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Author(s): E. Dimson and P. R. Marsh
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The Stability of UK Risk Measures and The Problem of Thin Trading

E. DIMSON and P. R. MARSH*

ABSTRACT

This paper examines the problems of estimating risk measures and their stability in thin markets. It shows analytically that conventional approaches used in previous studies can lead to serious overestimates of the stability of risk measures when shares are subject to thin trading. It then demonstrates, using UK data, that this is, in fact, a serious practical problem, and that the resultant biases are of precisely the form predicted. Finally, the paper presents reliable evidence on the stability of UK risk measures by using an estimation method designed to avoid thin trading bias. Using this approach, risk measures are found to be as stable in the UK as they are in the USA.

THIS PAPER EXAMINES THE stability of risk measures in the UK. The focus is on providing reliable evidence in a market where many of the shares suffer from thin trading. This is important because thin trading can seriously distort stability measures, leading to incorrect conclusions. Indeed, this is a problem which has almost certainly afflicted many previous studies of stability.

The paper makes four main contributions. First, it shows analytically that thin trading can lead to serious overstatements about stability. Second, it uses UK data to illustrate that this is, in fact, a serious practical problem, and that the biases are of precisely the form predicted. This is important for anyone who wishes to interpret previous claims on beta stability in thin markets. Third, the paper presents reliable evidence on the stability of UK betas by using an estimation method designed to avoid thin trading bias. Besides avoiding bias, our evidence, which covers several thousand estimates over a quarter of a century, is far more comprehensive than that of previous UK studies (see, for example, Brealey [8], Cunningham [15], Guy [32], Pogue and Solnik [46], and Theobald [63]). Finally, the paper provides the first published UK evidence on the stability of other risk measures, particularly the standard deviation of returns and the residual standard deviation.

We proceed by examining, in the next section, the standard methodology for measuring stability. We explain why thin trading is a problem, and review ways in which it can be circumvented. The subsequent three sections describe the empirical work: Section II provides evidence on the effects of thin trading bias

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and its impact on stability measures. Section III investigates the stability of UK risk measures when these are estimated using a method designed to avoid thin trading bias. Section IV then compares the stability of betas estimated using different variants of this basic technique. Finally, Section V summarizes our conclusions and their implications.

I. Beta Stability and the Problem of Thin Trading

The most common approach used to measure stability is to estimate betas over successive periods; and then either to calculate the correlation coefficient between successive estimates (see, for example, Blume [6]), or to construct transition matrices showing how the estimates change over time (see, for example, Sharpe and Cooper [58]). Essentially, these are variants of the same approach since transition matrices can be regarded as tabular representations of correlation coefficients. Thus in either case, the approach is dependent on the integrity of the correlation coefficient.

The correlation coefficient in question is, of course, the correlation between beta estimates rather than between the true, but unobservable, betas. Typically, beta estimates are derived from the simple regression (SR) market model (see Sharpe [55]),

$$\tilde{R}_{jt} = \alpha_{jp} + \beta_{jp}\tilde{R}_{mt} + \tilde{u}_{jt} \tag{1}$$

Normally, $\tilde{R}_{jt}$ is defined as the measured continuously compounded return on the j'th security during period t, while $\tilde{R}_{mt}$ is the corresponding return on the market. Furthermore, it is standard practice to assume that the estimate $\hat{\beta}_{jp}$ for security j in the estimation period p, where the latter commences at time $t = (p - 1)\ T + 1$ and terminates at time $t = pT$, has a zero mean error $\tilde{\varepsilon}_{jp}$ which is uncorrelated with the true (but unobservable) beta β_{jp}:

$$\hat{\beta}_{jp} = \beta_{jp} + \tilde{\varepsilon}_{jp} \tag{2}$$

It is normally implicitly assumed that the true underlying betas β_{jp} are constant within estimation period p, but vary stochastically between successive non-overlapping estimation periods such as p and $p + 1$, where period $p + 1$ runs from time $t = pT + 1$ to $t = (p + 1)T$. Then, to estimate the stability of beta estimates derived from (1), it is usual to calculate the correlation coefficient ρ' between beta estimates $\hat{\beta}_{jp}$ and $\hat{\beta}_{j,p+1}$:

$$\rho' = \{E(\hat{\beta}_{jp}\ \hat{\beta}_{j,p+1}) - E(\hat{\beta}_{jp})E(\hat{\beta}_{j,p+1})\}/\{\sigma(\hat{\beta}_{jp})\sigma(\hat{\beta}_{j,p+1})\} \tag{3}$$

Assume that the true betas are constant within each estimation period $(p, p + 1)$, with a cross-sectional mean of unity and standard deviation of σ_β. (Our results would be similar if the unweighted mean beta were not equal to unity or if σ_β varied from period to period.) Assume also that true betas are stochastic over time, with correlation ρ between their values in successive estimation periods p and $p + 1$. It follows that:

$$\begin{aligned}\rho' &= \{E[(\tilde{\beta}_{jp} + \tilde{\varepsilon}_{jp})(\tilde{\beta}_{j,p+1} + \tilde{\varepsilon}_{j,p+1})] - 1\}/\{\sigma_\beta^2 + \sigma_\varepsilon^2\} \\ &= \{\rho\sigma_\beta^2 + \rho^*\ \sigma_\varepsilon^2/\{\sigma_\beta^2 + \sigma_\varepsilon^2\}\end{aligned} \tag{4}$$

where ρ^* is the (unobservable) correlation coefficient between the error terms in the two successive periods.

Virtually all previous studies of beta stability make the implicit assumption that ρ^* is zero. Note that when the latter is the case, the estimated correlation ρ' is a reasonable criterion for judging the relative merits of different estimation methods, since it is inversely proportional to σ_ε^2. Furthermore, if ρ' is supplemented with estimates of σ_β^2 and σ_ε^2, Blume [7] has shown that it is possible to obtain a direct estimate of ρ, and thus make statements about the stochastic behaviour of the true betas over time. If, however, beta estimation errors are serially correlated ($\rho^* \neq 0$) then ρ' can prove a highly misleading statistic. In particular, for two sets of beta estimates with ρ and σ_β^2 identical the estimation method with the highest ρ^* will appear to give the most stable beta estimates. Yet in reality, this has nothing to do with stability in the underlying betas, but instead simply reflects persistence in the pattern of estimate errors.

This does, in fact, present a serious potential problem when estimating the stability of risk measures for thinly traded securities. As is well known, when shares are thinly traded, their beta estimates based on (1) are biased downwards (see, for example, Fisher [26], Cohen, Maier, Schwartz, and Whitcombe [12], Dimson [19], and Scholes and Williams [53]). If thin trading persists over time, these systematic biases will be serially correlated, and hence ρ^* will be greater than zero. For example, with a market index which suffers from negligible thin trading and with both daily market returns and true (but unobservable) daily security returns which are identically and independently distributed, SR beta estimates depend on the ages of month-end price observations. Defining L_{jt} as the age (in fractions of a month) of the last marked price in security j as at the end of month t, then

$$\begin{aligned} P\lim \hat{\beta}_{jp} &= \Big\{1 - T^{-1}\textstyle\sum_{t=(p-1)T+1}^{pT} \mathrm{Min}(L_{jt}, 1)\mathrm{Cov}(\tilde{R}_{jt}, \tilde{R}_{mt})/\mathrm{Var}(\tilde{R}_{mt}) \\ &= (1 - I_{jp})\beta_{jp} \end{aligned} \tag{5}$$

where $\hat{\beta}_{jp}$ is the beta estimate given by Equation (1) and where the trading infrequency of security j during estimation period p is defined as

$$I_{jp} = T^{-1}\textstyle\sum_{t=(p-1)T+1}^{pT} \mathrm{Min}(L_{jt}, 1)$$

where I_{jp} is independent of the security returns.

Trading infrequency is, of course, highly correlated with company size (see Dimson [19]). Since changes in company size are by assumption i.i.d., it follows that the level of company size is serially dependent. Thus I_{jp} is autocorrelated and ρ^* in (4) will be positive. Beta estimates may therefore appear stable simply because of the stability of trading infrequency.

Note that the source of the problem does not lie in the use of the correlation coefficient to evaluate the stability of beta, but instead in its derivation from biased beta estimates. Indeed, other approaches to measuring beta stability such as Sharpe and Cooper's [58] transition matrices, Blume's [6, 7] portfolio regression, Fisher and Kamin's [27] conditional returns, Kon and Lau's [38] shifting beta tests, or Klemkosky and Martin's [37] use of the mean squared error criterion would all suffer from the same drawback if (1) was used to estimate the betas of thinly traded securities. Furthermore, the problem arises from thin trading rather than from the precise form of the market model used in (1). Thus, although most studies do estimate betas from (1), the problem would also arise with regressions based on ex-post forms of the capital asset pricing model (Sharpe [56], Lintner [40]).

