Report prepared for the

Australian Energy Regulator

Advice on the Rate of Return for the 2015 AER Energy Network Determination for Jemena Gas Networks

John C. Handley

University of Melbourne

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PREAMBLE

The Australian Energy Regulator (AER) is currently undertaking a regulatory determination for Jemena Gas Networks (JGN). The AER published the draft decision in late November 2014. Following the receipt of JGN's revised regulatory proposal in late February 2015, the AER now seeks further expert advice on a number of matters in relation to the return on equity to inform its assessment of the rate of return for JGN.

In particular, the AER has asked that I respond to two experts reports – SFG (2015) and NERA (2015), and also a letter from the Australian Securities Exchange (ASX) to United Energy and Multinet Gas – ASX (2015), which relate to the estimation of the return on equity. I have also been asked to provide an overall view, based on the review of the above material whether I consider it necessary to change any of the findings in my earlier report – Handley (2014).

I consider each of the reports in turn.

1. RESPONSE TO THE NERA (2015) REPORT

NERA was asked by a number of service providers, including JGN, to review the theoretical and empirical literature on three asset pricing models – the Sharpe-CAPM, the Black-CAPM and the Fama-French three-factor model – in relation to estimating the return on equity and to also respond to matters raised by the AER in its draft decision. In this section, I set out one by one the main issues raised by NERA in its report followed by my response.

(a) It is important to measure the cost of capital accurately

NERA states: *"while no model is perfect and all models make assumptions, the costs of using a model that provides inaccurate estimates of the cost of equity will be far greater for a regulated firm than for a unregulated firm".*[1]

Response

There is nothing controversial here although the comparison of the implications of an incorrectly estimated cost of capital for a regulated firm verses an unregulated firm is unnecessary – it is well understood that we require an accurate rather than inaccurate estimate.

The key consideration in this regard is that the regulator set prices in accordance with the allowed rate of return objective – i.e. the rate of return for a service provider is to be commensurate with the efficient financing costs of a benchmark efficient entity with a similar degree of risk as that which applies to the service provider in respect of the provision of the regulated service.[2]

[1] NERA (2015 p.ii)

[2] The National Electricity Rules and National Gas Rules also require that in estimating the return on equity, regard must be had to the prevailing conditions in the market for equity funds.

(b) The AER's reference to Roll's critique in the Guideline is not relevant

NERA notes that in theory, the Sharpe-CAPM applies to all risky assets but because we can never identify the market portfolio of all risky assets, most practical applications of the model (including the AER's approach) use a market portfolio of stocks as a proxy for the market portfolio of all risky assets.

NERA then claims that: *"The AER uses difficulties in testing the predictions that the SL CAPM makes about the behaviour of the return to the market portfolio of all risky assets to shield the version of the model that it employs from scrutiny"* [3]

Response

Roll's critique says that we can never truly test the CAPM since we can never truly observe the return on all risky assets. NERA appears to be suggesting that the AER can't rely on Roll's conclusion – that the CAPM can never really be tested – as a reason to dismiss the empirical evidence on the CAPM, since the empirical evidence relates to a version of the model used by the AER (using a stock market index as a proxy for the true market portfolio) which by definition is different to the unobservable true market portfolio to which Roll's critique relates.

I interpret the AER's reference to Roll's critique in the Guideline in a different way – being, Roll also showed how sensitive empirical tests of the CAPM can be to the particular market proxy that is used in the test.[4]

In any event, I don't consider this to be a critical issue.

[3] NERA (2015 p.ii)

[4] Roll (1977 p.131) states: *"For the Black, Jensen and Scholes data, for example, there was a mean variance efficient 'market' proxy that supported the Sharpe-Lintner model perfectly and that had a correlation of 0.895 with the market proxy actually employed."*. In other words, Roll shows that if Black, Jensen and Scholes (1972) used a slightly different proxy for the market then their econometric results would have been perfectly consistent with the Sharpe CAPM.

(c) Using the Sharpe-CAPM will underestimate returns on low-beta stocks

NERA states: *"It has been known for well over 40 years that empirical versions of the SL CAPM tend to underestimate the returns to low-beta assets and overestimate the returns to high-beta assets."* [5]

Response

There is nothing new here. It is well known that an apparent weakness of the Sharpe-CAPM is the empirical finding, for example by Black, Jensen and Scholes (1972) and Fama and French (2004), that the relation between beta and average stock returns is too flat compared to what would otherwise be predicted by the Sharpe-CAPM – a result often referred to as the low beta bias.

But empirical evidence of a low beta bias is not sufficient on its own to justify a claim for additional compensation relative to the Sharpe-CAPM.

The key point is that there are multiple possible (but not necessarily mutually exclusive) explanations for the low beta bias.[6] In other words, we do not have a clear understanding of what the low beta bias represents. This uncertainty is critically important in the current context because it means that the low beta bias does not necessarily reflect risk, whereas the allowed rate of return objective is clear that risk is the key determinant of the rate of return.

