

REPORT TO THE AER

RETURN ON EQUITY (Updated)

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April, 2015

Acknowledgement

The first draft of this report was jointly written with Michael McKenzie. Sadly, Micheal died in an accident before the final report was completed. I acknowledge Micheal's talented contribution to this report and have retained use of the term we throughout the report.

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

A handwritten signature in black ink, appearing to read 'Graham Partington', written over a horizontal line.

Graham Partington

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Background

In an earlier report, McKenzie and Partington (2014), we were asked to review the use of the Foundation Model approach by the AER, using the Sharpe-Lintner Capital Asset Pricing Model (S-L CAPM) to provide a cost of equity. The previous report reviewed the foundation model in five main areas. To be consistent with the original request for advice we shall refer to each of these areas as A1 to A5 respectively. The titles of the five sections and the specific questions that we were asked to address in each section are given under the heading Questions from the Original Report: Part A1 to Part A5 below.

We were subsequently requested to provide further advice in two stages. The first stage involved the provision of preliminary advice, followed by a second stage where a formal updating of our prior report was to be undertaken consequent to reviewing submissions that the AER anticipated receiving during February 2015. The terms of reference for stages 1 and 2 in the provision of advice are given below.

Terms of Reference Stage 1

To inform its final decisions on the rate of return for TransGrid, TasNetworks, Directlink, Ausgrid, Essential Energy, Endeavour Energy, ActewAGL and JGN, the AER commissioned the authors to provide advice. We were requested to

- Review the revised proposals from TransGrid, TasNetworks, Directlink, Ausgrid, Essential Energy, Endeavour Energy, and ActewAGL. Advise whether any new technical information/reasoning has been submitted in the revised proposals relating to McKenzie & Partington's expert report, which could lead to McKenzie & Partington changing their findings in the reports. Advise as to the scope of any potential work required to respond to the new information/reasoning.

In preparing the initial advice, the authors were provided with copies of the following key regulatory proposal documents:

- TransGrid, Revenue proposal, May 2014
- Ausgrid, Regulatory proposal, May 2014
- ActewAGL, Regulatory proposal, May 2014
- Directlink, Revenue proposal, May 2014
- Directlink, Revenue proposal—Attachment 6.1—Transitional approach to estimating the cost of debt, May 2014
- Jemena Gas Networks, 2015-2020 access arrangement information—Appendix 9.10—Return on debt proposal, 30 June 2014
- Transgrid Revised revenue proposal 2014-2018, January 2015
- Ausgrid Revised regulatory proposal., January 2015
- ActeAGL-Revised regulatory proposal, January 2015

And the following documents were also provided

- CEG – Estimating the cost of equity (attachment 7.03) January 2015
- SFG - The required return on equity initial review of AER draft decision (attachment 7.04) January 2015
- Prof Bruce Grundy – Letter to Networks NSW (attachment 7.05) January 2015
- NERA – Memo of revised MRP estimates (attachment 7.06) January 2015
- David Newbery, Cambridge Economic Policy Associates: Expert report, Jan 2015 (Attachment 1.07) January 2015.
- Grant Samuel Response to AER Draft Decision (12 January 2015)

Terms of Reference Stage 2

In relation to stage 2 we received the following instructions from the AER:

In October 2014 McKenzie & Partington provided a report on return on equity to the AER. This report reviewed the use of the AER’s foundation model approach to estimate a return on equity for regulated Australian gas or electricity transmission or distribution businesses. The AER’s foundation model approach is set out in the AER’s rate of return guideline (published December 2013). The October 2014 McKenzie & Partington report specifically reviews the foundation model in five main areas:

- Part A1: review of the foundation model approach
- Part A2: review of the role provided to the Fama-French three-factor model in the foundation model approach
- Part A3: review of the role provided to the Black CAPM in the foundation model approach
- Part A4: review of the role provided to the dividend growth in the foundation model approach
- Part A5: review of the role provided to experts’ reports in the foundation model approach

A revised version of the report provided in October 2014 is sought. This revised report should:

- Re-visit the findings of the October 2014 report for Parts A1 through to A5, in particular examining:
 - whether the foundation model approach if applied now (as set out in the Guideline, but with updated market information) would be expected to result in a reasonable estimate of the return on equity for a benchmark efficient entity with a similar degree of risk as that which applies to a regulated Australian gas or electricity, transmission or distribution business.
- Have regard to all the documents provided and listed in the ‘relevant documents’ section below, which include those received from NSPs, consumer groups and the Consumer Challenge Panel on rate of return.
- Include any amendments considered necessary by McKenzie & Partington to address any new information or reasoning in the ‘relevant documents’ that relates to McKenzie & Partington’s October 2014 report. If no amendments are considered necessary, this should be noted in the report along with reasons.

Without in any way intending to limit the revised report referred to above, the revised report should also address the following issues:

- Whether or not, in the absence of estimating the Black CAPM, Fama-French three factor model, and dividend growth model; there remains sufficient information on the relative merits of these models to assign to them the roles as set out in the AER's foundation model approach.
- The likelihood that the current yields on ten-year government bonds reflect an abnormal investment environment, and whether or not the foundation model approach is likely to produce a return on equity estimate that contributes to the rate of return objective in current market conditions. In considering this issue, the report should address the current evidence on any relationship between movements in the risk free rate and movements in equity risk premium.
- The considerations that should be had when examining both raw and re-levered equity beta estimates and the ability to draw collective inferences from both sets of estimates.
- The advantages and disadvantages of excluding the mining boom time period from equity beta estimates, and the implications any mining boom exclusion may have for the estimation of other cost of equity parameters such as the market risk premium.
- The extent to which risks associated with disruptive energy technologies should be considered in setting a return on capital allowance.

The following submissions were supplied by the AER (note: numbers represent the index numbers provided to us by the AER)

- 1.1_ActewAGL submission - Feb 13 2015_Further Submisison to the AER_FINAL.pdf
- 1.2_CitiPower and Powercor - Submission on NSW ACT and TAS draft decisions - 6 February 2015.pdf
- 1.3_Ergon Energy - Submission on NSW ACT and TAS draft decisions - submission - 13 February 2015.pdf
- 1.4_NSW DNSPs - Submission to the AER on its draft determination.pdf
- 2.1_ActewAGL submission - 32 SFG The required return on equity for the benchmark efficient entity.pdf
- 2.2_ActewAGL submission - 30 SFG Report Using the Fama-French model to estimate the required return on equity.pdf
- 2.3_ActewAGL submission - 31 SFG Beta and the Black Capital Asset Pricing Model.pdf
- 2.4_ActewAGL submission - 28 SFG Share prices, the dividend discoutn model and the cost of equity for the market and a bechmark energy network.pdf
- 2.5_ActewAGL submission - 27 Incenta, Further update on the required return on equity from independent expert reports.pdf
- 2.6_ActewAGL submission - 29 NERA Historical estimates of the Market Risk Premium.pdf
- 2.7_ActewAGL submission - 26 NERA EmpiricalPerformance of Sharpe-Lintner and Black CAPMs.pdf
- 2.8_Energex_39. Estimating the required return on equity - SFG Consulting.pdf
- 2.9_Energex - Submission on Energex's regulatory proposal 2015-20 and AER issues paper - attachment 1 - 30 January 2015.pdf
- 3.1_RARE Infrastructure - Submission on NSW DNSPs draft decisions - 13 February 2015_0.pdf
- 3.2_Spark Infrastructure - Submission on NSW DNSPs draft decisions - 13 Feb 2015_1.pdf
- 3.3_SA Council of Social Services (SACOSS) - Submission on SAPN's regulatory proposal 2015-20 - 30 January 2015.pdf
- 3.4_Queensland Council of Social Service (QCOSS) - Submission on Qld distributors' regulatory proposals 2015-20 - 30 January 2015.pdf
- 3.5_Energy Consumers Coalition of SA (ECCSA) - Submission on SAPN's regulatory proposal 2015-20 - 31 January 2015.pdf
- 3.9_Consumer Challenge Panel - Submission on draft decision and revised proposal - TransGrid - 19 February 2015.pdf

Questions from the Original Report : Part A1 to Part A5

Below are the questions that we were originally asked to address in McKenzie and Partington (2014).

Part A1: Review of the AER's Foundation Model approach to estimating RoE and its application

- a) The Foundation Model approach if applied now (as set out in the Guideline, but with updated market information) would be expected to lead to a RoE that when combined with our allowed return on debt (assuming this is set at the true cost of debt of our benchmark efficient entity) would lead to an allowed rate of return (calculated for each year over the regulatory period as a weighted average of the return on equity for the regulatory control period in which that regulatory year occurs and the return on debt for that year) that is commensurate with the efficient financing costs of a benchmark efficient entity with a similar degree of risk as that which applies to a regulated Australian gas or electricity, transmission or distribution business.
- b) The Foundation Model approach if applied now (as set out in the Guideline, but with updated market information) would be expected to result in a reasonable estimate of the return on equity for a benchmark efficient entity with a similar degree of risk as that which applies to a regulated Australian gas or electricity transmission or distribution business.
- c) The Foundation Model approach if applied now (as set out in the Guideline, but with updated market information) would be expected to result in a return on equity estimate that is systematically downwardly biased relative to the true unobservable cost of equity capital of a benchmark efficient entity.
- d) The addition of direct cost of equity estimates from the other models/sources as proposed by each of the NSPs (FFM, Black CAPM, DGM, expert valuation reports etc) would be expected to result in a materially better estimate of the RoE relative to the RoE estimates coming from the application of the Foundation Model approach as set out in the Guideline (if applied now with updated market information).

Part A2: Review the decision of the AER to not provide any weight to the application of the Fama French three factor model (FFM) when estimating the allowed return on equity

- a) The FFM can be used to reliably estimate the required return on equity for a benchmark regulated network service provider, or estimates from the model are likely to be empirically stable.
- b) The use of the FFM, either alone, or in combination with other asset pricing models, would be expected to result in a materially better allowed return on equity estimate for a benchmark regulated network service provider than the use of the estimates from applying the Foundation Model approach as set out in the Rate of Return Guideline.
- c) The FFM should be used for estimating the return on equity of a benchmark regulated network service provider either alone or in combination with other the other models proposed by the NSPs.

Part A3: Review of the decision of the AER to only consider the theory underpinning the Black CAPM to adjust the equity beta estimate in the Foundation Model approach.

- a) It is reasonable to limit the use of the Black CAPM to having regard to the theory underpinning the Black CAPM in choosing the equity beta.
- b) The theory underpinning the Black CAPM necessarily supports an uplift to the estimate of beta to be used in the SLCAPM for low beta stocks.
- c) The empirical use of the Black CAPM, either alone, or in combination with other asset pricing models, would be expected to result in materially better allowed return on equity estimates for a benchmark regulated network service provider than the use of the estimates from applying the Foundation Model approach as set out in the Rate of Return Guidelines.
- d) The Black CAPM should be used for estimating the return on equity of a benchmark efficient entity in combination with other models proposed by the NSPs.

Part A4: Review of the decision of the AER to limit the use of the Dividend Growth Model (DGM) model to the estimation of the market risk premium to be used to inform the MRP to be used in the foundation model

- a) If it is appropriate to limit the use of the DGM to informing the estimate of the market risk premium.
- b) Assuming it is appropriate to use the DGM to estimate the regulated business cost of equity, is it appropriate to use the DGM to inform the systematic risk exposure of regulated firms?
- c) If the use of the SFG DGM model will lead to a materially better cost of equity estimate for a benchmark regulated network service provider relative to the AER's foundation model approach and if the answer is yes, if you consider any extra computational intensity of the SFG DGM model (relative to the AER's DGM model) for this purpose is justified.
- d) What you consider is a reasonable estimate of the long term growth rate that should be used in applying a DGM? In relation to this point, please consider:
 - If a long term growth rate should be set to exceed the growth in GDP.
 - If it is reasonable to determine the long term growth rate from historical dividends or earning growth rates.
 - If the long term growth rate can and should be determined as the product of the retention rate and the return on equity.
 - Assuming the long term growth rate can be determined as the product of the retention rate and the return on equity, how these can and should be

determined (e.g. endogenously using data as SFG do, using historical data, or some other means).

- e) If a term structure can be incorporated in the DGM analysis?
- f) What is the most appropriate method for adjusting dividends for the impact of imputation credits?

Part A5: Review of the use of Expert's reports in estimating the RoE

- a) Whether you consider the use of expert reports (as set out in the Guideline) is reasonable for the purposes of informing the estimate of the allowed return on equity for a benchmark efficient entity with a similar degree of risk as the service provider.
- b) Whether you consider the use of expert reports for directly estimating the RoE is appropriate. In answering this question please take into account as part of your consideration the rationale for the creation of these reports and the methods they use to value firms.
- c) If the use of experts reports to directly estimate the RoE would be expected to materially improve the allowed return on equity estimate for a benchmark regulated network service provider relative to the RoE estimated under the Foundation Model approach as set out in the guidelines.
- d) Which expert report, if any, are appropriate comparators to use for informing or estimating the RoE for a benchmark regulated NSP. In answering this, please set out what you consider are desirable characteristics for comparator expert reports.
- e) If the level of adjustments undertaken in any expert reports you consider appropriate comparators (e.g. adjustment relative to the base WACC estimated using the SLCAPM) demonstrates that the expected adjustment to the SLCAPM under the Foundation Model approach (e.g. via using a beta towards the upper end of our estimated range for beta) is of an insufficient magnitude to take into account any expected biases in SLCAPM used as the base model in the Foundation Model approach.

This report represents our consolidated findings taking into account our McKenzie and Partington (2014) report and all of the documents listed above.

Introduction

McKenzie and Partington (2014) review the use of the Foundation Model approach by the AER, which is used in estimating the cost of capital for electricity networks and gas pipelines. The Foundation Model approach refers to the AER's use of the Sharpe-Lintner Capital Asset Pricing Model (S-L CAPM) to provide a cost of equity that acts as a starting point for analysis. The parameters that the AER uses in this model are informed by a number of sources of information.

In their report, McKenzie and Partington (2014) review the use of the foundation model and consider a range of other relevant information. This discussion was broken down into five sections:

Part A1: Review of the AER's Foundation Model approach to estimating RoE and its application

- Part A2: Review the decision of the AER to not provide any weight to the application of the Fama French three factor model (FFM) when estimating the allowed return on equity
- Part A3: Review of the decision of the AER to only consider the theory underpinning the Black CAPM to adjust the equity beta estimate in the Foundation Model approach.
- Part A4: Review of the decision of the AER to limit the use of the Dividend Growth Model (DGM) model to the estimation of the market risk premium to be used to inform the MRP to be used in the foundation model
- Part A5: Review of the use of Expert's reports in estimating the RoE

The conclusions of the McKenzie and Partington (2014) report may be summarised as follows. With respect to the Foundation Model, McKenzie and Partington (2014) argue that its application will lead to an RoE that when combined with the allowed return on debt would lead to an allowed rate of return that is commensurate with the efficient financing costs of a benchmark efficient entity.

We continue to hold that view and consider that the foundation model approach if applied now (as set out in the Guideline, but with updated market information) would be expected to result in a reasonable estimate of the return on equity for a benchmark efficient entity with a similar degree of risk as that which applies to a regulated Australian gas or electricity, transmission or distribution business.