Obviously, one way of avoiding the thin trading problem is to adjust SR estimates to make them unbiased. Equation (5) shows that an unbiased estimate of beta is

$$\hat{\beta}'_{jp} = \hat{\beta}_{jp}/(1 - I_{jp}) \tag{6}$$

where $\hat{\beta}_{jp}$ is the SR estimator given by Regression (1). Frequently, however, information on the timing of transactions is unavailable and I_{jp} cannot be computed. In these instances it is necessary to resort to one of the beta estimation methods which utilize lagged and leading market returns (Dimson [19], Scholes and Williams [53], or Cohen, Hawawini, Maier, Schwartz, and Whitcomb [14]) or to use a more ad hoc procedure for avoiding thin trading bias (such as Dimson [17], Ibbotson [36], Schwert [54], Theobald [63], or Cohen, Hawawini, Maier, Schwartz, and Whitcomb [13]).

In this study, however, we do in fact, have information on the timing of the last trade within each period. Though some researchers (see Fowler and Rorke [28]) advise using Equation (6) to estimate beta, this is an inefficient technique when the timing of trades is known. Instead, we tackle the problem of thin trading directly by abandoning the use of equal length periods, as in Marsh [43]. Returns on the share are calculated between adjacent trades, and the market returns are then measured over the same calendar period. Beta is then estimated from the second coefficient of the trade to trade (TT) regression:

$$\tilde{R}_{jt_j}/\sqrt{\tilde{d}_{t_j}} = \alpha_{jp}/\sqrt{\tilde{d}_{t_j}} + \beta_{jp}\tilde{R}_{mt_j}/\sqrt{\tilde{d}_{t_j}} + \tilde{v}_{jt_j} \tag{7}$$

where $\tilde{R}_{jt_j}$ is the continuously compounded return on security j during period t_j which is the period between two recorded trades in security j, $\tilde{R}_{mt_j}$ is the continuously compounded return on the market over precisely the same calendar time period t_j, $\tilde{d}_{t_j}$ is the length (in days) of period t_j, and α_{jp} is now redefined as a continously compounded return per day during estimation period p. Note that the observations are weighted to allow for the different period lengths to ensure that the beta estimates are efficient as well as unbiased.

The alternative techniques which might have been used for beta estimation are Dimson [19], Scholes-Williams [53], or Cohen et al. [14]. The generalized Scholes-Williams estimator devised by Cohen et al. is a slight modification of the Dimson [19] method, and the difference between these two estimators is for practical purposes negligible. (This is shown both analytically and empirically in Dimson [21] and was previously conjectured by Roll [48, fn. 8]; also see Fowler and Rorke [28].) Consequently, decisions about the choice of estimator depend effectively on the relative efficiency of competing methods. When each share price is labelled with a transaction date and when, at each of these dates, recordings are available for a market index which does not suffer from thin trading, Dimson [19] has shown that the TT method is a more efficient method for handling thin trading than either the Dimson [19] or Scholes and Williams [53] approaches.

Although the methods discussed above have now been available for some years, very little attention has yet been given to quantifying the biases introduced by thin trading in empirical studies of, for example, beta stability. This is unfortunate, and makes it very difficult to interpret previous work on thin markets. There is, however, circumstantial evidence to suggest that thin trading may have caused serious distortions in previous studies. An example of this is the finding that in certain markets, daily returns give rise to more stable beta estimates than weekly returns (Brealey [8]), and weekly returns provide more stable estimates than monthly returns (Altman, Jacquillat, and Levasseur [2]). Also consider the counter-intuitive claims that betas are less stable in the USA than in the stock markets of France (Altman, Jacquillat, and Levasseur [2]) and of Belgium (Hawawini and Michel [34]). Clearly, such findings could simply be attributable to thin trading bias, and as such, should be treated with caution. Indeed, with the probable exception of stability studies carried out on monthly data for New York Stock Exchange securities, it could be argued that we really have very little reliable evidence on the stability of risk measures for other markets and for other countries. In this study, we set out to redress this imbalance.

The current study proceeds in three stages. First, in Section II, we investigate whether thin trading is, in fact, a serious practical problem. Then, in Section III, we examine the stability of UK risk measures estimated using the trade-to-trade technique which, as noted above, is the preferred method for avoiding thin trading bias. Finally, in Section IV, we compare the stability of betas estimated using different variants of this basic technique, and examine other issues such as the regression tendencies of betas.

II. An Empirical Investigation into the Effects of Thin Trading

Our initial step was simply to replicate the standard methodology on UK data, using SR betas estimated from (1). Our sample here consisted of all UK companies for which data were available on the London Share Price Database (LSPD). By including all companies, our sample has a slight bias towards larger (frequently traded) companies. (See Smithers [59, 60] for a description of the LSPD). Security returns data were taken from the LSPD monthly returns file. Monthly market returns were calculated from the broadly-based, capitalization weighted Financial Times-Actuaries All-Share Index (FTA).[1] Five equal length estimation periods of 60 months each were examined, beginning in January 1955 and ending in December 1979.[2]

[1] Prior to the inception of this index on 14 April 1962, we used the large company oriented geometric *Financial Times* Ordinary Share Index. Theobald and Whitman [64] have shown that the latter was a good proxy for the All Share Index. In both cases, index returns were calculated by incorporating an estimate of dividend yield based on the published yield on the index. For a fuller description of these indexes, see Financial Times Publications [25].

[2] The 1955–9 period has no opening price recorded for January 1955, and virtually all February 1956 prices are missing because of a newspaper strike. So this period in fact has only fifty-eight months of data.

The stability of these estimates was then examined using the standard methodology. More precisely, betas were estimated for each company for each five year estimation period for which the company had a continuous listing.[3] Correlation coefficients were then calculated between successive beta estimates both for individual shares and for portfolios. The shares were then assigned to quintiles on the basis of their ranked beta estimates for each estimation period in turn. Portfolio regression tendencies were then evaluated by monitoring changes in quintile beta estimates from the estimation period to the subsequent prediction period. Finally, transition matrices were constructed to show the proportion of shares which move from one quintile in the estimation period to another in the prediction period.

Table I summarizes our results. The figures in Panel A are broadly similar to Sharpe and Cooper's [58] findings for the USA. Individual security beta estimates exhibit a modest but nevertheless noticeable degree of stability. Panel B shows that portfolio betas, on the other hand, appear highly stable in line with the findings of Blume [6] and others. Finally, Panel C indicates that although portfolio beta estimates for successive periods exhibit high rank correlation, as in the USA, they show a tendency to regress towards the mean. Note that for the UK betas, the mean is considerably less than unity. While this would be consistent with downward bias introduced by thin trading, there could be other explanations. In particular, it is important to note that there is no reason to expect an unweighted mean beta of unity, since we are using a value weighted index. Thus, for example, if securities with lower market capitalizations tend to have lower betas, we would expect the unweighted mean to be less than unity.

We have already noted, however, that these results may overstate the true degree of stability because of the extent and persistence of thin trading bias. To investigate the seriousness of this problem, we need to measure the magnitude of thin trading bias. As indicated in Equation (5), this will be directly related to the extent to which security and market returns are nonsynchronous. Fortunately, we can obtain a measure of this from the LSPD, since this database records not only the last trading price of the month, but also the date on which the trade took place. We therefore computed the trading infrequency I_{jp} of security j during estimation period p for all securities and estimation periods. The range of values found for I_{jp} ranged from 0.92 months down to zero (though, when logarithms were used, this minimum was truncated at .00005). The mean value of I_{jp} was 0.13.

To examine the extent to which the SR beta estimates are subject to thin trading bias, we divided up the sample in each period into quintiles on the basis of trading infrequency, and calculated the mean beta estimate for each quintile. In addition, to establish whether there is likely to be bias in our stability measures arising from serial correlation in trading infrequency, we constructed transition

[3] The consequential survivorship bias is unlikely to influence our results on beta stability, but may have a very marginal impact on our study of the stability of specific risk. Note that just under 30% of the securities in the initial five year estimation periods failed to survive (or, at least, to have complete histories) throughout the subsequent five-year period.

