Thin-Trading Effects in Beta: Bias *v.* Estimation Error*

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Abstract

Two regression coefficients often used in Finance, the Scholes-Williams (1977) quasi-multiperiod "thin-trading" beta and the Hansen-Hodrick (1980) overlapping-periods regression coefficient, can both be written as instrumental-variables estimators. Competitors are Dimson's beta and the Hansen-Hodrick original OLS beta. We check the performance of all these estimators and the validity of the t-tests in small and medium samples, in and outside their stated assumptions, and we report their performances in a hedge-fund style portfolio-management application. In all experiments as well as in the real-data estimates, less bias comes at the cost of a higher standard error. Our hedge-portfolio experiment shows that the safest procedure even is to simply match by size and industry; any estimation just adds noise. There is a clear relation between portfolio variance and the variance of the beta estimator used in market-neutralizing the portfolio, dwarfing the beneficial effect of bias.

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Introduction

The "beta" coefficient of the market model—the slope in the regression of an asset's return onto the market return—is crucial in tests or applications of the CAPM or in event studies since Fama, Fisher, Jensen and Roll (1973). The standard, in much of the literature, is an OLS beta, which assumes IID idiosyncratic returns. If, in addition, also market returns are IID and if n-period returns are sums of n one-period returns—a feature that strictly holds only for log-change returns—the population beta is also independent of the observation interval used in the returns.[1] In the rest of the paper we refer to the model with IID and time-additive returns as the standard model.

This standard model has also been at the basis of beta variants that were proposed to handle measurement errors in returns, caused by thin trading, bid-ask bounce, differential reaction speeds to news, and so on. Its no-autocorrelation assumption echoes the stylized view of the early efficient-markets literature as popularised by *e.g.* Fama (1965). True, observed returns do not fully meet these early efficient-markets assumptions, but the violations are not massive, and many apparent infractions are potentially explained by thin trading.[2] The main effect of thin trading is that the purely contemporaneous correlation between asset and market return seems to get smeared out forward and backward when reported returns are used instead of true returns. The measured return on stock j becomes linked to the past L true market returns because j's period-t return may contain its bottled-up true returns over the past L

[1]Under these assumptions, indeed, also the total stock returns are IID whatever the holding period, and any correlation between asset return and market return is purely contemporaneous; as a result, the covariance in the beta's numerator and the variance in its denominator simply go up by the same multiple, the number of periods, leaving the ratio unaffected.

[2]For instance, autocorrelation in market-index returns is exactly what one would expect when not all stocks get quoted every day: the day $t+1$ reported return for stocks that did not trade on day t contains the stock's true day-t return and, therefore, introduces a spurious echo of the day-t market evolution into the reported index return for the next day. For the same reason, actively traded stocks seem to lead the market return by one day, and reported returns on thinly traded stock seem to be related to the once-lagged market. The residual for a thinly traded stock is negatively autocorrelated, when the stock seems to belatedly catch up with the market. And, lastly, the market-model residuals for actively traded stocks are positively autocorrelated, if only because the cross-sectional average autocorrelation is positive.

periods, which are logically linked to the past L true market returns. But also the leading market returns must be included, because the true market return for a period t can be smeared out over all periods $t, ..., t + L$ if the market index contains thinly-traded stocks.

The standard model is adopted by Scholes and Williams (1977), whose beta estimator accounts for non-trading that lasts at most one period. Fowler and Rorke (1983) generalize this to longer no-trading intervals. The Scholes-Williams-Fowler-Rorke (SWFR) solution is an instrumental-variable estimator with, as the instrument, a moving sum of the contemporaneous market return plus L leading and L lagging market returns, L being the longest period of non-trading.

The SWFR estimator has been used also in different contexts. Apte, Kane and Sercu (1993) consider a setting where Purchasing Power Parity holds for traded-goods prices and where CPI inflation sluggishly reacts to traded-goods prices. They show that a SWFR-like estimator is needed to extract the full impact of inflation on the exchange rate. In optimal-hedge problems, the variance-minimizing hedge ratio is likewise found by regressing price changes of the exposed asset on price changes of the hedge instrument (Johnson, 1960, and Stein, 1961). Sercu and Wu (2000) obtain good results from the SWFR estimator when some currencies partly follow others with a lag, as was the case in the Exchange Rate Mechanism.

There are two or three alternative estimators that are not strictly consistent but still reduce the thin-trading bias and might be useful in reducing other errors-in-data biases too. Dimson (1979) proposes a multiple regression with leading and lagging market returns as additional regressors. Dimson's beta is the sum of all these multiple coefficients. Another alternative is to measure returns at lower frequencies, using *e.g.* weekly holding-period returns rather than daily ones. The advantage of working with returns from longer holding periods is that while the amount of noise generated by thin trading is not affected, the true returns become larger, implying a better signal-to-noise ratio (see e.g. Stoll and Whaley, 1990). Similar possible motivators for lower observation frequencies are data problems like bid-ask bounce, reporting lags, or differential adjustment speeds due to differences in trading costs or liquidity: with longer holding periods, the signal-to-noise ratio improves. The cost is that extracting non-overlapping longer holding periods from a data base substantially reduces the number of observations. Hansen and Hodrick (1980), discussing a different econometric issue, point out that the use of overlapping multi-period returns mitigates the problem. They further show that, in such overlapping-return regressions, GLS is not the way to deal with the induced auto-correlation in the errors; OLS, in contrast, remains unbiased (in the absence of data problems, that is), and an asymptotic standard error for the OLS estimator is provided that takes into account the serial autocorrelation.

Most of the above literature thus far has focused on bias in the beta, but the issue of standard error can be important too. Famously, Brown and Warner's (1980) Monte-Carlo experiment on event studies shows that using OLS-estimated betas introduces more noise than the errors caused if all betas are set equal to unity, without any estimation. Thus, more in general, it is possible that a biased estimator is still more powerful than an unbiased one.

Accordingly, in this note we report Monte-Carlo results on the comparative performance of (*i*) one-day OLS, (*ii*) Scholes-Williams-Fowler-Rorke (SWFR), (*iii*) Dimson multiple OLS, (*iv*) Hansen and Hodrick's overlapping-return regressions (HH), and (*v*) an instrumental-variable variant of the overlapping-return regression. We rate the contending estimators not just on the basis of bias in the coefficient—there is little news on that front—but also observed estimator standard deviation and reliability of the theoretical large-sample standard deviation. Besides the simulation we also set up a real-world performance race. Specifically, we consider the problem of a portfolio manager who has a target beta—zero, in our application, like for a hedge fund—and considers using empirical betas to try and keep her portfolio market neutral. For such a manager, any non-zero true market sensitivity is equally bad, whether it comes from estimation error or from bias; variability of returns over time induced by estimation errors in the hedge ratios is as undesirable as otherwise unhedged risk. We check which estimator does best, and we also let the simple equal-beta model enter the race: while almost surely biased to some extent, it does have zero estimation error. Lastly, we check to what extent estimation error is diversifiable, a criterion that is not part of the regular statistician's toolbox.

From the Monte-Carlo experiments we conclude that smaller bias always comes at the cost of imprecision. OLS, which is the $L = 0$ version of all variants, has maximal bias but unparallelled standard error. Within each family of estimators, widening the window L steadily reduces bias but always at the cost of precision. The same holds across families. The ordering in terms of bias, from better to worst, is SWFR, Dimson, HHOLS, HHIV, and standard OLS, but in terms of standard error the ordering is basically reversed.

The experiments also show that in all cases the theoretical standard error is a reliable guide to the actual one, on average, even in mid-sized samples and with departures from the standard model. OLS is quite consistently good at this, in the sense that the standard deviation, across samples, of the computed standard errors is smallest. SWFR does worst in this respect, and the Dimson and overlapping-return models' performance is in-between. In short, noisier estimators also have noisier estimated standard errors.

All this bears on Monte-Carlo sampling. The real-world results similarly confirm that, within and across estimator families, bias disappears only at the cost of precision. This is found directly in the betas, and confirmed by variances of portfolios that, in hedge-fund style, are market-neutralized using beta estimates. The overwhelming winner of the portfolio experiment however is the equal-beta model: just match by industry and size, and assume without any estimation that matched stocks have identical market sensitivities. There may be bias here, but the standard error equals an unparalleled zero, and this seems to be the more important aspect.

1 The contenders

The familiar market-model regression is

$$R_{j,t} = \alpha + \beta_j R_{m,t} + \epsilon_{j,t}, \tag{1}$$

where $R_{j,t}$ is the simple percentage change, *cum* dividend, in the j-th stock price over period t and $R_{m,t}$ is the return on the market portfolio. The stylized assumptions are that the only correlation among these variables is the contemporaneous one between $R_{j,t}$ and $R_{m,t}$, without any auto- or cross-correlation anywhere.

1.1 Scholes-Williams-Fowler-Rorke (SWFR)

As illustrated in *e.g.* Dimson (1979), thin trading induces bias in the OLS beta because it leads to errors in both the regressand and in the regressor.[3] The interactions of these errors mean that for active stocks the beta is biased upward, while for thinly traded stocks the bias is negative, as Scholes and Williams show. Assuming that the duration of inaction never exceeds one period, they then derive a consistent instrumental-variable estimator for the market-model beta, where the instrument is a moving sum of the market returns for days $t-1$, t, and $t+1$.

