Model Adequacy Test Background

This appendix provides background information for a number of aspects of the model adequacy test.

Data

We use monthly data from January 1969 to December 2013 from SIRCA's Share Price and Price Relative (SPPR) database to evaluate the ability of a number of different pricing models to forecast equity returns.

The model that the ERA uses to estimate the cost of equity is a version of the SL-CAPM. This model implies that variation in required returns across equities will be completely explained by variation in betas across equities. So a sensible way of constructing portfolios to be used in testing the ability of the model to forecast equity returns is to form portfolios, like Black, Jensen and Scholes (1972) and Fama and MacBeth (1973), on the basis of past estimates of beta.

Forming portfolios on the basis of past estimates of beta

To form portfolios on the basis of past estimates of beta, we begin by extracting data from January 1969 to December 2013 for individual stocks from the SPPR database. The SPPR database does not provide market capitalisations before December 1973 and so we do not begin to record the returns to the portfolios that we construct until January 1974. We use past estimates of betas to allocate stocks to portfolios, however, and so we use data from before January 1974 to determine in which portfolios to place stocks in the early years of the time series that we construct. To minimise the impact of market microstructure effects, at the end of each year we use past data to estimate the betas only of stocks that are in the top 500 by market capitalisation. We choose the top 500 because the All Ordinaries Index is constructed from the top 500 stocks.

We form a number of value-weighted portfolios. First, we form a value-weighted portfolio of the top 500 stocks by market capitalisation and use the portfolio as a proxy for the market portfolio. Second, we form value-weighted portfolios on the basis of past beta estimates. At the end of December each year we use data for the prior five years to estimate the betas of all stocks relative to the market portfolio, dropping those that do not have a full 60 months of data. We then place the stocks into 10 portfolios on the basis of the estimates and record the returns to these portfolios for each month of the following year. So, for example, we compute beta estimates using data from January 1969 to December 1973 for stocks that are in the top 500 by market capitalisation at the end of December 1973. We allocate these stocks to 10 portfolios on the basis of these estimates and then record the returns to the portfolios for each month of 1974. Next, we compute beta estimates using data from January 1970 to December 1974 for stocks that are in the top 500 by market capitalisation at the end of December 1974, allocate these stocks to 10 portfolios on the basis of the estimates and then record the returns to the portfolios for each month of 1975. And so on. Thus we form portfolios in a way that is similar to the manner in which Black, Jensen and Scholes (1972) and Fama and MacBeth (1973) form portfolios.

Industry portfolios

We also use the returns to industry portfolios provided by SIRCA. These portfolios also display a considerable variation across the portfolios in beta. There are, however, two drawbacks to using the industry returns that SIRCA provides. First, industry portfolios are not as well diversified as portfolios formed on the basis of past estimates of beta – which are diversified across industries. This lack of diversification lowers the power of tests that use individual portfolios. Second, the way in which SIRCA forms the portfolios may lead to a survivorship bias (SIRCA (2013)).[1]

Imputation credits

We compute the returns to the portfolio that we use inclusive of a value assigned to imputation credits. In particular, we assign a value of 35 cents to each dollar of imputation credits distributed. Thus the partially franked returns that we use are the unfranked returns plus 35 percent of the difference between the fully franked and unfranked returns.

Risk-free rate

We use as a measure of the risk-free rate the yield, computed on a monthly basis, on a 10-year Commonwealth Government bond. We extract the yields on these bonds from the Reserve bank of Australia.

Missing observations

Some of the industry returns are missing. To compute Wald statistics that take into account the fact that some of the returns are missing, we use the following procedure.

Let N be the number of portfolios, and let $N-K_t$ be the number of missing forecast errors in month t. Also let J_t be an n-dimensional identity matrix whose *j*th column is eliminated if the *j*th forecast error is missing at time t. J_t will be an $N \times K_t$ matrix. Consider the model:

$$f_t = \alpha + \varepsilon_t, \tag{1}$$

where f_t is an $N \times 1$ vector of forecast errors, α is an $N \times 1$ vector of mean forecast errors and ε_t is an $N \times 1$ vector of disturbances. The vector of sample mean forecast errors, computed using all available data, will be:

$$\hat{\alpha} = \left[\sum_{t=1}^{T} J_t J_t' \right]^{-1} \sum_{t=1}^{T} J_t J_t' f_t \tag{2}$$

If $\mathrm{E}\left(\varepsilon_t \varepsilon_{t-w}'\right) = 0$ for $|w| > 0$ and the distribution of f_t is independent of whether the data are missing, then the variance-covariance matrix of the estimator (2) will be given by:

[1] SIRCA (2013) notes that:

> 'all indices based on GICS rely on industry definitions obtained since July 2001. Even so, many of the GICS based indices report values back to January 1974. Index values before July 2001 have been generated by imputing GICS codes back through time. GICS codes were extended back through time for company name segments with the same ASX industry classification as was present when GICS were first assigned. The assumption behind this was that unchanged ASX classification implied unchanging GICS codes. Although this method identifies companies whose industry interests are similar to those held later, it introduces implicit survivor biases. Biases arise because companies changing industry focus are not tracked in earlier periods. Nor are companies that delist before July 2001 because they are not present when GICS are first assigned and so are ineligible for GICS industry membership. Survivor biases can be expected to increase the further back in time present day GICS are imputed.'

$$\left[\sum_{t=1}^{T} J_t J_t'\right]^{-1} \sum_{t=1}^{T} J_t J_t' \mathrm{E}\left(\varepsilon_t \varepsilon_t'\right) J_t J' \left[\sum_{t=1}^{T} J_t J_t'\right]^{-1} \tag{3}$$

If the proportion of data for each portfolio that are missing is independent of the sample size, then under the usual regularity conditions:

$$\left[\sum_{t=1}^{T} J_t J_t'\right]^{-1} \sum_{t=1}^{T} J_t J_t' \hat{\varepsilon}_t \hat{\varepsilon}_t' J_t J' \left[\sum_{t=1}^{T} J_t J_t'\right]^{-1} \tag{4}$$

will be a consistent estimator for the variance-covariance estimator (3). Here:

$$\hat{\varepsilon}_t = f_t - \left[\sum_{t=1}^{T} J_t J_t'\right]^{-1} \sum_{t=1}^{T} J_t J_t' f_t \tag{5}$$

If, again, the proportion of data for each portfolio that are missing is independent of the sample size, then under the usual regularity conditions:

$$\begin{aligned} &\hat{\alpha}' \left[\left[\sum_{t=1}^{T} J_t J_t'\right]^{-1} \sum_{t=1}^{T} J_t J_t' \hat{\varepsilon}_t \hat{\varepsilon}_t' J_t J' \left[\sum_{t=1}^{T} J_t J_t'\right]^{-1}\right]^{-1} \hat{\alpha} \\ &= \sum_{t=1}^{T} f_t' J_t J_t' \left[\sum_{t=1}^{T} J_t J_t' \hat{\varepsilon}_t \hat{\varepsilon}_t' J_t J'\right]^{-1} \sum_{t=1}^{T} J_t J_t' f_t \end{aligned} \tag{6}$$

will be asymptotically χ_N^2 under the null hypothesis that the vector mean forecast errors $\alpha = 0$.

