001 (0.25)	-0.001 (-3.42)
Panel I	*NYilliq*	*1964–2007*	0.122 (4.16)	0.079 (2.96)	0.043 (5.82)
		1964–1982	0.104 (2.49)	0.073 (1.79)	0.032 (10.49)
		1983–2000	0.166 (2.99)	0.099 (2.05)	0.068 (3.86)
		2001–2007	0.056 (2.28)	0.044 (1.92)	0.012 (2.09)
	NAilliq	*1983–2007*	0.074 (5.32)	0.054 (3.93)	0.020 (5.22)
		1983–2000	0.074 (4.56)	0.053 (3.26)	0.021 (4.27)
		2001–2007	0.074 (2.72)	0.057 (2.19)	0.017 (3.33)

Table IV. Fama-MacBeth Regressions Including the *Illiq* Variable

Reported are results of implementing cross-sectional Fama-MacBeth regressions of monthly stock returns on NYSE-Amex stocks from 1964 to 2007 and on Nasdaq stocks from 1983 to 2007. Panel A through I report results for the different firm-specific characteristics similar to Table III but now with *Illiq* variable included. The coefficients reported in Column OLS are the time-series means of the monthly cross-sectional OLS regression estimates, while coefficients reported in Column RWLS are the time-series means of the monthly cross-sectional RWLS regression estimates, where the weighting variable is one plus previous month return. The coefficients reported in Column DIF are the time-series means of the difference between the OLS and RWLS coefficient. T-statistics are reported in parentheses.

		OLS (Mean t-stat.)	RWLS (Mean t-stat.)	DIF (Mean t-stat.)
Panel A	*Size*	-0.129 (-2.74)	-0.077 (-1.68)	-0.052 (-11.91)
	NYilliq	0.115 (4.65)	0.084 (3.90)	0.031 (4.18)
	NAilliq	0.062 (6.14)	0.048 (4.59)	0.014 (3.79)
Panel B	*log(BM)*	0.429 (6.30)	0.439 (6.50)	-0.010 (-1.49)
	NYilliq	0.114 (3.53)	0.081 (2.49)	0.033 (5.51)
	NAilliq	0.081 (4.41)	0.058 (3.13)	0.022 (4.64)
Panel C	*InvPrice*	0.194 (1.81)	0.060 (0.58)	0.134 (13.37)
	NYilliq	0.111 (4.59)	0.088 (4.21)	0.023 (3.05)
	NAilliq	0.063 (5.79)	0.053 (4.58)	0.010 (2.83)
Panel D	*Yld*	0.012 (1.03)	0.016 (1.32)	-0.003 (-4.88)
	NYilliq	0.126 (4.36)	0.083 (3.19)	0.042 (5.70)
	NAilliq	0.074 (5.36)	0.054 (3.98)	0.020 (5.19)
Panel E	*NYdvol*	-0.062 (-1.38)	-0.038 (-0.87)	-0.024 (-4.67)
	NAdvol	-0.174 (-1.93)	-0.117 (-1.30)	-0.057 (-5.86)
	NYilliq	0.112 (4.43)	0.079 (3.59)	0.033 (4.40)
	NAilliq	0.062 (5.10)	0.049 (3.99)	0.013 (3.64)
Panel F	*CIVOL*	0.128 (8.85)	0.110 (7.85)	0.017 (13.10)
	NYilliq	-0.028 (-1.30)	-0.050 (-2.33)	0.022 (5.69)
	NAilliq	-0.010 (-1.11)	-0.023 (-2.20)	0.012 (3.36)
Panel G	*Eiskew*	-0.781 (-2.88)	-1.105 (-4.34)	0.325 (6.93)
	NYilliq	0.106 (2.36)	0.077 (2.31)	0.029 (1.63)
	NAilliq	0.077 (4.65)	0.061 (3.70)	0.016 (3.65)
Panel H	*Ret23*	0.003 (1.10)	0.008 (2.88)	-0.005 (-11.79)
	Ret46	0.005 (2.02)	0.008 (3.34)	-0.003 (-9.63)
	Ret712	0.007 (4.89)	0.008 (6.06)	-0.002 (-9.73)
	NYilliq	0.118 (4.19)	0.076 (3.01)	0.042 (5.57)
	NAilliq	0.072 (5.45)	0.053 (3.98)	0.019 (5.22)

Table V. Multivariate Fama-MacBeth Regressions

Reported are results of implementing cross-sectional Fama-MacBeth regressions of monthly stock returns on NYSE-Amex stocks from 1964 to 2007 and on Nasdaq stocks from 1983 to 2007. The coefficients reported in Column OLS, Column RWLS and Column DIF are as explained in the previous tables. The dependent variable is *Return*. *T*-statistics are reported in parentheses.