[3] On the regressand side, a stock that is not traded on day t reports a return of zero, apparently without relation with the market's movement. Actually, the unobserved true day-t return shows up on day $t+1$ (as part of the return reported for that day), but again seemingly without relation with the true day-t market return. Thus, at least two returns are mis-measured. On the regressor side, thin trading likewise induces two types of error, each induced by the errors in the regressands. First, as yesterday's non-traded stocks catch up today, an echo of yesterday's market return is added into today's reported market return; this error is assumed to be uncorrelated with today's true return. Second, due to non-trading today, part of today's true market return is missing, an error that is negatively correlated with the true market return. It will also be obvious that the thin-trading error in the regressand is positively correlated with the one in the regressor.

Fowler and Rorke (1983) generalize this solution to longer no-trading intervals by extending the moving-sum window. So Scholes and Williams (1977) and Fowler and Rorke (1983) show that, when reported prices may really date from up to L periods ago, the downward bias is avoided if beta is estimated as

$$\beta_{j,H}^{SW} = \frac{\text{cov}(R_j, Z_H^{SW})}{\text{cov}(R_m, Z_H^{SW})} \text{ with } Z_{H,t}^{SW} = \sum_{l=-L}^{L} R_{m,t+l}. \tag{2}$$

The reason why this IV solution works here is that (*i*) any auto- and cross-correlations present in actually observed returns are, by assumption, induced by thin trading, and (*ii*) the $(2L+1)$-period moving sum of market returns does pick up all correlations induced by thin trading. Since the mis-timing of the true day-t return does not affect the moving sum of market returns, the instrument is uncorrelated with the error in the reported market return, the key requirement for a proper instrument.

The consistency of the SWFR estimator is not the issue, so our Monte-Carlo tests are intended as checks for small-sample unbiasedness and especially for the validity of the standard error, whose consistent estimator is:[4]

$$\text{asymptotic stdev}(\beta_{j,H}^{SW}) = \sqrt{\frac{\sigma_\epsilon^2(1 + 2\sum_{l=1,L}^{L} \rho_{l,j}\rho_{l,Z})}{\sum_{1+L}^{N-L}(R_{m,t} - \overline{R}_m)^2 R_{R_M,Z}^2}} \tag{3}$$

where σ_ϵ^2 is the residual variance, $\rho_{l,X}$ the lth-order autocorrelation of the returns from asset $X = \{j, m\}$ and $R_{R_M,Z}^2$ the squared correlation between the market return and the instrument.

1.2 The Hansen-Hodrick overlapping-observation regression

Under standard assumptions, in the absence of thin trading the true one- and multi-period betas are the same. For reasons outlined in the introduction, an overlapping-return regression may be preferred,

$$\left(\sum_{l=0}^{H-1} R_{j,t+l}\right) = \alpha + \beta_{j,H}^{HH}\left(\sum_{l=0}^{H-1} R_{m,t+l}\right) + \epsilon_{j,t,H}. \tag{4}$$

The overlapping returns obviously generate autocorrelation in the error terms. Hansen and Hodrick (1980) reject GLS as biased, in the case of overlapping returns, and propose OLS with an autocorrelation-consistent standard deviation which is consistently estimated as

$$\Theta = (X'X)^{-1}X'\Omega^{-1}X(X'X)^{-1}, \tag{5}$$

[4] The autocorrelations are new relative to the textbook IV case. See Scholes and Williams for the derivations for $L = 1$. The generalisation to $L \geq 2$ follows easily.

where X is the $N \times 2$ matrix of observations $\{1, R_{m,t}\}$ and Ω is a band matrix with the estimated variances and autocovariances of the regression errors u:

$$\Omega = \sigma_\epsilon^2 \begin{bmatrix} 1 & \rho_1 & ... & \rho_{H-1} & 0 & ... & ... & ... & 0 \\ \rho_1 & 1 & \rho_1 & ... & \rho_{H-1} & 0 & ... & ... & 0 \\ ... & \rho_1 & 1 & \rho_1 & ... & \rho_{H-1} & 0 & ... & 0 \\ \rho_{H-1} & ... & \rho_1 & 1 & \rho_1 & ... & \rho_{H-1} & 0 & 0 \\ 0 & & & & & & & & 0 \\ 0 & 0 & \rho_{H-1} & ... & \rho_1 & 1 & \rho_1 & ... & \rho_{H-1} \\ 0 & ... & 0 & \rho_{H-1} & ... & \rho_1 & 1 & \rho_1 & ... \\ 0 & ... & ... & 0 & \rho_{H-1} & ... & \rho_1 & 1 & \rho_1 \\ 0 & ... & ... & ... & 0 & \rho_{H-1} & ... & \rho_1 & 1 \end{bmatrix}. \tag{6}$$

1.3 An Instrumental-Variable Overlapping-Return Estimator

We easily derive a consistent IV estimator of the overlapping-return model. We start from the explicit objective that we want to estimate the beta over a horizon of H periods, and accordingly write the H-period price change as the sum of H one-period changes. Next, we use equalities like $\text{cov}(X(+1), Y(+3)) = \text{cov}(X, Y(+2))$; and lastly we regroup and interpret the estimator as an IV one:

$$\begin{aligned} \beta_{j,H}^{IV} &= \frac{\text{cov}(R_j + R_j(+1) + ... + R_j(+H-1), R_m + R_m(+1) + ...R_m(+H-1))}{\text{var}(R_m + R_m(+1) + ...R_m(+H-1))} \\ &= \frac{\sum_{k=0}^{H-1}\sum_{l=0}^{H-1} \text{cov}(R_j(+k), R_m(+l))}{\sum_{k=0}^{H-1}\sum_{l=0}^{H-1} \text{cov}(R_m(+k), R_m(+l))} \\ &= \frac{\sum_{k=-H+1}^{H-1}(H-|k|)\text{cov}(R_j, R_m(+k))}{\sum_{k=-H+1}^{H-1}(H-|k|)\text{cov}(R_j, R_m(+k))} \\ &= \frac{\text{cov}(R_j, Z_H^{IV})}{\text{cov}(R_m, Z_H^{IV}))}, \end{aligned} \tag{7}$$

where the instrument is a pyramid-weighted moving sum rather than the SWFR equally weighted one:

$$Z_H^{IV} = \sum_{k=-H+1}^{H-1} (H - |k|) R_m(+k). \tag{8}$$

The asymptotic standard error is the same as for SWFR.

In the next section we compare the performance of these contenders as far as bias is concerned, true standard error, and reliability of the standard error produced by standard software. This last issue is especially relevant when the method is applied to data where the standard model does not hold.

2 Simulation results

2.1 Monte Carlo results for the standard model

Each complete simulation contains 10,000 experiments, and one such experiment consists of the following steps. First, an IID market factor f is generated with zero mean and constant volatility equal to 0.01 *per diem*, i.e. about 0.15 *per annum (p.a.)*. This market factor has a fat-tailed Student's distribution with seven df, which produces a realistic level for the kurtosis and tail coefficient (Bauwens *et al.*, 2006). From this, returns for 2,000 assets are generated, all with a unit beta and idiosyncratic noise with 1.5 times the market standard deviation, generating the typical *p.a* volatility of about 0.30 for an individual stock. There are three classes of trading thinness: 1000 of the stocks trade every day, 500 trade with a probability of 0.9, and 500 trade with probability 0.75. If a stock is not traded, the recorded return is zero; simultaneously, its true return is added to a buffer until the stock does trade, at which time the cumulative return is recorded. Starting from 2000 initial value weights taken from NYSE in 1992, we then update the value weights. The initial market caps are ranked in descending order, so that large stocks tend to be active ones, and the smallest firms most prone to missing prices. The market return is computed as a value-weighted average of asset returns. For the market-model regressions we generate 250 such "daily" observations for all 2000 stocks, and pick three stock files out of these, one per thinness class, to run the regressions. We then start a new experiment, until we have gone through 10,000 of them. For SWFR and Dimson, L is set at 1, 2, 3, 4 and 19 each side,[5] while for the overlapping-observation regressions H is likewise set at 2, 3, 4, 5 (one week) and 20 (monthly holding periods). For each regression we compute the slopes and theoretical asymptotic standard error. For each set of 10,000 such computations (one per type of regression and thinness class) we then produce the mean and standard deviation. Table 1 summarizes the results.

Predictably, OLS does worst in terms of bias, with beta approximating unity minus the probability of no trade. There is no evidence of any upward bias for the active stocks. Scholes and Williams demonstrate the theoretical possibility: since the weighted average beta estimate tautologically equals unity, downward bias for sleepy stocks must come with upward bias for active stocks. The fact that our unweighted average beta is below unity, in this table, is still

[5] The chance that a stock with no-trade probabilty 0.25 would not trade for 3 (4) periods is $0.75^2 0.25^3 = 0.0088$ ($0.75^2 0.25^4 = 0.0022$), so $L = 3$ looks reasonable. We add 19 for comparability with the overlapping-return techniques, where monthly returns (approximately 20 trading days) are quite popular.