[5] NERA (2015 p.iii)

[6] It may reflect restrictions on riskless borrowing consistent with the Black (1972) CAPM. It may reflect the impact of barriers to international investment consistent with the international CAPM of Black (1974). It may reflect a specification error in the proxy for the market portfolio consistent with the suggestion by Roll (1977). It may reflect model misspecification consistent with the value and/or size effects of the Fama-French model. It was also initially thought that it may reflect the impact of differential personal taxes consistent with the after-tax CAPM of Brennan (1970) but this idea has since been dismissed by subsequent research. It may reflect price pressure exerted by leverage-constrained investors who tilt their portfolios towards high-beta stocks relative to low-beta stocks in seeking higher expected returns, consistent with Frazzini and Pederson (2014). It may reflect price pressure exerted by investors who seek lottery-like stocks consistent with Bali, Brown, Murray and Tang (2014). A lottery-like asset is one which has a relatively small probability of a large payoff. In addition, NERA notes how departures from a number of the assumptions underlying the Sharpe-CAPM – which it describes as an *"an attractively simple theory"* – including mean-variance preferences, single period, homogeneous beliefs, no taxes, and investors can borrow and lend at the risk free rate, may explain the poor empirical performance of the model.

(d) Using the Sharpe-CAPM will underestimate returns on value stocks and small stocks.

NERA states: *"These studies have also shown that the SL CAPM tends to underestimate the returns to value stocks and low-cap stocks."* [7]

Response

There is nothing new here. It is well known that an apparent weakness of the Sharpe-CAPM is the empirical finding that the average return on value stocks[8] and small stocks is higher than what would otherwise be predicted by the Sharpe-CAPM – a result which motivated Fama and French to propose their three-factor model in the early 1990s.

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But empirical evidence of a value effect is not sufficient on its own to justify a claim for additional compensation relative to the Sharpe-CAPM.

The key point is that we do not have a clear understanding of what the value effect represents.[9] This uncertainty is critically important in the current context because it means that the value effect does not necessarily reflect risk, whereas the allowed rate of return objective is clear that risk is the key determinant of the rate of return.

[7] NERA (2015 p.iii)

[8] A value stock is one that has a high book-to-market ratio. The higher average return on value stocks is often referred to as the "value effect" or "book-to-market" effect and the higher average return on small stocks is often referred to as the "size effect". The focus of the debate is on the value effect rather than the size effect. As SFG (2014 p.2) states: *"The performance of the size factor has fluctuated somewhat over time and across markets, but the performance of the book-to-market factor has not"* whilst NERA (2013 p.vii) states: *"We estimate the value premium, inclusive of a value assigned to imputation credits, to be 7.68 per cent per annum. On the other hand, our estimates indicate that, using data from SIRCA, the size premium, while economically significant, is not statistically significant."*

[9] Daniel and Titman (2012) suggest there is still no consensus concerning how the Fama-French model should be interpreted despite being the topic of debate for decades. Fama and French (1993) argue that the value and size factors are proxies for distress risk. Lakonishok, Shleifer and Vishny (1994) argue that the value factor instead proxies for mispricing. Daniel and Titman (1997) also suggest that the return premia on small stocks and value stocks cannot be viewed as compensation for risk. NERA (2015 p.vi) notes that the Black-CAPM does not explain the value effect.

(e) The Black-CAPM explains the low-beta bias

NERA states: "*By construction, the Black CAPM eliminates the tendency of the SL CAPM to underestimate the returns to low-beta assets and overestimate the returns to high-beta assets.* [10]

Response

The Black-CAPM and the low beta bias are not equivalent concepts. In other words, whilst the Black-CAPM implies an empirical result like the low beta bias, the low beta bias does not imply the Black-CAPM.

This means empirical evidence of a low beta bias is not sufficient to justify the use of the Black-CAPM.

It is further noted that Lewellen, Nagel and Shaken (2010) cast doubt on explanations of a low beta bias being attributable to borrowing costs.[11]

(f) The Fama-French three-factor model explains the value effect and size effect and so generates estimates of the cost of equity that are unbiased

NERA states: "*The Fama-French three-factor model, in its original form, is not a more general model than the SL CAPM but there is, nevertheless, less evidence against the model than against the SL CAPM. The model, unlike the SL CAPM and Black CAPM, does not tend to underestimate the returns to value stocks and low-cap stocks."* [12]

[10] NERA (2015 p.vi)
[11] Lewellen, Nagel and Shanken (2010 p.189) state: *"The table shows that none of the models explain the level of expected returns … Annualised, the zero-beta rates range from 7.8% to 14.3% above the risk-free rate. These estimates cannot reasonably be attributed to differences in lending versus borrowing costs"*
[12] NERA (2015 p.vi)

Response

Lewellen, Nagel and Shanken (2010) show, among other things, that the previously reported empirical success of the Fama-French model in explaining the determinants of past stock returns is grossly overstated – the problem being with the standard methodology used in the empirical tests. In particular, they show that the explanatory power of the Fama-French model is nothing like the reported 78% obtained using the standard OLS approach but rather is closer to only 6% when the empirical test is improved (using GLS and an expanded set of test portfolios).[13]

(g) The two additional factors in the Fama-French three-factor model – value (HML) and size (SMB) – are proxies for risk

NERA states: *"The R^2 values attached to these time series regressions range from 0.83 to 0.97 … the evidence that Fama and French provide suggests that the HML and SMB factors capture pervasive sources of risk – at least in US data."*[14]

Response

As explained under (d) above, there is no consensus as to what the additional Fama-French factors represent.