In terms of the other potentially relevant information that could be used to inform the foundation model derived estimate, McKenzie and Partington (2014) firstly considered the Fama and French Model (FFM). McKenzie and Partington (2014) concluded that the FFM does not have the ability to reliably estimate the required return on equity for a benchmark regulated network service provider and may indeed lead to invalid, incorrect or misleading inference. The evidence suggests that there has been little use of this model in regulatory settings in other jurisdictions and neither has it had significant use in estimating the cost of capital for Australian firms. The latter is unsurprising as attempts to estimate the FFM in Australia, have led to rather different results with respect to both the sign and significance of the model's factors. The report also highlights new challenges in the literature to the use of the Fama and French model. This is particularly important in light of the consultants' continual promotion of the significance of the FFM parameters as being evidence in favour of this approach. For example, ActewAGL (2014, p. 272) point to SFG's empirical study that finds evidence of a significant positive return premium for high book-to-market stocks in the Australian context. This evidence cannot be given any weight until it is assessed in the context of the most recent contributions from the literature. In particular Fama and French have replaced the three factor model with a five factor model, Fama and French (2014). The five factor model adds an investment and a profitability factor to the three factor

model, but as it turns out only a four factor model is needed. The addition of the investment and profitability factors kills the explanatory power of the book to market factor.

A second source of potentially relevant information is the Black CAPM. McKenzie and Partington (2014) argue that while this model may have a role to play, it is unclear exactly what that role is. Further, there are a great number of practical difficulties to be confronted when implementing the Black CAPM such that McKenzie and Partington (2014) do not recommend any weight be given to the estimates provided in the network service providers consultants' reports. This is an important point as McKenzie and Partington (2014) do not suggest that the Black model cannot be estimated. Indeed, the consultants reports clearly show that it can be done. What they do say however, is that it is unclear what those estimates represent.

The final source of potentially relevant information considered in McKenzie and Partington (2014) is the Dividend Growth Model (DGM). McKenzie and Partington (2014) argue that the DGM may have a role to play in informing the estimate of the market risk premium. They do not recommend the use of the DGM for direct estimation of the cost of equity for utilities however, as there is the risk of substantial inaccuracy at the level of the individual firm. There is a significant risk that the DGM will overestimate the cost of equity and hence also overestimate the market risk premium.

In preparing this report, we have considered the arguments presented in the revised proposals as detailed in the Background section of this document. In brief, our position is that none of the information and arguments presented in these reports would give us cause to change our previously stated position. That is to say, the findings of McKenzie and Partington (2014) would remain unaltered in light of these additional submissions.

The report is structured as follows. We begin by summarising some of the main issues from the various submissions, particularly those arising from the consultants' reports. We then address a number of the issues arising from the submissions. This is followed by our response to the AERs questions A1 to A5 as per McKenzie and Partington (2014). This is essentially unchanged from our prior report as the various submissions have given us no cause to revise our conclusions. Lastly we discuss the additional issues that we were asked to address for the stage 2 report.

Summary of the Consultants Reports

The arguments of the consultants reports are designed to support their contention that the AER is too narrow in its view of the evidence it uses to set the parameters in the foundation model. For example, ActewAGL (2014, p. 257) state:

“If the AER uses its foundation model approach, it should set the parameter values in applying the SL-CAPM to reflect all relevant evidence before it and allow alternative models to be used as cross checks on the overall rate of return on equity...”

Similarly ActewAGL (2015, p.449) state:

“One reason the AER's foundation model generates the lowest estimate of the return on equity is that the equity beta and MRP input parameters used by the AER do not incorporate all relevant information.”

More importantly, the consultants also argue that the AER's continued reliance on the Foundation

Model as the primary tool for analysis is misplaced and more weight needs to be given to alternative methods. For example, Ausgrid (2014, p. 79) state:

“The AER’s consideration of relevant evidence is too narrow and does not give proper weight to each source of evidence that should be considered when estimating the cost of equity.”

Similarly ActewAGL (2015, p.439) state:

“By relying on SL-CAPM as the foundation model the AER omits relevant information and constrains the use of information to the foundation model’s parameters resulting in some information being given disproportionate weight or preventing relevant information from being used.”

Arguments are typically made in favour of the Black CAPM, the FFM, the DGM (some reports do argue in favour of other sources). For example, Ausgrid (2014, p. 79) write:

“We agree that the SL CAPM can be used as a base model for estimating the allowed return on equity. We also agree that the following sources of information should be taken into account when estimating the allowed return on equity:

- evidence from the ‘Black CAPM’ ...
- evidence from the Dividend Growth Model ...
- empirical estimates of the equity beta from both domestic and overseas firms
- estimates of the MRP using historical excess returns...”

AusGrid (2015, pp. 188–189) state:

“In its final determination, we consider that the AER should have regard to the following evidence:

- Fama-French model based estimates of the cost of equity for the benchmark firm;
- Empirical evidence of the low beta bias of the SL CAPM;
- Black CAPM based estimates of the cost of equity for the benchmark firm (using zero beta premium estimates from SFG); and
- DGM based estimates of the cost of equity for the benchmark firm

All of these sources of evidence contain relevant information as to what the true cost of equity for a benchmark efficient energy network firm is likely to be and therefore represents relevant information within the meaning of clause 6.5.2(e)(1) of the NER.”

While Transgrid (2014, p. 185) write:

“TransGrid’s approach to estimating the return on equity concludes that there are a number of sources of relevant information that can be used to improve the estimate of the return on equity. These are:

- the Sharpe-Lintner Capital Asset Pricing Model (CAPM);
- the Black CAPM;
- the Fama-French Three Factor Model;
- the Dividend Growth Model (DGM);
- an independent capital market expert’s recent valuation of a business comparable to a benchmark efficient entity; and
- a comparison of the return on equity to observed debt yields as a means of a reasonableness check.”

We note that Transgrid initially justify this approach using the following quote from the consultant SFG, and subsequently argue that it is the intention of the rules.

“In our view it is difficult to make the case that allowing the regulator to consider more information about the required return on equity would systematically result in lower-quality estimates.” Transgrid (2014, p. 185)

We must say that we disagree with the underlying message of this statement and the quotations above. The message is that using more data and more models is likely to improve the quality of the estimates. In econometric modeling when you put garbage in, you get garbage out. It is by no means assured that more information will result in a better estimate if that information is of poor quality or is downright misleading. We have written on this in a number of previous reports and indeed, it is a prevalent theme in the McKenzie and Partington (2014) report where we argue that it cannot be taken for granted that a number is meaningful without fully understanding the context in which it is estimated. Of course, the regulator may consider additional information and decide upon its value, and if low value information is given low, or zero, weight then this would be not systematically result in lower quality estimates.

TransGrid (2015 pp. 113–114) return to the point above arguing that this material should be considered as relevant evidence under the regulatory rules. While the regulatory rules do now specify that the regulator must have regard to all of the relevant evidence, they do not specify how the regulator must have regard to that relevant evidence. SFG, (2015a, pp 3 - 4) view the AER as having invented the notion of “primary” and “secondary” evidence. They argue that “the AER “has regard to” the secondary evidence in such a way that it has no material effect on the primary parameter estimates” and that this “effectively guarantees that it will have no effect.” While we are unable to comment on the motivations of the AER, what we can say is that we have always been of the view that all relevant evidence should be taken into account. However, with regard to the use of alternative models to estimate the cost of equity, such as the Black CAPM, the Fama and French (1993) three-factor model and the Dividend Growth Model, it is clear that there are substantial problems in implementing these models, as we subsequently discuss. It is our view that there is sufficient uncertainty in the validity and applicability of these alternative models to justify the current focus on the estimates provided by the SL-CAPM. While it is fair to say that the SL-CAPM model does have a number of issues, many of which are shared with the Black and Fama and French models, it is at least in wide spread use for estimating the cost of capital and has a relatively well agreed and transparent method of implementation.

Issues Arising from the Submissions and Consultants Reports

Our reading of the submissions and associated consultants reports has raised a number of issues that we feel need further discussion and we begin by addressing the consultants call to use more models.

Multiple cost of capital models

We have repeatedly expressed our view that triangulation is a useful technique, for example in relation to measurement of the market risk premium. However, this does not mean that using all possible models for the rate of return will give you a better estimate of the rate of return. There needs to be a strong case for the use of the model.

There is a clear case for using the Sharpe Lintner CAPM and the dividend growth model. Both have had extensive use in practice and in regulation when calculating the cost of capital. Both have also withstood an important test, the test of time. The CAPM has been in use for over 50 years and the dividend growth model has been used for even longer. In contrast, when calculating the cost of capital, there has been relatively little use of the Fama and French three factor model or the Black CAPM in practice or in regulation.

The Fama and French model has had significant practical use as a benchmark for measuring fund managers' performance. For example, in addressing the question: Does the fund return demonstrate the manager's skill, or just a portfolio tilt towards high book to market stocks (known as value investing)? Investors tend to view this as an issue of portfolio management style, rather than reflecting risk factors, as Carhart et. al. (2014) state:

"Value investing in equities is a good example of an alternative risk premium. However, most investors think of value investing as a style or as a skill sourced from individual managers—not as a risk." (p.25).

Regulators have flirted with the use of the Fama and French model, but that has not encouraged its ultimate adoption in regulation. For example, Green, Lopez and Wang (2003) in relation to updating the method used by US Federal Reserve to estimate the cost of equity for US banks state:

"Multibeta models could be employed to calculate the equity cost of capital used in the PSAF. However, because there is no consensus on the factors, adoption of any particular model would be subject to criticism. Because the academic literature shows that multibeta models do not substantially improve the estimates, the gain in accuracy would likely be too small to justify the burden of defending a deviation from the CAPM. We therefore do not recommend using multibeta models to calculate the cost of equity capital in the PSAF. Nevertheless we present some numerical results based on the Fama and French (1993) model. These results indicate that any additional accuracy provided by multibeta models is clearly outweighed by the difficulties in specifying and estimating them." (p.73)

We agree with this view. We also note, as discussed above, that even Fama and French are having second thoughts about their three factor model.

We have previously pointed out the problems with the application of the alternative cost of capital models particularly in Australian context, and we expand on this elsewhere in this report. Here we simply note that there is little consensus on the implementation of these models in Australia and there is substantial variation in the estimated parameters. For example, the widely divergent estimates of zero beta returns in the Black model as previously supplied by consultants and the substantial variation

in the academic literature arising from attempts to estimate the Fama and French model in the Australian context. Consequently, it is little surprise to find that there that there is negligible evidence of practical use of these models to estimate the cost of capital in Australia.

Multi-model approaches and cherry-picking

Where a multi-model approach is used, the Queensland Council of Social Services (QCOSS) expresses a concern about cherry picking. QCOSS even go so far as to suggest that the AER should go back to simply using the Sharpe Lintner CAPM, without reference to any other models. QCOSS (2015, p75) state:

“This approach allows for the use of multiple models and approaches (specifically, the following four - SL CAPM, Black CAPM, DGM, Wright approach) in determining various parameters or the overall return. The approach encourages ‘cherry picking’ by permitting the distributors to choose whichever model delivers the highest rate of return at the time of a given RCP. Advice to QCOSS suggests that the approach of using a range of models together (e.g. using the Black CAPM to pick the point estimate for the equity beta within the range estimated by the SL CAPM) is flawed because:

- the models have conflicting conceptual bases and assumptions and are not compatible with each other
- distributors can vary the weight that they put on the models from one RCP to the next. This approach is clearly evident in the relatively arbitrary weighting placed by SFG on the outputs of different models in estimating WACC parameters such as the equity beta.

The historical approach under the SL CAPM was reasonably predictable and transparent. This reduced opportunities for distributors to cherry-pick outcomes, which was a central concern expressed by policy-makers and stakeholders.”

So is QCOSS right about the increased risk of cherry picking with multiple models? It is evident from current and past regulatory determinations that these determinations generate extensive debate about the inputs to the CAPM. There has been extensive debate about the magnitude of the two parameters, the risk free rate and the market risk premium, and the variable in the model, the equity beta with respect to the market. There has also been extensive debate about which form of the dividend growth model to employ and how to estimate the inputs to that model.

Adding additional models provides a field day for consultants. For example adding the Fama and French model to the mix adds the book to market premium and the size premium to the parameters to be determined and adds two new betas to estimate relative to these premiums, a new equity beta with respect to the market also has to be estimated for use in the Fama French model as it may well differ from the equity beta in the Sharpe Lintner CAPM.

There are three opportunities for cherry picking. First, with multiple models there are multiple unobservable parameters and betas to estimate and there is considerable latitude in how the parameters and betas can be estimated and in the choice of data to be used for this task. Second, when it then comes to combining the results from multiple models there is no agreed optimal weighting scheme and the desired result can be obtained by judicious choice of weights. Third, it is possible to raise arguments about abnormal conditions, either requiring the revision of parameters, or the exclusion of certain data in estimating inputs to the models.

In the light of the foregoing we think the QCOSS argument about the risk of cherry-picking has considerable merit and the risk expands quite rapidly as the number of models increases. Even with the best will in the world, there is a natural inclination to select the parameters that favour self interest as being the truth, so there is a natural tendency towards cherry picking. As a test of this we propose the following hypothesis: Where a choice of parameters are available, the regulated businesses will tend to select the values resulting in a higher rate of return and those groups representing users will tend to select the values resulting in a lower rate of return. This hypothesis is well supported by the submissions that we have been asked to review.

The relationship between r_f , r_z and the cost of debt

Recall that the Capital Asset Pricing Model (CAPM) may be written as:

$$E(R_i) = r_f + [E(R_M) - r_f] \beta_i \quad (1)$$

where

$$\beta_i = \frac{Cov(R_i, R_M)}{Var(R_M)} \quad (2)$$

In deriving the CAPM, Sharpe (1964) assumes that (i) investors are risk averse and seek to maximise expected utility of end-of-period wealth, (ii) investors are price takers with homogeneous expectations about normally distributed asset returns, (iii) there is a risk free asset and unlimited borrowing and lending is possible at r_f (iv), that all assets are fixed in supply and are perfectly marketable and divisible, (v) that asset markets are frictionless and finally (vi) that there are no market imperfections.

Where the assumption that the investor can borrow and lend unlimited amounts at the risk free rate is violated, the CAPM does not apply. Such a violation may occur when there is a restriction on short selling the risk free security, or where there is no risk free security at all - both cases are dealt with by Black (1972). Any attempt to compare the Black CAPM and S-L CAPM must be done with great care. In the case where the risk free security, r_f , exists then the zero- β orthogonal portfolio to the market, r_z must coincide with r_f .

For example, consider Figure 1 below:

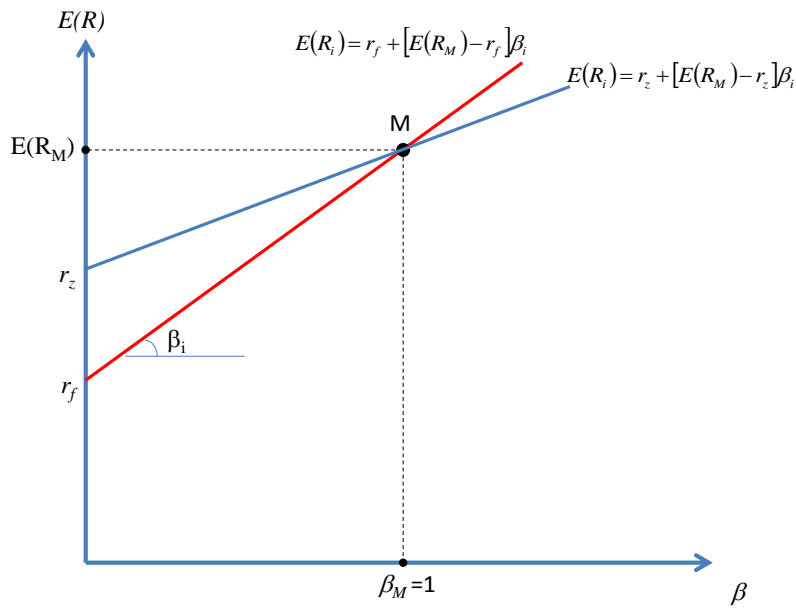


Figure 1: Security Market Line: Sharpe-Lintner and Black CAPM

We note that the efficient frontier in both the S-L and Black cases must be linear and given that there is one market portfolio the efficient frontier must pass through the point M. Here the efficient frontier is a locus of risk-expected return combinations where risk is measured by β . Recall that β measures undiversifiable risk. When r_f exists then the return to the zero beta portfolio r_z must equal r_f .