The Black and the SL-CAPM

Two of the models that we use are empirical versions of the Sharpe-Lintner Capital Asset Pricing Model (SL-CAPM) and the Black Capital Asset Pricing Model (CAPM).

The SL-CAPM implies that:

$$\mathrm{E}_{t-1}(z_{jt}) = \beta_{jt} \mathrm{E}_{t-1}(z_{mt}), \tag{7}$$

where $\mathrm{E}_{t-1}(.)$ denotes an expectation formed on all that is known at time $t-1$, z_{mt} is the return to the market portfolio from time $t-1$ to t in excess of the risk-free rate, quoted at time $t-1$ and to be earned from time $t-1$ to t,

$$\beta_{jt} = \frac{\mathrm{E}_{t\text{-}1}\left[\,(z_{jt} - E_{t-1}(z_{jt}))(z_{mt} - E_{t-1}(z_{mt}))\right]}{\mathrm{E}_{t\text{-}1}\left[(z_{mt} - E_{t-1}(z_{mt}))^2\right]}$$

and z_{jt} is the excess return to portfolio j from time $t-1$ to t.

The Black CAPM implies that:

$$\mathrm{E}_{t-1}(z_{jt}) = (1-\beta_{jt})\mathrm{E}_{t-1}(z_{0t}) + \beta_{jt}\mathrm{E}_{t-1}(z_{mt}), \tag{8}$$

Australian regulators have not used the Black CAPM explicitly but have suggested that they will use the model implicitly. The Australian Energy Regulator (2013), for example, states that:

> 'To the extent the Black CAPM may have some support, we will use the model (in addition to other evidence) to inform the selection of the equity beta.'

A regulator using the Black CAPM explicitly would set the cost of equity for a firm equal to:

$$(1-\hat{\beta}_{jt})\,\hat{z}_{0t} + \hat{\beta}_{jt}\hat{z}_{mt}, \tag{9}$$

where $\hat{z}_{0t}$ denotes the regulator's assessment of the zero-beta premium to be earned from $t-1$ to t and $\hat{z}_{mt}$ is the regulator's assessment of the market risk premium. The regulator might set $\hat{z}_{mt}$ and $\hat{z}_{0t}$ equal to the means of all past excess returns to the market portfolio and all past estimates of the zero-beta premium but may choose to use other information.

The expression (9), however, can also be rewritten as:

$$\beta^*_{jt}\hat{z}_{mt}, \tag{10}$$

where

$$\beta^*_{jt} = \left(1-\frac{\hat{z}_{0t}}{\hat{z}_{mt}}\right)\hat{\beta}_{jt} + \left(\frac{\hat{z}_{0t}}{\hat{z}_{mt}}\right) \tag{11}$$

Thus a regulator using the Black CAPM implicitly could use (10) to set the cost of equity for a firm.

We label the adjusted estimate of beta given by (10) 'betastar'. The estimates of betastar that we use employ estimates of the zero-beta premium and market risk premium that are the means of all past excess returns to the market portfolio and all past estimates of the zero-beta premium that use individual security data provided by NERA (June 2013, October 2013) and updated by NERA to the end of 2013. We describe below how NERA has updated estimates of the market risk premium (MRP). The details of how NERA estimates the zero-beta premium are contained in its June 2013 report *Estimates of the zero-beta premium*.

We compute the standard error of betastar using the delta method (see, for example, Hayashi (2000)). Betastar will be a function of the ordinary least squares (OLS) estimate of the beta of the portfolio computed using monthly data from $t-T$ to $t-1$, an estimate of the zero-beta premium computed using monthly data from $t-S$ to $t-1$ and an estimate of the MRP computed using annual data from month $t-M$ to $t-1$, where $t>M>S>T$. The standard error of betastar will depend in part on an estimate of the covariance matrix of a vector containing these three estimates. The difficulties in estimating the covariance matrix are that (a) the estimates use samples that only partially overlap and (b) two of the estimates are based on monthly data while one of the estimates is based on annual data. We circumvent these difficulties using methods similar to the ones that we describe above for dealing with missing data.

NERA update of the MRP

NERA's estimates of the MRP are based on estimates that Brailsford, Handley and Maheswaran (2008, 2012) produce.

The data that Brailsford, Handley and Maheswaran (2008, 2012) employ from 1883 to 1957 are constructed from:

- the Commercial and Industrial index assembled by Lamberton (1958) from 1882 to 1936;
- the Sydney Stock Exchange (SSE) All Ordinary Shares price index from 1936 to 1957; and
- the Lamberton/SSE yield series from 1883 to 1957;

Brailsford, Handley and Maheswaran (2008, 2012) lower the yields provided by Lamberton (1961) between 1883 and 1957 by multiplying them by 0.75. NERA (June 2103, October 2013) shows that while some downward adjustment of Lamberton's yield series is warranted, data from original sources indicate that the adjustment should be smaller than the adjustment that Brailsford, Handley and Maheswaran make. NERA shows that an estimate of the downwards bias generated by inappropriately adjusting Lamberton's yield series is 36 basis points for the period 1883 to 2012. NERA (October 2013) provides a time series of returns to the market portfolio from 1883 to 1957 that use adjustments to the dividend yields that Lamberton supplies that are indicated by data drawn from original sources.

From 1958 to 2010, the data that NERA uses are identical to the data that Brailsford, Handley and Maheswaran (2012) use while the data that NERA uses from 2011 to 2013 are updates of the data that Brailsford, Handley and Maheswaran employ. We explain below how NERA updates the data from 1958 to 2010 that Brailsford, Handley and Maheswaran supply.

NERA extracts daily data (for days on which the market was open) for the All Ordinaries Index (AS30) and the All Ordinaries Accumulation Index (ASA30) from Bloomberg.

Like Brailsford, Handley and Maheswaran (2008, 2012), NERA extracts imputation credit yields for December of each year from the Australian Taxation Office (https://www.ato.gov.au/rates/company-tax---imputation--average-franking-credit---rebate-yields/?page=2#List_of_yields).

Like Brailsford, Handley and Maheswaran (2008, 2012), NERA takes 90-day bank accepted bill rates, the yields on three-month Treasury notes and the yields on 10-year Commonwealth Government bonds from the Reserve Bank of Australia (http://www.rba.gov.au/statistics/index.html).