Table 1: **Simulation results: base case**

	Mean $\hat{\beta}$			mean SE ($\hat{\beta}$)s			stdev across $\hat{\beta}$			stdev of SE ($\hat{\beta}$)s		
prob no trade	.00	.10	.25	.00	.10	.25	.00	.10	.25	.00	.10	.25
	One-period regression											
OLS	.99	.89	.74	.10	.10	.10	.09	.11	.12	.008	.008	.009
SWFR ±1	.99	.98	.92	.17	.18	.18	.17	.17	.18	.020	.021	.023
SWFR ±2	.99	.98	.97	.22	.23	.25	.22	.23	.23	.036	.037	.040
SWFR ±3	.99	.99	.98	.27	.28	.30	.27	.28	.28	.055	.058	.062
SWFR ±4	.99	.98	.98	.32	.33	.35	.32	.32	.32	.078	.082	.087
Dimson±1	.99	.98	.92	.14	.14	.15	.14	.14	.15	.012	.012	.013
Dimson±2	.99	.98	.97	.17	.17	.18	.17	.17	.17	.016	.016	.017
Dimson±3	.99	.99	.98	.19	.20	.21	.19	.20	.20	.019	.020	.021
Dimson±4	.98	.99	.98	.22	.23	.24	.22	.22	.22	.023	.024	.026
Dimson±19	.99	.99	.99	.51	.53	.55	.52	.51	.52	.100	.105	.110
	H-period overlapping-returns regression											
IV 2	.99	.93	.83	.12	.12	.13	.12	.12	.13	.010	.010	.011
IV 3	.99	.95	.87	.14	.15	.15	.14	.15	.15	.012	.013	.014
IV 4	.99	.96	.90	.16	.17	.18	.16	.17	.17	.015	.016	.017
IV 5	.99	.96	.92	.18	.19	.20	.18	.18	.18	.018	.018	.020
IV 20	.99	.98	.96	.41	.42	.44	.39	.40	.41	.074	.077	.084
OLSHH 2	.99	.95	.88	.13	.13	.13	.14	.14	.15	.014	.014	.014
OLSHH 3	.99	.96	.90	.15	.15	.16	.16	.16	.16	.018	.018	.019
OLSHH 4	.99	.96	.92	.17	.17	.17	.18	.18	.18	.023	.023	.023
OLSHH 5	.99	.97	.93	.19	.19	.19	.20	.20	.20	.027	.027	.027
OLSHH 20	.99	.98	.96	.33	.33	.33	.38	.39	.39	.089	.091	.089

Key. 250 "daily" returns for 2000 stocks are generated by a unit-beta market model with a thick-tailed IID market factor with stdev 0.01/day plus idiosyncratic noise (R^2=0.3). 1000 of the 2000 stocks trade daily, 500 nine days out of ten, and 500 three days out of four. On no-trade days a zero return is reported, and the invisible price change is cumulated until the first trade. Value-weighted market returns are computed from these 2000 returns, and three individual-stock return series are stored, one for each thinness class. This experiment is repeated 10,000 times. OLS uses daily data. SWFR uses as the instrument a moving-window sum of market returns with $\pm L$ leads/lags. Dimson uses a multiple regresion with $\pm L$ leading/lagging market returns as additional regressors; the beta is the sum of these $2L+1$ coefficients. The overlapping-return IV regression uses $H-1$ proximity-weighted leading and lagging market returns as the instrument. OLSHH runs OLS with autocorrelation-adjusted SE's. The IVSE's also account for thin-trading-induced autocorrelation in regressand and regressor.

compatible with a value-weighted average beta equal to unity because of the positive covariance between weights and estimated beta:

$$1 = \sum_j w_j \hat{\beta}_j = (\sum_j w_j)\overline{\beta} + \sum_j (w_j - \overline{w})(\hat{\beta}_j - \overline{\beta}) = \overline{\beta} + \sum_j (w_j - \overline{w})(\hat{\beta}_j - \overline{\beta}) > \overline{\beta}. \qquad (9)$$

Equally predictably, SWFR does a good job in eliminating even severe thin-trading biases like $p = .25$ with modest values like $L = 3$. But Dimson's beta does as well in terms of bias. For both estimators, the reduction in bias upon increasing the window size $2L + 1$ comes quite rapidly: with no-trading probabilities not exceeding 0.25, there seems to be no point in going beyond $L = 4$. The overlapping-return regressions, whether OLS or IV, are good at handling

Table 2: **Simulation results: autocorrelation in R_m**

	Mean $\hat{\beta}$			mean SE $(\hat{\beta})$s			stdev across $\hat{\beta}$			stdev of SE $(\hat{\beta})$s		
prob no trade	.00	.10	.25	.00	.10	.25	.00	.10	.25	.00	.10	.25
	One-period regression											
OLS	.99	.90	.75	.10	.10	.10	.10	.11	.12	.008	.008	.009
SWFR ±1	.99	.98	.93	.16	.17	.18	.16	.16	.17	.020	.021	.022
SWFR ±2	.99	.99	.98	.20	.21	.22	.20	.20	.21	.032	.033	.035
SWFR ±3	.99	.99	.99	.23	.24	.26	.23	.23	.24	.043	.045	.049
SWFR ±4	.99	.99	.99	.26	.27	.28	.26	.26	.26	.055	.058	.061
Dimson±1	.99	.98	.93	.13	.14	.14	.13	.13	.14	.012	.013	.013
Dimson±2	.99	.99	.97	.16	.17	.17	.16	.16	.17	.016	.017	.018
Dimson±3	.99	.99	.98	.18	.19	.20	.18	.18	.19	.021	.021	.022
Dimson±4	.99	.99	.98	.20	.21	.21	.20	.20	.21	.025	.026	.027
Dimson±19	1.00	.99	.99	.35	.36	.38	.35	.35	.35	.084	.087	.090
	H-period overlapping-returns regression											
IV 2	.99	.94	.85	.12	.12	.13	.12	.12	.13	.010	.011	.011
IV 3	.99	.96	.89	.14	.14	.15	.14	.14	.14	.013	.013	.014
IV 4	.99	.96	.92	.16	.16	.17	.15	.16	.16	.016	.016	.017
IV 5	.99	.97	.93	.17	.18	.19	.17	.17	.18	.019	.019	.021
IV 20	1.00	.99	.98	.32	.34	.35	.32	.32	.32	.072	.075	.078
OLSHH 2	.99	.96	.89	.12	.13	.13	.13	.14	.14	.014	.014	.014
OLSHH 3	.99	.96	.92	.14	.14	.15	.15	.15	.16	.019	.019	.019
OLSHH 4	.99	.97	.93	.16	.16	.16	.17	.17	.17	.023	.023	.023
OLSHH 5	.99	.97	.94	.17	.17	.17	.18	.18	.19	.027	.027	.027
OLSHH 20	1.00	.99	.98	.25	.25	.25	.30	.30	.30	.080	.080	.080

Key. There is a partial autocorrelation schedule in the market factor that starts at $\rho_1 = 0.04$, and linearly falls to zero at lag 15. The implied autocorrelation of monthly market factors is 0.16. Otherwise, the procedure is like in Table 1.

moderate thin-trading like $p = .10$; but for the $p = .25$ stocks, convergence toward unity seems to be slow: even monthly returns ($H = 20$) achieve only .97, on average.

Equally interesting are the standard deviations of the betas. There is a good connection between the theoretical standard errors (SE) and the actual one computed from the cross-section of betas. OLS is quite precise, despite the slight autocorrelation and heteroscedasticity induced by thin trading. But for thinly-traded assets, autocorrelation and heteroscedasticity also make the OLSSE's underestimate the true error margins. SWFR betas are 1.5 to 3 times noisier than OLS ones, a feature easily explained from the asymptotic formula (3). Specifically, OLS is identical to IV with the regressor as instrument, so the main difference between the OLS and IVSE's comes from the factor R^2 in the numerator of (3), which drops from 1 (OLS) to about 1/3 (IV with $L = 1$) or 1/7 (IV with $L = 3$). These R^2s, in turn, imply that the IV asymptotic errors rise by a factor $\sqrt{3} = 1.7$ for $L = 1$ and $\sqrt{7} = 2.6$ for $L = 3$. The result is that the SWFR estimator is actually the noisiest of the entire set. The reason why we show

Table 3: **Simulation results: crash added in R_m**

	Mean $\hat{\beta}$			mean SE $(\hat{\beta})$s			stdev across $\hat{\beta}$			stdev of SE $(\hat{\beta})$s		
prob no trade	.00	.10	.25	.00	.10	.25	.00	.10	.25	.00	.10	.25
	One-period regression											
OLS	1.06	.97	.83	.10	.10	.11	.10	.12	.15	.015	.015	.015
SWFR ±1	1.00	.99	.96	.15	.16	.17	.15	.15	.17	.025	.026	.026
SWFR ±2	.99	.99	.99	.19	.20	.21	.19	.19	.20	.035	.036	.036
SWFR ±3	.99	.99	.99	.22	.23	.24	.22	.22	.23	.045	.046	.048
SWFR ±4	.99	.99	1.00	.24	.25	.27	.24	.24	.25	.055	.057	.059
Dimson ±1	1.00	.99	.96	.13	.14	.14	.13	.13	.15	.020	.020	.020
Dimson ±2	.99	.99	.99	.15	.16	.17	.15	.16	.16	.024	.025	.025
Dimson ±3	.99	.99	.99	.17	.18	.19	.17	.17	.18	.028	.029	.029
Dimson ±4	.99	.99	.99	.19	.20	.21	.19	.19	.20	.032	.033	.033
Dimson ±19	.1.00	1.00	.99	.37	.35	.37	.34	.34	.34	.085	.087	.090
	H-period overlapping-returns regression											
IV 2	1.03	.98	.90	.12	.12	.13	.12	.12	.14	.018	.018	.018
IV 3	1.02	.99	.93	.14	.14	.15	.13	.14	.15	.021	.021	.021
IV 4	1.01	.99	.95	.15	.16	.17	.15	.15	.16	.024	.024	.024
IV 5	1.01	.99	.96	.17	.17	.18	.16	.17	.17	.027	.027	.028
IV 20	1.00	.99	.99	.31	.32	.34	.30	.31	.31	.075	.077	.080
OLSHH 2	1.02	.99	.93	.12	.12	.13	.13	.14	.15	.020	.020	.020
OLSHH 3	1.01	.99	.95	.14	.14	.14	.15	.15	.16	.025	.025	.025
OLSHH 4	1.00	.99	.96	.15	.15	.16	.16	.16	.17	.029	.029	.029
OLSHH 4	1.00	.99	.97	.16	.17	.17	.18	.18	.18	.033	.033	.032
OLSHH 20	1.00	.99	.99	.24	.24	.24	.29	.29	.29	.080	.080	.079

Key. With probability 0.001 per day, a crash occurs with size uniformly distributed between (minus) 10 and 20. Otherwise, the procedure is like in Table 2.

no results for the case SWFR $L = 19$ (i.e. about one month either way) is that the variance simply is absurdly high (five decimals before the dot). Dimson offers more precision for the same unbiasedness, and the overlapping-observation models do even better (at the cost of some extra bias). To eliminate bias for all stocks, one needs to be willing to accept SE's that are at least half as large again as those of OLS.