Table I

Summary of Study on Stability of Simple Regression Betas

Panel A: Stability of Simple Regression Betas for Individual Shares[a]						
Risk Class of Share in Beta Estimation Period		High	2	3	4	Low
Mean % in Same/Adjacent Class in Subsequent Period		72	80	71	76	79
Corresponding Results for US (Sharpe and Cooper [58])		79	82	76	77	82
Panel B: Effect of Increased Portfolio Size on Simple Regression Beta Stability						
Number of Shares in Portfolio		1	2	4	10	50
Mean Correlation with Subsequent Beta		.56	.68	.80	.91	.97
Corresponding Results for US (Blume [6])		.62	.74	.83	.91	.98
Panel C: Regression Tendency of Simple Regression Beta Estimates for Portfolios						
Risk Class of Portfolio		High	2	3	4	Low
Mean Portfolio Beta	Estimation Period	1.29	.97	.77	.53	.16
	Subsequent Period	1.03	.91	.77	.62	.38
Corresponding Results for US (Blume [5])	Estimation Period	1.58	1.16	.93	.71	.43
	Subsequent Period	1.43	1.14	1.01	.80	.53

[a] This table is based on 2,905 simple regression betas (Equation (1)) estimated over the periods 1955–9, 1960–4, 1965–9, 1970–4 and 1975–9. The number of securities in each pair of periods was 949, 772, 626, and 558 respectively. The procedures followed in drawing up Panels A, B, and C follow those described in the footnotes to Table V, VI, and VII respectively. Fuller details of this study are available from the authors.

matrices of shares partitioned into trading infrequency quintiles for each consecutive pair of five year periods. We also calculated the correlation coefficients between trading infrequencies in each adjacent pair of periods.

Table II shows the results of this analysis. The figures in the second column show the extent of thin trading. While this is negligible for the most frequently traded shares, thin trading is obviously a substantial problem for the shares in the least frequently traded quintile, where the degree of nonsynchronization (I_{jp}) averages some 43 per cent. At the same time there is clearly a very strong relationship between trading infrequency and the mean SR beta estimates. The latter tend to decline rapidly as trading becomes less frequent, which is consistent with the notion that these estimates suffer from serious thin trading bias. Furthermore, it is clear from Table II that any such bias will be persistent, since trading infrequency tends to be highly stable over time. On average, over two-thirds of all securities fall in the same trading infrequency quintile in the subsequent five year period, and no less than 98 per cent fall in either the same or an adjacent quintile. As further confirmation, it is worth noting that the correlation coefficients between the individual shares' trading infrequencies (I_{jp}) in the successive pairs of non-overlapping five year periods were respectively 0.84, 0.89, 0.89 and 0.91.

It is clear, therefore, that our SR beta estimates are subject to thin trading bias, that this bias persists over time, and that our stability estimates will, to some extent, reflect this bias. In order to quantify the extent of this bias, and its

Table II

Trading Infrequency: Stability and Relationship with Simple Regression Beta Estimates[a]

Trading Infrequency Class in Period p	Trading Infrequency (I_{jp})	Average Simple Regression Beta Estimate	Percentage of Securities in Trading Infrequency Class in Period $p+1$: Frequent	2	3	4	Infrequent	Same or Adjacent Class
Frequent	.001	1.01	83	15	1	0	0	98
2	.013	.96	19	56	24	1	0	99
3	.051	.81	1	22	54	22	1	98
4	.145	.62	1	3	18	60	19	96
Infrequent	.429	.32	0	0	3	17	80	97

[a] This table was constructed by taking the securities described in Table I, and dividing them into quintiles on the basis of their ranked trading infrequency within the period. The transition matrix was estimated over the same set of periods used in Table I. The chi-squared statistic is 4,665, which is significant at the 1 per cent level.

impact on our stability estimates, we require a set of bias-free beta estimates to use as a benchmark. As we noted above, the availability of transactions data from the LSPD, and of daily readings on the FTA index (which Dimson [17] has demonstrated to be relatively free from thin trading effects), makes it possible for us to estimate unbiased betas in the potentially most efficient way using the TT regression, Equation (7).[4] We therefore used this approach to obtain TT beta estimates for exactly the same set of securities and for the same five estimation periods as described above.[5]

To investigate whether one would reach different conclusions on stability using these TT estimates rather than SR betas, we partitioned our sample into two on the basis of trading infrequency. For each half, we calculated the correlations between security betas in successive periods for both SR and TT beta estimates. Although we would expect the TT estimates to be the more efficient,[6] they might nevertheless appear less stable than their SR counterparts if the value of ρ^* for the latter is sufficiently large due to the persistence of thin trading bias over time. In particular, SR might appear more stable than TT beta estimates for the less frequently traded half of our sample, since thin trading bias will be at its most serious here. This, however, is an empirical question, since we would also expect the relative efficiency of the TT estimates to be correspondingly greater for the less frequently traded securities.[7]

[4] In using Equation (7) as our specification for the TT regression, we are implicitly assuming that σ^2_{dt}/d_t is constant, where σ^2_{dt} is the variance of continuously compounded returns for periods of length d_t. This would be true if successive returns were identically and independently distributed. This seems a reasonable assumption, given the evidence of Fama [24], Brealey [9], and Dryden [22]. It will not hold precisely, however, for returns measured over very short intervals because of the bid-asked spread. While this will not introduce any bias, as long as the variances of both security and market returns share a common relationship with d_t, it may result in our estimates being somewhat inefficient if the weighting scheme used in (7) is non-optimal. In principle, we could avoid this problem by estimating the empirical relationship between σ^2_{dt} and d_t, and using this as our weighting scheme in (7). To establish whether this was worthwhile, we checked whether violations of the assumption actually affected our results. Since such violations are likely to be most serious in the case of returns measured over very short periods, we checked whether our results were sensitive to TT returns spanning periods of one week or less. Eliminating these returns had a negligible impact on the results reported below. We therefore concluded that the effort involved in estimating an empirical weighting scheme would not prove worthwhile.

[5] In estimating the TT betas for each subperiod, we used only those returns which fell wholly within the subperiod, with the exception of the initial return for each security, where we took the starting date and price as being the date and price of the last marked trade as at the start of the subperiod. This procedure ensured that all available return observations were utilized, and that no observation was allocated to more than one subperiod.

[6] We would expect the TT method to be more efficient since it is making direct and full use of all available information by taking account of the actual timing of trades. Because of this, however, the TT beta estimate will always be based on fewer or, at best, the same number of observations as the SR estimate. While superficially, it might seem that this lower number of observations could result in TT estimates being less efficient, this is misleading, since in both cases, the number of true underlying price and return observations is identical. The number of observations simply appears higher for SR estimates because of the method's disregard for the actual timing and occurrence of trades.

[7] More formally, we are comparing the values of ρ' obtained using both SR and TT beta estimates. From (4), $\rho' = \{\rho\sigma^2_\beta + \rho^*\sigma^2_\epsilon\}/\{\sigma^2_\beta + \sigma^2_\epsilon\}$. Since ρ and σ^2_β are the same for both sets of estimates, differences in ρ' will be related to differences in the magnitude of ρ^* and σ^2_ϵ. A priori, we would expect

In fact, for the more frequently traded group, the average correlation coefficient was 0.42, whether we used SR or TT beta estimates. This is precisely what we would expect if the securities in this subsample suffered from relatively little thin trading. It is clear from the second column of Table II that this was, in fact, the case. However, Table II also indicates that thin trading is far more serious with a much greater cross-sectional dispersion in the less frequently traded half of our sample. For this group of securities, the average correlation coefficients were 0.43 and 0.34 for SR and TT beta estimates, respectively.

Since both sets of estimates were obtained using the same samples and underlying returns information, any differences between them must be related to the adjustment for thin trading bias made in the TT approach. By construction, we would have expected this adjustment to provide more efficient and hence more stable estimates. These results, therefore, suggest that the higher mean correlation coefficient (ρ') of 0.43 for the SR beta estimates is upward biased, in line with the analysis of Section I. In other words, SR estimates appear more stable, in spite of their inefficiency, because of the relative magnitude of ρ^* arising from the persistence of thin trading over time.

Finally, we measured the extent of thin trading bias more directly by estimating the following relationship between beta estimates and trading infrequencies for each estimation period p:

$$\hat{\beta}_{jp} = \alpha + \gamma I_{jp} + \tilde{u}_{jp} \qquad j = 1(1)n_p \tag{8}$$

where n_p is the number of securities examined in estimation period p. If thin trading bias is a serious problem, we should find that Equation (8) has significantly lower explanatory power with TT estimates than with SR estimates.

The results in Table III confirm that this is, in fact, the case. The top panel shows that regression (8) of SR beta estimates on trading infrequency produced large slope coefficients in all periods. On average, 40 per cent of the cross-sectional variance in betas is explained by trading infrequency. However, the centre panel of Table 3 shows that when the same regression is run for TT beta estimates, the average slope coefficient is reduced and the percentage of variance explained by trading infrequency falls to 24 per cent. Clearly, this reduction is attributable to the thin trading bias adjustment made in estimating TT betas, since this is the only real difference between TT and SR estimates.

The question remains, however, as to why TT beta estimates should continue to be so strongly related to trading infrequency. By construction, the TT approach, using dated returns and a daily capitalization weighted index, must remove virtually all thin trading bias, except for any slight residual effect arising from the daily index failing exactly to match trading times within the day. While it is possible to devise techniques to eliminate this residual bias (e.g. Marsh [42]

ρ^* to be virtually zero for the TT estimates, but positive for SR estimates with its magnitude increasing with trading infrequency. Similarly, σ_ϵ^2 should be smaller for the TT estimates, and the relative efficiency of the latter should increase with trading infrequency. When we compare ρ' for the SR and TT estimates for the securities in the less frequently traded half of the sample, SR estimates could appear more stable if ρ^* was large enough to dominate the increased efficiency of the TT estimates.