[13] NERA also suggests that the theoretical foundation for the Fama-French three factor model is superior to the theoretical foundation for the Sharpe-CAPM since it is based on arbitrage rather than equilibrium considerations. In particular, NERA (2015 p.15) states: *"Ross (1976) provides a no-arbitrage framework that is an alternative to the mean-variance framework of Sharpe, Lintner and Black. While Sharpe and Lintner assume that no investor faces borrowing constraints and Black assumes that no investor faces short-sale constraints, Ross assumes only that at least one investor faces no borrowing or short-sale constraints. In the Arbitrage Pricing Theory (APT) of Ross investors are rewarded for risks that are pervasive and that they cannot diversify away but are not rewarded for risks that are idiosyncratic and that they can diversify away".* There are two theoretical risk based justifications for a multifactor model – the APT of Ross (1976) and the Intertemporal CAPM of Merton (1973). Brennan (1992 p.291) notes that the APT and ICAPM are empirically indistinguishable in the absence of prior information about the relevant state variables. In any event, I don't consider this to be a critical issue as the more important consideration is whether the Fama-French model actually reflects risk or not.

[14] NERA (2015 p.18-19)

As explained under (f) above, Lewellen, Nagel and Shanken (2010) show that the OLS R^2 is not an appropriate measure of the success of an asset pricing model.

(h) Lewellen, Nagel and Shanken (2010) provide empirical evidence in support of using the Fama-French three-factor-model and against using the Sharpe-CAPM

NERA states: *"the evidence that Lewellen, Nagel and Shanken provide indicates that the SL CAPM does not generate unbiased estimates of the cost of equity."*[15]

and further:

"One way of looking at the results that Lewellen, Nagel and Shanken (2010) provide on the empirical performance of the Fama-French model is to follow Cochrane (2001) and plot the sample mean excess return to each portfolio against the mean excess return that the Fama-French three-factor model predicts the portfolio should earn." [16]

Response

In relation to the Sharpe-CAPM: NERA summarises the empirical evidence (from Lewellen, Nagel and Shanken) in their Figure 1[17] which shows a plot of the sample mean excess returns[18] against the (estimated) beta for 25 size and book-to-market sorted portfolios and 30 industry portfolios using quarterly U.S. data from 1963 to 2004 and conclude: *"The figure indicates that there is little relation between the sample mean return to a portfolio and an estimate of its beta."*. [19]

[15] NERA (2015 p.iv)
[16] NERA (2015 p.vi)
[17] NERA (2015 p.v)
[18] The excess return is the return over and above the risk-free rate
[19] NERA (2015 p.v)

This evidence – that the Sharpe-CAPM performs poorly in explaining past stock returns – is not really new. Lewellen, Nagel and Shanken (2010) show that the CAPM has zero explanatory power when the set of test assets is expanded to include 30 industry portfolios (rather than just the standard 25 Fama and French size-B/M portfolios) and the more relevant GLS R^2 rather than the OLS R^2 is used as the goodness-of-fit measure.

In relation to the Fama-French model: NERA summarises the empirical evidence (from Lewellen, Nagel and Shanken) in their Figure 2[20] which shows a plot of the sample mean excess returns against the Fama-French predicted mean excess return for 25 size and book-to-market sorted portfolios and 30 US industry portfolios using quarterly U.S. data from 1963 to 2004 and concludes*: "The figure shows that the Fama-French three-factor model provides better predictions of the mean excess returns to the portfolios than does the SL CAPM in Figure 1, which use the same data."* [21]

This evidence – that the Fama-French three-factor model *apparently* performs well in explaining past stock returns – is also not really new as it effectively corresponds to a graphical representation of the OLS R^2 which we know from past studies is high.[22] More importantly, NERA's presentation in Figure 2 is arguably misleading given Lewellen, Nagel and Shanken (2010) concerns with using the OLS R^2 as the goodness-of-fit measure. In fact, Lewellen, Nagel and Shanken (2010) show that the explanatory power of the Fama-French three-factor model is nothing like the reported 78% using the standard OLS approach (based on the 25 test portfolios) or even the reported 31% (based on OLS and the expanded set of 55 test portfolios) but rather is closer to only 6% when the empirical test is improved using GLS and the expanded set of test portfolios.

In my opinion, a 6% explanatory power does not constitute meaningful empirical support for a model.

[20] NERA (2015 p.vii)
[21] NERA (2015 p.vi)
[22] It is not clear however why there are not 55 data points (corresponding to the 55 test assets) shown in the figure.