Finally, like Brailsford, Handley and Maheswaran (2008, 2012), NERA uses the percentage change in the All Groups CPI for Australia from the last quarter of one year to the last quarter of the next year, provided by the Australian Bureau of Statistics, as a measure of inflation (http://www.abs.gov.au/AUSSTATS/abs@.nsf/DetailsPage/6401.0Sep%202014?OpenDocument).

Like Brailsford, Handley and Maheswaran (2008, 2012), NERA computes the annual with-dividend return to the market portfolio in data from 1981 onwards as the percentage change from one year to the next in the average December level of the All Ordinaries Accumulation Index.

To produce gross returns, NERA adds to the with-dividend return 35 per cent of the credit return – that is, the ratio of the credits provided by the All Ordinaries within a year to the level of the index at the start of the year.

Like Brailsford, Handley and Maheswaran (2008, 2012), NERA computes an estimate of the *MRP* by averaging the difference between each year's gross return and the yield on a 10-year Commonwealth Government bond at the end of each year.

The gross return to the All Ordinaries from December 2013 to December 2013 was 18.66 per cent while the yield on a 10-year Commonwealth Government bond at the end of 2013 was 4.23 per cent. Thus the excess return to the market portfolio computed in the same way that Brailsford, Handley and Maheswaran (2008, 2012) compute the return was 14.43 per cent – considerably above its long-run average. As a result, estimates of the *MRP* rise with the addition of 2013's data. The table below shows how estimates of the *MRP* have been affected by the addition of 2013's data for a variety of sub-periods that the Australian Energy Regulator (AER) has in the past used.

Table 1

Estimates of the *MRP*

Period	MRP estimate	Standard error	Period	MRP estimate	Standard error
1883-2012	6.50	1.45	1883-2013	6.56	1.44
1937-2012	5.67	2.26	1937-2013	5.79	2.23
1958-2012	6.16	3.02	1958-2013	6.31	2.97
1980-2012	5.84	3.90	1980-2013	6.09	3.79
1988-2012	5.12	3.68	1988-2013	5.48	3.55

There are three points that are worth making about this table and these estimates.

First, as the table above makes clear, estimates of the *MRP* are imprecise and estimates that use shorter time series are less precise than estimates that use longer time series.

Second, the AER's habit of using overlapping sample periods like those that appear in the table above amounts to placing a larger weight on more recent data than on older data. While this may appear sensible, the impact of weighting more recent data more heavily than older data is to reduce the precision of the estimates (see section 5 of NERA (June 2013)). So we would not endorse this way of summarising the data.

Thirdly, the estimates are based on arithmetic means. While compounding these means would produce estimates that are biased, there is no evidence that the AER or ERA compounds the estimates (see section 4 of NERA (June 2013)).

Testing whether forecasts of the return on equity are unbiased

Rule 74 of the National Gas Rules, relating generally to forecasts and estimates, states:

(1) Information in the nature of a forecast or estimate must be supported by a statement of the basis of the forecast or estimate.

(2) A forecast or estimate:

(a) must be arrived at on a reasonable basis; and

(b) must represent the best forecast or estimate possible in the circumstances.

Since it is difficult to see that a forecast of the return on equity that can be shown to be systematically biased could meet these criterion, we test whether forecasts of the return on equity are unbiased that use a number of different pricing models. Our tests use two methods and we label these methods: Method A and Method B. Method A uses explicit forecasts of the MRP, the zero-beta premium and the Fama-French high-minus-low (HML) and small-minus-big (SMB) premiums. Method B, on the other hand does not do so.[2] Method B merely assumes that a regulator will use rational forecasts, that is, forecasts that are unbiased.[3]

Method A

If forecasts generated by the SL-CAPM are unbiased, then:

$$\mathrm{E}\left(z_{jt} - \hat{\beta}_{jt}\hat{z}_{mt}\right) = 0, \tag{12}$$

where, again, $\hat{z}_{mt}$ is the regulator's assessment of the market risk premium and where $\hat{\beta}_{jt}$ is an estimate of the beta of portfolio j that uses data from months up to (and including) month $t-1$.

We test whether the restriction (12) holds true by examining whether its sample counterpart:

$$\frac{1}{T}\sum_{t=1}^{T}(z_{jt} - \hat{\beta}_{jt}\hat{z}_{mt}) \tag{13}$$

differs significantly from zero. The quantity (13) is the mean forecast error associated with forecasts of excess returns that use the SL-CAPM.[4]

We test the other pricing models in the same way. Thus, for example, we test whether forecasts generated by the Black CAPM are unbiased by examining whether the mean forecast error:

$$\frac{1}{T}\sum_{t=1}^{T}(z_{jt} - (1-\hat{\beta}_{jt})\,\hat{z}_{0t} - \hat{\beta}_{jt}\hat{z}_{mt}) = \frac{1}{T}\sum_{t=1}^{T}(z_{jt} - \beta^{*}_{jt}\hat{z}_{mt}) \tag{14}$$

differs significantly from zero.

Note that Wald statistics that test whether the means of the forecast errors

$$z_{jt} - (1-\hat{\beta}_{jt})\,\hat{z}_{0t} - \hat{\beta}_{jt}\hat{z}_{mt}, \qquad j = 1, 2, \ldots, N \tag{15}$$

differ from zero will be identical to Wald statistics that test whether the mean of the forecast errors

$$z_{jt} - \beta^{*}_{jt}\hat{z}_{mt}, \qquad j = 1, 2, \ldots, N \tag{16}$$

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[2] Aside from forecasts of the zero-beta premium and MRP required to compute β^{*}_{jt}.

[3] See Keane and Runkle (1989) for a discussion of what it means for a forecast to be rational.

[4] Since the risk-free rate for an investment made at the end of month $t-1$ that matures at the end of month t is known at the end of month $t-1$, it is also the mean forecast error associated with forecasts of returns that use the SL-CAPM.

differ from zero. This is because

$$(1-\hat{\beta}_{jt})\,\hat{z}_{0t}+\hat{\beta}_{jt}\hat{z}_{mt}=\beta^{*}_{jt}\hat{z}_{mt} \tag{17}$$

Method B

If the regulator's assessment of the market risk premium is rational, that is, unbiased, then:

$$\mathrm{E}\left(\hat{z}_{mt}\right)=\mathrm{E}\left(z_{mt}\right) \tag{18}$$

It follows that if forecasts generated by the SL-CAPM are unbiased and the regulator's assessment of the market risk premium is rational, then:

$$\mathrm{E}\left(z_{jt}-\hat{\beta}_{jt}z_{mt}\right)=0 \tag{19}$$

We test whether the restriction (19) holds true by examining whether its sample counterpart:

$$\frac{1}{T}\sum_{t=1}^{T}(z_{jt}-\hat{\beta}_{jt}z_{mt}) \tag{20}$$

differs significantly from zero. The quantity (20) is the mean difference between two zero-investment strategies.