Table III

Regression of Beta Estimates on Trading Infrequency[a]

	Dependent Variable	Independent Variable	Statistic	Period 1	Period 2	Period 3	Period 4	Period 5	Average
Panel A	Simple Regression Beta Estimate (Equation 1)	Trading Infrequency	Est. Slope	−1.210	−1.599	−1.556	−1.295	−1.391	−1.410
			(t-statistic)	(−24.8)	(−18.3)	(−21.4)	(−17.4)	(−24.9)	(−21.4)
			$\bar{R}$-Squared	.39	.30	.42	.35	.53	.40
Panel B	Trade-to-Trade Beta Estimate (Equation 7)	Trading Infrequency	Est. Slope	−.857	−1.327	−1.146	−.742	−1.042	−1.023
			(t-statistic)	(−16.3)	(−14.6)	(−15.3)	(−9.9)	(−18.0)	(−14.8)
			$\bar{R}$-Squared	.22	.22	.27	.15	.37	.24
Panel C	Adjusted Trade-to-Trade Beta Estimate	Adjusted Trading Infrequency	Est. Slope	−.054	−.050	−.043	−.031	−.002	−.036
			(t-statistic)	(−8.5)	(−5.9)	(−5.4)	(−3.9)	(−0.3)	(−4.8)
			$\bar{R}$-Squared	.07	.04	.04	.02	.00	.04

Results of Regression: Period 1 – Period 5, Average.

[a] The results in the first two panels of this table are derived from a cross-sectional regression of the SR and TT beta estimates used in Tables I and V respectively on trading infrequency (see Equation (8)). The final panel is based on a regression of trade-to-trade beta estimates on trading infrequency (see Equation (9)), where the data have been adjusted to remove the effects of the empirical relationship between these variables and equity capitalization. The adjusted data are thus the residuals from regressions of both TT beta estimates and the logarithm of trading infrequency on the logarithm of equity capitalization. The reason for taking logarithms in this case was to ensure that the residuals of the regressions underlying Panel C were, like the residuals from Panels A and B, distributed approximately normally.

incorporated a lag/lead market term in his TT regressions), the potential gains would seem to be very small; and in any event, this slight effect clearly cannot explain the relationship between TT betas and trading infrequency. Nor can the relationship be attributed to thin trading in the index, since Dimson [19] shows this would cause betas to be over-estimated. Furthermore, it seems unlikely that the effect is connected with the tendency for thinly traded shares to experience large intraspread price movements or to suffer from price rounding (see Dimson [20]).

It seems clear, therefore, that the relationship between TT estimates and trading infrequency must, because of its magnitude, be attributable to some effect other than thin trading bias. There are two possible explanations. First, trading infrequency may be related to some other variable such as company size which in turn is related to the security's true underlying beta. If this were the case, we would still expect γ to be positive, even if the TT method completely eliminated bias. Certainly trading infrequency and company size are highly correlated (see Dimson [19]), and, in fact, the correlation coefficient between the logarithms of trading infrequency and of market capitalization averaged 0.81 over the five time periods represented in Table III. At the same time, there is also evidence to suggest an association between beta and company size (see Pountain and Fitzgerald [47] and Rosenberg and Marathe [49] for UK and US evidence respectively). While these intercorrelations make it difficult to disentangle individual relationships, we nevertheless attempted to gain some insight into the effect of company size by estimating the relationship:

$$\hat{\beta}'_{jp} = \alpha + \gamma I'_{jp} + \tilde{w}_{jp} \qquad j = 1(1)n_p \tag{9}$$

where $\hat{\beta}'_{jp}$ is the residual from a regression of TT betas on the logarithm of equity capitalization and I'_{jp} is the residual from a similar regression of the logarithm of trading infrequency on the logarithm of equity capitalization. The bottom panel of Table III shows that when Equation (9) was estimated using TT beta estimates and trading infrequencies which had been adjusted in this way to remove their association with company size, the explanatory power of the equation was only four per cent.

While the observed relationship between TT beta estimates and trading infrequency may well, therefore, arise from a common relationship with company size, a possible alternative explanation is that actual transaction prices may, for some reason, lag behind market movements for the smaller, more thinly traded companies. Treynor[8] has suggested a possible rationale for this in terms of dealing costs and information motivated trading. He argues that traders acting on information about the value of the market as a whole will choose to profit from this by transacting in the most marketable securities with the lowest transactions costs. As a result of their actions, the large companies with heavily traded securities will find their prices responding more quickly than the small companies. The prices of small companies will respond only when investors in

[8] We are grateful to Barr Rosenberg for passing on this argument which was put forward to him by Jack Treynor in personal communication.

these securities, who may be oriented to a fundamental concept of value rather than to an analysis of the pattern of market movements, come to reflect the movement in the overall market into their thinking about the individual asset. Common factors which influence some but not all of the market will be even more likely to generate a nonsynchronous response, since the exposure of individual assets to those factors may not be clearly conceived. In general, the adjustment in stock prices will only be complete when investors' information processing costs are covered by the gains from analyzing the impact of such common factors. As a result, security price responses may be nonsynchronous with movements in common factors and with market movements as a whole (see also Rosenberg and Rudd [50]).

Such lagged responses would affect beta estimates in a very similar manner to thin trading bias. For example, smaller, less frequently traded companies would have lower contemporaneous beta estimates, and positive lagged beta coefficients. Some evidence that this is, indeed, the case is presented in Marsh [42]. This study found positive and significant lagged coefficient terms when TT returns were regressed on both contemporaneous and lagged market returns. Furthermore, when two different samples were compared, the lagged coefficients proved larger for the sample which, on average, contained the smaller sized companies. Fama, however, has pointed out that such differential lagged relationships could arise even in an efficient market with synchronous prices if there were nonstationarities in the risk or expected return of securities.[9]

However, whether the observed lagged relationships reflect nonsynchronous prices (Treynor) or nonstationarities (Fama), the effects are relatively small, especially in an efficient security market, and are conceptually quite distinct from conventional "Fisher [26] Effect" thin trading bias. Since we have such strong priors that the TT methodology both by construction and definition must remove virtually all conventional thin trading bias, it seems clear that the empirical results in Table III must be attributable to one or both of the above explanations. In all of our subsequent analysis, therefore, we shall assume that the TT method is effective in eliminating thin trading bias, and we shall work exclusively with TT estimates.

[9] We are grateful to Gene Fama for helpful discussion on this point. Fama has pointed out that lagged relationships can be expected in an efficient market if there are nonstationarities in the risk or expected return of securities. As one possible illustration of nonstationarity, suppose that for a less frequently traded security j there is an association between market returns in period t, $\tilde{R}_{mt}$, and the true beta in subsequent period(s), $\beta_{j,t+n}$. There could be a positive association if company j has less financial leverage than the market index; this would imply that (ceteris paribus) the contemporaneous beta β_{jt} is less than unity, while there is a positive slope β_{jt}^{n} in a regression of stock returns on lagged market returns. The association between market returns and beta could also be positive if the price of share j impounds above-average growth opportunities; this would imply that (ceteris paribus) $\beta_{jt} > 1$ and $\beta_{jt}^{n} > 0$. If the lower leverage observed in smaller British companies (Dimson [18]) or the higher growth prospects observed in their American counterparts (Rosenberg and Marathe [49]) have a dominant impact, or if there are other nonstationarities in expected returns, then a lagged relationship will exist when TT returns are regressed on lagged as well as contemporaneous market returns.

III. Evidence on the Stability of UK Risk Measures

While Table I showed that SR beta estimates are relatively stable, the results discussed in Section II demonstrate that this finding may be extremely misleading. On the one hand, thin trading bias imparts spurious stability to the SR estimates, while on the other hand the SR estimator is also inefficient. To identify the magnitude of thin trading bias, we therefore computed an unbiased estimate of beta using Equation (6). In Panels A and B of Table IV we compare the stabilities of the biased SR estimator (equation 1) with the unbiased estimator. It can be seen that the average correlation of 0.56 between successive SR beta estimates falls to 0.33 when these estimates are corrected for thin trading. Much of the apparent stability of SR beta estimates suggested by Table I is, indeed, an artefact of thin trading.

The average correlation shown in Panel B, however, understates the stability of betas. This is because Equation (6) is an inefficient estimator, particularly for the most infrequently traded stocks. The stability of the TT estimator (Equation (7)) is therefore examined in Panel C, where it can be seen that the correlation between successive TT estimates averages 0.47. It is interesting to observe that while efficient, unbiased (TT) estimates of beta are less stable than biased SR estimates, they are considerably more stable than unbiased SR estimates.