To see this point note that, from the derivation of the CAPM, we know that at the point M the slope of the efficient set is equal to the slope of the Capital Market line, which is tangential to the investment opportunity set at M. This is illustrated in Figure 2.

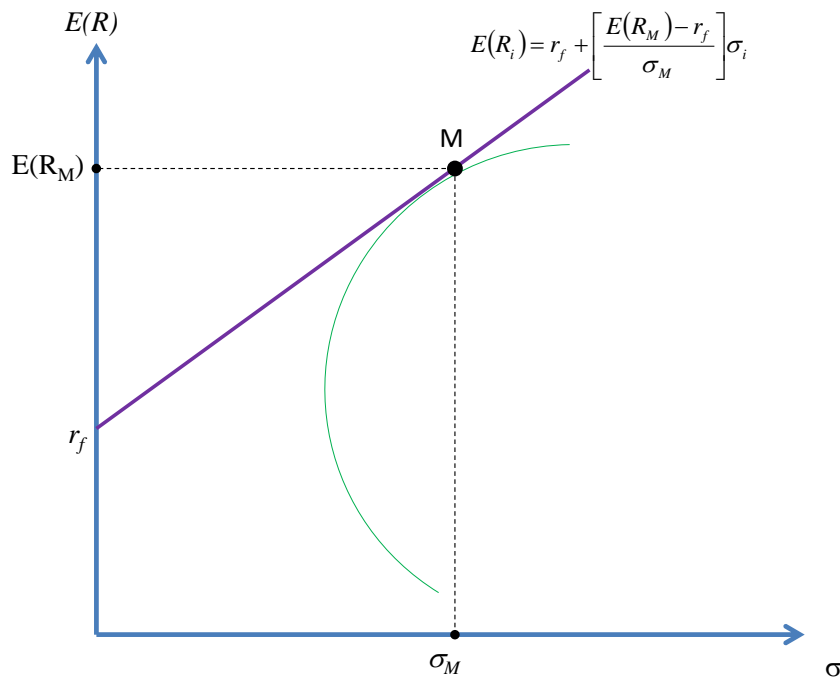


Figure 2: The Capital Market Line

The efficient frontier in Figure 2 is a locus of risk-expected return combinations where risk is measured by σ . Recall that σ measures the total risk of an asset, whether diversifiable or undiversifiable. Here the

slope of the Capital Market Line at M equals the slope of the Security Market line at M because M is an efficient portfolio. In Figure 1, therefore, it is incorrect to draw a comparison between the SML for the S-L CAPM and the SML for the Black CAPM. In equilibrium, the slopes of the S-L and Black CAPM must be equal where both r_f and r_z exist and short selling of the risk free asset is allowed.. Given that the slopes are equal and the linear efficient frontier contains the point $(E(R_M), 1)$, the intercepts must coincide. Necessarily therefore, given the common definition of β in the S-L and Black models the slopes of both lines must be equal the slope of the CML at M and the minimum variance orthogonal portfolio and the risk-free portfolio must coincide. That is $r_f = r_z$. Where the two diverge, as shown in Figure (1), then the figure is misleading.

Following this logic, we should also point out that statements such as:

“The return on a zero beta asset is expected to be above the risk-free rate”
ActewAGL (2014, p. 267)

are also misleading and are symptomatic of the confusion that surrounds the Black model in the consultants reports (see McKenzie and Partington, 2014 pp. 20 - 25).

Similarly, CEG (2015, p. 7) present the following equation:

$$\frac{E(R_M) - E(R_{\beta=0})}{E(R_M) - \text{Govt. bond rate}} = 0.50$$

This equation is again misleading and we note our comments above about exercising great care when comparing the S-L and Black CAPM apply equally here. In an equilibrium pricing model, one aspect that is important is that the model produce a single price. The relationship in the CEG report is consistent with multiple prices and is therefore incorrect. Where both r_f and r_z exist, equilibrium requires that:

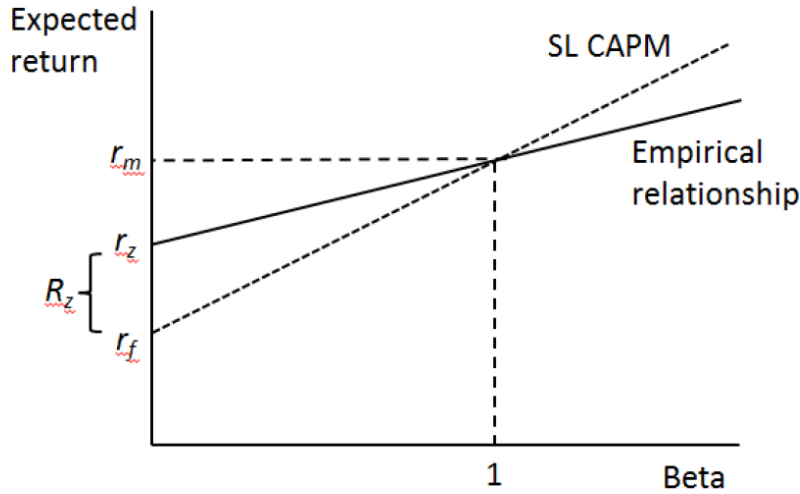
$$\frac{E(R_M) - r_f}{E(R_M) - E(R_z)} = 1$$

Where this ratio is different to one, it suggests that multiple prices for an asset exist, which is inconsistent with equilibrium as there will be unexploited arbitrage opportunities.

Theoretical and empirical versions of the CAPM

ActewAGL (2014, p. 266) present Figure 10.3, which is taken from an earlier SFG report. We note that SFG (2015a, p. 15) also includes this as Figure 1 and we reproduce it below for the purposes of discussion.

Figure 10.3 Sharpe-Lintner CAPM versus empirical relationship (Black CAPM)



Source: SFG Consulting, Cost of equity in the Black Capital Asset Pricing Model, p 8

In the ActewAGL (2014) report, the discussion below Figure 10.3 makes it clear that the Black CAPM represented in this figure corresponds to the case where no risk free security exists. In our view, Figure 10.3 is therefore somewhat misleading as it compares two states of the world that do not simultaneously exist.

While the empirical evidence in tests of the CAPM generally gives a flatter relation between measured risk and realized return than that implied by realised values of the market return, testing realisations is not the same thing as testing an *ex-ante* relation. The S-L CAPM is an *ex ante* model with a linear and positively sloped relation in equilibrium expected returns. Conversely, the empirical relationship is an *ex post* model that predicts a linear relationship between actual returns. It is not necessary that the empirical CAPM be positively sloped and may, in fact, be horizontal or downward sloping. This can be seen as follows: if, on average, the expected rate of return to an asset equals the actual rate of return then the asset is said to be a fair game. This may be written as

$$R_{it} = E(R_{it}) + [R_{Mt} - E(R_{Mt})]\beta_i + u_{it}, \quad (3)$$

where u_{it} is a zero mean white noise error. Taking expectations of both sides of (3) it follows that $E(R_{it}) = E(R_{it})$ so on average the investor's expectations are realised. Substitution of the definition of $E(R_{it})$ from (1) into (3) yields

$$\begin{aligned} R_{it} &= r_{ft} + [E(R_{Mt}) - r_{ft}]\beta_i + [R_{Mt} - E(R_{Mt})]\beta_i + u_{it} \\ &= r_{ft} + [R_{mt} - r_{ft}]\beta_i + u_{it} \end{aligned} \quad (4)$$

Subtraction of r_{ft} from both sides of (4) yields the empirical CAPM

$$R_{it} - r_{ft} = [R_{mt} - r_{ft}]\beta_i + u_{it} \quad (5)$$

It is important to note that the empirical CAPM, which is an *ex-post* model, can have a negative slope, while the both Black and Shape Lintner CAPM models are *ex-ante* models and must have an upward

slope as the expected return to the risky market portfolio must always exceed r_f . If $R_{Mt} > r_{ft}$ then the empirical CAPM given by (5) will slope downwards.

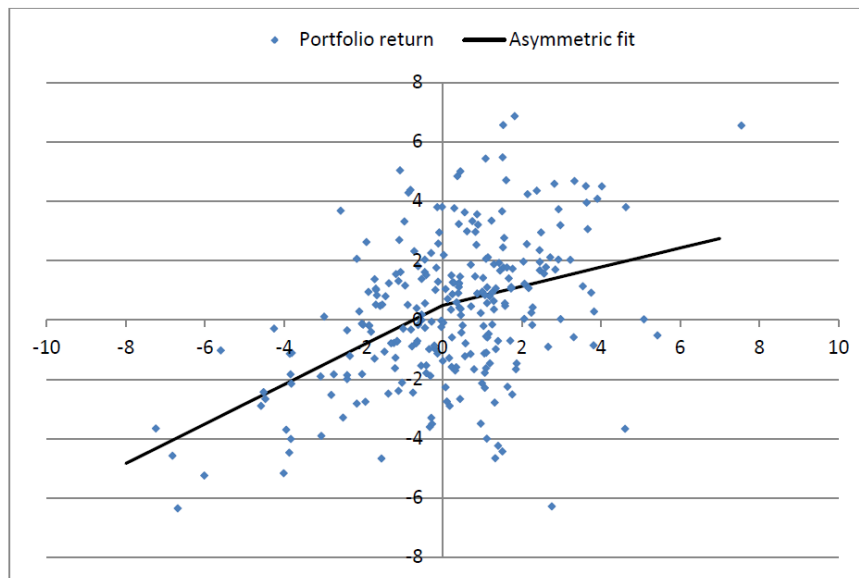
Beta stability returns and statistical significance

In the discussion presented by AGL, Figure 10.2 (2014, p. 264 and reproduced below) depicts an alleged asymmetric relationship between an equally weighted portfolio of energy stock and the Market Portfolio. Denoting the return to the equally weighted portfolio as R_{pt} , then a model of the type

$$R_{pt} = a_o + a_1 R_{Mt} + e_{pt}, \quad (6)$$

can be used to obtain an estimate (a_1) of the portfolio beta using Ordinary Least Squares regression analysis. We note that AGL do not present their regression results, nor do they define the point of asymmetry.

Figure 10.2 Demonstration of asymmetric market risk



However, it seems likely that they define a dummy variable D_t such that $D_t = 1$ if $R_{Mt} < 0$, and zero otherwise and then estimate a regression similar to:

$$R_{pt} = a_o + b_0 D_t + a_1 R_{Mt} + b_1 (D_t \times R_{Mt}) + u_{pt} \quad (7)$$

Where b_0 and b_1 differ significantly from zero individually and/or jointly then a figure such as 10.2 obtains. However, the question is whether b_0 and b_1 differ *significantly* from zero. There is no way to determine the joint and individual significance of the estimates of b_0 and b_1 as neither parameter estimates nor standard errors are provided. Moreover, it might be equally valid to define a dummy variable D_t such that $D_t = 1$ if $R_{Mt} < \lambda$ where λ is an unknown constant to be estimated. There is too much uncertainty about the validity of the assumptions and/or methodology underlying Figure 10.2 for it to be given much weight.

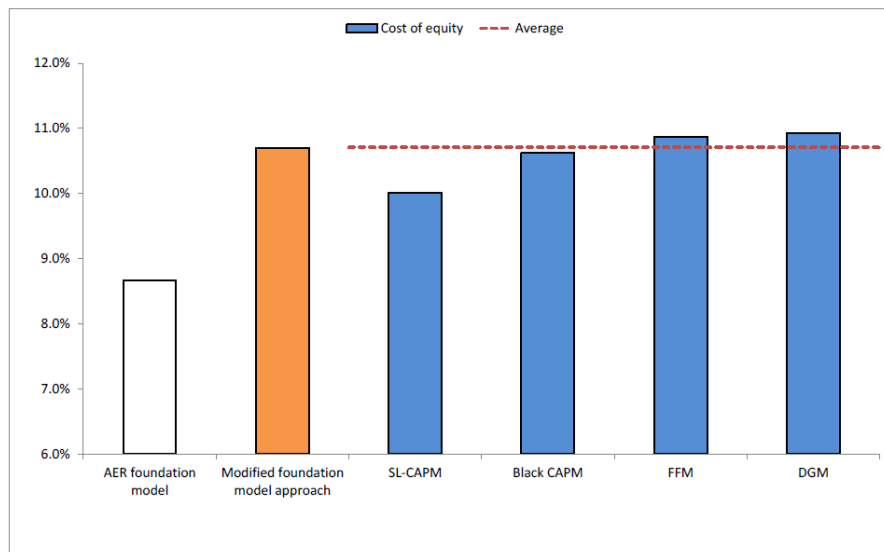
At issue here is the question as to whether equation (7) provides a superior conditional characterisation of the data over equation (6)? One cannot assess the answer to that question using the scant information provided. We note that Henry (2008, 2009, 2014) estimates a range of models consistent with (6) and finds no evidence of serial correlation in the residuals from these models. This is consistent

with the view that there are no omitted variables such as D_t or $D_t \times R_{Mt}$ in those models and we take this evidence as suggesting that there is no such asymmetry.

We note that the differences in return arising from the claimed asymmetry in this figure will arise from differences in estimated betas. What is provided by such estimates is a point estimate of the return on equity. We suspect that none of the differences in the point estimates of return are likely to be significant given the likely standard errors of the beta estimates.

We note that ActewAGL (2015, p. 261) also provide the following figure:

Figure 10.1 Estimate of the return on equity using different equity models



We further note that there is no *ceteris paribus* in the information underlying this figure. The market risk premium, the risk free rate and the beta may vary to produce the estimated return on equity. It is impossible to determine whether any of these differences are meaningful and, in particular, whether the difference between the AER foundation model result and the other estimates is statistically meaningful. All we can say using this figure is that if you vary the inputs to a model, you will necessarily get variation in the predictions.

A final comment may be made with reference to a number of the reports that allege instability in the estimates of β . Henry (2008, 2009, 2014) provides a range of evidence demonstrating the stability of the estimates.

Being clear about the “Black” model

It is unhelpful to continue to refer to the Black, Vasicek and Brennan models as the ‘Black’ model. Notwithstanding the lengthy arguments of NERA (2015, pp. 17-18), to the best of our knowledge, there is no general usage, such that the Brennan model is referred to as the Black model. This distinction is important if these models are being considered as supplementary sources of information to augment the SL-CAPM model.

In the Black model calculating a zero beta premium above the risk free rate makes little sense. To compute such a premium we have to estimate a return on Black’s minimum variance zero beta portfolio, a return that cannot be directly observed, and then subtract from that a risk free rate that does not exist in the world of the Black CAPM. This hardly seems to be a compelling basis for computing

a regulated return

In justification of this, NERA (2015, p. 18) imply that they are really using the Brennan model:

“NERA (2012, 2013), to conserve space, chooses to briefly describe only one of the Black, Vasicek and Brennan models. Since Brennan’s model includes the other two models as special cases NERA chooses to describe his model while noting – correctly, as we demonstrate – that all three models are often labelled the Black model.”