(i) Handley (2014) misrepresents the results of Lewellen, Nagel and Shanken (2010)

There are a number of statements in my previous report that NERA objects to:

- First, my statement: *"The empirical evidence supporting the Fama-French model (and a number of other asset pricing models for that matter) has recently been called into question by Lewellen, Nagel and Shanken (2010)."* [23]

  To which NERA comments: *"Keyword searches of Lewellen, Nagel and Shanken's (2010) paper for the expressions 'Fama' and 'FF' do not uncover a statement to that effect".* [24]

- Second, my use of the following quote from Lewellen, Shanken and Nagel (2010 p.189) which summarises the poor explanatory power of five asset pricing models:

  "The third key result is that none of the models provides much improvement over the simple or consumption CAPM when performance is measured by the GLS R^2 or q …. The average GLS R^2 is only 0.08 across the five models using size-B/M portfolios and 0.02 using the full set of 55 portfolios."

  To which NERA comments: *"A close inspection of Lewellen, Nagel and Shanken's (2010) Table 1 … reveals that the expression 'none of the models' refers to the set of five alternatives to the Fama-French three-factor model and not to a set of models that includes the Fama-French three-factor model."* [25]

[23] Handley (2014 p.7)
[24] Handley (2014 p.7)
[25] NERA (2015 p.39)

- Third, my statement: *"In regards to the Fama-French model in particular, Lewellen, Nagel and Shanken (2010) show that using Fama and French's 25 size-B/M portfolios as test assets results in an apparently impressive OLS R^2 of 0.78 but when the set of test assets is expanded to include 30 industry portfolios, then the more relevant resultant GLS R^2 is only 0.06"* [26]

  To which NERA comments: *"The results indicate that of the models that Lewellen, Nagel and Shanken examine, the Fama-French three-factor model has the highest GLS R^2 when their preferred combination of 25 book-to-market and size portfolios and 30 industry portfolios is employed. Thus Handley has misrepresented the results that Lewellen, Nagel and Shanken provide."* [27]

Response

In my opinion, the above comments from NERA completely miss the relevance of the Lewellen, Shanken and Nagel (2010) paper to the current debate.

One can get a very quick sense of what the Lewellen, Nagel and Shanken (2010) paper is about by simply looking at the title of the paper – "A Skeptical Appraisal of Asset Pricing Tests" and the abstract, in which they state: "*It has become standard practice in the cross-sectional asset pricing literature to evaluate models based on how well they explain average returns on size-B/M portfolios, something that many models seem to do remarkably well. In this paper, we review and critique the empirical methods used in the literature. We argue that asset pricing tests are often highly misleading, in the sense that apparently strong explanatory power (high cross-sectional R^2s and small pricing errors) can provide weak support for a model. We offer a number of suggestions for improving empirical tests and evidence that several proposed models do not work as well as originally advertised*". [28]

[26] Handley (2014 p.7)
[27] NERA (2015 p.39)
[28] Lewellen, Nagel and Shanken (2010 p.175)

Lewellen, Nagel and Shanken examine eight asset pricing models in total, one of which is the Fama-French three-factor model, and show (among other things) in their Table 1, how poorly the Fama-French three-factor model performs under the improved test methodology (GLS and an expanded set of test portfolios) despite appearing to perform well under the standard test methodology (OLS and Fama-French 25 size-B/M test portfolios).

In relation to my first statement, the inference that NERA's keyword search somehow shows that my statement is incorrect or misleading is in itself misleading. My statement is based on Table 1 in Lewellen, Nagel and Shanken (2010).

In relation to my use of the above mentioned quote from Lewellen, Nagel and Shanken (2010), NERA's inference that it is irrelevant or misleading, because the "five models" does not include the Fama-French three-factor model, again misses the point of their critique. When read as a part of the surrounding statements in my previous report, the quote illustrates the problem with the standard test methodology and therefore how little we know about empirical asset pricing. In any event, my point is still valid. Lewellen, Shanken and Nagel (2010) compare the performance of five models to three benchmark models – the Sharpe-CAPM, the Consumption-CAPM and the Fama-French three-factor model – and in doing so examine eight asset pricing models. They find that all eight models perform poorly. [29]

In relation to my third statement, whilst there is strictly nothing wrong with NERA's statement that the Fama-French three-factor model has the highest GLS R^2 of the eight models that Lewellen, Nagel and Shanken (2010) examine, this again misses the point of their critique. The GLS R^2 is only 6% !

[29] The average GLS R^2 across the five models is 8% using size-B/M portfolios and 2% using the expanded set of test portfolios. The average GLS R^2 across all eight models is also 8% using size-B/M portfolios and 2% using the expanded set of test portfolios.

If one wants to argue that the Fama-French three-factor model is an appropriate model for determining compensation, because it can explain past stock returns, then I suggest you need more than a 6% explanatory power to make that case.

In summary, there is nothing misleading in the way I have referenced Lewellen, Nagel and Shanken (2010).