	OLS (Mean t-stat.)	RWLS (Mean t-stat.)	DIF (Mean t-stat.)	OLS (Mean t-stat.)	RWLS (Mean t-stat.)	DIF (Mean t-stat.)	OLS (Mean t-stat.)	RWLS (Mean t-stat.)	DIF (Mean t-stat.)
		(1)			**(2)**			**(3)**	
Size	-0.154	-0.093	-0.060	0.290	0.311	-0.021	-0.229	-0.196	-0.032
	(-2.89)	(-1.80)	(-11.90)	(7.82)	(8.41)	(-8.35)	(-3.40)	(-3.00)	(-3.82)
log(BM)	0.342	0.377	-0.035	0.705	0.697	0.007	0.319	0.359	-0.039
	(4.51)	(5.04)	(-4.47)	(11.77)	(11.51)	(1.42)	(2.62)	(2.99)	(-2.86)
InvPrice	–	–	–	–	–	–	–	–	–
Yld	–	–	–	–	–	–	–	–	–
Nydvol	–	–	–	–	–	–	–	–	–
Nadvol	–	–	–	–	–	–	–	–	–
CIVOL	–	–	–	0.168	0.152	0.016	–	–	–
				(13.24)	(12.26)	(10.56)			
Eiskew	–	–	–	–	–	–	-1.285	-1.486	0.200
							(-7.11)	(-8.03)	(5.70)
Ret23	–	–	–	–	–	–	–	–	–
Ret46	–	–	–	–	–	–	–	–	–
Ret712	–	–	–	–	–	–	–	–	–
Nyilliq	–	–	–	–	–	–	–	–	–
Nailliq	–	–	–	–	–	–	–	–	–
		(4)			**(5)**			**(6)**	
Size	–	–	–	-0.115	-0.082	-0.033	0.206	0.184	0.021
				(-2.60)	(-1.87)	(-9.24)	(2.76)	(2.43)	(1.73)
log(BM)	–	–	–	0.343	0.363	-0.020	0.560	0.548	0.012
				(5.20)	(5.52)	(-3.07)	(8.68)	(8.18)	(1.33)
InvPrice	-0.815	-0.866	0.051	–	–	–	-0.472	-0.505	0.033
	(-7.24)	(-7.68)	(4.20)				(-4.60)	(-4.93)	(2.37)
Yld	–	–	–	–	–	–	0.005	0.005	0.001
							(1.75)	(1.55)	(1.17)
Nydvol	-0.461	-0.467	0.005	–	–	–	-0.564	-0.567	0.002
	(-5.42)	(-5.52)	(0.78)				(-4.03)	(-4.05)	(0.15)
Nadvol	-0.602	-0.579	-0.023	–	–	–	-0.586	-0.571	-0.014
	(-4.16)	(-3.95)	(-1.82)				(-3.32)	(-3.23)	(-0.74)
CIVOL	0.151	0.136	0.014	–	–	–	0.158	0.145	0.012
	(10.05)	(9.44)	(6.01)				(10.63)	(10.21)	(5.15)
Eiskew	-1.548	-1.599	0.050	–	–	–	-1.317	-1.371	0.054
	(-11.55)	(-11.10)	(1.40)	–	–	–	(-11.35)	(-10.51)	(1.41)
Ret23	–	–	–	0.003	0.007	-0.004	0.003	0.007	-0.003
				(1.26)	(2.88)	(-10.84)	(1.38)	(2.53)	(-6.47)
Ret46	–	–	–	0.005	0.008	-0.002	0.005	0.007	-0.001
				(2.52)	(3.62)	(-8.90)	(2.32)	(3.09)	(-5.99)
Ret712	–	–	–	0.005	0.007	-0.001	0.004	0.004	-0.001
				(4.38)	(5.27)	(-7.76)	(3.39)	(3.95)	(-3.23)
Nyilliq	–	–	–	0.097	0.075	0.021	-0.017	-0.034	0.016
				(3.75)	(2.85)	(3.61)	(-0.43)	(-0.83)	(1.62)
Nailliq	–	–	–	0.061	0.045	0.016	-0.032	-0.047	0.014
				(4.82)	(3.30)	(3.53)	(-2.57)	(-3.67)	(3.46)

Table BI. Simulation Evidence: The ABK Correction when True Returns are Autocorrelated and Lagged Returns as used as a Regressor

Simulated monthly returns R_t with firm-specific return standard deviations of 0.10 are regressed on spreads and lagged R_{t-2} returns. The true spread slope is equal to 0.1, while the true lagged return slope is equal to 0.025. All simulations are performed with 50,000 replications on 2,500 simulated securities. Reported are average estimated slope coefficients, the t-statistic for the hypothesis that the mean estimated coefficient equals the true parameter, and the standard deviation of the estimates across the 50000 simulations. Columns (1) and (2) in each panel include results when where R_t is subject to microstructure noise in prices, but R_{t-2} is not. Columns (3) and (4) report the results when both R_t and R_{t-1} are measured with microstructure noise in prices.

Panel A: Low Mean Spread

$\mu_{spread} = 0.05$

	(1) OLS	(2) RWLS	(3) OLS	(4) RWLS
Lagged Return Measure	True R_{t-2}	True R_{t-2}	Observed R_{t-2}	Observed R_{t-2}
Mean Spread Slope	0.12663	0.10061	0.12614	0.10077
T-statistic	77.8847	1.7879	76.0733	2.21913
Standard Deviation of Estimates	0.076461	0.076734	0.076835	0.077114
Mean Lagged Return Slope	0.024916	0.024889	0.021118	0.021109
T-statistic	-0.864466	-1.1322	-42.8266	-42.7739
Standard Deviation of Estimates	0.021846	0.021951	0.02027	0.020343

Panel B: High Mean Spread

$\mu_{spread} = 0.1$

	(1) OLS	(2) RWLS	(3) OLS	(4) RWLS
Lagged Return Measure	True R_{t-2}	True R_{t-2}	Observed R_{t-2}	Observed R_{t-2}
Mean Spread Slope	0.15288	0.10012	0.1531	0.1011
T-statistic	245.0121	0.57261	247.5345	5.116985
Standard Deviation of Estimates	0.048259	0.048352	0.047963	0.048086
Mean Lagged Return Slope	0.025068	0.02498	0.01435	0.014264
T-statistic	0.5685097	-0.16751	-110.9262	-111.7554
Standard Deviation of Estimates	0.026562	0.026641	0.021468	0.021482