The quantity z_{jt} is the return to a zero-investment strategy that is long portfolio j and short the risk-free asset.

The quantity $\hat{\beta}_{jt}z_{mt}$ is the return to a zero-investment strategy that is long the market portfolio and short the risk-free asset.

If the SL-CAPM generates forecasts that are unbiased and the regulator's assessment of the market risk premium is rational, then the mean difference between the returns to the two zero-investment strategies should be zero.

We test the other pricing models in the same way. Thus, for example, we test whether forecasts generated by the Black CAPM are unbiased by examining whether the mean forecast error:

$$\frac{1}{T}\sum_{t=1}^{T}(z_{jt}-(1-\hat{\beta}_{jt})\,z_{0t}-\hat{\beta}_{jt}z_{mt}) \tag{21}$$

differs significantly from zero. Here z_{0t} and z_{mt} denote the realised returns to a zero-beta portfolio and the market portfolio from month $t-1$ to month t in excess of the risk-free rate and not the regulator's assessments of what the zero-beta and market risk premiums should be based on data from months up to (and including) month $t-1$.[5]

Note that Wald statistics that test whether the means of the forecast errors

$$z_{jt}-\beta^{*}_{jt}z_{mt}, \qquad j=1,2,\ldots,N \tag{22}$$

[5] In tests of (15) we use Fama-MacBeth estimates of the realised return to a zero-beta portfolio from month $t-1$ to month t in excess of the risk-free rate provided by NERA.

differ from zero will not be identical to Wald statistics that test whether the mean of the forecast errors

$$z_{jt} - (1-\hat{\beta}_{jt})\, z_{0t} - \hat{\beta}_{jt} z_{mt} \tag{23}$$

differ from zero because

$$(1-\hat{\beta}_{jt})\, z_{0t} + \hat{\beta}_{jt} z_{mt} \neq \beta^{*}_{jt} z_{mt} \tag{24}$$

Discussion

We anticipate that the properties of tests that use Method A and the properties of tests that use Method B will display some interesting differences. As Fama and French (1997) point out, much of the variability in estimates of the cost of equity that use either the SL-CAPM or the Fama-French three-factor model can be traced to variability in estimates of the market risk premium or the Fama-French factor risk premiums. We anticipate that variability in estimates of these premiums will lower the power of tests for individual portfolios that use Method A relative to tests that use Method B. We anticipate, on the other hand, that the power of Wald joint tests that use Method A will differ little from the power of Wald joint tests that use Method B. This is because Wald joint tests take into account common variation across portfolios.

Assessing the power of the tests

We conduct bootstrap simulations to examine the behaviour of the test statistics that we use and the power of our tests.

To begin with, we use least squares to estimate for each of the N portfolios that we employ the time series regression:

$$z_{jt} = \alpha_j + \beta_j z_{mt} + \varepsilon_{jt}, \qquad j = 1,2,\ldots,N, \quad t = 1,2,\ldots,T, \tag{25}$$

where z_{jt} and z_{mt} are the returns to portfolio j and the market portfolio in excess of the risk-free rate, α_j and β_j are an intercept and slope coefficient and ε_{jt} is a regression disturbance.

We place in each row of a $T \times (N+1)$ matrix E the vector $(\hat{\varepsilon}_{1t}, \hat{\varepsilon}_{2t}, \ldots, \hat{\varepsilon}_{Nt}, z_{mt})$ where $\hat{\varepsilon}_{jt}$ is a least squares residual.

We simulate data for T months using the fitted regression, random drawings with replacement from the rows of the matrix E and, initially, the restriction that:

$$\alpha_j = 0, \; j = 1,2,\ldots,N. \tag{26}$$

In this way we create data that may display heteroskedasticity but are drawn from a model in which the SL-CAPM is true.

Where some of the data are missing we proceed in the following way. First, we delete from the data all months where at least one portfolio return is missing. Let the number of months of data that we delete be M. Then the length of the time series with which we are left will be $T - M$.

Second, we use these time series to estimate the parameters of the regression (25) and to produce a $(T-M)\times(N+1)$ matrix of residuals and market excess returns.

Third, we simulate data for T months using the fitted regression, random drawings with replacement from the rows of the matrix and, initially, the restriction (26)

Fourth, we eliminate observations that were missing in the original set of data. In other words, we ensure that the simulated set of data has the same pattern of missing observations as the original set.

To begin with, we examine the behaviour of the t-test statistics and Wald statistics under the null hypothesis that the SL-CAPM is true. We then examine their behaviour under the alternative that the SL-CAPM is false. The results of the simulations for the 10 portfolios formed on the basis of past estimates of beta appear in Table 2 below and indicate that while the finite-sample distributions of the test statistics that we use differ, under the null hypothesis, from their theoretical asymptotic counterparts, the differences are small.

Table 2

Distribution under the null of statistics used to test the SL-CAPM: 10 portfolios formed on the basis of past estimates of beta

	Method A				Method B			
	Probability that null is rejected at a significance level of				Probability that null is rejected at a significance level of			
Portfolio	0.995	0.975	0.025	0.005	0.995	0.975	0.025	0.005
1	0.993	0.976	0.029	0.008	0.994	0.971	0.025	0.004
2	0.997	0.976	0.037	0.007	0.995	0.971	0.029	0.006
3	0.996	0.979	0.035	0.009	0.995	0.976	0.028	0.007
4	0.996	0.975	0.031	0.008	0.994	0.972	0.029	0.005
5	0.996	0.978	0.031	0.007	0.994	0.972	0.023	0.005
6	0.997	0.979	0.035	0.008	0.994	0.974	0.027	0.006
7	0.997	0.978	0.034	0.009	0.995	0.975	0.031	0.007
8	0.996	0.978	0.029	0.007	0.995	0.974	0.021	0.005
9	0.995	0.978	0.028	0.007	0.994	0.972	0.022	0.005
10	0.995	0.975	0.026	0.006	0.994	0.972	0.023	0.005
	Method A				Method B			
	Probability that null is rejected at a significance level of				Probability that null is rejected at a significance level of			
	0.500	0.100	0.050	0.010	0.500	0.100	0.050	0.010
Wald	0.540	0.124	0.065	0.017	0.535	0.122	0.065	0.017

Notes: The results are generated using bootstrap simulations, 10, 000 replications and the returns to 10 portfolios formed on the basis of past estimates of beta from January 1974 to December 2013. The theoretical asymptotic distribution of the t-test statistic used to test whether the mean forecast error for an individual portfolio differs from zero is standard normal. The theoretical asymptotic distribution of the Wald statistic used to test whether the mean forecast errors for all 10 portfolios are zero is chi-squared with 10 degrees of freedom.