In terms of SE, the IV version of overlapping-observation regression does better than SWFR's IV estimator, for the same reason: putting more weight on the contemporaneous market return, its instrument has higher correlation with the regressor than has the SWFR instrument with the same window size L. The asymptotic SE's for the IV- and OLS versions of overlapping-return regressions are quite similar, which is as expected because the estimators are asymptotically equivalent. They also beat Dimson's beta for precision (if not for bias, it should be recalled). In terms of actual estimation error the IV version does somewhat better than the Hansen-Hodrick OLS one unless the window becomes quite large ($H = 20$, one month).

Table 4: **Simulation results: thin trading as a market-wide random factor**

	Mean $\hat{\beta}$			mean SE $(\hat{\beta})$s			stdev across $\hat{\beta}$			stdev of SE $(\hat{\beta})$s		
prob no trade	.00	.10	.25	.00	.10	.25	.00	.10	.25	.00	.10	.25
	One-period regression											
OLS	1.07	.98	.84	.10	.10	.11	.10	.12	.15	.015	.015	.015
SWFR ±1	1.00	.97	.91	.15	.16	.16	.15	.16	.17	.025	.025	.025
SWFR ±2	1.00	.99	.96	.19	.20	.21	.19	.19	.20	.035	.035	.036
SWFR ±3	1.00	1.00	.98	.22	.23	.24	.22	.21	.22	.045	.046	.047
SWFR ±4	1.00	1.00	.99	.24	.25	.27	.24	.24	.25	.055	.056	.058
Dimson ±1	1.00	.97	.91	.13	.14	.14	.13	.14	.16	.020	.020	.020
Dimson ±2	.99	.99	.97	.15	.16	.17	.16	.16	.17	.024	.025	.025
Dimson ±3	.99	1.00	.99	.17	.18	.19	.18	.18	.18	.028	.029	.029
Dimson ±4	.99	1.00	.99	.19	.20	.21	.19	.19	.20	.032	.033	.033
Dimson ±19	.99	.99	1.00	.34	.35	.37	.34	.34	.35	.084	.087	.089
	H-period overlapping-returns regression											
IV 2	1.03	.98	.88	.12	.12	.13	.12	.13	.15	.018	.018	.018
IV 3	1.02	.98	.91	.14	.14	.15	.14	.14	.15	.021	.021	.021
IV 4	1.01	.99	.93	.15	.16	.17	.15	.15	.16	.024	.024	.024
IV 5	1.01	.99	.94	.17	.17	.18	.17	.17	.17	.027	.027	.028
IV 20	1.00	1.00	.98	.31	.32	.34	.31	.30	.31	.076	.077	.080
OLSHH 2	1.02	.98	.91	.12	.12	.13	.14	.14	.15	.020	.020	.020
OLSHH 3	1.01	.99	.93	.14	.14	.14	.15	.15	.16	.025	.025	.025
OLSHH 4	1.01	.99	.94	.15	.15	.16	.17	.16	.17	.029	.029	.028
OLSHH 4	1.01	.99	.95	.16	.17	.17	.18	.18	.18	.032	.033	.032
OLSHH 20	1.00	1.00	.98	.24	.24	.24	.29	.28	.29	.078	.080	.079

Key. A daily number p_{2000} is generated that distributes uniformally between 0.5 and 1 and represents the probability that the most illiquid stock trades on that day. (The ordering by liquidity remains determined by initial market value.) Of the other 1999 stocks, the top 1000 stocks remain 100 percent active, and the others get a probability that is linearly interpolated between 1 and p_{2000} on the basis of their liquidity ranking. We select stock 1000 as the representative active firm, and stocks 1200 and 2000 as the representative medium- and poorly-traded stocks, respectively. (Their unconditional probabilities of being traded equal 0.90 and 0.75, like in the base case.) Otherwise, the procedure is like in Table 3.

Lastly, the pecking order on the basis of actual standard error is also echoed by the standard deviations of the SE's. That is, when the estimator is more imprecise, also the estimated theoretical SE becomes more sensitive to sample coincidences, with OLS doing best, followed by overlapping returns with IV, overlapping returns with OLS, Dimson, and finally SWFR.

2.2 Robustness to Various Departures from the Stylized Model

In this section we check the robustness of these results when key assumptions of the stylized model are relaxed. All changes are cumulative, that is, they are also retained in subsequent experiments.

The first change is autocorrelation in the market factor. We give the factor a 15-th order

Table 5: **Simulation results: magnified thin-trading problem**

	Mean $\hat{\beta}$			mean SE $(\hat{\beta})$s			stdev across $\hat{\beta}$			stdev of SE $(\hat{\beta})$s		
prob no trade	.00	.25	.50	.00	.25	.50	.00	.25	.50	.00	.25	.50
	One-period regression											
OLS	1.23	.99	.75	.12	.13	.13	.12	.16	.19	.017	.016	.017
SWFR ±1	1.06	1.07	.87	.16	.17	.18	.16	.16	.20	.025	.025	.025
SWFR ±2	1.02	1.02	.98	.19	.21	.22	.19	.19	.21	.033	.033	.034
SWFR ±3	1.01	1.01	1.02	.22	.24	.25	.22	.22	.23	.043	.043	.045
SWFR ±4	1.01	1.01	1.02	.25	.26	.28	.24	.25	.25	.052	.053	.055
Dimson ±1	1.03	1.08	.88	.15	.16	.17	.15	.15	.19	.022	.022	.022
Dimson ±2	1.01	1.01	1.02	.17	.18	.19	.17	.17	.19	.026	.026	.027
Dimson ±3	1.01	1.01	1.04	.19	.20	.21	.19	.19	.20	.030	.031	.031
Dimson ±4	1.00	1.00	1.02	.20	.22	.23	.20	.20	.21	.034	.035	.035
Dimson ±19	1.00	.99	1.00	.34	.37	.40	.35	.35	.35	.087	.092	.095
	H-period overlapping-returns regression											
IV 2	1.12	1.04	.82	.13	.14	.15	.13	.14	.18	.019	.019	.020
IV 3	1.08	1.03	.88	.15	.16	.17	.15	.15	.18	.022	.022	.023
IV 4	1.06	1.03	.92	.17	.18	.19	.16	.17	.18	.026	.025	.026
IV 5	1.05	1.02	.95	.18	.20	.21	.18	.18	.19	.029	.029	.029
IV 20	1.01	1.00	.99	.32	.35	.37	.31	.31	.32	.078	.080	.084
OLSHH 2	1.08	1.03	.89	.13	.14	.14	.15	.15	.17	.022	.022	.022
OLSHH 3	1.06	1.03	.93	.15	.15	.16	.16	.16	.18	.027	.027	.027
OLSHH 4	1.05	1.02	.95	.17	.17	.17	.18	.18	.19	.031	.031	.031
OLSHH 4	1.04	1.02	.96	.18	.18	.18	.19	.19	.20	.035	.035	.034
OLSHH 20	1.01	1.01	.99	.24	.24	.24	.30	.30	.30	.083	.082	.082

Key. There are 666 active stocks, 667 stocks that trade 3 days out of 4, and 667 stocks that trade one day out of two. Otherwise, the procedure is like in Table 3.

autocorrelation (prior to the autocorrelation induced by thin trading), starting at $\rho_1 = 0.04$ and linearly falling to 0 at lag 16. This, it can be checked, induces an autocorrelation of monthly returns of 0.16. Adding thin trading, we get a total autocorrelation in the index of about 0.20, which is realistic for monthly intervals. As can be verified in Table 2, the results do not differ in any meaningful way from the base-case output.