To be clear, we repeat the statement of McKenzie and Partington (2014) on this issue:

“We note that the model of Brennan (1971) is not the Black CAPM. Rather, the Brennan model imposes a differential between borrowing and lending rates in an economy where *a risk free security exists*. In the zero beta CAPM of Black (1972), there is *no risk free security* and consequently there is a role for the zero beta portfolio.”

The consultant’s construct of the zero beta premium does not appear to have much, if any, use elsewhere. In the interest of clarity and avoiding ambiguity, it might be better to avoid its use in the current context.

Variability and plausibility of the zero beta return

SFG (2015a, pp. 13 - 14) and makes the point that

“The AER does not reject the SFG estimate because it considers the estimation approach to be inappropriate or because it considers the estimate to be implausible – it rejects the SFG estimate because there are other estimates that use different approaches that produce estimates that the AER considers to be implausible.”

While we have not validated the SFG estimates, nor the reasons for the AER rejection of them, we would again point to our previous discussion on this issue. McKenzie and Partington (2012b) present a numerical example, which was designed to highlight the sensitivity of the Black model to the choice of proxy for the market portfolio. Our point is simply that ‘what you get depends very heavily on what you do’ and we expand on this point in the next section below. While SFG has managed to produce an estimate that might be regarded as ‘plausible’,¹ the sensitivity of the BLACK CAPM estimates means that this is entirely unsurprising. As our simple example shows, with minor variations in inputs, a wide range of results are possible, some of which are ‘plausible’ and some of which will be ‘implausible’. As we also point out below the basis for determining plausibility is subjective rather than objective. We might argue that this is perhaps a major reason why the Black CAPM has not seen widespread application in this context.

In any event the plausibility of the SFG estimate is highly questionable. As we discuss below given the way the consultants have estimated r_z it is not constrained to lie within any particular bounds. However one reasonableness test for the value of r_z is that the return on the zero beta portfolio should not exceed the return on risky debt. SFG propose a zero-beta premium of 3.34%. With current risk free rate of 2.5% this gives r_z of 5.84%. In comparison, the current rates (April) on BBB-rated corporate debt, as reported by Bloomberg are under 4.5% at maturities up to 10 years and even extrapolated out to a 30 year maturity remain well below 5%. Clearly, the SFG estimate fails this reasonableness test.

¹ We would consider it more plausible than the NERA estimate, but that it is not plausible for current use.

Sensitivity to choice of the market portfolio

NERA has disputed our point that the zero beta return is sensitive to the choice of proxy for the market portfolio. We call this proxy portfolio the reference portfolio, because both beta and the zero beta return are computed with reference to the chosen market proxy. It is critical to understand that NERA's argument relates to where the reference portfolio sits on the efficient set. This is important because the actual reference portfolio that NERA uses does not lie on the efficient set.

In mean variance space, the efficient frontier is described by a parabolic shape. The sensitivity of the zero beta return to the choice of a reference portfolio (market proxy) will depend on the curvature of the parabola segment separating alternative reference portfolios. It is curvature that determines the slope of tangents to the parabola. In turn, it is the slope of the tangent to the point represented by the reference portfolio that determines where the tangent cuts the return axis and this intercept determines the magnitude of the return on the zero beta portfolio. The greater the curvature, the greater the resulting difference in the estimated zero beta returns for different reference portfolios.

The curvature will be greater the closer the reference portfolio is to the minimum variance portfolio and so the greatest sensitivity will be in the region of the minimum variance portfolio. However, it does not follow, as asserted by NERA, that there will be no problem in sensitivity arising from the choice of reference portfolio. NERA argues that alternative reference portfolios (market proxies) sit on the efficient frontier well above the minimum variance portfolio, where there is less curvature and hence less sensitivity. The key question, however, is not how far the reference portfolio sits above the minimum variance portfolio on the efficient frontier, but rather what is the curvature of the parabola segment separating alternative reference portfolios. This depends both on the form of the parabola and where on the parabola the reference portfolio sits. A nice dynamic demonstration of the effect of these two factors on the slope of tangents can be found at: <http://demonstrations.wolfram.com/TangentLinesToAParabola/>, and a figure from this demonstration is given below.

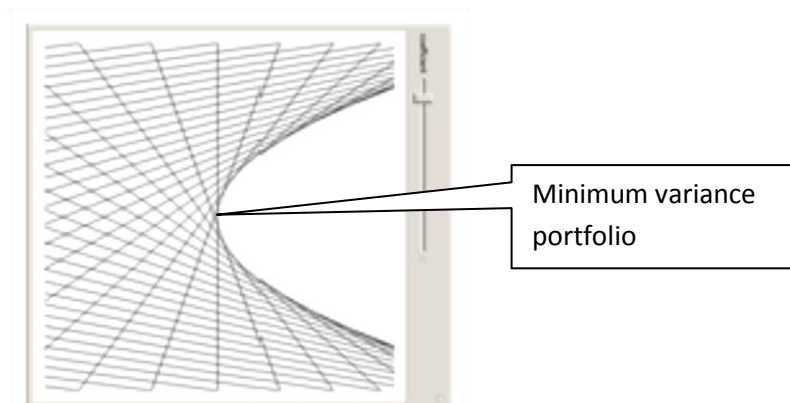


Figure 3 "Tangent Lines to a Parabola" from the Wolfram Demonstrations Project

Source: <http://demonstrations.wolfram.com/TangentLinesToAParabola/> **Contributed by:** Greg Radighieri

The figure demonstrates that, for this parabola, there is sensitivity of the tangent slope to the point of tangency, and although the sensitivity gets smaller as the point of tangency moves away from the minimum variance portfolio, it still persists. A narrower parabola would tend to show more sensitivity and a wider parabola would tend to show less sensitivity. What is clear is that it cannot be simply

assumed that the alternative market proxies sit in a region and on a parabola where the sensitivity to curvature is not an issue.

In any event, the key question is not what happens in relation to efficient portfolios, but rather what happens in the case of inefficient portfolios. This is because the results of NERA's various empirical analyses (most recently NERA, 2015) show that the reference portfolio they use is not on the efficient set ex-post. If it were, then there would be a perfect linear relation between the returns on securities and their betas calculated relative to the reference portfolio. Empirically, however, this is not the case. Therefore, the reference portfolio is not on the efficient set.

The implication of a reference portfolio that is not on the efficient set is that there is an infinite set of zero beta portfolios with differing returns that can be associated with the reference portfolio. In this case, the zero beta return can be more or less arbitrarily chosen. NERA and SFG restrict the choice by fitting a regression model to the data in order to obtain a single estimate. However, the estimates obtained then depend upon how the data is organised and the regression model used. This is amply demonstrated by the different estimates of the so called zero beta premium² over the risk free rate, which is estimated at 10.75% by NERA and 3.34% by SFG.

There is no objective benchmark to choose between the two estimates because, when the reference portfolio does not lie on the efficient set, the zero beta return is unconstrained. The estimate, for example, is not constrained to lie between the lending and borrowing rates, as prescribed by the Brennan (1971) model. Such constraints might be regarded as sensible, but given an inefficient reference portfolio they are not objectively imposed either by a theoretical model of returns, or by the estimation process. It is very much a case of what you get depends on what you do. The resulting latitude in estimates and the lack of an objective benchmark renders these zero beta estimates unsuitable for determining required rates of return.

Stability of estimates

SFG (2015c) considers the Fama and French (1993) model. In this paper, the authors argue that the AER is incorrect in dismissing this model on the basis that it is sensitive to different estimation periods and methodologies as "this applies to all models" (p. 2).

It is our view that the empirical evidence on the FFM suggests that it is unlikely to produce estimates that are empirically stable and that it involves relatively arbitrary choices in measuring the risk premium on the factors. We have discussed this point on numerous occasions previously and documented the fact that the estimated factor estimates vary considerably in both sign and significance. Furthermore as we have pointed out previously there is plenty of questioning of what factors should be included in the model.

We do not agree with SFG however, that "this applies to all models". We agree that estimated values may vary over data sets, the question is do they vary moderately or do they vary so much as to be considered unstable and/or unreliable? In this context we note that Henry (2008, 2009, 2014) tests for, and finds no evidence of, structural instability in the estimates of the equity beta in the SL-CAPM.

² As we have pointed out, this concept does not make sense in the context of the Black (1972) model, but does make sense in the context of the Brennan (1971) model.

Dividend growth model and real returns

The problems of the dividend growth model have been amply covered in our prior work and several of these issues are subsequently covered in this report at section A4. However, we must point out the error that SFG (2015d) make on pages 13 through 15, where they incorrectly adjust the growth rate calculations from McKenzie and Partington (2013b*) in rows 4 through 7 of our nominal dividend growth calculations. We have previously pointed out their mistake in this regard in relation to an earlier paper by SFG. We reiterate here that there is no error in relation to our computation of the nominal long run growth rates in rows 4 through 7. SFG make the basic error of not understanding that the difference between two real rates of return is approximately the same as the difference between the two rates converted to nominal rates of return. We find this surprising. We reiterate our conclusion that in comparison to the average nominal growth rate estimated across all the data sources in the table the growth rate adopted by the AER seems on the generous side.

To prevent any further misconceptions we re-explain the data and calculations below. The table that contains the growth rates is reproduced below, with the long run nominal growth rates given in the last column. The sources of the data for this table, as explained in the text that originally accompanied the table, were:

Bernstein and Arnott (2003) and Ritter (2005): historic average long run real dividend growth rate in Australia 0.9%.

Bernstein and Arnott (2003) : long run real dividend growth rate equals long run real GDP minus 2.4%.

Barra (2010): long run real dividend growth rate equals real EPS growth equals 0.5% or equals the long run real GDP growth rate minus 2.7%.

Lally (2013): long run real dividend growth equals the long run real GDP growth of 3.0%, with growth adjusted down by 0.5%, 1.0% and 1.5%.

CEG (2013): long run real dividend growth equals the long run real growth rate in GDP of 3.9%, or it equals the historic average long run real dividend growth rate of 2.76%.

Lally (2013) and CEG (2013): expected inflation 2.5%.

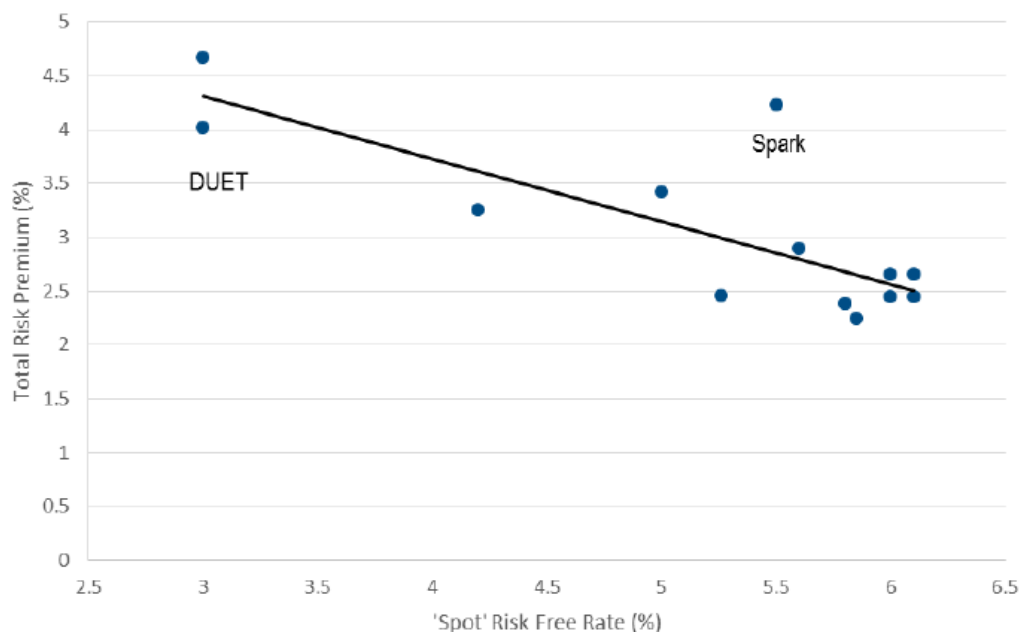
Comparison of this data with the table shows that all the information in columns one to six comes from these sources and there are no errors of transposition. The nominal long run dividend growth rate in for the first three rows is computed as $(1 + \text{Average real dividend})(1 + \text{Inflation}) - 1$. The nominal long run dividend growth rate in the next four rows is computed as $(1 + \text{Inflation})(1 + \text{Real GDP}) - (1 + \text{Inflation})(1 + \text{Real GDP adjustment})$ and this gives a result that approximates the difference between real GDP and the real GDP adjustment. The nominal long run growth rate in rows seven through 14 is given by $((1 + \text{Inflation})(1 + \text{Real GDP}) - 1 - \text{Lally GDP Adjustment})$. In this latter case we have treated the Lally GDP adjustment as a nominal growth adjustment. Had we treated the Lally adjustment as an adjustment to the real growth rate the resulting average growth rate across all the estimates would only have been 3%, making the AER estimates look even more generous.

Main Source	Average Real Dividend	Inflation	Real GDP	Real GDP adjustment	Lally GDP Adjustment	Long run growth
Barra	0.50%	2.50%				3.01%
Bernstein/Ritter	0.90%	2.50%				3.42%
CEG	2.76%	2.50%				5.33%
Lally/Bernstein		2.50%	3.00%	2.40%		0.62%
CEG/Bernstein		2.50%	3.90%	2.40%		1.54%
Lally/Barra		2.50%	3.00%	2.70%		0.31%
CEG/Barra		2.50%	3.90%	2.70%		1.23%
CEG		2.50%	3.90%			6.50%
CEG		2.50%	3.90%		0.50%	6.00%
CEG		2.50%	3.90%		1.00%	5.50%
CEG		2.50%	3.90%		1.50%	5.00%
Lally		2.50%	3.00%		0.50%	5.08%
Lally		2.50%	3.00%		1.00%	4.58%
Lally		2.50%	3.00%		1.50%	4.08%

Small date sets and misleading inference

Incenta (2015) argue that it is incorrect to assume that the total risk premium is independent of the risk free rate. In support of this view they provide figure ES.1 (reproduced below).

Figure ES.1: Independent expert reports (regulated infrastructure) – Total risk premium vs risk free rate



Source: AER (November, 2014), p.3-90 to p.3-92, Grant Samuel, Incenta analysis

In figure ES.1 the 13 data points are AER estimates of the total risk premium, adjusted for the Grant Samuel uplift factor, plotted against the 'spot' risk-free rate prevailing at the time of each independent expert report. We note that there are no coefficient estimates, nor standard

errors provided. Moreover, we note that making reliable inference in a sample of 13 observations is extremely difficult.