The results of the simulations for the 26 industry portfolios appear in Table 3 below and indicate that the differences between the finite-sample distributions of the test statistics that we use and their theoretical asymptotic counterparts are larger than is documented in Table 2 for the 10 portfolios formed on the basis of past estimates of beta.

Table 3

Distribution under the null of statistics used to test the SL-CAPM: 26 industry portfolios

	Method A				Method B			
	Probability that null is rejected at a significance level of				Probability that null is rejected at a significance level of			
Portfolio	0.995	0.975	0.025	0.005	0.995	0.975	0.025	0.005
1	1.000	0.998	0.142	0.048	0.993	0.972	0.024	0.006
2	1.000	0.999	0.211	0.086	0.994	0.970	0.022	0.005
3	1.000	0.999	0.203	0.085	0.994	0.973	0.023	0.004
4	1.000	0.999	0.190	0.079	0.993	0.968	0.025	0.006
5	1.000	0.999	0.169	0.065	0.995	0.976	0.026	0.006
6	0.999	0.994	0.084	0.023	0.995	0.977	0.028	0.006
7	0.999	0.993	0.084	0.022	0.991	0.966	0.024	0.004
8	0.999	0.994	0.080	0.020	0.992	0.969	0.024	0.004
9	1.000	0.995	0.089	0.028	0.995	0.975	0.030	0.007
10	1.000	0.999	0.146	0.055	0.994	0.974	0.024	0.005
11	0.999	0.995	0.091	0.023	0.992	0.966	0.015	0.002
12	1.000	0.999	0.189	0.077	0.994	0.974	0.026	0.006
13	1.000	0.999	0.178	0.073	0.996	0.976	0.031	0.007
14	1.000	0.997	0.159	0.058	0.994	0.974	0.025	0.005
15	0.999	0.995	0.093	0.024	0.995	0.973	0.021	0.004
16	0.999	0.997	0.148	0.050	0.991	0.969	0.019	0.003
17	1.000	0.999	0.189	0.081	0.996	0.976	0.033	0.009
18	1.000	0.998	0.159	0.057	0.992	0.970	0.020	0.003
19	1.000	0.999	0.215	0.095	0.994	0.976	0.033	0.007
20	1.000	0.998	0.156	0.060	0.994	0.974	0.030	0.006
21	0.999	0.994	0.088	0.024	0.994	0.971	0.025	0.006
22	0.997	0.987	0.052	0.012	0.994	0.969	0.026	0.005
23	0.999	0.995	0.096	0.027	0.992	0.973	0.027	0.005
24	1.000	0.996	0.103	0.032	0.994	0.975	0.026	0.005
25	1.000	0.997	0.144	0.049	0.993	0.971	0.022	0.004
26	0.999	0.995	0.094	0.024	0.990	0.961	0.014	0.002
	Method A				Method B			
	Probability that null is rejected at a significance level of				Probability that null is rejected at a significance level of			
	0.500	0.100	0.050	0.010	0.500	0.100	0.050	0.010
Wald	0.697	0.271	0.180	0.069	0.620	0.196	0.118	0.037

Notes: The results are generated using bootstrap simulations, 10, 000 replications and the returns to 26 industry portfolios from January 1974 to December 2013. The theoretical asymptotic distribution of the t-test statistic used to test whether the mean forecast error for an individual portfolio differs from zero is standard normal. The theoretical asymptotic distribution of the Wald statistic used to test whether the mean forecast errors for all 26 portfolios are zero is chi-squared with 26 degrees of freedom.

We next examine the behaviour of the test statistics that we use under the alternative that:

$$\alpha_j = 0.005 \times (1 - \beta_j), \; j = 1, 2, ..., N. \tag{27}$$

In other words, we examine the behaviour of the test statistics under the alternative that the Black CAPM is true but the SL-CAPM is false and the zero-beta premium is 0.5 percent per month.

The results of these simulations for the 10 portfolios formed on the basis of past estimates of beta appear in Table 4 below and the results for the 26 industry portfolios appear in Table 5.

The results in Table 4 indicate that tests of the SL-CAPM that use Wald statistics and Method A have marginally more power than tests of the SL-CAPM that use Wald statistics and Method B. On the other hand, tests of the SL-CAPM for individual portfolios that use *t*-statistics and Method B have, consistent with our intuition, substantially more power than tests that use *t*-statistics and Method A. Wald statistics are useful for indicating whether the forecasts that the SL-CAPM generates are unbiased. *t*-statistics are useful for revealing for which portfolios any failure of the model to produce unbiased forecasts is important.

Table 4

Power of the test: 10 portfolios formed on the basis of past estimates of beta

	Method A			Method B		
	Probability that null is rejected at a significance level of			Probability that null is rejected at a significance level of		
Portfolio	0.100	0.050	0.010	0.100	0.050	0.010
1	0.289	0.195	0.060	0.414	0.289	0.131
2	0.215	0.131	0.051	0.316	0.210	0.077
3	0.199	0.126	0.034	0.306	0.204	0.076
4	0.133	0.067	0.015	0.179	0.101	0.033
5	0.103	0.054	0.011	0.116	0.060	0.011
6	0.098	0.048	0.011	0.106	0.057	0.012
7	0.104	0.052	0.013	0.096	0.051	0.009
8	0.118	0.068	0.015	0.179	0.105	0.033
9	0.143	0.083	0.021	0.263	0.160	0.050
10	0.132	0.072	0.016	0.176	0.102	0.028
Wald	0.359	0.239	0.078	0.349	0.234	0.067

Notes: The results are generated using bootstrap simulations, 10, 000 replications and the returns to 10 portfolios formed on the basis of past estimates of beta from January 1974 to December 2013. Inference is drawn by comparing the t-test statistics and Wald statistic to their simulated distributions.

Unlike the results in Table 4, the results in Table 5 indicate that tests of the SL-CAPM that use Wald statistics and Method B have marginally more power than tests of the SL-CAPM that use Wald statistics and Method A. As in Table 3, however, tests of the SL-CAPM for individual portfolios that use *t*-statistics and Method B have substantially more power than tests that use *t*-statistics and Method A. Table 5 also indicates that the power of the tests for individual industry portfolios is – aside from the portfolio of REITs (Industry 20) – low.