(j) Handley (2014) misrepresents the results of Fama and French (2014)

NERA states: *"The implicit suggestion that Handley and McKenzie and Partington are making is that Fama and French have abandoned their three-factor model and are ready to accept the use of the SL CAPM. This is not the case … [Fama and French] are suggesting that there may be a four or five-factor model that performs better than their three-factor model."* [30]

Response

I have made no such claim – explicitly or implicitly.

Fama and French (2014) show that the value factor of the Fama-French three-factor model becomes redundant for describing average returns when using the new Fama-French five-factor model, in the sample they examine. This begs the question – how important can the value factor be ?

[30] NERA (2015 p.43)

(k) The value effect may not reflect risk

NERA states: *"It has been known for well over 25 years that the SL CAPM misprices value stocks. There is, however, no consensus among academics about whether these deviations from the SL CAPM represent compensation for risks not priced by the SL CAPM or mispricing by the market."* [31]

Response

This is consistent with the discussion under (d) above – there is no consensus as to what the value effect represents and in particular whether it represents additional compensation for risk.

(l) It is not relevant whether the value effect reflects risk

NERA states: *"In other words, a belief that deviations from the SL CAPM represent compensation for risks not priced by the SL CAPM – even if false – need not lead one to mismeasure the cost of equity in using the Fama-French three-factor model. If investors irrationally require higher returns on value stocks, then the cost of capital for firms whose stock can be so classified will be higher as a result."* [32]

Response

NERA does not appear to be concerned whether the value effect reflects risk or does not reflect risk.

In contrast, I consider this to be a critically important issue since risk is the key determinant of the rate of return according to the allowed rate of return objective. Unless the Fama-French model determines returns on the basis of risk then the model would not be appropriate for compensation purposes.

[31] NERA (2015 p.44)
[32] NERA (2015 p.45)

2. RESPONSE TO THE SFG (2015) REPORT

SFG was asked by a number of service providers, including JGN, to provide an opinion on the AER's use of a foundation model to estimate the cost of equity and on the advice in my previous report. In this section, I set out one by one the main issues raised by SFG in its report followed by my response. A number of the issues raised by SFG correspond to similar issues raised by NERA.

(a) The AER does not take into account the empirical evidence relating to the low beta bias

SFG states: *"We explain that the AER does not take into account two important pieces of empirical evidence: a) stocks with low beta estimates earn higher returns than predicted by the Sharpe-Lintner CAPM – which could be dealt with by implementing the Black CAPM to derive one estimate of the cost of equity".* [33]

Response

The low beta bias refers to the empirical finding that that the relation between beta and average stock returns is too flat compared to what would otherwise be predicted by the Sharpe-CAPM. Alternatively this can be described as the finding that observed returns are above predicted returns for low beta stocks.

SFG suggests that the low beta bias reflects risk[34], but as discussed under section 1(c) above, this is not necessarily the case and so additional compensation is not necessarily appropriate.

SFG suggests that the Black-CAPM should be used to take account of the low beta bias[35], but as discussed under section 1(e) above, the Black-CAPM and the

[33] SFG (2015 p.3)
[34] SFG (2015 p.6): *"So either the [Sharpe-CAPM] is incomplete, in that it leaves out an important risk factor, or the manner in which risk is measured (that is, estimating beta by regressing stock returns on market returns) is flawed."*

low-beta bias are not equivalent concepts, which means the Black-CAPM is not necessarily the correct (or only) way to take account of the low-beta bias, in the event that additional compensation is considered to be appropriate.

In my opinion, the AER has taken into account the empirical evidence relating to the potential for low beta bias in deciding not to use the Black-CAPM and in deciding to take a beta estimate from the high end of its estimated range.

(b) The AER does not take into account the empirical evidence relating to the value effect

SFG states: *"We explain that the AER does not take into account two important pieces of empirical evidence: … b) stocks with high book-to-market ratios persistently earn higher returns than predicted by the Sharpe-Lintner CAPM – which could be dealt with by implementing the Fama-French model to derive one estimate of the cost of equity".* [36]

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Response

SFG suggests that the value effect reflects risk, [37] but acknowledges that we do not really know what it represents.[38] More importantly, SFG does not appear to be concerned about whether it reflects risk or does not reflect risk.[39] But as

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[35] SFG (2015 p.6): *"But if we adopt the Black CAPM we increase the expected return on low beta stocks, and decrease the expected return on high beta stocks. So the use of the Black CAPM addresses the first empirical shortcoming of the Sharpe-Lintner CAPM".*

[36] SFG (2015 p.3)

[37] SFG (2015 p.6): *"The basis for using the HML factor to estimate expected returns, as opposed to the stock characteristic of the book-to-market ratio itself, is the idea that there is a risk factor associated with the book-to-market ratio … So the use of the Fama-French model addresses the second empirical shortcoming of the Sharpe-Lintner CAPM."*

[38] SFG (2015 p.6): *"While there is debate amongst academics and practitioners as to why this empirical result holds, there is no substantial disagreement that the result itself is true".*

[39] SFG (2015 p.11): *"So according to the AER, the high returns to stocks with a high book-to-market factor might represent something other than compensation to investors for bearing the risk of those stocks … Yet there is no question that the book-to-market effect is present in the stock return data over time and across markets, and neither the AER nor Handley (2014) reach any conclusion on how this empirical evidence can be accommodated in their estimate the cost of equity."*

discussed under sections 1(d) above, additional compensation is appropriate only if the value effect reflects risk.