In addition to measuring the stability of our risk estimates using the correlation coefficient, we also examined their performance as predictors of subsequent beta estimates. To do this, we calculated the mean squared forecast error (MSE), defined as:

$$\begin{aligned} \text{MSE} &= \textstyle\sum_{j=1}^{n_p} (\hat{\beta}_{j,p+1} - \hat{\beta}_{jp})^2/n_p \\ &= (\bar{\beta}_{j,p+1} - \bar{\beta}_{jp})^2 + (1 - \hat{\gamma})^2 \operatorname{Var}(\hat{\beta}_{jp}) + (1 - \rho'^2) \operatorname{Var}(\hat{\beta}_{j,p+1}) \end{aligned} \tag{10}$$

To provide additional information, the MSE is broken down in the second line of this equality into its three standard components: bias, inefficiency, and random error (see Theil [62]). In this context $\bar{\beta}_{jp}$, $\bar{\beta}_{j,p+1}$ are the mean beta estimates in periods p, $p + 1$; $\hat{\gamma}$ is the slope of a regression of $\hat{\beta}_{j,p+1}$ on $\hat{\beta}_{jp}$; and ρ' is the correlation coefficient between these successive beta estimates. Bias arises from a shift in the mean betas over time (this is, of course, an entirely different concept from thin trading bias). The second component, inefficiency, is associated with a regression of beta estimates towards their mean. The random error term, on the other hand, is closely related to the correlation coefficient, so that the higher the correlation coefficient, the smaller the random error. Note that the reason for using the MSE here rather than simply the correlation coefficient, is that the latter fails to capture the problems of bias and inefficiency. The MSE not only incorporates these effects, but the Theil decomposition allows us to examine how the different components of the MSE vary with the choice of estimator. In fact it can be seen that, compared to the unbiased SR estimator, the TT estimator not only has a lower random error (resulting from its higher correlation), but it is also much more efficient.

Finally, we investigated the TT method by means of a test which does not prejudge which estimator is "correct." Our test is to examine whether TT betas

Table IV

Stability of Share Betas Estimated in Successive Periods Using Different Estimators[a]

Panel	Beta Estimate in Period $p+1$	Beta Estimate in Period p	Average Correlation Between Estimates in Successive Periods	Average MSE in Predicting Beta Estimates: Bias	Inefficiency	Random Error	Total MSE
Panel A	Simple Regression (SR) (Equation 1)	SR (Equation 1)	.56	.01	.03	.12	.16
Panel B	Unbiased SR (Equation 6)	Unbiased SR (Equation 6)	.33	.01	.09	.21	.31
Panel C	Trade-to-Trade (TT) (Equation 7)	TT (Equation 7)	.47	.01	.04	.12	.17
Panel D	Unbiased SR (Equation 6)	TT (Equation 7)	.41	.01	.04	.19	.24
Panel E	TT (Equation 7)	Unbiased SR (Equation 6)	.40	.01	.09	.13	.23

[a] All of these average correlation coefficients and mean squared errors (MSE) were computed from 2,905 betas estimated over the periods 1955–9, 1960–6, 1965–9, 1970–4 and 1975–9. The number of securities in each pair of periods was 949, 772, 626, and 558 respectively. The ranking of Panels A to E by the correlation coefficient or by any of the components of the MSE was the same in each of the four pairs of periods as in this table.

are better predictors of unbiased SR betas (6), as well as of TT betas (7). The rest of Table IV displays our results. The TT technique provides superior forecasts of subsequent unbiased SR beta estimates (compare Panels D and B), as well as of subsequent TT beta estimates (compare Panels C and E). The TT estimators outperform in terms of both the correlation coefficient and the components of MSE. The substantial magnitude of this outperformance is attributable to the nature of the adjustment for thin trading bias. This adjustment, Equation (6), tends to have greatest impact on those stocks which, because of infrequent trading, have large standard errors. Yet Equation (6) rescales the standard errors as well as the betas of these securities, increasing the cross-sectional dispersion of beta estimates. Thus, the TT method is superior not merely because of the virtual elimination of thin trading effects, but because it is a far more efficient method of estimating security risk.

Given the unbiased nature of the TT beta estimates, it is clear that they provide a much more trustworthy picture of beta stability. We, therefore, used the same sample and procedures as before to re-estimate all the stability results using TT beta estimates. This involved calculating correlation coefficients, constructing transition matrices, and examining the regression tendencies of portfolio betas.

The results shown in Table V–VII thus provide a more reliable view of beta stability since they are based on TT betas. Individual security beta estimates

Table V

Transition Matrices for UK Risk Measures[a]

Panel	Risk Measure	Risk Class in Period p	Percentage in Risk Class in Period $p+1$: High	2	3	4	Low	Same or Adjacent	Chi-Squared Statistic
Panel A	BETA (Trade-to-Trade Estimates)	High	41	27	16	10	6	68	
		2	25	27	23	16	9	75	704
		3	18	22	25	20	15	67	(Significant at 1
		4	10	16	23	28	25	76	percent level)
		Low	6	9	13	26	46	72	
Panel B	RESIDUAL RISK	High	62	23	11	3	1	85	
		2	24	33	27	12	4	84	1917
		3	9	25	29	27	9	81	(Significant at 1
		4	3	14	23	37	23	83	percent level)
		Low	1	6	9	21	63	84	
Panel C	STANDARD DEVIATION	High	59	23	11	5	2	82	
		2	21	31	25	15	8	77	1324
		3	12	21	27	27	13	75	(Significant at 1
		4	5	17	22	29	27	78	percent level)
		Low	2	8	15	24	51	75	

[a] These matrices were estimated using individual security risk measures calculated using trade-to-trade returns (Equation (7)) using the same sample as in Table I to IV above, and over the same set of time periods. The number of securities examined varied from period to period ranging from 949 in Periods 1 and 2 down to 558 in Periods 4 and 5 (see Table VI below). In total 2, 905 observations were used in estimating these matrices.

Table VI

Correlation Coefficients between Trade-to-Trade Portfolio Betas[a]

Number of Securities in Portfolio	Correlation Between Portfolio Beta Estimates in Successive Periods				
	Periods 1 and 2	Periods 2 and 3	Periods 3 and 4	Periods 4 and 5	Average
1	.46	.44	.44	.53	.47
2	.59	.57	.56	.67	.60
4	.70	.71	.71	.79	.73
7	.81	.82	.79	.86	.82
10	.84	.82	.82	.90	.82
20	.91	.92	.92	.96	.93
35	.93	.96	.94	.96	.95
50	.97	.97	.94	.97	.96
100	.99	.99	.99	.98	.99
Number of Observations	949	772	626	558	726

[a] These correlation coefficients were estimated using the same sample and set of beta estimates as in Table I–V above. The final row of the table shows the number of securities in the sample. Securities were first ranked by their beta estimates. Portfolios of n securities were then constructed by taking the first n securities for Portfolio 1, the next n for Portfolio 2 and so on. Thus in calculating the correlation coefficients between betas estimated in Periods 1 and 2, for example, 949 single security portfolios were examined, 474 two-security portfolios, and nine 100-security portfolios. Portfolio betas were calculated by averaging the individual security betas.

exhibit modest stability, with, on average, some 33 per cent falling in the same risk quintile five years after and some 72 per cent falling in the same or an adjacent quintile (see Panel A of Table V). Stability increases rapidly with portfolio size, with the average correlation coefficient increasing from 0.47 to 0.99 as the number of holdings in the portfolio is increased from 1 to 100 (see Table VI). And finally, betas show a marked tendency to regress towards the mean (see Panel A of Table VII). Note that, in general, TT beta estimates appear somewhat less stable than their SR equivalents. This suggests that the stability imparted by thin trading to the SR estimates more than overcame their poorer quality as predictors of true betas. This is consistent with the analysis of Sections I and II.

These results are, in fact, very similar to those for the USA. Whereas we find that the proportions of shares falling in the same (same or adjacent) quintile are 33 (72) per cent, the corresponding US figures are 40 (79) per cent, 33 (70) per cent and 40 (79) per cent in the periods 1931–67 (Sharpe and Cooper [58]), 1950–67 (Baesel [3]), and 1961–71 (Francis [29]). Similarly, the average correlation for individual security betas of 0.47 is similar to the US figures of 0.60, 0.37, and 0.45 recorded for the periods 1954–68 (Blume [6]), 1961–71 (Francis [29]), and 1962–75 (Alexander and Chervany [1]). Note that Blume's figures, which are the most widely quoted, also show the greatest stability. This may be partly due to his longer estimation period, but it could also reflect the earlier time period covered. While stability may have been greater then, the results for this period could also reflect some thin trading (see Fisher [26]).