Table 5

Power of the test: 26 industry portfolios

	Method A			Method B		
	Probability that null is rejected at a significance level of			Probability that null is rejected at a significance level of		
Portfolio	0.100	0.050	0.010	0.100	0.050	0.010
1	0.112	0.061	0.013	0.115	0.057	0.010
2	0.104	0.051	0.012	0.104	0.055	0.012
3	0.109	0.061	0.012	0.142	0.076	0.019
4	0.103	0.051	0.010	0.107	0.056	0.010
5	0.106	0.053	0.009	0.117	0.061	0.012
6	0.174	0.104	0.032	0.191	0.112	0.027
7	0.106	0.059	0.014	0.101	0.056	0.013
8	0.116	0.061	0.011	0.111	0.060	0.014
9	0.109	0.056	0.013	0.117	0.060	0.012
10	0.134	0.072	0.018	0.166	0.092	0.023
11	0.159	0.090	0.027	0.180	0.109	0.040
12	0.118	0.066	0.014	0.135	0.074	0.020
13	0.100	0.052	0.011	0.106	0.056	0.010
14	0.100	0.052	0.009	0.101	0.051	0.010
15	0.111	0.061	0.015	0.114	0.063	0.014
16	0.107	0.054	0.012	0.121	0.065	0.014
17	0.104	0.055	0.011	0.115	0.062	0.013
18	0.117	0.061	0.012	0.137	0.076	0.017
19	0.112	0.056	0.012	0.120	0.061	0.014
20	0.407	0.297	0.125	0.627	0.509	0.291
21	0.106	0.056	0.009	0.105	0.054	0.009
22	0.100	0.055	0.011	0.108	0.056	0.013
23	0.105	0.053	0.012	0.106	0.051	0.010
24	0.111	0.057	0.013	0.118	0.057	0.014
25	0.121	0.067	0.013	0.137	0.074	0.018
26	0.104	0.057	0.011	0.100	0.051	0.013
Wald	0.353	0.228	0.078	0.370	0.254	0.103

Notes: The results are generated using bootstrap simulations, 10, 000 replications and the returns to 26 industry portfolios from January 1974 to December 2013. Inference is drawn by comparing the t-test statistics and Wald statistic to their simulated distributions.

To examine the impact of excluding REITs from the tests, we also run bootstrap simulations in which REITs are removed. The results of these simulations are briefly summarised in Table 6. The table indicates that much of the power of Wald tests that use industry portfolios documented in Table 5 can be traced to the inclusion of REITs. Without REITs the power of the tests that use industry returns is substantially lower than the power of tests that use portfolios formed on the basis of past estimates of beta.

Table 6

Distribution under the alternative of statistics used to test the SL-CAPM: 25 industry portfolios – REITs excluded

	Method A			Method B		
	Probability that null is rejected at a significance level of			Probability that null is rejected at a significance level of		
	0.100	0.050	0.010	0.100	0.050	0.010
Wald	0.236	0.127	0.043	0.252	0.141	0.032

Notes: The results are generated using bootstrap simulations, 1, 000 replications and the returns to 25 industry portfolios (REITS excluded) from January 1974 to December 2013. Inference is drawn by comparing the t-test statistics and Wald statistic to their simulated distributions.

Industry test results

Although we note that there are concerns over survivorship bias raised by the way in which SIRCA constructs industry returns and there are concerns over the power of tests that use industry returns, we nevertheless report, for completeness, the results of tests of the SL-CAPM that use these returns.

The results of tests of the SL-CAPM that use industry returns appear in Table 7 below while the results of tests of the ERA's version of the SL-CAPM, which uses the 95th percentile of an estimate of the distribution of an OLS estimator for beta rather than an estimate of the mean of the distribution (the OLS point estimate), appear in Table 8.

As in the main body of the submission, the mean forecast errors in Tables 7 and 8 (and also, later, in Table 9) are formed as predicted minus observed which means that negative t-statistics are indicative that the model has under-predicted actual returns and the firm is receiving an NPV-negative outcome. [6]

The results are similar to the results of tests that use the 10 portfolios formed on the basis of past estimates of beta. There is a tendency for the SL-CAPM and the ERA's version of the SL-CAPM to underestimate the returns required on low-beta portfolios. In particular, the SL-CAPM underestimates the returns required on the Retailing, Pharmaceuticals and Utilities portfolios and the ERA's version of the SL-CAPM underestimates the returns required on the Retailing and Pharmaceuticals portfolios. The low power of the tests is illustrated by the fact that a Method B test of the null hypothesis that the ERA's version of the SL-CAPM provides an unbiased estimator of the return required on a portfolio of utilities is unable to reject at the five percent level the null despite the mean forecast error associated with the estimator being 0.557 percent per month.

[6] Once more, DBP is aware that standard statistical convention has errors as actual minus predicted, not predicted minus actual, but this form allows the non-statistician reader to easily interpret our results in light of the ARORO. Presenting our results in a more conventional statistical manner would make no difference whatsoever to our conclusions.

Table 7

ERA's version of the SL-CAPM

		Method A		Method B	
Wald statistic		22.900		29.621	
	Beta	Mean forecast error	t-test	Mean forecast error	t-test
Energy	1.595	0.168	0.444	0.042	0.146
Materials	1.014	0.126	0.472	0.020	0.142
Metals & mining	1.365	0.264	0.763	0.127	0.668
Capital goods	0.920	0.085	0.288	-0.007	-0.039
Commercial services	0.992	-0.136	-0.468	-0.216	-1.028
Transportation	0.523	0.109	0.351	0.053	0.187
Automobiles	0.669	0.069	0.174	-0.002	-0.007
Consumer durables	0.734	0.650	1.560	0.541	1.478
Consumer services	0.901	-0.039	-0.137	-0.239	-1.123
Media	1.235	-0.362	-0.788	-0.479	-1.271
Retailing	0.472	-0.573	-1.854	-0.654	-2.472
Food retailing	0.840	-0.175	-0.750	-0.254	-1.483
Food, beverage & tobacco	0.963	-0.189	-0.674	-0.279	-1.211
Health care	0.958	-0.040	-0.141	-0.128	-0.590
Pharmaceuticals	0.662	-0.599	-1.766	-0.660	-2.155
Banks	1.082	-0.232	-0.852	-0.295	-1.356
Diversified financials	0.868	-0.118	-0.465	-0.203	-1.238
Insurance	1.031	-0.338	-0.899	-0.451	-1.515
Real estate (excluding REITs)	1.339	0.080	0.240	-0.022	-0.096
REITs	0.503	0.026	0.133	-0.015	-0.096
Software & services	1.046	0.455	0.836	0.311	0.640
Technology hardware	0.847	0.349	0.603	0.151	0.287
Telecommunication services	1.932	0.105	0.179	-0.040	-0.075
Utilities	0.865	-0.435	-1.272	-0.557	-1.958
GICS code unassigned	1.364	0.163	0.435	0.008	0.032
GICS code unknown	0.882	0.022	0.050	-0.039	-0.119

Note: The tests use SIRCA data from January 1974 to December 2013.