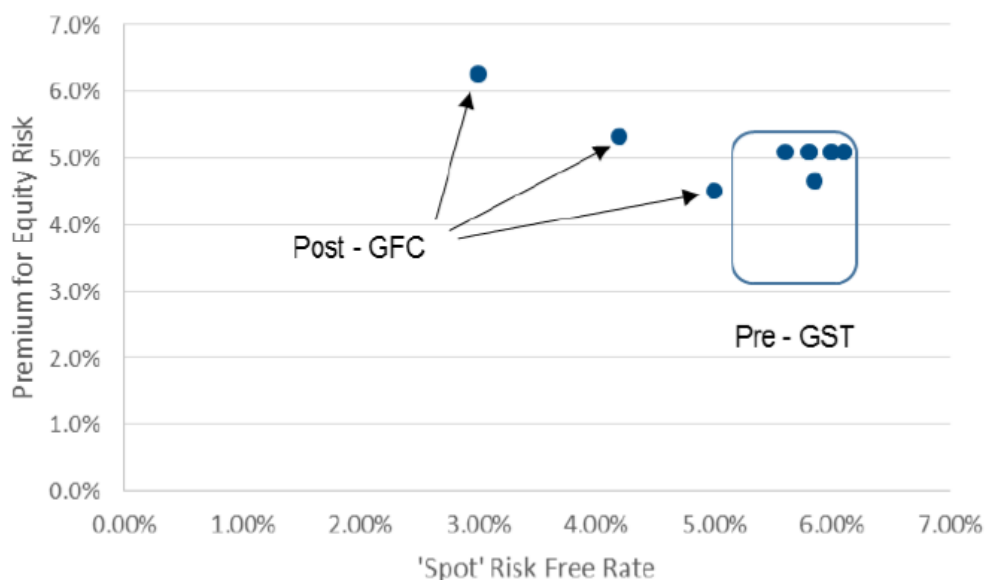
In ES.1 the two leftmost observations above the word 'DUET' are unusual. Both observations are associated with a risk-free rate of 3% compared with 4-6% for the remaining 13 observations. Moreover, if the Total Risk Premium estimates were ranked from largest to smallest, these leftmost observations would be first and third in magnitude. In fact 10 of the 13 Total Risk Premium estimates lie in the range 2.5-3.5%. It is likely that the apparent negative relationship in ES.1 is driven entirely by the high degree of leverage obtaining to the two leftmost observations. Omitting these unusual observations would be likely to yield a flat regression line, consistent with the view that there was in fact no relationship between estimates of the total risk premium and the 'spot' risk-free rate.

Similarly, figure ES.2 (reproduced below) attempts to compare a sample of 3 post GFC observations with a sample of 5 pre-GST (which it seems should be GFC) observations, there is little need to comment as any inference is likely to be in the eye of the beholder and not supported by meaningful statistical analysis.

We note in passing that ES.1 and ES.2 (reproduced below) are repeated in Incenta (2015) as Figure 3.2 on page 20 and Figure 3.3 on page 21. Again the inference in the report is highly speculative at best.

We note also that Incenta (2015) also rely heavily on an 'uplift analysis' by Grant Samuel that is of questionable academic merit.

Figure ES.2: Independent expert reports (regulated infrastructure) – Risk premium for equity vs risk free rate



Source: AER (November, 2014), p.3-90 to p.3-92, Grant Samuel, Incenta analysis

Part A1: Review of the AER’s Foundation Model approach to estimating RoE and its application

The AER’s proposal for estimating the expected return on equity using the S–L CAPM as a ‘foundation model’ provides a starting point, which is firmly based in a mature and well accepted theoretical and empirical literature. As no framework is perfect, the foundation model has its weaknesses, but these are well-documented and in many cases can either be diagnosed or perhaps compensated for in empirical practice. The final estimate of the expected return on equity may have regard to a broad range of relevant material including a range of multifactor models such as the Fama and French (1993) and the APT of Ross (1976), *inter alia*. Many of these competing models nest this foundation model and so potentially make more use of available information. In that sense, they may prove to be useful in validating this foundation model estimate.

With regard to the CAPM, its efficacy comes from the test of time. This model has been around for in excess of half a century and has become the standard workhorse model of modern finance both in theory and practice. The CAPM’s place as the foundation model is justifiable in terms of its simple theoretical underpinnings and relative ease of application. The competing alternatives, which build upon the CAPM, serve to add a level of complexity to the analysis. It remains the case that the majority of international regulators currently base their decisions primarily on the CAPM framework. (see Table 1).

Table 1
Models used by Regulators to Estimate the Cost of Equity

	Australia	Germany	New Zealand	USA	Canada	UK
Regulator	AER	FNA	CC	NYSPUC	OEB	Ofgem
Primary model	CAPM	CAPM/RPM	CAPM	DDM	RPM	CAPM
Secondary model				CAPM		
Other use of DDM	Cross-check on MRP		Cross-check on MRP		Cross-check on MRP	Cross-check*

Source: Sudarsanam, Kaltenbronn and Park (2011)

Notes: * - on the overall cost of equity but not for individual firms, RPM = Risk Premium Model, DDM = Dividend Discount Model.

The consultants raise concerns with the ability of the CAPM to provide an adequate characterisation of the relationship between risk and return. Their concerns are largely driven by the ability of modern multifactor asset pricing models to provide a more adequate explanation of the cross section of realised average returns. It is important to recognise that the cross section of average returns is only one dimension of interest when modelling the risk-return relationship. Further, recent work suggests that the evidence against the CAPM may not be as robust as previously thought. For example, Ray, Savin and Tiwari (2009) show that the statistical evidence for rejecting the CAPM is weaker than previously thought when more appropriate statistical tests are used. More importantly, Da, Guo and

Jagannathan (2012) argue that the empirical evidence against the capital asset pricing model (CAPM) based on stock returns does not invalidate its use for estimating the cost of capital for projects in making capital budgeting decisions. Their argument is that stocks are backed not only by projects in place, but also by the options to modify current projects and even undertake new ones. Consequently, the expected returns on equity need not satisfy the CAPM even when expected returns of projects do. Thus, their findings justify the continued use of the CAPM irrespective as to one's interpretation of the empirical literature on asset pricing.

In terms of the consultants reports, SFG (2014a) states:

“79. McKenzie and Partington (2012) advise that:

the level of systematic risk for the firm comes down to a question of the extent to which the higher leverage *per se* offsets the lower business risk of the firm”⁴⁹

and we agree that this is the nub of the issue. That is, what is required is a way of determining how business risk (asset beta) and leverage (the proportion of debt financing) jointly determine the equity beta. SFG (2014a) continue:

“80. McKenzie and Partington (2012) go on to note that there are a number of ways of specifying the relationship between equity beta, asset beta, and leverage, but that all of them:

find in favour of an increasing equity beta as financial leverage increases.”

We note that this is somewhat selective quotation of the text of McKenzie and Partington (2012), that potentially serves to mislead the reader (the fact that they ‘*go on to note*’, but the actual quote comes from 3 pages earlier in the report serves to warn the reader that something is afoot). The latter quote is drawn from a section of our report where McKenzie and Partington (2012) summarises the ‘neo-traditional’ position that challenges the M&M framework and the full quote is:

“Regardless, this alternative view does still find in favour of an increasing equity beta as financial leverage increases.” (p. 10)

McKenzie and Partington (2012) note that assuming the beta of debt is zero (an incorrect assumption), the equity (β_E) and asset beta (β_A) are theoretically linked according to the following equation:

$$\beta_E = \beta_A \left(1 + \frac{D}{E} \right)$$

where D (E) is the market value of debt (equity). Thus, this basic formula shows that as the financial leverage of the firm increases, so must the equity beta. McKenzie and Partington (2012) note however, that things are more complicated than this simple formula suggests given that:

“The possibility of a non-zero debt beta can make quite a difference to the analysis of de-leveraging and re-leveraging beta.” (p. 11)

and

“A further problem with the de-levering re-levering process is that it gets more complicated when we introduce tax. ... While this discussion certainly has implications for the estimates of beta, it is unclear exactly what conclusions we should draw. The end result of any analysis will

rely on a host of factors: which theory of capital structure you assume, whether you assume the firm targets particular levels of debt in absolute terms, or in terms of a leverage ratio, how frequently you assume the firm rebalances to the target debt level and what adjustments you assume for the effects of imputation. In short, there are so many twists and turns that the de-leveraging and re-levering exercise can take you to a range of different destinations depending on what you assume.” (p. 11)

This is why the basic unadjusted industry betas of Damodoran are presented in Appendix 2 of McKenzie and Partington (2012) as a simple example of evidence from a well-known dataset that serves to highlight the basic point – utility betas are likely to be amongst the lowest of all industries. A recent publication by Huy (2013) provides further evidence in support of this point.

For the reasons stated above, the SFG (2014a) relevering exercise is of little merit and the results nonsensical - surely SFG are not suggesting that protected and regulated utility companies, with inelastic demand functions caused by a lack of substitutes and prices set by regulation, have a higher level of risk than the market? It must also be said that we do not understand how they generate their re-levered beta estimates in paragraph 89. By our calculations, for example, Electric Utility West relevers to 1.01 and not 1.33 as stated.

In fact, McKenzie and Partington (2012) are clearly agnostic on the extent to which extra leverage increases the equity beta and state that:

“...while the assumption of 60% debt financing for a regulated network distribution or transmission business is (approximately) twice that of the average firm, it is unclear what impact this extra debt will have on the benchmark firm’s financial risk...” (p. 7)

McKenzie and Partington (2012, p. 15) undertake a conceptual analysis and argue that there is:

“...evidence to suggest that the theoretical beta of the benchmark firm is very low. While it is difficult to provide a point estimate of beta, based on these considerations, it is hard to think of an industry that is more insulated from the business cycle due to inelastic demand and a fixed component to their pricing structure. In this case, one would expect the beta to be among the lowest possible and this conclusion would apply equally irrespective as to whether the benchmark firm is a regulated energy network or a regulated gas transmission pipeline.”

We note the work of Simshauser and Catt (2012, p. 82) who also comment on the stability of the revenue streams of network utilities, describing them as operating in an:

“... extremely favourable regulatory environment, which is characterised by virtually bullet-proof revenue streams.”

Thus, while a theoretical trade off does exist between business and leverage risk, as stated in McKenzie and Partington (2012, p. 15):

“To the extent that the firm is able to pass on the borrowing costs, the likelihood of bankruptcy as the leverage of the firm increases is low...”

Further to this point, they reiterate the argument made by the AER (2009, p. 253) that:

“... a regulated utility can pass through much higher borrowing costs through higher prices and not expect its profitability to diminish. In contrast, if a business in a competitive market was

faced with much higher borrowing costs it would likely have to wear some of those higher cost (as attempting to pass those costs through via higher prices may lead to lower profitability caused by a loss of market share or consumers substituting away from the product or service)."

Thus, McKenzie and Partington's (2012, p. 14) conceptual discussion leads them to conclude that it is the intrinsic risk of the firm which is the primary, if not sole, driver of its systematic risk.

The importance of intrinsic risk in estimating beta is not just limited to regulated utilities. McKenzie and Partington (2012) references a number of articles to support this view for the *general market*, including Schlueter and Sievers (now 2014), but also Chung (1989), Mensah (1992) and Griffin and Dugan (2003). The fact that Schlueter and Sievers (2014) is based on accounting data is irrelevant (SFG 2014a, p. 19-20). In fact, Schlueter and Sievers (2014) motivate their paper by drawing on the general literature which acknowledges our point:

"Although prior literature acknowledges the impact of financial- and operating leverage on market beta (e.g., Mandelker and Rhee 1984; Gahlon and Gentry 1982; Hill and Stone 1980; John et al. 1994), research meanwhile finds that intrinsic business risk (i.e., the demand volatility of a firm's output due to macroeconomic conditions) is the main component of market beta (e.g., Griffin and Dugan 2003; Mensah 1992; Chung 1989). Based on the same theoretical framework these three studies independently suggest different business risk proxies and find that their supposed measure is the main determinant of market beta." (p. 536)

SFG (2014a, p. 20) are incorrect when they claim the evidence of Schlueter and Sievers (2014) does not apply to utilities based on the evidence of Table 1 – this is a table of summary statistics. The actual analysis of the determinants of market beta is presented in the following section and is a cross-sectional study across all industries. In the robustness tests, detailed in Section 5, Schlueter and Sievers (2014) attempt to provide individual industry insights and they report:

"... prior research shows that cost of equity estimates differ across industries (Fama and French 1992), leading to terms of industry costs of equity (Easton and Monahan 2005 and Gong et al. 2006 for a careful analysis of the transportation industry). Consequently, we re-estimate the above specifications, but we additionally include industry indicator variables in all of our regressions. All our results (unreported, but available upon request) are again confirmed."

The results they are referring to are best summarised in the abstract:

"... we find that growth risk (i.e., the risk of firm sales variations that are inconsistent with the market wide trends), is the business risk that explains cross-sectional variations in market beta best."

Thus, SFG's (2014a) claim of problems with McKenzie and Partington's (2012) approach are not valid. The first 'problem' (para 86. a) SFG allude to doesn't make sense. Despite the fact that we provide a clear conceptual analysis of the logic behind our beliefs (summarised above), SFG claims our analysis is solely empirical. It is true that we do cite *a number* of empirical articles, including Schlueter and Sievers (2014), and evidence from the well-known dataset of Damodoran. As discussed above, the interpretation of Schlueter and Sievers (2014) is clearly wrong and the SFG re-levering exercise is also wrong (thus, dismissing the second and third stated problems mentioned by SFG).

Questions

Given the discussion above, we now turn to consider the application of the Foundation Model

approach and the specific questions that have been posed.

- a) The Foundation Model approach if applied now (as set out in the Guideline, but with updated market information) would be expected to lead to a RoE that when combined with our allowed return on debt (assuming this is set at the true cost of debt of our benchmark efficient entity) would lead to an allowed rate of return (calculated for each year over the regulatory period as a weighted average of the return on equity for the regulatory control period in which that regulatory year occurs and the return on debt for that year) that is commensurate with the efficient financing costs of a benchmark efficient entity with a similar degree of risk as that which applies to a regulated Australian gas or electricity, transmission or distribution business.

We interpret this question as asking whether, in the context of the vanilla WACC formula [ie. $WACC = r_d (D/V) + r_e (E/V)$], if you use S-L CAPM to get r_e and use the true r_d , then would you get a WACC that is expected to be commensurate with the efficient financing costs of the benchmark firm? In which case, the answer to this question is yes. The Foundation Model approach if applied now would be expected to lead to a RoE that when combined with the allowed return on debt would lead to an allowed rate of return that is commensurate with the efficient financing costs of a benchmark efficient entity with a similar degree of risk as that which applies to a regulated Australian gas or electricity, transmission or distribution business.

- b) The Foundation Model approach if applied now (as set out in the Guideline, but with updated market information) would be expected to result in a reasonable estimate of the return on equity for a benchmark efficient entity with a similar degree of risk as that which applies to a regulated Australian gas or electricity transmission or distribution business.

We interpret this question as asking whether the S-L CAPM gives an estimate of the return on equity, r_e , that is 'reasonable' in this context. The answer to this is again yes, the Foundation Model approach if applied now would be expected to result in a reasonable estimate of the return on equity for a benchmark efficient entity with a similar degree of risk as that which applies to a regulated Australian gas or electricity transmission or distribution business.

The objections raised by the consultants concerning the ability of the CAPM to explain the cross section of average returns can be dismissed as the cross section is only one dimension along which you measure the success of the model. Further, recent work suggests this cross sectional evidence may not be as compelling as previously thought. See our discussion above and below for further details.

- c) The Foundation Model approach if applied now (as set out in the Guideline, but with updated market information) would be expected to result in a return on equity estimate that is systematically downwardly biased relative to the true unobservable cost of equity capital of a benchmark efficient entity.

We are of the view that the foundation model does not provide a downwardly biased estimate in this context.

The theoretical justification for a downward bias has previously been considered in McKenzie and Partington (2012, p. 19-20) and they do not find in favour of this argument in this context. We also do not view the statistical justification (see SFG (2013a, p. 5), SFG (2014a, p. 10-12) for a discussion of the Vasicek adjustment) as valid in this context. For the latter, we note the work of Henry (2008), who finds no evidence that would support the use of the Vasicek model for Australian data. The results of

the Henry (2008) study:

“... suggest that there is little convincing evidence of regression to unity in this data. Therefore, it is difficult to justify the application of the Blume or Vasicek adjustments.” (p. 12)

- d) The addition of direct cost of equity estimates from the other models/sources as proposed by each of the NSPs (FFM, Black CAPM, DGM, expert valuation reports etc) would be expected to result in a materially better estimate of the RoE relative to the RoE estimates coming from the application of the Foundation Model approach as set out in the Guideline (if applied now with updated market information).