The second change is skewness in the returns. From Bauwens *et al.* (2006), active stocks exhibit no skewness at the daily level as long as there is no crash; so we generate skewness via a crash factor in the market. In the experiment we report the crash occurs with a quite generous daily probability of 0.001 (about once every 4 years), and when it does take place the fall is uniformly distributed between –10 and –20 percent. Unlike the regular market factor f, which retains its 15th-order autocorrelation, the crash factor is not autocorrelated. Table 3 has the results. The relevant conclusions are unaffected; in fact, there is but one pervasive change: all betas are higher. This reflects the fact that there is an extra common factor that affects all stocks in the same way without increase in the errors-in-variables problems. As a result, there now is a noticeable upward bias in the active stocks, as predicted theoretically. The reason why it shows up now and not in the earlier experiments is that the the covariance between beta error and asset weight is weakened if a common crash factor affects all stocks alike, whether big or small. The OLS beta is still below unity on average, but the active subgroup no longer is.[6]

The above modifications were cumulative, and also show up in the next two experiments, which have to do with the way we modeled the thin-trading problem. In the first robustness check we let the probabilities of no trade vary randomly over time, while creating correlation between the no-trading events across stocks. Specifically, a daily number p_{2000} is generated that distributes uniformally between 0.5 and 1 and represents the probability that the most illiquid stock gets traded on that day. (The ordering by liquidity remains determined by initial market value.) Of the other 1999 stocks, the top 1000 stocks remain 100 percent active as before, but the others get a probability that is linearly interpolated between 1 and p_{2000} on the basis of their liquidity ranking. We select stock 1000 as the representative active firm, and stocks 1200 and 2000 as the representative medium- and poorly-traded stocks, respectively. Thus, comparable with the base case, the three stocks we study still have unconditional probabilities of being traded equal to 1.00, 0.90 and 0.75, but now the non-trading problem is correlated across stocks, and assets' errors-in-variables are more strongly correlated with the measurement errors in the market. Despite this, the only remarkable effect in Table 4 is how small and unsystematic the changes are relative to the previous case.

In our second robustness check w.r.t. the details of the thin-trading mechanism we return to the original setup except that we worsen the thin-trading problem substantially. Instead of 1000 active stocks, 500 moderately traded, and 500 thinly traded ones we make the groups equally large, and we increase the probability of no trade from 0.10 to 0.25 for the middle group and from 0.25 to 0.50 for the worst affected stocks. In Table 5, summarizing the results, we see many effects magnified. The gap between the active and sleepy stocks' OLS betas is still equal to the (now larger) probability of no trade, but the active betas are substantially overestimated. This upward bias for the active stocks gets weakened quite quickly when we use any of the other estimators, and is no longer a major problem when $L = 4$; for monthly windows it is entirely

[6] Results for lower crash probabilities are, not surprisingly, in-between those of Tables 3 and 1 and are available on request. The same holds for experiments that are identical to those already summarized except for thicker tails in the market and idiosyncratic returns (five df instead of seven): the changes are minute.

Table 6: **Simulation results: stochastic liquidity, big market variance**

	Mean $\hat{\beta}$			mean SE $(\hat{\beta})$s			stdev across $\hat{\beta}$			stdev of SE $(\hat{\beta})$s		
prob no trade	.00	.25	.50	.00	.25	.50	.00	.25	.50	.00	.25	.50
	One-period regression											
OLS	1.07	.99	.85	.07	.07	.08	.07	.09	.11	.007	.008	.008
SWFR ±1	1.01	.98	.92	.10	.11	.12	.10	.11	.13	.014	.015	.015
SWFR ±2	1.00	1.00	.97	.13	.14	.16	.13	.13	.14	.021	.022	.023
SWFR ±3	1.00	1.00	.99	.15	.16	.18	.15	.15	.15	.028	.030	.032
SWFR ±4	1.00	1.00	.99	.17	.18	.20	.17	.17	.17	.035	.037	.040
Dimson±1	1.00	.97	.92	.09	.10	.11	.09	.10	.12	.010	.011	.011
Dimson±2	1.00	.99	.97	.11	.12	.13	.11	.11	.12	.013	.014	.014
Dimson±3	1.00	1.00	.99	.12	.13	.14	.12	.12	.13	.016	.017	.017
Dimson±4	1.00	1.00	1.00	.13	.14	.16	.13	.13	.14	.018	.020	.020
Dimson±19	.99	1.00	1.00	.23	.25	.27	.23	.23	.24	.056	.059	.063
	H-period overlapping-returns regression											
IV 2	1.04	.98	.89	.08	.09	.10	.08	.09	.11	.009	.010	.010
IV 3	1.02	.99	.92	.09	.10	.11	.09	.10	.11	.011	.012	.012
IV 4	1.02	.99	.94	.11	.11	.13	.11	.11	.11	.013	.014	.014
IV 5	1.01	.99	.95	.12	.12	.14	.12	.11	.12	.015	.016	.017
IV 20	1.00	1.00	.99	.21	.23	.25	.21	.21	.21	.049	.051	.056
OLSHH 2	1.02	.99	.92	.08	.09	.09	.09	.10	.11	.011	.011	.012
OLSHH 3	1.02	.99	.94	.10	.10	.10	.10	.11	.11	.014	.014	.014
OLSHH 4	1.01	.99	.95	.11	.11	.11	.11	.11	.12	.017	.017	.017
OLSHH 5	1.01	.99	.96	.11	.11	.12	.12	.12	.13	.019	.020	.019
OLSHH 20	1.00	1.00	.99	.16	.16	.16	.20	.19	.20	.052	.053	.053

Key. The market-factor volatility is increased by one-half to 1.5% per diem. Otherwise, the procedure is like in Table 4.

gone. The other effects that we observed in the base case remain unaffected: Scholes-Williams, Dimson, and Hansen-Hodrick handle the bias well (in that order), and the IV version of the overlapping-return regression rather well. Actual precision of OLS now substantially overstates the calculated one, but still does not do badly relative to the other estimators; the OLS and IV overlapping-observations models still come next, followed by Dimson and, lastly, SWFR. We conclude that in all experiments, lower bias comes at the cost of higher SE's and noisier estimates of these SE's.

We end with three encores. In all of these, we return to the stochastic-liquidity setup of Table 4. First, we increase the market factor volatility from 1% per day to 1.5%, i.e from about 16% *per annum* to 24%, at constant idiosyncratic noise. Predictably, with a clearer signal and unmodified residual risk, the precision is higher, but otherwise the outcomes are similar to those Table 4, as can be seen from Table 6. For the remaining two variants we again return to the scenario of Table 4, with base-case market variance (and stochastic liquidity and a crash), but now we add standard negative skewness in both the market and the idiosyncratic factor.

Table 7: **Simulation results: stochastic liquidity, extra skewness, extra kurtosis**

	Mean $\hat{\beta}$			mean SE $(\hat{\beta})$s			stdev across $\hat{\beta}$			stdev of SE $(\hat{\beta})$s		
prob no trade	.00	.25	.50	.00	.25	.50	.00	.25	.50	.00	.25	.50
	market-factor $\gamma = 0.92, ndf = 6$ (skewness -0.32, kurtosis 6.23)											
	One-period regression											
OLS	1.07	.99	.85	.12	.10	.11	.12	.12	.15	.019	.015	.015
SWFR ±1	1.00	.97	.91	.18	.16	.17	.19	.16	.17	.032	.025	.026
SWFR ±2	1.00	.99	.97	.23	.20	.21	.23	.19	.20	.044	.035	.037
SWFR ±3	.99	1.00	.99	.27	.23	.24	.27	.22	.22	.056	.045	.047
SWFR ±4	.99	1.00	.99	.30	.25	.27	.30	.24	.25	.068	.056	.059
Dimson±1	1.00	.97	.92	.16	.14	.14	.16	.14	.16	.026	.020	.021
Dimson±2	.99	.99	.97	.19	.16	.17	.19	.16	.17	.031	.025	.025
Dimson±3	.99	1.00	.99	.21	.18	.19	.21	.18	.18	.036	.029	.030
Dimson±4	.99	1.00	1.00	.23	.20	.21	.23	.19	.20	.041	.033	.034
Dimson±19	.99	.99	.99	.41	.35	.37	.42	.34	.34	.108	.089	.093
	H-period overlapping-returns regression											
IV 2	1.03	.98	.88	.14	.12	.13	.15	.13	.15	.023	.018	.018
IV 3	1.02	.98	.91	.17	.14	.15	.17	.14	.15	.027	.021	.022
IV 4	1.01	.99	.93	.19	.16	.17	.19	.16	.16	.031	.024	.025
IV 5	1.01	.99	.95	.20	.17	.18	.20	.17	.17	.035	.027	.028
IV 20	1.00	.99	.98	.38	.33	.35	.38	.31	.31	.096	.078	.082
OLSHH 2	1.02	.98	.91	.15	.12	.13	.17	.14	.15	.026	.020	.021
OLSHH 3	1.01	.99	.93	.17	.14	.14	.19	.15	.16	.032	.025	.025
OLSHH 4	1.01	.99	.95	.19	.15	.16	.20	.17	.17	.037	.029	.029
OLSHH 5	1.01	.99	.96	.20	.17	.17	.22	.18	.18	.041	.032	.033
OLSHH 20	1.00	.99	.98	.29	.24	.24	.36	.29	.29	.101	.081	.081
	market-factor $\gamma = 0.96, ndf = 4$ (skewness -0.37, kurtosis 16.18)											
	One-period regression											
OLS	1.07	.98	.84	.14	.10	.11	.14	.13	.16	.027	.016	.017
SWFR ±1	1.00	.97	.91	.21	.16	.17	.22	.16	.18	.044	.028	.028
SWFR ±2	1.00	.99	.96	.27	.20	.21	.27	.19	.20	.060	.038	.039
SWFR ±3	1.00	.99	.99	.31	.23	.24	.31	.22	.23	.074	.048	.050
SWFR ±4	.99	.99	.99	.34	.26	.27	.35	.25	.25	.087	.058	.061
Dimson±1	1.00	.97	.91	.18	.14	.14	.19	.14	.16	.036	.022	.023
Dimson±2	.99	.99	.97	.22	.16	.17	.22	.16	.17	.044	.027	.027
Dimson±3	.99	.99	.99	.25	.18	.19	.25	.18	.19	.050	.031	.032
Dimson±4	.99	.99	.99	.27	.20	.21	.27	.20	.20	.056	.035	.036
Dimson±19	.99	.99	.99	.48	.36	.37	.49	.35	.35	.136	.093	.095
	H-period overlapping-returns regression											
IV 2	1.03	.98	.88	.16	.12	.13	.17	.13	.15	.032	.02	.02
IV 3	1.02	.98	.91	.19	.14	.15	.19	.14	.16	.038	.023	.024
IV 4	1.01	.98	.93	.21	.16	.17	.22	.16	.17	.043	.027	.027
IV 5	1.01	.99	.95	.24	.18	.19	.24	.17	.18	.048	.030	.030
IV 20	1.00	.99	.98	.44	.33	.35	.45	.31	.32	.123	.082	.085
OLSHH 2	1.02	.98	.91	.17	.12	.13	.19	.14	.16	.036	.022	.022
OLSHH 3	1.01	.98	.93	.20	.14	.15	.22	.16	.16	.043	.027	.027
OLSHH 4	1.01	.99	.95	.22	.16	.16	.24	.17	.17	.049	.031	.031
OLSHH 5	1.01	.99	.95	.24	.17	.17	.25	.18	.18	.055	.035	.035
OLSHH 20	1.00	.99	.98	.34	.24	.24	.42	.29	.29	.123	.084	.083