Table 8

Vanilla SL-CAPM

		Method A		Method B	
Wald statistic		24.278		44.276	
	Beta	Mean forecast error	t-test	Mean forecast error	t-test
Energy	1.436	0.083	0.218	-0.043	-0.158
Materials	0.959	0.096	0.361	-0.006	-0.045
Metals & mining	1.295	0.226	0.653	0.091	0.484
Capital goods	0.857	0.051	0.172	-0.036	-0.195
Commercial services	0.909	-0.181	-0.622	-0.257	-1.236
Transportation	0.380	0.032	0.102	-0.020	-0.070
Automobiles	0.518	-0.012	-0.031	-0.074	-0.204
Consumer durables	0.584	0.569	1.366	0.482	1.296
Consumer services	0.721	-0.135	-0.482	-0.289	-1.346
Media	1.067	-0.452	-0.985	-0.553	-1.458
Retailing	0.347	-0.641	-2.071	-0.715	-2.598
Food retailing	0.772	-0.212	-0.908	-0.286	-1.696
Food, beverage & tobacco	0.866	-0.242	-0.860	-0.321	-1.417
Health care	0.866	-0.090	-0.318	-0.174	-0.813
Pharmaceuticals	0.523	-0.674	-1.988	-0.731	-2.378
Banks	0.984	-0.285	-1.045	-0.342	-1.642
Diversified financials	0.802	-0.154	-0.606	-0.234	-1.426
Insurance	0.863	-0.429	-1.140	-0.539	-1.793
Real estate (excluding REITs)	1.235	0.024	0.072	-0.075	-0.339
REITs	0.440	-0.008	-0.043	-0.047	-0.297
Software & services	0.817	0.331	0.609	0.205	0.417
Technology hardware	0.608	0.222	0.382	0.086	0.161
Telecommunication services	1.565	-0.093	-0.160	-0.242	-0.466
Utilities	0.736	-0.504	-1.475	-0.615	-2.139
GICS code unassigned	1.260	0.106	0.284	-0.042	-0.160
GICS code unknown	0.841	-0.000	-0.001	-0.059	-0.183

Note: The tests use SIRCA data from January 1974 to December 2013.

Tests of a naïve model

Pricing models are designed to explain the cross-section of mean returns. Thus for any pricing model to be taken seriously, it should outperform a naïve model that restricts the returns required on all equities to be the same. Within a regulatory context, any pricing model that a regulator uses to compute a return on equity should outperform a model that sets the return on equity equal to the return on the market.

Table 9 below provides the results of tests of a naïve model that use the 10 portfolios formed on the basis of past estimates of beta. The table shows that there is considerably less evidence against a naïve model than against the SL-CAPM – be it the conventional form of the model or the ERA's version of the model. The performance of a naïve model, though, comes close to matching the performances of the Black model and betastar model and we can provide some intuition for why this is so.

The Black model and betastar model work by examining the historical relation between returns and estimates of beta and using this historical relation to forecast returns. We emphasise that at each point in time, these models use only past data. At each point in time, the past data suggest that there is little relation between returns and estimates of beta and so both models predict that, going forward, there should be little variation in the returns realised by the 10 portfolios formed on the basis of past estimates of beta.

The predictions generated by a naïve model can also be generated by using the SL-CAPM and setting beta to one. So this model too predicts that, going forward, there should be little variation in the returns realised by the 10 portfolios formed on the basis of past estimates of beta.

Table 9

Tests of a naïve model

		Method A		Method B	
Wald statistic		10.011		9.992	
Portfolio	Beta	Mean forecast error	t-test	Mean forecast error	t-test
1	0.536	-0.151	-0.759	-0.247	-1.327
2	0.608	-0.181	-0.867	-0.277	-1.594
3	0.576	-0.123	-0.578	-0.219	-1.354
4	0.766	-0.262	-1.167	-0.358	-2.394
5	0.857	-0.126	-0.495	-0.222	-1.519
6	0.882	-0.009	-0.038	-0.105	-0.841
7	0.966	0.167	0.607	0.071	0.503
8	1.182	0.105	0.330	0.009	0.053
9	1.362	0.416	1.168	0.320	1.567
10	1.384	0.309	0.699	0.213	0.659

Note: The tests use SIRCA data from January 1974 to December 2013.

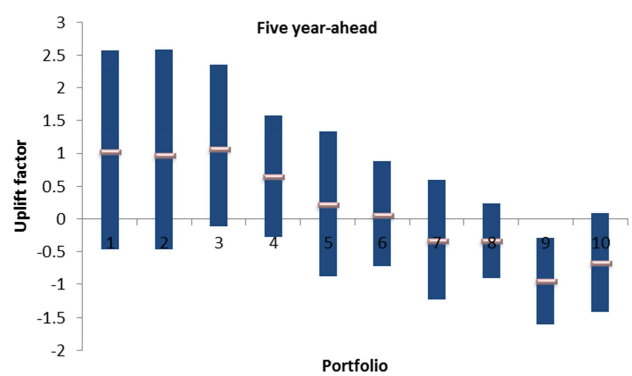
One-month versus five-year returns

As noted in the report, we make use of month-ahead returns in our forecasts. That is, we form estimates based on data up to month t, and then use these estimates to predict month $t+1$, recording the error. We are aware that the ERA sets rates of return for five years, not one month, but comparing predictions across an average of 60 months creates two significant issues. Firstly, it reduces the number of independent data points to around 8, because we have around 40 years of data in our tests (the 60-month forward average from month t has 59 of the same data points as the 60-month forward average from month $t+1$), and secondly it induces a great deal of serial correlation into the analysis which did not exist previously. For aggregate measures such as the Wald test, removing this serial correlation proved highly problematic. Both reduce the robustness of results, and thus we determined that, provided similar information is being provided by month-ahead compared to (corrected for serial correlation) five-year average results, then using the month-ahead model is likely to provide results about which inference is likely to be more robust.

Figure 1 below presents a comparison of the results of a month-ahead and five-year ahead model. This is for the SL-CAPM and shows the uplift factor that would be required to remove bias in each model; the extent of the bias. The pink bars are the uplift factor, and the blue bars are the confidence interval around the uplift factor given the uncertainty in the data; in essence, this is a graphical representation of the information contained in the t-statistic results. As is clear, though the year-ahead results are less certain (wider confidence intervals) the scale of the bias in each case is the same.

Figure 1

Comparison of month-ahead and five-year-ahead models



Mincer-Zarnowitz tests

Mincer and Zarnowitz (1969) provide a method for examining whether a forecast is both efficient – in that it uses appropriately all available information – and unbiased.

Consider the set of regressions:

$$z_{jt} = \gamma_j + \delta_j \hat{z}_{jt} + \varepsilon_{jt}, \qquad j = 1, 2, ..., N, \tag{28}$$

where, again, z_{jt} is the excess return to portfolio j from month $t-1$ to t, $\hat{z}_{jt}$ is a forecast of the return made at the end of month $t-1$, γ_j and δ_j are an intercept and slope coefficient and ε_{jt} is a disturbance.