In our view, the answer to this question is unclear. To the extent that these alternative estimates are well founded, unbiased and appropriately combined, then we would say that such models might be useful in triangulating the cost of equity. However, as subsequent discussion will make clear we have significant reservations about the implementation of such models as proposed by network service providers. We do however, maintain the view that triangulation across a range of sources can be useful and we have previously made this point in the context of the market risk premium (see McKenzie and Partington, 2011). When considering alternative estimates however, it is important to recognise that there is the possibility of introducing extraneous noise, via the use of multiple estimates, given the potential for multiple sources of estimation error, which may not necessarily average out to zero. For some of the pitfalls in these alternative approaches see the discussion in sections A2 to A5.

Part A2: Review the decision of the AER to not provide any weight to the application of the Fama French three factor model (FFM) when estimating the allowed return on equity

Ross (1976) introduces the Arbitrage Pricing Theory (APT) as an alternative to the CAPM. The central prediction of the CAPM is that the returns to risky securities are driven by a single common factor. In essence, under the CAPM all returns are linearly related to the rate of return on the market portfolio. By way of contrast, the APT predicts that the return to any risky security is linearly related to a set of k factors. The APT is commonly represented as:

$$R_i = E(R_i) + b_{i1}F_1 + \dots + b_{ik}F_k + \varepsilon_i \quad (1)$$

Here R_i is the risky rate of return on the i^{th} asset with expectation $E(R_i)$, b_{ik} is the sensitivity of the return on the i^{th} asset to the k^{th} factor, F_k is the k^{th} factor return, and ε_i represents the mean zero asset specific risk term for asset i . Central to the APT is the notion that a portfolio that bears no risk and costs nothing to establish should pay a zero return in equilibrium.

In order to operationalise the APT, the number of assets, n , must exceed the number of factors, k . Furthermore, the asset specific noise term should be purely random, that is, ε_i must be uncorrelated with all factors and all other asset specific noise terms. Given a set of orthogonality conditions, Ross shows that the APT in (1) may be written in terms of factor risk premia as:

$$R_i = \lambda_0 + b_{i1}\lambda_1 + \dots + b_{ik}\lambda_k \quad (2)$$

Rewriting the APT in excess returns form with $\lambda_0 = r_f$ allows a very specific interpretation to be placed on the coefficients. In this form the APT may be represented as

$$R_i - r_f = b_{i1}[X_1 - r_f] + \dots + b_{ik}[X_k - r_f] \quad (3)$$

Equation (3) predicts that the excess return on the i^{th} asset over the risk-free rate is linearly related to the k risk premia associated with the k common factors. In (3), X_k is the return on a portfolio with unit sensitivity to the k^{th} factor, F_k , and zero sensitivity to the remaining $k-1$ factors.

In the case where the market portfolio is the only common factor, and returns are assumed to be normally distributed, then the CAPM and the APT converge. Similarly, the Black CAPM corresponds to a two-factor model, where the two factors are the return to the market portfolio and the return to the zero beta portfolio. It is important to note, that unlike the CAPM, the APT is agnostic about the factors that drive returns and a careful factor analysis is necessary to identify the underlying factors. This is an important point as the S-L and Black CAPM are both very specific about what factors are priced. The APT, however, is not and this has opened a veritable Pandora's box in terms of researchers claiming that their empirically significant factor is an unidentified F_k .

Fama and French (1993) present a three factor model of asset returns. Their model incorporates the predictions of the CAPM by including the return on the market portfolio as a factor. Fama and French (1993) also include two further variables that had been found to be statistically significant in explaining the cross section of average returns - firm size, as measured by market capitalisation, and the ratio of

the book value of equity to the market value of equity.

Since the publication of Fama and French's (1993) paper, an amazing array of models have been proposed each of which introduce further factors to the model. For example, Carhart (1997) introduces a factor designed to capture momentum in returns, in what is commonly referred to as the Carhart four factor model. More recently, Foye et al (2013) replaces the market value of equity factor with a proxy for accounting manipulation. Even Fama and French (2014) themselves have moved on from the three factor model. Extending Hou, Xue and Zhang, (2012), Fama and French develop a five factor model that forms portfolios based on firm size, the ratio of book to market value, profitability and investment. We do note that they motivate this model by linking it back to the dividend discount model. Fama and French (2014) conclude that their new model provides better descriptions of average returns than their three-factor model. They also find that market to book factor is no longer "priced" when included in the five factor model, although they suggest that this may be sample specific.

Following the work of Roll and Ross (1980), Chen (1983), Chen, Roll and Ross (1986), Burmeister, and Wall (1986), Burmeister and McElroy (1988) and McElroy and Burmeister (1988) *inter alia*, an alternative strand of the literature explains equilibrium returns using macroeconomic factors. These include factors such as unanticipated shock to industrial production or inflation, movements in the default premium or shifts to the slope of the term structure of interest rates. We note that there is no real overlap between the factors used in this literature and those used in Fama and French (1993, 2014 *inter alia*) type studies.

The discussion of the previous two paragraphs highlights the more general point that the sheer size of the set of potential factors is daunting. By way of example, Subrahmanyam (2010) documents over 50 variables that have been used to predict stock returns and comments:

"The research at this point presents a rather unsatisfying picture of a morass of variables, and an inability of us finance researchers to understand which effects are robust and which do not survive simple variations in methodology and use of alternative controls." (p. 35)

"As a central theme, I maintain that our learning about the cross-section is hampered when so many predictive variables accumulate without any understanding of the correlation structure between the variables, and our collective inability or unwillingness to adequately control for a comprehensive set of variables." (p. 28)

Green et al (2014) document over 330 predictive return signals and concludes that,

"(g)iven the large number of R(eturn) P(redictive) S(ignals) that have already been reported in the literature and the high degree of multidimensionality we empirically find to be present in returns, we propose that an important avenue for future research is to understand why returns are so highly dimensional, and why the most important multidimensioned RPS are priced the way they are." (p. 26)

Green et al (2014) find that 24 of 100 readily programmed signals are multidimensionally priced (ie. the mean coefficient estimates produced t-statics in excess of 3). The authors suggest that increasing the dimensionality of the cross-section is important as the size and book-to-market factors are not the most statistically significant predictive signals. This is an interesting point in the current context as

recall from our earlier discussion that in order to operationalise the APT, the number of assets, n , must exceed the number of factors, k . Given that we have so few assets in the Australian context, this presents a serious problem for operationalising a model with many factors.

Harvey et al (2014) present a useful survey of the literature seeking to explain asset returns. Their work omits papers that focus on small groups of stocks or employ data that is collected over short periods of time, finding 313 papers suggesting a total of 316 factors. It is important to note that Harvey et al (2014) are quick to acknowledge that this list of factors is not exhaustive. Most importantly, Harvey et al (2014, p. 35) state that,

“... it is a serious mistake to use the usual statistical significance cutoffs (e.g., a t-ratio exceeding 2.0) in asset pricing tests. Given the plethora of factors and the inevitable data mining, many of the historically discovered factors would be deemed “significant” by chance.”

Harvey et al (2014) argue for t-statistics in excess of 3 and even then they argue that this may still be too low. It is interesting to note that they argue for allowing theoretically inspired factors to have a lower rate, although it should still be above 2.0.

Recall that the Fama and French (1993) model was empirically motivated. As SFG (2014b, p. 27) point out, ex-post the *BTM* and *HML* factors have been interpreted in an APT and ICAPM framework. We note that the latest work by Fama and French (2014), motivates their work using the dividend discount model. Irrespective, of the rationalisation of the three factor model, the point remains that it was initially an empirical model. While we agree with SFG (2014b, p. 28) that this does not mean “we should ignore the empirical evidence”, the work of Harvey et al (2014) suggests that the empirical foundation is an important consideration when it comes to interpreting empirical results and that a higher standard of evidence should apply when assessing the statistical significance of empirically based asset pricing models

Harvey et al’s (2014) paper contributes to the literature on biases and inefficiencies in cross sectional regression studies. On a similar theme, Lewellen, Nagel and Shanken (2010) raise empirical concerns about the method and the way the results are interpreted. They observe that,

“... one gets the uneasy feeling that it seems a bit too easy to explain the size and B/M effects”
(p. 175)

Progress is being made in this area and Balduzzi and Robotti (2008), for example, argue for an alternative testing approach to the standard cross sectional regression approach. On a similar theme, Daniel and Titman (2012) constructs more powerful tests and rejects some recently proposed factor models.

Harvey et al (2014) also suggest that we may need to change the way in which we think about factors as being important. They note that there are currently two possible options – one is to introduce additional testable assumptions that a systematic risk factor has to satisfy before it can claim to be significant. For example, Pukthuanthong and Roll (2014) propose a seven-stage protocol for identifying and measuring important factors. The other possibility is provided by Harvey and Liu (2014) who argue that an evaluation of the economic contribution of a risk factor should be used to determine its importance.

Questions

Given the discussion above, we now turn to review the role of the Fama and French model (FFM) in the context of the specific questions that were posed.

- a) The FFM can be used to reliably estimate the required return on equity for a benchmark regulated network service provider, or estimates from the model are likely to be empirically stable.

We do not view the FFM as having the ability to reliably estimate the required return on equity for a benchmark regulated network service provider. There are many unresolved issues with respect to such multi-factor models and even Fama and French are moving away from the three factor model. The evidence suggests that the estimates for Australia using the Fama and French approach are unstable and depend on both the cross section of firms selected and the sample period chosen.

Our view of the empirical evidence on the FFM is that it is unlikely to produce estimates that are empirically stable. To that end, we disagree with SFG (2014b, p. 24) who states:

“In our view, the fact that a range of studies of variable quality produce a range of estimates should not be used as the basis for the outright rejection of the entire model. Otherwise, relevant and informative models (that would otherwise have been used to inform the estimate of the required return on equity) could be rejected due to the publication of one or more low quality studies with divergent results. A better approach is to consider the robustness and the reliability of the best available estimates of each model.”

What are the objective criteria for low quality studies? Surely, SFG are not suggesting that empirical studies coming from academic colleagues such as Robert Faff, one of Australia’s top finance professors, is a low quality study (Eg: Faff (2004)) just because it produces estimates that do not support the consultant’s view. We simply view the evidence of parameter instability from the empirical literature as symptomatic of the weakness of the model.

Further to this point, the main discussion of this section of our report highlights the nascent literature suggesting that the use of the Fama and French model is under question, and may indeed lead to invalid, incorrect or misleading inference. Even the originators of this model, Fama and French (2014) themselves, have contributed to this literature. It would seem unusual to adopt a model 21 years after its publication, when its weaknesses are becoming more evident and contemporary research is just beginning to understand the possible causes and potential solutions.

- b) The use of the FFM, either alone, or in combination with other asset pricing models, would be expected to result in a materially better allowed return on equity estimate for a benchmark regulated network service provider than the use of the estimates from applying the Foundation Model approach as set out in the Rate of Return Guideline.

We would view the answer to this question as unclear given the state of the literature and the issues that are yet to be resolved (see the main text of this section of our report for a full discussion of these issues). It is also the case that the outcome would depend critically on how this additional information is incorporated into the estimate of the return on equity. However, given the issues in the current literature and in particular the problems in Australian implementation of the Fama and French three factor model, it is clear that the use of this model alone would not result in a better estimate of the

return on equity.

- c) The FFM should be used for estimating the return on equity of a benchmark regulated network service provider either alone or in combination with other the other models proposed by the NSPs.

Given the uncertainties that surround the use of this model, as highlighted in the literature, the answer at this time is undoubtedly no.

Part A3: Review of the decision of the AER to only consider the theory underpinning the Black CAPM to adjust the equity beta estimate in the Foundation Model approach.

The CAPM tells us that where an investor's aim is to maximise their expected utility, which depends only on the mean return and variance of end-of-period wealth, then the optimal investment choice is an efficient portfolio and this choice will be made irrespective of the investor's attitudes towards risk. The CAPM is commonly represented in algebraic form as:

$$E(R_i) = r_f + [E(R_m) - r_f] \beta_i \quad (1)$$

Here $E(R_i)$ represents the expected return on the i^{th} asset, r_f is the return paid on the riskless security, $E(R_m)$ is the expected return to the market portfolio (which is a portfolio made up of all assets held in proportion to their value weights) and β_i measures the riskiness of asset i . This measure of risk is the covariance of the return on the i^{th} asset with the return on the market portfolio, scaled by the variance of return on the market portfolio. The CAPM is a single factor model, where the factor driving returns is the covariance of the return on the i^{th} asset with the return on the market portfolio. Rearranging (1) reveals:

$$E(R_i) = (1 - \beta_i)r_f + \beta_i E(R_m) \quad (2)$$

This demonstrates that, under the CAPM, the expected return to any asset i can be written as a linear combination of the expected return on the market portfolio and the known return on the risk-free asset.

It is a normal part of research to ask whether the conclusions of a model are robust to changes in the assumptions. The CAPM is no different and one of the first questions considered was whether the CAPM remains valid in the absence of the risk-free asset.

Black (1972) examined what occurred when the investor is unable to borrow or lend unlimited amounts at the risk free rate, r_f , as is assumed in deriving (1) above. The Black approach relies on our ability to identify all portfolios that have zero covariance with the market portfolio and also to select from this set of orthogonal portfolios the one with minimum variance. For example, in Figure A3.1, the market portfolio M is shown to be efficient. Portfolios A and B are both orthogonal (ie. have zero covariance) to M and offer the same expected return, $E(R_z)$. Portfolio B lies on the boundary of the investment opportunity set and is thus the minimum variance uncorrelated portfolio. In practice, choosing B is an exercise in quadratic programming. While this example refers to the market portfolio, it is important to recognise that that Black approach applies to any efficient portfolio.

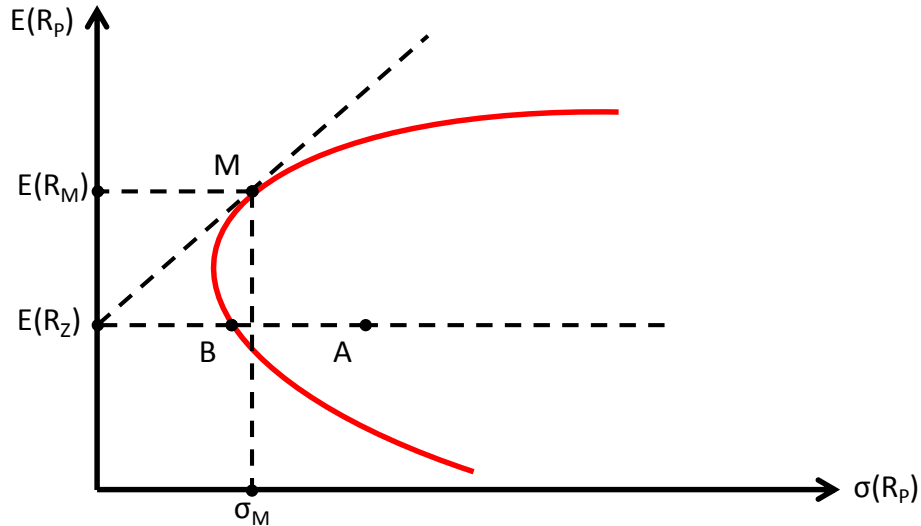


Figure A3.1: The market portfolio M and two orthogonal portfolios A and B

Black (1972) shows that the required rate of return on the i^{th} asset, in the absence of the ability to borrow and lend unlimited amounts at the risk free rate, is given by

$$E(R_i) = (1 - \beta_i)E(R_z) + \beta_i E(R_m) \quad (3)$$

Thus, Equation (3) describes the expected return to any asset i as a linear combination of the expected return on two assets - the market portfolio and the unique zero beta portfolio with minimum variance. As with the CAPM, the weight invested in the market portfolio is β_i . Rearranging (3) yields the Black CAPM, also referred to as the Zero-Beta CAPM:

$$E(R_i) = E(R_z) + [E(R_m) - E(R_z)]\beta_i \quad (4)$$

The definition of β_i is identical in (3) and (4).