Key. The market factor is skewed t, with γ chosen to as to produce a skewness of about –0.32 as in Bauwens *et al.* (2006). In the second half of the table, ν is decreased from 6 to 4 df to boost tail-thickness. Otherwise, the procedure is like in Table 4.

Table 8: **Betas for N=1998 US stocks, daily data 1999-2000**

L or	SWFR		Dimson		HH overlap		IV overlap	
$H-1$	mean	stdev	mean	stdev	mean	stdev	mean	stdev
(OLS) 0	0.62	0.36	0.62	0.36	0.62	0.36	0.62	0.36
1	0.73	0.44	0.72	0.43	0.69	0.40	0.67	0.39
2	0.73	0.45	0.74	0.45	0.74	0.42	0.69	0.40
3	0.89	0.54	0.81	0.50	0.78	0.44	0.74	0.42
4	0.94	0.58	0.84	0.54	0.82	0.46	0.78	0.44
19	—	—	1.06	0.92	1.04	0.67	1.05	0.66

The distribution is a skewed student's, this time, with its skewness parameter chosen so as to match the *per diem* skewness of –0.32 estimated for NASDAQ by Bauwens et al. (2005). In the second variant we also boost tail-thickness by lowering the ndf from 6 to 4. Table 7 shows that the main consequence of higher skewness and kurtosis is to lower the precisions across the board; neither the biases nor the patterns in the precisions are affected; .

3 Field Experiments

In this section we verify to what extent the effects observed above are noticeable in practice. We do so in two ways. First we simply compute and describe the various beta estimates on real-world stocks. Second, we see how a hedge-fund manager would fare if she would aim for market-neutrality on the basis of a particular estimator of the assets' betas.

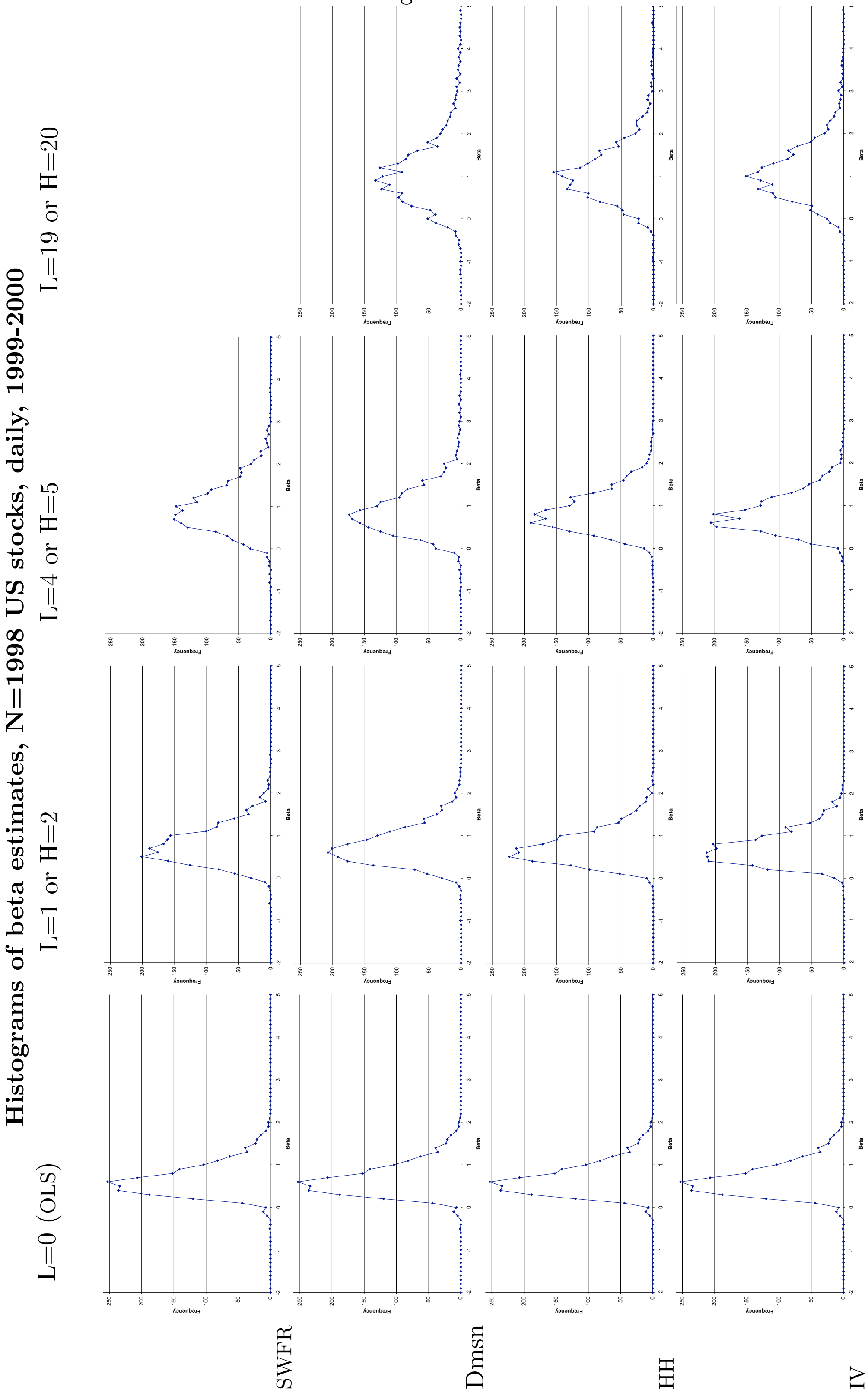
3.1 Field experiment 1: estimating betas in real-world data

The data are 10 years of US stock prices, daily, 1994-2003, on originally 2675 stocks in Datastream's market list early 2004. This is an unbalanced panel, but heterogeneities across stocks that stem from data availability are equally present for each estimator. We compute the betas for all of the 1998 stocks that made it into the hedge experiment described in the next subsection, using daily prices starting between days 1000 and 1480 for all stocks present on day 1480. Table 8 shows the cross-sectional mean and standard deviation for each estimator, while Figure 1 presents some of the histrograms.

Unlike in the Monte-Carlo study, now the market-averaged expected beta (absent thin-trading problems) is not known a priori;[7] and the cross-sectional standard deviation contains

[7]We only know the value-weighted mean value assuming constant weights. Value weighting would swipe

Figure 1:



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much of the thin-trading problem under the carpet.

the unknown variability of true betas across stocks and must be affected by heterogeneity of SE's too. Despite these complicating factors, the similarities with the simulation results are striking. Within each and every class we see increasing average betas, indicating a falling bias, but at the cost of a rising standard deviation. We again note that the two estimators that are explicitly set up to cope with thin trading do best *re* bias, but again at the cost of precision. Even within each class (SWFR versus Dimson; and OLS overlapping versus IV overlapping) we see the same effect. In sum, like in our Monte-carlo experiments the bias-versus-precision trade-off also holds across estimators.

Whether bias should be the overwhelming consideration rather than precision depends on the application. We consider one such application in the remainder of this section.