Table VII

Stability and Regression Tendency of Risk Estimates Grouped by Quintiles[a]

Panel	Risk Measure	Quintile	Period 1*	Period 2	Period 2*	Period 3	Period 3*	Period 4	Period 4*	Period 5
			Average Risk Measures within Quintiles in the Estimation Period (Asterisked) and Subsequent Period							
Panel A	Trade-to-Trade Beta Estimates	1	1.20	1.04	1.37	1.02	1.20	1.09	1.36	1.03
		2	.90	.82	.98	.90	1.03	.99	1.08	.92
		3	.72	.71	.73	.81	.85	.94	.94	.88
		4	.52	.58	.49	.67	.64	.83	.79	.74
		5	.20	.34	.05	.53	.29	.71	.50	.49
		Mean	.71	.70	.73	.78	.82	.91	.93	.81
Panel B	Specific Risk Estimates	1	.52	.43	.46	.46	.47	.51	.54	.49
		2	.30	.29	.30	.32	.30	.40	.38	.37
		3	.24	.25	.24	.27	.24	.33	.32	.32
		4	.20	.22	.20	.23	.21	.30	.27	.28
		5	.16	.19	.15	.19	.16	.23	.20	.21
		Mean	.28	.29	.27	.29	.28	.35	.34	.33
Panel C	Standard Deviation Estimates	1	.53	.43	.47	.47	.49	.56	.60	.54
		2	.34	.31	.32	.33	.33	.45	.45	.45
		3	.28	.28	.27	.31	.28	.41	.39	.40
		4	.24	.25	.23	.27	.25	.37	.34	.39
		5	.20	.22	.18	.24	.20	.34	.28	.34
		Mean	.32	.30	.29	.32	.31	.43	.41	.42
Total Number of Securities			949		772		626		558	

[a] These quintiles were constructed by ranking the securities described in Table I and VI by their estimated risk measures in the earlier of each pair of periods. For betas, the quintile averages can be interpreted as portfolio betas. For specific risk and standard deviation, the quintile averages have no portfolio interpretation and are simply the average risk measures for the individual securities within the portfolios (quintiles).

It is also interesting to note that the overall mean beta estimate appears to shift over time, and is less than unity in every period examined (see Table VII). An explanation for the low mean is that smaller UK companies, unlike their US counterparts (see Rosenberg and Marathe [49] and Alexander and Chervany [1]), appear to have lower betas (see Dimson [19], Marsh [43], and Theobald [63]). This implies an unweighted mean beta which is less than the capitalization weighted mean beta of unity. The changes over time in the mean beta may well be due to the changing riskiness of smaller companies, a secular decrease in the proportion of smaller companies, and the shifting weights of high and low beta index constituents during this rather volatile period of UK stock market history.[10]

Finally, we examined the stability of residual risk and standard deviation estimates. Note that conventional estimates of these two items using (1) will, like betas, also suffer from severe distortions from thin trading. When there is slight thinness in trading the standard deviation will be overestimated, while severe nontrading will lead to an underestimate of the standard deviation.[11] To avoid this problem, we estimated both measures using the TT approach. Thus our standard deviation estimate was taken as the annualized value of the standard deviation of the dependent variable in (7). Note that in calculating the standard deviation from scaled returns in this way, we are implicitly assuming that the variance of continuously compounded returns is directly proportional to the scaling factor, namely the length of the time period over which returns are measured.[12] This same assumption applies to our estimates of residual risk which

[10] The changing riskiness of small companies might be explained by varying levels of gearing and reliance on trade credit (see Dimson [18] and Davis and Yeomans [16] respectively), and by the "small company" factor in the returns generating process (see Rosenberg and Marathe [49] for US evidence and Marsh [43] for British evidence). The number of UK registered companies with equity listed on the London Stock Exchange fell from 3,816 in 1955 (Smithers [59]) to 2,052 at the end of 1979, at which date over half the equity capitalization of the market was represented by the largest fifty companies (Stock Exchange [61]). Finally, the volatility of the market during the 1970s was greater than in any other volatile period (see Brealey, Byrne, and Dimson [10]), and the consequential changing weights of index constituents would also tend to lead to apparent changes in mean betas (see Schaefer, Brealey, Hodges, and Thomas [51]).

[11] When monthly prices represent transactions occurring before the month-end, the measured "monthly" returns used in (1) will represent a mixture of true returns spanning more than one month, and those spanning less than one month. If the variance of true returns is proportional to the length of the periods spanned by the returns, then thin trading will result in overestimation of the underlying standard deviation of true monthly returns. When there is severe thin trading, however, a large number of the measured monthly returns may be zero, and in extreme cases, this can lead to underestimates of the standard deviation.

[12] This assumption, discussed in Footnote 4 above, is far more important for standard deviation and residual risk estimates than for betas. A nonlinear relationship between the variance of returns and the length of the time period over which returns are measured could lead to biased standard deviation and residual risk estimates, particularly for thinly traded shares where some of the returns are measured over very short time intervals. This could in turn induce spurious correlation between estimates in successive periods so that risk measures appear stable simply because of the persistence of trading infrequency over time. Once again, we checked, therefore, to see whether violations of this assumption actually affected our results. In fact, excluding all returns measured over short periods of one week or less had a negligible impact on our results. Any bias in our empirical results which is induced by the employment of a non-optimal weighting scheme in (7) can clearly, therefore, be ignored.

we took as being the annualized value of the standard deviation of the error term in (7). To examine the stability of both of these sets of estimates, we once again followed the standard approach of calculating correlation coefficients and transition matrices, and examining regression tendencies.

Panel B of Table V shows the average transition matrices for residual risk estimates for individual securities, estimated using the same sample and periods as above. Residual risk exhibits a noticeable degree of stability, and on average, 45 per cent of the estimates fall in the same quintile five years later, while 83 per cent fall in either the same or an adjacent class. From these results, and from our previous findings on beta stability, we would clearly expect to find a corresponding degree of stability in the equivalent standard deviation (total risk) estimates. Panel C of Table V confirms this, showing that 39 per cent of the standard deviation estimates fall in the same quintile five years later, while 77 per cent fall in the same or an adjacent class. While there is a paucity of US evidence in this area, these results are very close to the figures of 40 and 84 per cent for standard deviations obtained by Francis [29] for the period 1961–71. We can therefore conclude that residual risk and standard deviation estimates for UK securities exhibit moderate stability of roughly the same magnitude as that found in the USA.[13] Finally, Panels B and C of Table VII show that residual risk and standard deviation estimates demonstrate the same kind of propensity as betas to regress towards the mean.

IV. Alternative Estimation Methods and the Stability of Betas

Having described a general method for estimating betas which are not subject to thin trading bias, we now examine the important practical question of how variants on this approach affect the quality of beta estimates. In particular, we look at the effects of varying the estimation period, and of making Bayesian adjustments and adjustments for heteroscedasticity. Finally, we look at the issue of betas and their regression tendencies.

A. The Estimation Period

From (1), it is clear that if true betas, β_{jp}, were stationary, we would obtain better estimates, the longer the time period examined. The estimation error would simply decline with the square root of the number of observations. It would then follow from (4) that the correlation coefficient between successive beta estimates would be higher the longer the time period examined. In practice, however, changes in the company's capital structure, in the nature of its business

[13] Note that our cross-sectional data shows that a share with a relatively high standard deviation in one period is likely to have a relatively high standard deviation in the next. This is not inconsistent with empirical applications of the Black-Scholes [4] model which suggest that market-implied standard deviations have an absolute value which is erratic over time (see Latane and Rendleman [39], Trippi [65], Chiras and Manaster [11], and Schmalensee and Trippi [52]). While the *absolute* value may be erratic, the *relative* standard deviation (which is what the correlation coefficient is measuring) may be moderately stable.

or in economic conditions will, certeris paribus, affect its beta (see Hamada [33], Sharpe [57], Masulis [44], Rosenberg and Marathe [49], and Francis and Fabozzi [30], respectively). There is therefore a tradeoff between statistical efficiency and contemporaneity, and the choice of estimation period will depend on the process by, and speed with which, betas change over time.

To examine the effects of varying the length of the estimation period, betas were estimated from (7) over successive estimation periods of 1, 2, 4, 5, 8, and 12 years. To facilitate comparisons, the same sample was used in each case, namely, the sample of all LSPD companies which had a continuous listing over the 25 year period 1955–79. For each length of estimation period, we examined how these estimates performed as predictors of betas in the subsequent period, which was either of the same length as the estimation period, or a fixed length of five years.[14] This involved calculating the average correlation coefficient between beta estimates in succesive pairs of adjacent periods. In addition, since we are concerned with the efficiency of our estimates as forecasts of subsequent betas, we also calculated the mean squared forecast error (MSE) as defined by Equation (10). Use of the Theil decomposition here enables us to identify how the components of the MSE vary with the length of the estimation period. We might find, for example, that the random error declines with the length of the period, while bias and inefficiency increase.