SFG (2014e, p. 12) argue that the Black CAPM is based on a,

“... more realistic assumption that investors would have to pay a premium above the risk-free rate when borrowing.”

This comment seems to suggest that there is some confusion among the consultants between the Black and the Brennan model. We note that the model of Brennan (1971) is not the Black CAPM. Rather, the Brennan model imposes a differential between borrowing and lending rates in an economy where a *risk free security exists*. Black (1972) questions the assumption that an investor may take unlimited long or short positions in any security, including the risk free security, which underlies the CAPM. Black (1972) considers two separate scenarios one in which there is *no risk free security* and another where the investor is precluded from holding short positions in the *risk free security*. In the absence of the riskless asset, there is a role for the zero beta portfolio, and the expected return on any asset is a linear function of the β of the asset. In the second scenario the resulting market equilibrium is more complex, but equilibrium asset returns again depend linearly on the β of the asset. The question that

arises is why NERA (2012, p. 4) and NERA (2013b, p. 6) appear to be treating the Brennan and Black models as substitutes? The implication of the Black model under either of his two scenarios is that borrowing cost are higher when there are restrictions on trading the riskless asset. This differs from the proposition “that investors would have to pay a premium above the risk-free rate when borrowing” as in the scenario where there is no risk-free security, such a statement is meaningless. Only under the Brennan (1971) model is the proposition that restrictions on trading in the riskless security result in the investor having to pay a premium above the risk-free rate when borrowing.

Returning to the arguments of SFG, they then state:

“The Black CAPM requires that investors can short sell. While in reality investors do not have unlimited ability to sell short, short-selling is a feature of the equity market. The more realistic assumptions of the Black CAPM are a potential reason why this model provides a better fit to the data.” (p. 12)

To understand why short sales are important, recall that in practice, asset returns are predominantly positively correlated with each other. Thus, in order to construct an uncorrelated portfolio, one would need to establish a portfolio containing both long and short positions in the risky assets. This portfolio made up of long and short positions and paying $E(R_Z)$, is orthogonal to the market portfolio, that is, there is no systematic relationship between movements in the market and the zero beta portfolio.

In theory, theory and practice are the same. In practice, however, theory and practice are different. It is important to understand that the conditions under which investors can short sell in the real world are very different to the conditions assumed in the Black model. As SFG point out, investors in the real world do not have an unlimited ability to short sell. The differences go far beyond that however, and short selling is actually a very risky and expensive exercise. In order to short sell, an investor must typically borrow the stock and most stock loan agreements require the investor to post in excess of 100% of the value of the loan in cash or equivalent, they must pay a fee for lending the stock (termed the rebate rate), loans are typically on 24-hour recall, investors face the constant risk of a short squeeze, etc.. For details on the process of stock lending for short selling see Faulkner (2002) and for academic research on the costs and impact of short selling see Henry and McKenzie (2006), McKenzie, (2012), Berkman and McKenzie (2012), McKenzie and Henry (2012) Jain, Jain, McInish and McKenzie (2013).

Thus, it is incorrect to suggest that the Black model is based on more realistic assumptions. The Black model simply relaxes one of the underlying assumptions of the S-L CAPM. Of course, debates over the realism of assumptions are secondary to the far more important issue, which is the claim by SFG that the Black model better explains the data. This is unsupported as, unless one accounts very carefully for the increased transactions costs and risks associated with holding long-term short positions, comparing estimates of β_i and/or $E(R_i)$ across (1) and (4) is unlikely to be a useful exercise at best, and may lead to unwarranted conclusions at worst.

On this point, Ross (1977, p. 183) says:

“To mention one difficult case of particular importance, if there are collateral restrictions on short sales ... the CAPM will have to be analyzed anew.”

Ross (1977) shows that in the presence of short sales restrictions, the absence of a risk-free asset

actually invalidates the linear CAPM. We refer to interested reader to Ross (1977) for further details.

To be clear on this point, empirical results for the Black and S-L CAPM are not directly comparable as they each involve very different investment strategies. In the S-L CAPM, the investor may hold the risk free asset. In the Black CAPM however, the investor may hold the zero beta portfolio, which consists of long and short positions. It is entirely reasonable to expect that these two strategies will have different payoffs, given their different risks and costs.

The fact that the S-L CAPM produces a relationship between beta and average return that is too flat (as exemplified in Figures 2, 5 and 6 in SFG, 2014e), cannot be interpreted as evidence in support of the Black CAPM, or indeed as evidence against the S-L CAPM. It does remain an outstanding issue as to why these empirical predictions differ to the theoretical predictions of the CAPM. As noted earlier, Ray, Savin and Tiwari (2009) shows that the statistical evidence for rejecting the CAPM is weaker than previously thought when more appropriate statistical tests are used.

It has been argued that empirical evidence, such as that presented in Figures 2, 5 and 6 of SFG (2014e), justifies an adjustment to the S-L CAPM beta. While we do not think such an adjustment is appropriate, if one were to consider making an adjustment, it is not clear what adjustment you should make to the CAPM return estimate. The problem is that the difference varies between studies, between sample periods (even with addition of relatively few data points) and is potentially sensitive to the choice of market index or proxy for the risk-free rate. In fact, it is entirely possible that the error bounds around these point estimates may mean that they are actually not significantly different from the CAPM securities market line.

McKenzie and Partington (2012b) present a numerical example, which was designed to highlight the sensitivity of the Black model to the choice of proxy for the market portfolio. NERA (2013b) go to great lengths to critically examine each aspect of the example. We find this approach somewhat unnecessary since we clearly state at the beginning of our analysis,

“... that the lessons we draw from our analysis do not depend upon the specific values we have chosen for the data.” (p. 11)

For example, NERA (2013b, p. 31) argue that:

“Although there are a number of different value-weighted indices of Australian stocks, their composition does not vary greatly. As a result, an estimate of the beta of a security will not in general be sensitive to the choice of an index and, consequently, an estimate of the zero-beta rate will also not be sensitive to the choice of an index. Thus this issue that McKenzie and Partington (2012) raise is also of no practical significance.”

This is contradicted by, Roll’s (1977, p. 130) fifth conclusion which states that with reference to the market portfolio,

“ ... most reasonable proxies will be very highly correlated with each other and with the true market whether or not they are mean-variance efficient. This high correlation will make it seem that the exact composition is unimportant, whereas it can cause quite different inferences.”

More generally, the example provided in McKenzie and Partington (2012b) is an illustration of Roll’s

work and to be clear, Roll's work does not rely on a specific set of numbers or circumstances. Our point that 'what you get depends very heavily on what you do' is well illustrated by the SFG estimate of the zero beta premium, which is quite different to the NERA estimate (see SFG, 2014e, and NERA, 2012, 2013b).

We do acknowledge the error in our labelling of the figures in McKenzie and Partington (2012b). As NERA correctly point out, Figure 1 corresponds to portfolio 2 and Figure 2 corresponds to portfolio 1. This does not, however, serve to alter any of the conclusions. That is to say the discussion of the sensitivity of the intercept to the portfolios remains valid. Further, the comparison of efficient portfolio 2 with an intercept of -0.85% (presented in the incorrectly labelled 'Figure 1') and the inefficient portfolios (presented in Figures 3 and 4) still shows that we move from a negative intercept to a positive intercept.

Questions

Given the discussion above, we now turn to review the role of the Black model in the context of the specific questions that were posed.

- a) It is reasonable to limit the use of the Black CAPM to having regard to the theory underpinning the Black CAPM in choosing the equity beta.

The answer to this question is a qualified yes - the theory of the Black CAPM may have a role to play in choosing the equity beta, although exactly how is still not clear to us. We do note that it is important that the discussion is actually referring to the Black CAPM and not other variants of this model, such as the Brennan (1971) model. Furthermore, it is important to recognise the additional risks and costs inherent in applying the Black CAPM. Given the practical difficulties in implementing the Black CAPM we would not recommend the use of the current estimates from the network service providers to inform any beta adjustment.

- b) The theory underpinning the Black CAPM necessarily supports an uplift to the estimate of beta to be used in the S-L CAPM for low beta stocks.

In our view, the theory underpinning the Black CAPM does not necessarily support an uplift to the estimate of beta to be used in the S-L CAPM for low beta stocks. We refer the reader to the main body of text above for details as to why this is so.

- c) The empirical use of the Black CAPM, either alone, or in combination with other asset pricing models, would be expected to result in materially better allowed return on equity estimates for a benchmark regulated network service provider than the use of the estimates from applying the Foundation Model approach as set out in the Rate of Return Guidelines.

We view the answer to this question as unclear given the state of the literature and the issues that are yet to be resolved. It is also the case that the outcome would depend critically on how this additional information is incorporated into the estimate of the return on equity. However, given the practical problems in implementation, we would not recommend using the Black CAPM alone.

- d) The Black CAPM should be used for estimating the return on equity of a benchmark efficient entity in combination with other models proposed by the NSPs.

The Black CAPM might in principle be used for estimating the return on equity of a benchmark efficient entity in combination with other models proposed by the NSPs. The problem in practice is estimating the return on the zero beta portfolio. This can be very sensitive to the choices made in its estimation as our prior work and the estimates of the consultants demonstrate.

Part A4: Review of the decision of the AER to limit the use of the Dividend Growth Model (DGM) model to the estimation of the market risk premium to be used to inform the MRP to be used in the foundation model

Introduction

A model that expresses price as the discounted value of future dividends is given by,

$$P_0 = \sum_{t=1}^{\infty} \frac{E(D_t)}{(1 + r_e)^t}$$

where P_0 is the current price, $E(D_t)$ is the expected dividend at time t , and r_e is the cost of equity.

This model, and its many variants, are used to value equity and also, given an observed share price, to reverse engineer the cost of equity. For the latter, estimating the cost of equity boils down to finding the internal rate of return that equates the cash flows (dividends) to the current price. This approach to estimating the cost of equity assumes that market price is efficient and that the inputs to the model such as cash flow forecasts are correctly measured. Additionally, in versions of the model where a constant dividend growth estimate (g) is required and it is estimated as the product of the reinvestment rate and the return on equity arising from that reinvestment, it is also assumed that no equity will be subsequently issued and that the leverage ratio will be held constant. Meeting the assumptions when estimating the cost of equity is where the problems begin. For example, it is common to use analysts' estimates of earnings and dividends in such models and it is well understood that analyst's forecasts are upward biased. Consequently, estimates of the cost of equity from the discounted dividend model will also be upward biased.³

The discounted dividend model of prices can appear in many forms. One form is the Gordon growth model which the AER and several consultants refer to as the DGM. This model assumes constant growth in dividends. This assumption can be relaxed and the model can be extended to allow for different phases of growth. For example, as in the AER's two stage and three stage growth models. Such models allow for dividends and/or growth rates to vary before settling down to a constant growth rate in perpetuity. Differing assumptions could be made about the pattern in the transition from the current growth to the constant long term growth rate. A common, but not necessarily correct assumption is a steady reduction in growth bringing the current above average growth down to the long term growth rate. The H-model of Fuller and Hsia (1984) for example, allows for a smooth linear transition to the constant growth rate.

Alternatively, instead of assuming a constant growth rate, we can assume that at some future date the return on equity from the firm's investments is the same as the cost of equity, or equivalently that investments have a zero NPV. This is an attractive assumption because it describes the natural outcome of competition, where prices are driven to a level such that the NPV of investments in an industry is zero. It is also consistent with value maximising behaviour, such that firms should expand their investments to the point where the return on equity from the firm's investments is the same as the cost of equity. This could be seen to be what regulation is intended to achieve, which is to eliminate

³ We have previously made this point at length McKenzie and Partington (2013b*) and it does not seem to have been contentious so we will not belabour the point here.

economic rents and achieve an equilibrium in which investments are zero NPV. The assumption that the return on equity from the firm's investments is the same as the cost of equity, gives rise to the finite horizon discounted dividend model of Gordon and Gordon (1997). In order to implement this model, no long run growth forecast is required. Instead we require dividend forecasts to the date where investments become zero NPV and the earnings at that date. The model is given by,

$$P_0 = \sum_{t=1}^n \left(\frac{E(D_t)}{(1+r_e)^t} \right) + \frac{E(EPS_n)}{r_e(1+r_e)^n}$$

where $E(EPS_n)$ is the expected earnings at time $t=n$ and n is the date at which the expected return on equity is the same as the cost of equity.

Utilising accounting identities, it is possible to transform the dividend model to an accounting based model such as the residual income based model of Ohlson (1995), which has subsequently been extended in papers such as Gebhardt, Lee and Swaminathan (2001) and Gode and Mohanram (2003).

All of the foregoing dividend and accounting models can and have been used to estimate the cost of equity. So, there is plenty of choice and these choices are further extended by various assumptions about the long term growth rate. For example, the growth rate has been set to the inflation rate, the interest rate, the growth in GDP, and the growth in GDP less a reduction to allow for future capital raising. There have also been attempts to jointly estimate the cost of equity and the growth rate as in Easton (2006). SFG (2013f) have added another choice to the mix, jointly estimate the cost of equity, the return on equity investment and the dividend growth rate, utilising a relation between the dividend growth rate the return on equity and the reinvestment rate. Clearly this has not yet become the definitive choice. As an additional choice among many, we are unconvinced about the merits of the SFG model. A reasonable requirement, before adopting the SFG model as a preferred choice over well established models, would be substantial agreement on its superiority in the research literature and/or extensive use in practice.

While there are many methods to choose from, the most widely used method when backing out the cost of equity from a valuation model, is some variant of the DGM. Following the CAPM, the DGM is reported as the second most popular model used by regulators (see Sudarsanam, Kaltenbronn and Park, 2011). Indeed, before the CAPM rose in prominence, the DGM was arguably the most popular model for estimating the cost of equity. A key problem with the DGM has been its sensitivity to estimates of the long term growth rate. It also suffers from problems relating to the interaction between the dividend yield and the growth rate as we discuss below.

Financing Dividends

The fundamental source of value in a firm is the cash flow it generates from its operations and the fundamental source of value for equity is the share of the operating cash flow available for owners. We will call this the free cash flow to equity or FCFE. The equity of the firm can be valued as the present value of FCFE or as the present value of dividends and, of course, the two present values are constrained to be equal. However, the dividend in any particular period is not constrained to be the same as the FCFE for that period. In reality, dividends are a smoothed version of the FCFE.

Differences between the FCFE and the dividend in a particular period may arise as a consequence of financing transactions. For example it is possible to pay a dividend to existing shareholders that is

higher than the FCFE by either borrowing or by issuing new shares. The result, however, is that that dividends in the future will be smaller, or equivalently growth will be lower. This is because either future FCFE will be smaller because of a need to service debt, or FCFE will be unchanged but spread over more shares. Either way the future dividends per share go down.