3.2 Field experiment 2: setting up market-neutral portfolios

We consider the problem of a hedge-fund manager who has selected an underpriced stock and now wants to add a position in another stock so as to make the combination market-neutral. Common sense would already suggest we match by industry and size. The question is whether this suffices: can one just invest equal amounts (up to the sign), implicitly assuming the two stocks have the same beta, or is it helpful to look at estimated betas and come up with a non-unit hedge ratio? In this problem, bias and estimation variance are equally bad as they enter into the portfolio-return as a sum. This can be seen as follows. Regard, in Bayesian style, the portfolio's true beta as a random variable. For simplicity and without loss of generality, let all returns be mean-centered. The portfolio return then equals $\tilde{r}_p = \tilde{\beta}_p \tilde{r}_m + \tilde{e}_p$. In line 3 of the set of equations below, we use the fact that, $\tilde{r}_m$ being centered, the expected cross product equals the covariance, which in turn equals zero as the portfolio beta is fixed at the beginning of the period.[8] In line 4 we work out the expectation of a product and set the resulting covariance zero, for the same reason. Line 5 again uses the zero-mean property of $\tilde{r}_m$.

$$\begin{aligned}
\text{var}(\tilde{r}_p) &= \text{var}(\tilde{\beta}_p \tilde{r}_m) + \text{var}(\tilde{e}_p), \\
&= [\text{E}(\tilde{\beta}_p^2 \tilde{r}_m^2) - \text{E}(\tilde{\beta}_p \tilde{r}_m)^2] + \text{var}(\tilde{e}_p), \\
&= [\text{E}(\tilde{\beta}_p^2 \tilde{r}_m^2) - \underbrace{\text{cov}(\tilde{\beta}_p, \tilde{r}_m)^2}_{=0}] + \text{var}(\tilde{e}_p),
\end{aligned}$$

[8] In the empirical work, another argument is relevant, but the conclusion is similar: the beta is determined by decisions based on realized returns prior to t, while the market return is subsequent to t. This rules out strong links.

$$= [\mathrm{E}(\tilde{\beta}_p^2)\mathrm{E}(\tilde{r}_m^2) - \underbrace{\mathrm{cov}(\tilde{\beta}_p^2, \tilde{r}_m^2)}_{=0}] + \mathrm{var}(\tilde{e}_p),$$

$$= [\mathrm{var}(\tilde{\beta}_p) + \mathrm{E}(\tilde{\beta}_p)^2]\mathrm{var}(\tilde{r}_m) + \mathrm{var}(\tilde{e}_p). \quad (10)$$

The square-bracketed factor collapses to the familiar β_p^2 if there is no uncertainty about beta, the standard textbook case. In the presence of estimation error, however, what matters is the sum of uncertainty about beta (estimation variance) and bias, the squared deviation from the target beta of zero.

3.2.1 Procedure

There are four sets of computations, of which we show three. In the first variant, every month we take all US stocks of a given industry, and rank them by size (market cap). For the long positions (subscript l) we pick, sequentially, assets ranked 1, 4, 5, 8, 9, 12, ..., and match each of them with a short positions (subscript s) in a size-wise close stock of the same industry, notably those ranked 2, 3, 6, 7, 10, 11, etc. Each pair's hedge ratio must then be set so as to produce a market-neutral position for each such pair. For the portfolio to have a zero beta, the weights of the two risky assets have to satisfy

$$\frac{w_l}{w_s} = -\frac{\beta_s}{\beta_l}. \quad (11)$$

In a first round of experiments we have one asset that is deemed underpriced, which we then hedge; so we set $w_l = 1$, implying $w_s = -\hat{\beta}_l/\hat{\beta}_s$ and a risk-free position $w_0 = 1 - w_l - w_s = \hat{\beta}_l/\hat{\beta}_s$. The data are again our 10 years of US stock prices, daily, 1994-2003, on the 2004 Datastream market list. Of these 2675 stocks, 2×999 have at least one period of 480 days of data and could be well matched by size and industry. We divide the time line into 123 20-trading-day periods which we somewhat inaccurately refer to, below, as "months". Betas are estimated using the first 480 daily data (24 months). Given the portfolio weights we then hold the two stocks and a risk-free deposit for one month, and note the portfolio returns per estimator. We next re-estimate the betas using a one-month-updated 24-month sample, re-rank the stocks by value, and form new pairs, etc. This produces 99 non-overlapping out-of-sample tests per pair; and since we have 999 US matched pairs with a 10-year history, there are, for each of the 99 test months, 999 such two-asset-portfolio returns available for performance analysis.

We see three problems with this approach. First, it studies pairs of assets in isolation, as standard in the optimal-hedge-ratio literature; but a portfolio manager may reckon that much of this risk must be diversifiable. Second, some estimated beta pairs are occasionally so egregious, say 2 and 0.1, that hedge ratios of 20 would be implied, generating absurd variances for the "hedged" portfolios. It is hard to believe a manager would adopt such positions. Third, the result of an individual hedge depends very much on which stock happens to be stock l or stock s. If, for instance, the betas are 2 for l and 0.5 for s, the weights would be ($w_l = 1$, $w_s = 4$), producing on average a sixteen-times higher portfolio variance than if the betas had been the other way around and the weights, accordingly, had been ($w_l = 1$, $w_s = 0.25$). This random element in the portfolio strategy would add noise to the variance of the investment strategy. In the standard hedging literature the position to be covered is given exogenously, but in the current portfolio-management setting this position is a decision variable. If the manager actually believed a hedge ratio of 4 is needed, then she would probably scale down both sides.

The last two of the above problems turned out to be quite large: portfolio-return variances were quite absurd, thus demonstrating that the naive procedure does not make a lot of sense. We show, instead, the results for three variants. In one amended version of the experiment we make two adjustments. First, we truncate the estimated betas at 0.25 and 4, reckoning that no real-world manager worth her salt would believe estimates outside this range. This provision was rarely needed in the actual computations. To weaken the effect on the portfolio weights of switching the betas, we rescale the first-pass weights by their geometric average. Elementary algebra show that this gives us the following weights (with $\hat{\beta}^{tr}$ denoting a truncated beta):

$$w_l = \sqrt{\frac{\hat{\beta}_s^{tr}}{\hat{\beta}_l^{tr}}}; \;\; w_s = -\sqrt{\frac{\hat{\beta}_l^{tr}}{\hat{\beta}_s^{tr}}}; \;\; w_0 = 1 - w_l - w_s, \tag{12}$$

With this rule it hardly matters whether the stock held long happens to be the smaller-beta one or not. With estimated betas equal to 2 and 0.5, for example, the weights could now be either 2 and 0.5 or 0.5 and 2 (depending on whether the high-beta stock acts as stock l or not), but this has no predictable impact on the portfolio variance.

In the third variant we want to obtain an impression of to what extent estimation risk is diversifiable. We proceed as in version 2, except that we now work with portfolios of ten stocks held long and ten held short. For instance, in the first portfolio the stocks with size rank 1, 4, 5, 8, ... , 17, 20 of industry 1 are held long, and assets with ranks 2, 3, 6, 7, ... , 18, 19 short. The second portfolio proceeds similarly with the next 20 stocks in the industry's size ranking, and so on. Betas are computed for the entire long side of the portfolio, and for the entire short side, and then handled as before: truncated, and converted in balanced weights. There are 88 such portfolios per month.

Table 9: **Average and Median variances of market-neutral portfolios (single share exposure hedged by single share matching industry and size)**

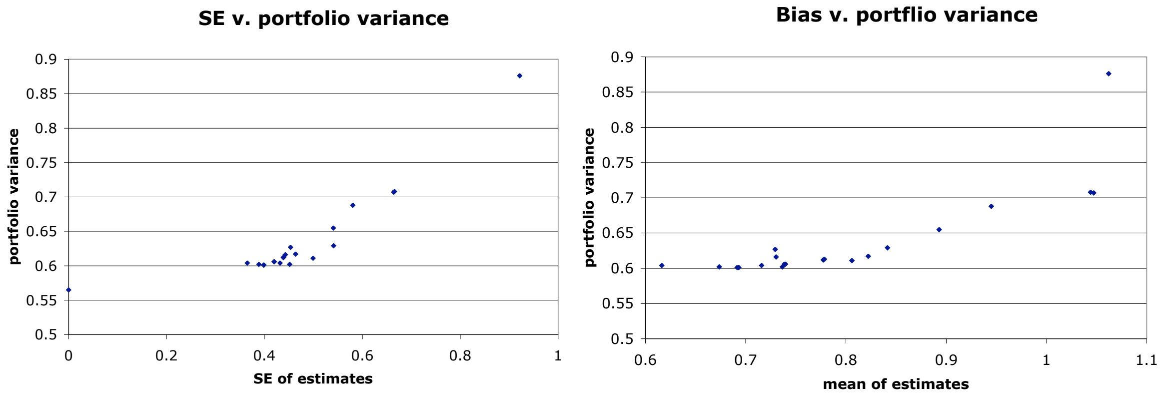
	Panel 1				Panel 2				Panel 3			
	one stock long, one short				ten stocks long, ten short							
	$w_l = \sqrt{\frac{\hat\beta_s^{tr}}{\hat\beta_l^{tr}}}$, $w_s = -\sqrt{\frac{\hat\beta_l^{tr}}{\hat\beta_s^{tr}}}$				$w_l = \sqrt{\frac{\hat\beta_s^{tr}}{\hat\beta_l^{tr}}}$, $w_s = -\sqrt{\frac{\hat\beta_l^{tr}}{\hat\beta_s^{tr}}}$				$w_l = 1$, $w_2 = \hat\beta_l/\hat\beta_s$			
	# times		var (E–2)		# times		var (E–2)		# times		var (E–2)	
estimator	top	btm	avg	med	top	btm	avge	med	top	btm	avg	med
equal betas	85	1	4.50	3.46	43	4	.565	.408	40	1	.565	.408
OLS	0	1	5.05	3.98	10	3	.599	.420	9	1	.604	.432
SWFR ±1	1	0	5.36	4.12	5	1	.604	.437	8	2	.616	.417
SWFR ±2	0	3	5.99	4.48	5	6	.617	.438	5	4	.627	.435
SWFR ±3	0	8	6.25	4.66	3	5	.627	.442	4	5	.655	.457
SWFR ±4	0	5	6.32	4.74	0	8	.643	.451	1	14	.688	.468
Dimson±1	5	0	5.21	4.02	9	1	.604	.428	8	2	.604	.427
Dimson±2	1	0	5.59	4.15	2	1	.599	.448	5	0	.602	.433
Dimson±3	1	0	5.56	4.26	7	0	.608	.450	4	0	.611	.442
Dimson±4	0	0	5.81	4.41	2	4	.612	.453	3	4	.629	.452
Dimson±19	0	75	8.17	5.86	3	57	.748	.504	1	54	.876	.527
OLSHH 2	2	0	5.20	3.96	1	0	.595	.422	0	0	.601	.424
OLSHH 3	1	0	5.30	4.05	0	0	.597	.422	1	0	.606	.423
OLSHH 4	0	0	5.38	4.08	0	0	.601	.430	1	0	.612	.423
OLSHH 5	0	0	5.43	4.15	1	0	.604	.441	1	0	.617	.430
OLSHH 20	0	2	6.32	4.88	1	6	.660	.470	1	8	.708	.479
IV 2	3	0	5.06	3.97	3	0	.595	.427	3	0	.602	.424
IV 3	0	0	5.19	3.97	0	0	.595	.422	2	0	.601	.425
IV 4	0	0	5.30	4.04	1	0	.597	.424	1	0	.606	.421
IV 5	0	0	5.37	4.10	0	0	.602	.434	1	0	.613	.422
IV 20	0	4	6.34	5.01	3	3	.660	.477	0	4	.707	.484