Table VIII shows how the average correlation coefficient between beta estimates in successive periods changes as the length of the estimation period is varied. The results suggest that stability, as measured by either the correlation coefficient or the MSE, increases substantially as the estimation period is lengthened. Furthermore, not only does the overall MSE decline with the length of the estimation period, but so do each of its component parts. Thus, there is certainly no evidence to suggest that a longer estimation period leads to an increase in bias or inefficiency. These results are consistent with the US findings of Baesel [3], Gonedes [31], and Eubank and Zumwalt [23].

We can therefore conclude that, for estimation purposes, the additional data obtained from extending the length of the estimation period more than compensates for the problems introduced by the use of older data.[15] Two notes of caution are required here, however. First, beyond five years, the improvement from extending the estimation period becomes more and more marginal. Since the longer estimation periods provide fewer observations to calculate the average correlation coefficient, we should not place too much confidence in these figures. Second, our conclusions are based on a sample which survived for 25 years. Non-survivors may well have less stable risk characteristics, and for these firms a shorter estimation period could be more appropriate.

[14] For most purposes, we would be concerned with a fixed length prediction period, and our results will certainly be easier to interpret in this context. We examine the cases where the prediction period is of the same length as the estimation period, largely to facilitate comparisons with previous US studies such as those by Baesel [3] and Eubank and Zumwalt [23].

[15] This should not be interpreted as meaning that it is worthwhile extending the length of the estimation period *while holding constant the number of observations*. When we did this, we found that 48 quarters' data produced beta estimates whose correlation (0.27) with subsequent beta estimates was lower than that shown in Table VIII for 48 months' data (0.35).

Table VIII

Stability of Betas Estimated Using Alternative Estimation Period Lengths[a]

Length of Estimation Period (Years)	Length of Prediction Period (Years)	Average Correlation between Successive Beta Estimates	Average Mean Squared Error (MSE) in Predicting Beta Estimates			
			Bias	Inefficiency	Random Error	MSE
1	1	.18	.02	.45	.57	1.04
2	2	.27	.01	.16	.27	.44
4	4	.35	.01	.08	.15	.23
5	5	.44	.01	.05	.12	.18
8	8	.46	.00	.04	.09	.14
12	12	.53	.00	.03	.06	.09
1	5	.24	.00	.29	.12	.42
2	5	.33	.00	.16	.13	.29
4	5	.43	.01	.06	.11	.18
8	5	.49	.01	.03	.09	.13
12	5	.51	.01	.02	.08	.10

[a] The correlation coefficients and mean squared errors (MSE) which have been averaged to provide the figures in this table were calculated from TT betas estimated using Equation (7). To facilitate comparisons, the same sample was used in each case, namely the sample of all LSPD companies which had a continuous history over the full 25 year period from 1955–79. Thus the average correlation coefficient for the five-year periods shown in this table (0.44) differs slightly from the average correlation figures shown in Table IV, VI, and IX (0.47). This is entirely attributable to the slight difference in the sample used.

Note: The number of observations used in calculating these averages varied with the length of the estimation period, ranging from 24 in the case of the one year estimation and prediction period down to one in the case of the twelve-year estimation and prediction period.

B. Adjustments for Regression Bias

It is well known that betas estimated from (1) or (7) will suffer from regression bias (see, for example, Blume [6, 7]). That is, there will be a tendency for high beta estimates to overstate true beta values and vice versa. Unless adjustments are made for this, the quality of the estimates for both securities and portfolios will be severely impaired. We therefore examined the two most commonly suggested adjustment procedures to determine which was the most efficient at removing regression bias for UK data (see Klemkosky and Martin [37] for results for the USA). First, we calculated adjusted betas $\hat{\beta}'_{jp}$using Blume's [6] procedure:

$$\hat{\beta}'_{jp} = \hat{\alpha}_p + \hat{\gamma}_p \beta_{jp} \tag{11}$$

where $\hat{\alpha}_p$ and $\hat{\gamma}_p$ are the constant term and slope coefficient from a cross-sectional regression of beta estimates in one period (p) on estimates for the previous period ($p - 1$). Second, we used the Bayesian approach[16] of Vasicek [66] and

[16] This is a true Bayesian approach only if there is no cross-sectional correlation between residual risk and beta. For our data, this does not seem an unreasonable assumption since the average cross-sectional correlation coefficient over the five periods examined was −0.09. This contrasts with the US findings of Rosenberg and Marathe [49] and others which show that residual risk and beta have

Litzenberger, Ramaswamy, and Sosin [41], whereby the adjusted beta is defined as:

$$\hat{\beta}'_{jp} = (\hat{\beta}_{jp}\sigma_{\beta}^2 + \bar{\beta}_{jp}\sigma_u^2)/(\sigma_{\beta}^2 + \sigma_u^2) \tag{12}$$

where σ_u^2 is the error variance of the estimate $\hat{\beta}_{jp}$, and where $\bar{\beta}_{jp}$ and σ_{β}^2 are the capitalization-weighted mean and cross-sectional variance of the true betas.[17]

Since in comparing these two approaches, our major concern is with the efficiency of the estimates as forecasts of subsequent betas, we once again used the MSE as our criterion. To facilitate comparisons, we also calculated the MSE for the raw betas estimated using (7), and for a naïve forecast $\hat{\beta}_{jp} = \bar{\beta}_{jp-1}$. It is conceivable, for example, that regression bias could be so serious that it would be better to use a naïve forecast than the raw beta estimates. Note that once again, it is important to focus on the MSE and its components rather than just looking at the correlation coefficient. For example, adjustment (11) will have no effect on the correlation coefficient but it should, nevertheless, reduce inefficiency. Adjustment (12) on the other hand should not only reduce inefficiency, but should also increase the correlation coefficient and hence decrease the random error. The contest between these two methods, however, is by no means a foregone conclusion. If, for example, the true underlying betas themselves are not stable over time, but instead tend to regress towards the mean (see Blume [7]) then adjustment (11) could prove a more effective method of reducing inefficiency.[18]

The results in Panels A and B of Table IX demonstrate just how serious the problem of regression bias can be. The MSE figures show that unless raw betas are adjusted for regression bias, we will actually obtain better forecasts from the naïve model that all betas will be equal to their historical mean. Fortunately, the use of Blume and Bayesian adjustments leads to a substantial reduction in the MSE, and both methods result in far better forecasts than the naïve model. Both approaches are highly effective in reducing inefficiency, with the Bayesian adjustments proving slightly superior. These results are broadly similar to the US findings of Fisher and Kamin [27], Klemkosky and and Martin [37], and Eubank and Zumwalt [23], although the latter found that Blume adjustments had a slight edge over the Bayesian approach.

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a strong positive cross-sectional correlation, which is probably explained by the fact that underlying determinants of risk such as operating and financial leverage affect specific and systematic risk in the same way. Superimposed on the negative relationship between residual risk and company size, however, is the positive relationship between the beta of British equities and company size. This phenomenon may well explain the slight negative correlations found for UK data.

[17] We took the cross-sectional variance of true betas as being equal to $\sigma^2(w_j\hat{\beta}_{jp}) - \sum_{j=1}^{n_p} w_j\hat{\sigma}^2(\hat{\varepsilon}_{jp})$ where w_j is the capitalization weight for security j, $\sigma^2(w_j\hat{\beta}_{jp})$ is the capitalization weighted cross-sectional variance of the beta estimates and $\sigma^2(\hat{\varepsilon}_{jp})$ is the standard error of $\hat{\beta}_{jp}$. This conforms to the standard procedure of Hodges [35], Blume [7], and others. Strictly speaking, however, this estimator will be unbiased only if the $\hat{\varepsilon}_{jp}$'s all have the same variance.

[18] If true betas were stationary over time and if the error variance were the same for all securities, the Blume and Bayesian approaches would be formally equivalent with $\gamma = \sigma_{\beta}^2/(\sigma_{\beta}^2 + \sigma_u^2)$. Note that in principle, the two approaches could be combined, so that we were applying the Blume adjustment (11) to Bayesian adjusted betas from (12).

Table IX

Stability of Share Betas Estimated in Successive Periods Using Alternative TT Estimation Methods[a]

Panel	Method	Average Correlation between Estimates in Successive Periods	Average MSE in Predicing Beta Estimates: Bias	Inefficiency	Random Error	Total MSE
Panel A	Beta equals historical mean	na	.01	.00	.16	.16
	Trade-to-trade (TT) (Equation 7))	.47	.01	.04	.12	.17
	As above, Bayesian adjustment (Equation (12))	.45	.01	.00	.12	.13
Panel B	Blume adjustment (Equation (11))	.47*	.01*	.01*	.10*	.12*
	Bayesian adjustment (Equation (12))	.46*	.00*	.00*	.10*	.11*
Panel C	TT weighted by cross-sectional variance of residual returns	.48	.01	.04	.12	.17
	As above, Bayesian adjusted	.46	.01	.00	.12	.13

[a] All of these average correlation coefficients and mean squared errors (MSE) were estimated using the same sample and over the same set of time periods as in Tables I–VI above. Asterisked results indicated that the averages are based on predictions for Periods 3, 4, and 5 only. This facilitates comparison between the Bayesian and Blume adjustments. Using the latter, no predictions are available for Periods 1 or 2, because of the need for two start-up periods.