SFG suggests that the Fama-French three-factor model should be used to take account of the value effect because the model can explain past stock returns.[40] But this purported justification is very weak – as discussed under sections 1(f) and 1(h) above, the previously reported empirical success of the Fama-French three factor is grossly overstated. It is now known that the Fama-French three-factor model has only a 6% explanatory power which leaves 94% unexplained.

In my opinion, the AER has taken into account the empirical evidence relating to the value effect in deciding not to use the Fama-French three-factor model.

(c) The AER's foundation model imposes a constraint on the estimation process and so does not produce an appropriate estimate of the cost of equity.

SFG states: *"By implementing the foundation model approach with this constraint, the foundation model approach will not deliver an estimate of the return on equity that is consistent with the allowed rate of return objective, and is reflective of prevailing conditions in the market for equity funds."*[41]

Response

SFG's criticism relates to the AER's estimation of beta. Specifically, SFG argues that the AER's beta estimate does not correctly take into account the low beta bias or give enough weight to the beta estimates of international comparable firms.

[40] SFG (2015 p.7): *"we expressed an expert view that the following weights could be applied … Half weight to the Fama-French model (due to its usefulness in explaining the stock returns that we actually observe)."*
[41] SFG (2015 p.3)

The AER has had regard to the potential for low beta bias and international betas in deciding to take a beta estimate (0.7) from the high end of its estimated range (0.4 to 0.7). This range was derived from a sample of Australian listed firms only. SFG claims that this approach imposes a constraint on the impact of the low beta bias and international betas in estimating the cost of equity: *"There is an implicit assumption [by the AER] that, in aggregate, the two sets of secondary evidence will be insufficient to adjust the beta estimate above 0.7"*. [42]

Instead, SFG suggests a multi-model approach should be used.[43] Under this approach, the beta is given by a weighted average of two sets of beta estimates (one for domestic firms and one for international firms) and the return on equity is given by a weighted average of the return on equity estimates from three asset pricing models (the Sharpe-CAPM, the Black-CAPM and the Fama-French three-factor model).

SFG argues that this approach is more appropriate since it separately and more directly takes into account the low beta bias (by using the Black-CAPM) and international betas (by applying a specific weight to international beta estimates): *"There is an explicit assumption about the impact that evidence from firms listed in other markets has on the cost of equity, and a separate explicit assumption about the empirical limitation of the Sharpe-Lintner CAPM…the explicit assumptions we adopt have not constrained the cost of equity estimate to within a boundary defined according to the first stage information"*. [44]

In my opinion, the foundation model approach does not constrain the estimation process. The AER has determined that the Black-CAPM and Fama-French three-factor model are not appropriate models for estimating the return on equity for a benchmark efficient entity. The AER has also determined not to give a

[42] SFG (2015 p.7)
[43] SFG (2015 p.7): *"In our view the best approach to estimating the required return on equity is to set out the estimate from every model that is considered to be relevant and then to weight each according to an assessment of the relative strengths and weaknesses of each. … the key point is that the AER's implementation of the foundation model approach constrains the impact of material, relevant information and the use of a weighted average approach does not impose such a constraint."*
[44] SFG (2015 p.9)

specific weighting to international betas since domestic betas and international betas are not strictly comparable – in this case, any weighting scheme would effectively be combining "apples and oranges".[45] It has, however, exercised regulatory judgment to give some effect to the potential for low beta bias and international beta estimates by taking a beta estimate from the high end of its estimated range (0.4 to 0.7) – resulting in a higher return on equity than would otherwise be the case.

(d) Handley (2014) reaches no conclusion on what the value effect represents or how to take it into account

SFG states:

- *"Neither the AER, nor Handley (2014) reach a conclusion on why stocks with high book-to-market ratios earn high returns".* [46]

- *"There is no question that the book-to-market effect is present in the stock return data over time and across markets, and neither the AER nor Handley (2014) reach any conclusion on how this empirical evidence can be accommodated in their estimate the cost of equity".*[47]

- *"The reason for this is that neither the AER, nor Handley (2014), reach conclusions on the basis of published evidence from the last four decades".* [48]

- *"While Handley (2014) introduces the recent work by Fama and French (2014) into his discussion, he makes no interpretation of the evidence."*[49]

[45] See Handley (2014 p.23-24) for further discussion
[46] SFG (2015 p.15)
[47] SFG (2015 p.11)
[48] SFG (2015 p.14)
[49] SFG (2015 p.21)

Response

The first statement is correct and importantly highlights the key issue concerning the value effect in the context of determining an appropriate level of compensation for a regulated firm – we simply don't know what it represents.