Key In Panel 1, each stock is matched with another one from the same industry and size, and one is held long and the other short. In Panel 2 and 3 they are grouped into equally weighted portfolios of ten stocks each; for instance the stocks with size rank 1,4,5,8, 14 ... are held long, and assets with ranks 2, 3, 6, 7, 10, 11 ... short. The size ranking is updated every month. The weighting of the short v. long positions depends on the asset's or portfolio's beta in the way indicated in the second line of the heading: in the first and second panel, betas are truncated at 4 and 0.25, and the long and short weights are made more balanced by the square-root procedure, while in Panel 3 we use naive weights (no truncation, no balancing). A time-series variance is computed for each beta and each portfolio (like large hi-tech and so on). We show the average ("avg var") and median ("med var") variance across all portfolios; the number of times a particular estimator shows up with the best variance across all portfolios ("times top"); and the number of times a particular estimator shows up with the worst variance ("times btm"). Variance is in E–2.

In the fourth variant we still work with 20-asset portfolios, but leave out the truncation and the square-root adjustment, reckoning that for portfolios the beta problems should be substantially attenuated.

We compute, for each beta estimator and month, the cross-sectional variation of the returns

Figure 2: **Portfolio Return variance plotted against variance or mean of betas**



across the 999 2-asset portfolios or the 88 20-asset portfolios, which leaves us with, for each estimator, a time series of 99 variances. For each such time series we report the mean and the median. In every month we also identify the best- and worst-performing estimator; we then report how often, out of 99 months, the estimator came out top and bottom.[9]

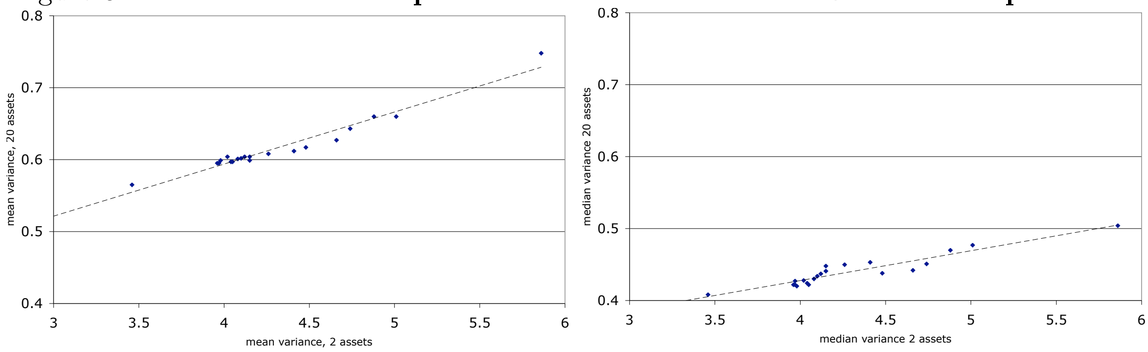
3.2.2 Results

The results for the three procedures are shown in Table 9. The equal-beta model does best on all counts. In the two-asset case (Panel 1) it comes up as the best procedure in 85 of the 99 months, with the runner-up (Dimson, one lag) achieving just 5 gold medals. While the lead of the equal-beta model is smaller when portfolios of 10 are used, it still comes up 43 or 40 times as the lowest-variance portfolio, with the runner-up (OLS) collecting a mere 9 or 10 firsts. Dimson1 is still a very close third. The equal-beta procedure also delivers the smallest average variance, followed by OLS. Dimson19 fares systematically worst; in fairness, recall that we threw out SWFR19 as producing simply too outlandishly imprecise numbers, otherwise that estimator would have brought up the rear.

The strong side of the equal-beta model is that it surely has zero estimation variance, and probably only a small bias, which is likely to be diversifiable across portfolios to a large extent. The suggestion that the beta's SE is the more important item, not its bias, gets more support

[9] The alternative is to compute time-series variances per portfolio, and average these across the portfolios instead of computing cross-sectional variances across portfolios and then averaging these across the months. In a fully balanced sample both procedures produce the same general average, and it is hard to see why it would systematically affect the other metrics. But since the number of portfolios changes over time, the second approach is hard to implement.

Figure 3: **Mean and median portfolio-return variances: 20- v 2-asset portfolios**



if we recall that OLS, our runner-up, has the worst bias but the highest precision, while last-comer Dimson19 combines zero bias with the highest SE's. The same picture emerges when we compare the average variances of within each family. Recall that, for Dimson, SWFR, HH and IV, the bias disappears as one increases the window L or H, but at the cost of less precision. We now see that the cost of imprecision outweighs the benefit of a lower bias: within each family of estimators, the portfolio-return variance steadily increases as the H- or L-window widens, with only two exceptions out of 57 comparisons of adjacent pairs of variances. Figure 2 plots, for each given beta estimator, the average portfolio-return variance obtained in Table 9 Panel 3 against either the cross-asset standard deviation or the mean as taken from Table 8. We see that returns become more volatile when betas are noisier, but also when the mean beta rises, that is, when the bias becomes smaller. The last result makes no sense except if bias comes along with lower precision and if the latter effect dominates. We conclude that the niceties about bias are dwarfed by issues of standard error when the problem is one of building market-neutral portfolios.

Predictably, the variances for 20-asset portfolios are substantially lower than those of 2-asset ones. But the ratio is not a constant ten-to-one, as one would expect if pairwise matching on size and industry left only purely idiosyncratic noise. In Figure 3 we plot the means or medians of the 20-asset portfolio variances against those of 2-asset ones. While there is a good preservation of order, the portfolio variances clearly do not plot on a ray from the origin with slope 1/10. There is, first, some randomness and, second, a large dose of attenuation: the slope is 0.04 or 0.07 rather than 0.10, and there is an intercept. The attenuation must be to some extent explained by the randomness. The reason is that if the 2-asset variances are noisy estimates of large-sample values, there is an errors-in-variables bias towards zero in the slope of the trendline. The observation that the more imprecise estimators seem to benefit relatively more from attenuation than the precise ones is consistent with this: noisiness in the estimates goes together with noisiness in the variance. But ascribing all of the attenuation to noisiness in the 2-asset return variances would be going too far: to shrink a slope from 0.10 to 0.04, the error variance for the variable on the horizontal axis would have to be 1.5 times larger than the true variance, which seems hard to reconcile with the strength of the observed relation. If part of the flattening out is real, then high-variance estimators benefit more from diversification than low- variance ones. There is another piece of evidence in that direction: the lead of the equal-beta rule of thumb shrinks when the portfolios are larger, suggesting the estimators do become less bad. While we do not have enough data to work with 100- or 1000-asset portfolios, our exploratory evidence on diversification raises the possibility that, with a great many assets, a regression may actually not do much harm relative to the equal-beta rule of thumb. Still, at this point there is no evidence that estimation would ever positively help.

4 Conclusion

Two regression coefficients often used in Finance, the Scholes-Williams (1977) quasi-multiperiod "thin-trading" beta and the Hansen-Hodrick (1980) overlapping-periods regression coefficient, can both be written as instrumental-variables estimators. We check the performance of these IV-estimators and the validity of the theoretical standard errors in small and medium samples, gauge the robustness of the Scholes-Williams estimator outside its stated assumptions, and report performances relative to the Dimson beta, standard OLS, and the equal-beta model in a hedge-fund style application. We learn that, across and within "families" of estimators, less bias comes at the cost of a higher standard error. The hedge-portfolio experiment shows that the safest procedure is to simply match by size and industry; any estimation just adds noise. There is a clear relation between portfolio variance and the variance of the beta estimator, dwarfing the effect of bias.

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