If, using the terminology of Mincer and Zarnowitz, the forecast $\hat{z}_{jt}$ is both unbiased and efficient, then the intercept γ_j should be zero and the slope coefficient δ_j should equal one.

If the slope coefficient were not equal to one, then there would be a better way of using the information contained in the forecast. If the slope coefficient were equal to one, then the intercept would be the mean forecast error and this should be zero for the forecast to be unbiased.

We use the Generalised Method of Moments (GMM) to test whether the forecasts that we generate using a number of different pricing models are both efficient and unbiased. Since under the null hypothesis, the intercept and slope coefficient in (28) should be identical across portfolios, we impose this restriction and estimate the system:

$$z_{jt} = \gamma + \delta \hat{z}_{jt} + \varepsilon_{jt}, \qquad j = 1, 2, \ldots, N \tag{29}$$

We test whether $\gamma = 0$ and $\delta = 1$.

Daily, weekly and monthly returns

Our estimates of the beta of a benchmark efficient entity use weekly data. Here we explain why we choose to use weekly data.

A general principle in statistics is that it is better to use more data than less – unless there is something wrong with the data. Since there are more daily returns in any given period of time than there are weekly returns and more weekly returns than there are monthly returns, this principle suggests that it is better to use daily returns to estimate the equity beta of a firm than to use weekly returns and better to use weekly returns than to use monthly returns.

There are two things, however, that may be 'wrong' with daily data. First, the stock of the firm whose equity beta one wishes to estimate may trade only infrequently relative to the market as a whole. If the stock trades infrequently, then least squares estimates of the equity beta of the firm computed from a simple regression of the return to the stock on the return to the market will be biased towards zero. Scholes and Williams (1977) show how to circumvent this problem.

Second, Gilbert, Hrdlicka, Kalodimos and Siegel (2014) show that difficulties in assessing the impact of market-wide news on individual firms can lead the returns to less complex firms predicting the returns to more complex firms.[7] As a result, even in the absence of infrequent trading, the high-frequency betas of some firms may fall below their low-frequency counterparts.

In prior work, we have examined whether there is evidence of infrequent trading in the stocks of regulated energy utilities and have found none. In particular, we have examined whether there is

[7] A complex firm is a firm for which it is costly to understand the impact on firm value of news about aggregate economic conditions. Gilbert, Hrdlicka, Kalodimos and Siegel use Berkshire Hathaway, a multi-industry conglomerate, as an example of a complex firm and Exxon Mobil, an oil company, as an example of a firm that is less complex.

evidence that the return to the market on one day or over one week can predict the return to the stock of a regulated energy utility on the next day or week. We found no evidence of a significant predictive relation. Using weekly data, Henry (2008) and the ERA (2013) similarly found no evidence of infrequent trading in the stocks of regulated energy utilities.

Testing whether the issue that Gilbert, Hrdlicka, Kalodimos and Siegel (2014) raise is an important one for regulated energy utilities is difficult because of the relatively short time series with which one has to work. Gilbert, Hrdlicka, Kalodimos and Siegel compare daily to quarterly estimates of betas using a large quantity of US data from 1969 to 2010 – a 42-year period. There are few listed Australian regulated energy utilities with data available before 2000.

To illustrate the gains to be had from using daily data to estimate the equity beta of a firm we present the results of some simulations. For convenience, the simulations employ estimates of betas computed using continuously compounded returns. Using continuously compounded returns is convenient because the continuously compounded return over a week is simply the sum of the continuously compounded returns over each day of the week.

We assume that the return to asset j from day $t-1$ to day t in excess of the risk-free rate, z_{jt}, satisfies that following relation

$$z_{jt} = \beta_j z_{mt} + \varepsilon_{jt}, \qquad \beta_j = 1, \qquad \begin{pmatrix} z_{mt} \\ \varepsilon_{jt} \end{pmatrix} \sim \mathrm{N}\left(\begin{pmatrix} 0.0002 \\ 0.0000 \end{pmatrix}, \begin{pmatrix} 0.01^2 & 0 \\ 0 & 0.01^2 \end{pmatrix} \right), \tag{29}$$

where z_{mt} is the excess return to the market portfolio from day $t-1$ to t, ε_{jt} is a disturbance and β_j is the beta of the stock.

Table 10 below shows the results of using weekly data to estimate the beta of the stock and of using daily data. There is a small benefit to computing an estimate of the beta using the five alternative estimates of weekly returns and then averaging the estimates rather than using only one of the estimates. The standard deviation of the estimator falls by about one sixth from around 0.062 to 0.051. There is a much larger benefit, though, to using daily data rather than averaging the five weekly estimates. The standard deviation of the estimator falls by almost one half from 0.051 to 0.028.

Table 10

Simulation results

	Weekly data						
	Monday-to-Monday	Tues-to-Tues	Wed-to-Wed	Thurs-to-Thurs	Friday-to-Friday	Average	Daily data
Mean	1.000	1.000	1.000	1.000	1.001	1.000	1.000
Std Dev	0.062	0.063	0.062	0.062	0.061	0.051	0.028

Notes: The simulations are based on 10,000 replications each of which uses ordinary least squares and five years of data to estimate the beta of the stock.

In practice, there appears to be only a relatively small difference between estimates that use daily data, estimates that use weekly data and estimates that use monthly data. As one would expect, though, the standard errors of the daily estimates are far smaller than the standard errors of their weekly or monthly counterparts.

Table 11 below provides estimates of the beta of a regulated energy utility that use a value weighted portfolio of the stocks, APA, AST, DUE, ENV, HDF and SKI and data from 1 September 2009 to 30 September 2014.

Our analysis suggests that there is little evidence to indicate that estimates of the equity beta of a regulated energy utility that use daily data suffer from any bias generated by infrequent trading. It is difficult to know whether the estimates suffer from any bias produced by challenges that the market faces in determining the impact on the value of the utility of changes in aggregate economic conditions. So a cautious approach would be to employ an estimate of the equity beta of a regulated energy utility that uses weekly data. Our analysis indicates that an estimate of the equity beta of a regulated energy utility constructed using weekly data will be conservative – we find that an estimate of the equity beta of a regulated energy utility constructed using weekly data lies below an estimate constructed using daily data.

Table 11

Estimates of the equity beta of a regulated energy utility

	Daily	Weekly	Monthly
Beta estimate	0.621	0.550	0.504
Standard error	0.023	0.049	0.094

Notes: The estimates are computed using the re-levered returns to a value-weighted portfolio of regulated energy utilities and data from 1 September 2009 to 30 September 2014. We re-lever the betas to a target debt-to-value ratio of 0.6.

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