C. Adjustment for Heteroscedasticity

Up to this point, all betas have been estimated using OLS regression, implicitly assuming that the error terms have constant variance. Yet Brealey, Byrne, and Dimson [10] found that the variance of UK market returns was not stable over time, but appeared to be subject to periodic shocks followed by a return to normalcy (see also Officer's [45] similar findings for the USA). If the residual variances are also nonstationary, then regressions using (7) will be heteroscedastic and will result in inefficient estimates. In an attempt to avoid the latter, we employed a weighting scheme based on the assumption that the residual variance for stock j in month t was proportional to the cross-sectional residual variance of all stocks in month t. This involved re-estimating the betas by weighting each observation in (7) by the reciprocal of the cross-sectional standard deviation of residual returns for that month.[19] We then compared the quality of these estimates with those obtained from (7), by calculating correlation coefficients and MSE's.

Panel C of Table IX shows the stability of beta estimates obtained using this weighting scheme. There is no measureable decline in MSE, suggesting that the gains in efficiency from weighting schemes of this sort are, at best, only marginal.

D. The Regression Tendencies of Beta Estimates

We have already noted that the observed regression tendencies of beta estimates may arise partly from estimate error, and partly from a regression of true betas towards their mean. Blume [7] suggests that the latter might occur because new projects taken on by companies tend to have less extreme risk characteristics than that of their existing business. In fact, any form of "lumpiness" in either investment or financing decisions will tend to produce this effect.

To investigate whether there is any UK evidence of true betas exhibiting regression tendencies, we followed Blume's [7] procedure. This involved forming quintiles on the basis of ranked beta estimates, and tracking the movement of portfolio betas from the estimation period to the two subsequent time periods. Since we had five available estimation periods, we were able to carry out this tracking exercise for each of the first three periods.

Table X shows how portfolio betas tend to regress towards their mean. On average over the three estimation periods, the unadjusted portfolio betas for the riskiest and least risky quintiles range from 1.29 down to 0.18. Keeping the constituents of each quintile the same in the first subsequent five-year period, the range narrows to 1.05 to 0.52. In the second subsequent period, the average range narrows further to 1.00 to 0.62. Comparing the second with the first subsequent period, there would appear to be evidence of slow true regression to the mean.

[19] This is an optimal weighting scheme if the interval between trades is constant, and if successive returns are independent, identically distributed normal variates. These assumptions are violated by a stochastic interval between trades, and the existence of a bid-ask spread (see Footnote 4 above). Since such violations would have their biggest impact on returns spanning a brief period, we once again checked whether our results were sensitive to TT returns spanning periods of less than one week. Eliminating these returns had a negligible impact on the results reported below.

Table X

Betas and Regression Bias[a]

Panel A: Average Portfolio Betas in Estimation Period and in Subsequent Period

Portfolio	Estimation Period (Raw Estimates)	Estimation Period (Bayesian Adjusted)	First Subsequent Period	Second Subsequent Period
1	1.29	1.10	1.05	1.00
2	.97	.95	.91	.93
3	.77	.84	.82	.84
4	.55	.73	.70	.75
5	.18	.60	.52	.62

Panel B: Pooled Regression (13) of Subsequent Unadjusted Beta Estimates on Unadjusted and Bayesian Estimates

Dependent Variable	Independent Variable	Slope	t-statistic	No. of Observations	R-Squared
Unadjusted Beta Estimate in First Subsequent Period	Unadjusted Beta Estimate	.46	23.5	2347	.19
	Bayesian Beta Estimate	.85	21.6	2347	.17
Unadjusted Beta Estimate in Second Subsequent Period	Unadjusted Beta Estimate	.33	16.1	1783	.13
	Bayesian Beta Estimate	.57	13.6	1783	.09

[a] The portfolios in Panel A are identical to those employed in Table VII, though the number of shares available in the second subsequent five-year period is somewhat lower. The regression results in Panel B are based on pooled data for individual shares over all three available estimation periods. The t-statistics are shown in parentheses.

We also investigated whether beta estimates still show a tendency to regress towards the mean even after they have been adjusted for estimate error. This involved comparing the mean Bayesian adjusted betas for the five portfolios in the estimation period with the portfolio betas in the two subsequent time periods. Table X confirms that the Bayesian adjustment procedure is effective in pulling the raw estimates in towards the mean, with the average range in the estimation period narrowing from 1.29 to 0.18 for the raw estimates to 1.10 to 0.60 for the Bayesian adjusted betas. Furthermore, if we assume that the Bayesian method makes the correct adjustment for regression bias, then a comparison of the average range of the Bayesian estimates with the figures for the first subsequent period would again seem to indicate that true betas regress towards the mean. However, these results would also be consistent with two alternative explanations. First, the Bayesian approach may be underadjusting for regression bias, and there may be little or no regression of true betas. Second, it may be over-adjusting, and thus obscuring the degree of true regression by pulling the estimates in too far towards the mean.

To help us choose between these joint hypotheses, we estimated the rate (λ) at which security betas regress towards the mean in both the first and second subsequent periods by fitting the model:

$$(\hat{\beta}_{jq} - \bar{\beta}_{jq}) = \lambda(\hat{\beta}^*_{jp} - \bar{\beta}^*_{jp}) + u_j \qquad j = 1(1)n_q \tag{13}$$

Both the dependent and independent variables in (13) are defined in terms of differences from the mean in order to abstract from any shifts in the unweighted mean beta of our sample between estimation periods. We estimated four different versions of (13). In the first two, period q was taken as the first subsequent period, $p + 1$, while $\hat{\beta}^*_{jp}$ was taken in turn as being the raw and the Bayesian adjusted beta from the estimation period. We then repeated this procedure taking period q to be the second subsequent period, $p + 2$. In each case, we estimated (13) by pooling data for all three available estimation periods. Note that, in doing this, we are implicitly assuming that the parameters of the model are constant across all shares and time periods.

The results of regression (13) are shown in panel B of Table X. We find that there is a slope of 0.46 in the regression of betas for the first subsequent period on raw betas for the estimation period. Presumably, this is partly attributable to regression bias, and partly to true regression towards the mean. When betas in the second subsequent period are regressed on the same raw betas from the initial estimation period, the slope falls to 0.33. Since the effect of regression bias should be the same regardless of which subsequent time period we examine, this reduction in the slope coefficient from 0.46 to 0.33 provides evidence that true betas regress towards the mean over time. Similar results were obtained when regression (13) was run on both residual risk and standard deviation estimates, providing evidence that these two risk measures are also subject to both regression bias and true regression towards the mean. For residual risk, the slope coefficients for the first and second subsequent periods were 0.70 and 0.56 respectively, while the corresponding figures for standard deviation were 0.71 and 0.66.

When beta estimates for the first subsequent period were regressed on the

Bayesian predictor, the slope coefficient was 0.85. This implies that, even after making Bayesian adjustments, there is still modest regression towards the mean. When estimates from the second subsequent period were regressed on the Bayesian estimator, the slope coefficient fell to 0.57. Note that this reduction from 0.85 to 0.57 is, as we should expect, roughly proportionately equivalent to the decline in slope from 0.46 to 0.33 which we found using raw estimates. This implies that the slope coefficient for the true regression to the mean is of the order of 0.7. Thus the Bayesian adjustment, with its slope coefficient of 0.85, would appear to be overadjusting for regression bias in the initial period estimates. By so doing, however, it actually improves these estimates as predictors of subsequent period betas by incorporating a partial adjustment for the true movement of betas towards their mean over time.

V. Summary and Conclusions

Thin trading can lead to serious bias in risk measures. Furthermore, since trading frequency is stable over time, this bias will be persistent, and will impart a spurious stability to estimates of beta and other risk measures.

This study has analyzed these distortions in estimated stability. Using an extensive sample over 25 years of history, we have shown that thin trading is indeed a serious problem. Fortunately, it is one which can be overcome by using the trade-to-trade method for estimating risk measures which are largely free from thin trading bias. In our empirical study, these risk measures are shown to be as stable in the UK as they are in the USA. Beta estimates are found to be moderately stable for individual shares and extremely stable for portfolios. The quality of these estimates can be improved by extending the estimation period and by making appropriate adjustments for regression bias. Even after such adjustments, however, UK beta estimates still appear to regress at a slow rate to their mean. Finally, estimates of the total and residual risk of shares appear to be at least as stable as those of beta.

Our research has a number of implications. First, it serves as an example of the importance of making adjustments for thin trading bias in any research study on thin markets. Second, it helps us interpret previous research on stability in such markets. We have already referred to previous work on European markets in this context. Finally, it provides important evidence that risk measures can be applied with the same degree of confidence in the UK as in the USA.

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