The second statement is incorrect – the conclusion is that the value effect should only be taken into account in determining returns if it reflects risk, otherwise the resultant estimate of the cost of equity would be inconsistent with the allowed rate of return objective.

The third statement is plainly incorrect and therefore surprising. Key conclusions including: (i) we don't know what causes the low beta bias; (ii) we don't know what causes the value effect; and (iii) we now know that the empirical performance of the Fama-French three-factor model is extremely poor, has been based on the recent literature as documented in this and other reports.

The fourth statement is incorrect – this issue is similar to the one considered under section 1(j) above.

(e) Handley (2014) misrepresents the results of Lewellen, Nagel and Shanken (2010)

There are a number of statements in my previous report that SFG objects to:

- The quote from Lewellen, Shanken and Nagel (2010) – which summarises the poor explanatory power of five asset pricing models – does not appear to be relevant since the five models do not include the Sharpe-CAPM or Fama-French models and *"it is not clear how the empirical performance of five different models can reasonably be used as*

the basis for eliminating the Fama-French model from further consideration".[50]

- Lewellen, Shanken and Nagel (2010) show that the Sharpe-CAPM has zero explanatory power whilst the Fama-French model generally has statistically significant explanatory power and uniformly outperforms the Sharpe-CAPM.[51]

- Lewellen, Shanken and Nagel (2010) suggest that a more meaningful interpretation of the explanatory power of an asset pricing model is obtained by considering the confidence interval for the R^2 and so SFG presents Figure 1 to summarise the empirical superiority of the Fama-French model.[52]

- It is unreasonable to conclude that Lewellen, Shanken and Nagel (2010) provides support for the exclusive use of the Sharpe-CAPM.[53]

Response

In my opinion, the above comments completely miss the relevance of the Lewellen, Shanken and Nagel (2010) paper to the current debate.

Lewellen, Shanken and Nagel (2010) show, among other things, that the previously reported empirical success of the Fama-French model is grossly overstated.

Lewellen, Shanken and Nagel (2010) do indeed show that the Fama-French model outperforms the Sharpe-CAPM (with a 6% explanatory power compared to a 0% explanatory power) but the key point is that the empirical performance of both models and not just the Sharpe-CAPM, is extremely poor.

[50] SFG (2015 p.17)
[51] SFG (2015 p.17)
[52] SFG (2015 p.17-18)
[53] SFG (2015 p.18)

It is not suggested here that Lewellen, Shanken and Nagel (2010) provides support for the exclusive use of the Sharpe-CAPM.

SFG's comment concerning the five asset pricing models is similar to the comment by NERA discussed under section 1(i) above.

SFG's presentation in Figure 1 is arguably misleading. Lewellen, Shanken and Nagel (2010) do not suggest that confidence intervals should be used to compare alternative asset pricing models. Rather they simply suggest that confidence intervals make the sampling issues associated with asset pricing tests more transparent.[54]

(f) Handley's (2014) assessment of the non-risk explanations of the value effect is problematic

SFG states:

- *"The problem with this assessment by Handley (2014) is that neither Handley, nor the AER, reaches any conclusion as to whether the book-to-market effect has a risk or non-risk based explanation."*[55]

- *"The analysis conducted by Daniel and Titman (1997) demonstrates that using the Fama-French model to estimate expected returns might be an incomplete description of returns".*[56]

- *"So the key point made by Daniel and Titman (2012) is that some newly identified factors published in recent papers might not in reality capture distinct risks, but rather are just different proxies for unspecified risks"*[57]

[54] According to Lewellen, Shanken and Nagel (2010 p.190): *"because the problems are exacerbated by sampling issues, our fourth suggestion is to report confidence intervals for cross-sectional R^2 s and other test statistics".*
[55] SFG (2015 p.19)
[56] SFG (2015 p.20)
[57] SFG (2015 p.21)

- *"At no stage do Daniel and Titman (2012) express support for the continued use of the Sharpe-Lintner CAPM as the single asset pricing model to explain expected returns:"*[58]

- *"We do not consider it reasonable to exclude the Fama-French model from consideration on the grounds that researchers continue to debate the underlying economic reasons for the book-to-market effect."* [59]

Response

The first statement is similar to the one considered under section 2(d) above. Daniel and Titman (2012) suggest there is still no consensus concerning how the Fama-French model should be interpreted despite being the topic of debate for decades.

The second statement misses the relevance of the Daniel and Titman (1997) paper to the current debate – Daniel and Titman (1997) suggest that the return premia on value stocks cannot be viewed as compensation for risk.

The third statement misses the relevance of the Daniel and Titman (2012) paper to the current debate – consistent with the findings of Lewellen, Nagel and Shanken (2010), Daniel and Titman (2012) identify problems with the standard methodology used in tests of asset pricing models.

In relation to the fourth statement, I have made no such claim.

I completely disagree with the fifth statement in the light of the allowed rate of return objective.

[58] SFG (2015 p.21)
[59] SFG (2015 p.16)

(g) Dividend discount models should be used in estimating the relative risk

SFG states: *"In estimating the relative risk of listed energy networks compared to the market, we recommend that dividend discount model estimates be given 25% weight with the remaining 75% weight allocated to estimation techniques that rely upon historical returns estimation (that is, implementing the Sharpe-Lintner CAPM, Black CAPM and Fama-French model with regression-based estimates of risk)."*[60]

Response

I repeat part of my previous advice on this issue:

"A DGM is no more than the fundamental principle of discounted cash flow (DCF) applied to the pricing of stocks. A similar approach is a well-established technique used in the pricing of bonds. But stocks differ from bonds in two important ways – cash flow and maturity – which in turn reduces the efficacy of the model when applied to stocks … Unlike bonds, future cash flows on stocks are not contractually specified nor have a finite maturity and so are subject to non-trivial estimation error – particularly the further ahead one looks … The [SFG] model is interesting but the regulatory environment involving an aggregate regulatory asset base measured in the tens of billions of dollars is not an appropriate setting to trial a new model whose widespread use and acceptance is yet to be established."[61]

[60] SFG (2015 p.25)
[61] Handley (2014 p.13-15)

3. RESPONSE TO THE ASX (2015) LETTER

According to Brian Goodman, Product Development Manager, Australian Securities Exchange:

- *"On Friday 24th October 2014, I received a query from Simon Wheatley, of NERA Economic Consulting, about two time series that the Australian Energy Regulator (AER) has been using to determine appropriate rates of return for regulated energy utilities. Apparently, the AER has claimed that the two time series that it has been using were supplied by the Australian Securities Exchange (ASX). Simon Wheatley wanted to know whether the time series are truly ASX products."*[62]

- *"NERA further reported that: Associate Professor John Handley of the University of Melbourne, in a paper co-authored with Tim Brailsford and Krishnan Maheswaran, used a series that is based on the two series (price and dividend yields) with an adjustment made to Lamberton's series of dividend yields. They multiplied the series of dividend yields that Lamberton had supplied by 0.75. The AER has used this series and has claimed that the adjustment has the blessing of the ASX."* [63]

- *"A search of the ASX email archive was not able to find any emails between ASX staff and the authors of the Brailsford, Handley and Maheswaran (2008) study. Please note however that email archiving system did not come into use until 2006 so anything prior is unavailable."* [64]

[62] Australian Securities Exchange (2015 p.1)
[63] Australian Securities Exchange (2015 p.2)
[64] Australian Securities Exchange (2015 p.3)

Response

The inference in the first statement that the stock and dividend data underlying the Brailsford, Handley and Maheswaran (2008) – BHM – dataset is not genuine is incorrect and troubling.

The claim (by NERA) in the second statement that BHM, rather than the ASX, made the adjustment to the dividend data is incorrect. I repeat my previous advice:

"Contrary to the claim by SFG – and it is not clear whether this view is also shared by NERA – the adjustment was not something which BHM took upon themselves to apply to the Lamberton data. Rather, the data that the ASX provided to BHM had already had been adjusted by the ASX. In other words, the ASX had many years earlier decided in their knowledge and wisdom that some adjustment was necessary and it was the ASX who determined the amount and adjusted the data accordingly. BHM simply sought to confirm their understanding of the data series provided by the ASX by reconciling it back to original sources."[65]

The source of the data and the dividend adjustment is well documented in the text and footnotes of Brailsford, Handley and Maheswaran (2008) as follows:

"A stock accumulation index for the period 1882-1979 was constructed retrospectively by the SSE in the mid-1980s based on the above (quarterly) price index data and three historical dividend yield series available at that time…[13]

[13] *Email correspondence from the ASX to the authors dated 11 April 2003 and 26 May 2004."* [66]

and:

[65] Handley (2014 p.19)
[66] Brailsford, Handley and Maheswaran (2008 p.79)

"Consequently, the SSE determined that the reported Lamberton/SSE yield series was prima facie not appropriate for the purposes of constructing an accumulation index and 'it was concluded that the real weighted dividend yield was probably overstated about a third on average and therefore the [Lamberton/SSE yield] was reduced by 25% in the early years of the accumulation index where we didn't have any other dividend yields to guide us'[16]

[16] *Email correspondence from the ASX to the authors dated 26 May 2004."* [67]

CONCLUSION

Overall, based on my review of NERA (2015), SFG (2105) and ASX (2015), I do not consider it necessary to change any of the findings in my earlier report – Handley (2014).

[67] Brailsford, Handley and Maheswaran (2008 p.80)

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Expert Witness Compliance Declaration

I have read the Guidelines for Expert Witnesses in proceedings in the Federal Court of Australia and this report has been prepared in accordance with those guidelines. As required by the guidelines I have made all the inquiries that I believe are desirable and appropriate. No matters of significance that I regard as relevant have, to my knowledge, been withheld.

Signed

John Handley

20 May 2015