Dividend reinvestment plans are a popular way of financing bigger dividends. When these occur, there is a continuing increase in the number of shares on issue and a continuing dilution in the growth rate. To illustrate this point consider an all equity firm that has an expected operating cash flows forever of \$1.1 million per year.⁴ There are one million shares on issue and they are worth \$11 million in total. Currently the expected FCFE is equal to the operating cash flow and the firm pays all the cash flow out as dividends. The company has just paid a dividend of \$1.10 and the expected growth in dividends is zero. The share price is \$11 (ex-dividend) and the dividend yield is 10%, which equals the cost of equity.

Now suppose it is discovered that as from next year the company will have to reinvest \$100,000 every year in order to sustain the operating cash flow. The FCFE is now expected to be \$1 million per year and the value of the company drops to \$10 million or \$10 per share. As a consequence the dividend yield rises to 11%, which overstates the cost of equity. The expected dividend growth rate is now negative because the sustainable dividend is now only \$1 per share. Next year the firm holds the dividend at \$1.10 and recoups \$100,000 for reinvestment via a dividend reinvestment plan. Ex-dividend the shares are worth \$9.90 and so the company must issue 10,101 shares to recoup the \$100,000 required. The \$1m FCFE is now spread over the original one million share plus the 10,101 shares newly issued, so the sustainable dividend goes down to \$0.99 per share. However, the dividend yield is now 11.11% ($\$1.1/\9.90). If the financing of the dividend by the dividend reinvestment plan is repeated year by year, the dividend yield continues to climb, the sustainable dividend continues to shrink and the growth rate becomes more negative.

Had we started our analysis with a positive growth rate, for example by allowing less than a 100% payout of FCFE, the same consequences would apply, except that initially the effect would be a reduction in the rate of growth rather than negative growth rates. The moral is that, in the presence of dividend reinvestment plans, the DGM can give upward biased estimates of the cost of equity unless the growth rate is appropriately adjusted.

Financing the dividend, therefore, has consequences for the transition to the long term growth rate and possibly for the long term growth rate as well. For example, as a consequence of financing dividends that are larger than the FCFE, the dividend growth rate may drop below the long term growth rate in the transition period and might even become negative before reversing and climbing back towards the long term rate. Thus, there might be a U or V shaped transition to the long term growth rate. The expected long term growth rate might also be reduced.

Negative growth rates are more than a remote theoretical possibility. In a study of UK water utilities, Armitage (2012) finds that the utilities have been financing dividends and incurring debt, with the consequence that dividend cuts will be inevitable. In the Australian context, Simshauser and Catt (2012)⁵ observe that:

⁴ We note the SFG (2014f) p30-33 present an example which they mislabel as a dividend reinvestment plan. In their example there is a share issue before the dividend at the cum-dividend price, whereas in a dividend reinvestment plan the share issue is after the dividend and is at the ex-dividend price.

⁵ It is salient that this is a paper prepared by utility industry insiders.

“The extent to which dividends were financed by the raising of ordinary equity capital in the utilities sector was material, but more striking is the more recent sectoral run-up in dividends...” (p. 16).

The run-up they refer to was a sharp rise beginning with dividends of under \$500 million in 2006 rising to about \$1.5 billion by 2008, a level that was maintained thereafter. Simshauser and Catt (2012) make the case that, given an impending need to fund investment, utilities should be cutting dividends. The more so as Simshauser and Catt show that relative to utilities in the EU and USA, Australian utilities have higher payout ratios. With respect to network utilities they make the following observation:

“Australian Network Utilities have the highest gearing, and highest dividends: gearing of Australian transmission and distribution network utilities is highest, averaging about 62.2%, and also, have very high dividend payout ratios averaging about 79%. This almost diametrically opposed financial position reflects the extremely favourable regulatory environment, which is characterised by virtually bullet-proof revenue streams. Of course, payout ratios are distinctly at odds with our investment megacycle thematic.” (p.18)

It is clear that where there is significant financing of dividends and/or where substantial investment demand for funds is anticipated, there is a considerable risk that dividend growth will slow or even turn negative for a period. Unless this is properly accounted for in the application of the dividend growth model an upward biased estimate of the cost of equity is likely. This is a significant risk at the level of the firm or industry and appears to be a particular risk for utilities. It may be less of a problem at the level of the market, but this is not guaranteed, particularly in times of crisis.

Sticky Dividends

Dividends are a smoothed version of both FCFE and profits. As profits go up and down, dividends follow slowly and indeed profits can change without any change being made in the dividend. Dividends are therefore said to be sticky. They are particularly sticky downwards because companies are particularly averse to cutting the dividend. Thus, profits and FCFE may drop and if this leads investors to revise their growth expectations downwards the share price may drop significantly, but the dividend is likely to be held unchanged. An unchanged dividend divided by a smaller share price results in a higher dividend yield. The outcome, unless appropriate downward adjustments are made to the growth parameter in the DGM, is that the combination of higher yield and overestimated growth gives an upward biased estimate of the cost of equity.

We suspect that the downward growth adjustment, if any, is likely to be insufficient. Firstly, if we rely on analyst’s forecasts for our growth estimates we know that their adjustment to the information already impounded into prices is sluggish (see inter alia Guay, Kothari and Shu, 2011). So, the change in the dividend yield is likely to lead any growth rate updates. In any case, we know that analysts’ forecasts are upward biased. Secondly, in our observation the long term growth rate is rarely changed, whereas in reducing the price investors may well have revised their expected long term growth rate down.

Of course there is the reverse effect when FCFE and profits rise, but the greater reluctance to cut dividends as opposed to increasing them is likely to create an asymmetry in the effects. Thus, the potential bias from dividend financing and the effect of dividend yield inflation are likely to be greater when profits and FCFE fall. This is likely to be a particular and market wide problem in times of crisis. During these times, we are likely to see a market wide drop in prices, profits and cash flow, but most

companies try and hold their dividends. This means that dividend yields will rise sharply and this is what we observed during the GFC. Some downward adjustment to growth rates is almost certainly required, but exactly how much is obscured by debates about how much of the price change is due to reduced estimates of growth and how much is due to an increase in the cost of equity. Thus, in times of sharply moving prices, the dividend growth model is at its most unreliable. When there has been a sharp rise in dividend yields resulting in historically high dividend yields, there is a particular risk that overestimates of the cost of equity will result and the reverse when there has been a sharp fall in dividend yields resulting in historically low dividend yields.

Analysts Forecasts and Target Prices

We observed earlier that analysts' forecasts of earnings and dividends are upward biased and so are their estimates of target prices. Consequently when estimating the cost of equity using analysts' forecasts for dividends and substituting target prices for market prices, there should be some offsetting of the biases. The result, as SFG (2014d), show is that the cost of equity estimates are lower using dividend forecast and target prices compared to combining analysts' dividend forecasts with actual prices.

Reducing bias is desirable, but the use of target prices is problematic. The assumption in using the DGM to estimate the cost of equity is that prices are efficient. Analysts' target prices are not efficient, they are upward biased. In order to justify the use of target prices, therefore, the argument must be that the target prices are efficient with respect to the analysts' dividend forecasts. If so combining target prices and dividend forecasts gives the cost of equity implicit in the analysts' forecasts assuming the DGM for prices. The result would be rather like an implied opinion survey of analysts. As we put it before, McKenzie and Partington (2013 b*) p.20:

“We ask analysts what they think dividends and prices are going to be and then infer what they think discount rates are. An alternative would be to ask then about discount rates directly.”

One problem with this approach to inferring the analysts' discount rates is that not all analysts use the DGM when forming target prices. Some would use a price earnings multiple applied to forecast earnings, some would use other methods.⁶ In a study of target prices, Parslow (2012) states that “I find that a large proportion of target price error is attributable to error in PE ratio forecasts.” (Thesis Abstract). The quote comes from an honours thesis, so we should not overweight this empirical evidence, but it is credible that analysts make errors in their PE ratio forecasts. If so, using PE ratios to estimate target prices mixes errors in the analysts' PE ratios with errors in their earnings forecast. Seen in this light, we are using an upward biased forecast of dividends to back out the implied cost of equity in a price given by a PE times the earnings forecast, where both the PE and the earnings forecast contain errors. Furthermore, we must make the assumption that if analysts had used the DGM, they would have got the same price as multiplying the PE by the earnings forecast. In our opinion, therefore, the use of target prices is not very convincing as a means of accurately estimating the cost of equity.

SFG (2014d) point to the lower volatility of cost of equity estimates using target prices, compared to using actual prices, as a desirable attribute. We agree that volatility can be an undesirable attribute for the cost of capital for regulatory purposes. However, we understood that a key argument for use of the DGM was that it had the potential to track changes in the cost of equity through time. If so, it might

⁶ Some financial analysts may use the present value of the FCFE, and some may discount net operating cash flow and then subtract the value of debt.

inform the regulator's decisions, without being the only determinant of the regulatory cost of equity. It seems a little inconsistent to argue for tracking ability on the one hand and, at the same time, claim the benefit of stability. Surely the appropriate criterion is not stability, but whether the estimate accurately tracks changes in the cost of equity. We note that based on our earlier discussion, we are not convinced that such tracking of changes in the cost of equity will be accurate.

In a comparison of two sets of data, there is always the question of whether the observed differences represent real differences or are of a size that could be attributed to the inherent variation in samples of data. The differences in standard deviation between the two estimates of the cost of equity seem to be small in absolute terms and relative to the mean. The question is whether null hypothesis of no significant difference can be rejected.

Time Matching Prices and Forecasts

Conceptually matching the date of observation of the price to the date at which the analyst made their dividend forecast has merit. The problem is that analysts' adjustment to the information in prices is sluggish. Consequently matching the date of the forecast and the date of the price is not the same thing as matching the information in the analysts' forecast with the information in the price. Matching of the information sets would require using lagged prices, but unfortunately it is not known what that lag should be and it could be variable across analysts and through time.

SFG (2014d) make the point that their matching of the individual analysts forecasts with the date of the price does not substantively affect the mean estimate of the cost of equity. They state:

“The analysis presented in this section demonstrates this technique does not lead to market cost of equity estimates that are, on average, any higher or lower than those resulting from consensus forecasts, including the cost of equity estimates compiled by the AER. This technique will, however, lead to lower variability of the market cost of equity estimates over time, which is a direct result of using more relevant information.” (p. 16)

So, empirically the case for matching rests on a lower estimate of volatility, with a standard deviation of 0.67% for the time matched data and 0.97% for the consensus forecasts. SFG (2014d, p.12) refer to this as a material reduction in volatility. The difference of 0.30% is a reduction in the volatility of 31% ($0.30/0.97$) but the effect is small relative to the mean cost of equity. Expressed as a percentage of the mean cost of equity the reduction in volatility is about a quarter of one percent ($0.3/11.5 = 0.26$). Is this effect really material and is it statistically significant? The range of the estimates, from maximum to minimum taken over 11.5 years of monthly estimates, is 2.45% for the time matched sample as against 3.63% for the consensus forecast sample. In our view, to treat the difference in range of 1.18% as being material attaches more precision to DGM estimates than is warranted.

In terms of tracking changes in the cost of equity, time matching would be expected to give better tracking, but this is contingent on correctly adjusting estimates of growth as the dividend yield changes. For the reasons that we have discussed earlier, we are not confident that such adjustments will be correct and, in particular, we expect the DGM to be at its most unreliable in times of crisis. Indeed, we would caution against relying on month by month, or even year by year, estimates from the DGM. Averaging measurement error over several periods is likely to reduce the error and therefore, we would recommend taking the mean over several years. In this way the DGM could be used to get a ball park - although likely upward biased figure - for the cost of equity. However, there would then be limited benefit in time matching since SFG's (2014d) results suggest that the means from the time matched, or consensus estimates are likely to be similar.

Two Stage versus Three Stage DGM

A three stage DGM is conceptually better than a two stage DGM as a period of transition seems more likely than a sharp disjunction with a sudden jump to the long term growth rate. Whether increased accuracy results from the three stage model in practice depends on whether the right pattern of transition is selected and whether the period of transition is appropriate. Clearly, if growth rates are expected to be negative during the transition phase, then assuming that they are positive and steadily declining to the long term rate is likely to give a worse result than the two stage model. The point is that the expected transition might not be a steady linear adjustment, but could for example, be U shaped or inverted U shaped, V shaped, or might involve exponential decay.

With regard to the length of the transition, we have previously argued for shorter transitions of three to five years based on business and equity market cycles (McKenzie and Partington, 2013b*). This is based on our view that changes in company cash flows follow the business cycle. As we pointed out earlier, dividends are a smoothed version of cash flows and this smoothing in times of cyclically depressed cash flows reduces future dividend growth. The reality of stock market cycles is that prices in the bear phase of the market go down for a reason: either the cost of equity rises, or expectations of future dividend growth go down and quite possibly both happen together. It is stretching credibility to argue that all bear markets are driven by a rising cost of equity. In our opinion, therefore, it is also stretching credibility to believe in a smooth one directional adjustment of expected dividends to the long term growth rate over a period significantly greater than business and market cycles. Does anyone really believe that over the eight year period 2006 to 2013 that it would have been appropriate to assume a smooth one directional adjustment of expected growth rates to a constant long term growth rate?

We agree with SFG (2014d, p. 7) who comment that:

“The business cycle data provides an indication of how long it would take for a boom economy or recession economy to revert to a normal growth state. It does not provide an indication of how long it would take for a high growth firm to revert to a normal growth firm.”

The objective, however, is not to estimate the growth rate for a specific high growth firm, but rather to estimate the market growth rate in order to get the market cost of equity. Not all firms are high growth firms - some are and they may take an extended period to revert to normal growth, but if the economy has returned to a normal growth state, it hardly seems appropriate to assume all firms will still be in the process of transition to normal growth and that this transition will continue for a significant number of years.

SFG (2014d)p.7 argue for longer transition periods on the grounds that the short term growth forecasts for listed firms are above the AER's 4.6% (nominal) long term growth rate over the entire period for which data is available. This could be interpreted as the AER's rate being too low, or as the analysts' forecast being upward biased, which plenty of research shows them to be. It also accords with the tendency we noted in McKenzie and Partington (2013b*), for the almost invariably optimistic assumption that whatever the current period happens to be, it is a period of dividend growth rates above the long run rate. While this is feasible for some periods, it is not possible for all periods. We also note that dividend growth rate comparisons are best done in real terms as they can otherwise be distorted by variation in rates of inflation.

We accept that there is considerable debate about what the long run growth rate should be and this

debate reflects the uncertainty over its estimation. This is another reason for not placing too great a reliance on cost of equity estimates from the DGM, irrespective of how the growth estimates are derived. Reflecting the debate about the dividend growth estimates, McKenzie and Partington (2013b*, p.15 - 16) listed a range of these estimates and took the average as 3.78%. SFG (2014d, p. 43) suggested that there was a transpositional error with a failure to adjust for inflation and corrected the estimate to 4.40%. However, SFG do say in footnote 65:

“If we have mis-interpreted the table we apologise”.

SFG have misinterpreted the table, as we point out that in McKenzie and Partington (2013b*) footnote 9:

“The resulting difference should not be adjusted for inflation. A slightly more accurate calculation is to convert the real values to nominal values and then take the difference.”

To explain why, we reference the case we labelled Lally/Bernstein in our analysis. This case showed real GDP as 3.0% and the downward real GDP adjustment as 2.4%, resulting in a long run growth rate of 0.62%. This value is the exact nominal growth rate. This nominal rate could be approximated as the difference in real GDP numbers: 3.0% - 2.4% = 0.6%. No adjustment for inflation is made in this approximation as both real DGP numbers would be adjusted to nominal values by adding the same inflation rate, leaving the answer unchanged. Using the more accurate Fisher equation and the 2.5% inflation rate to adjust for inflation we would have $((1.03)(1.025)-1) - ((1.024)(1.025)-1) = 0.62\%$, not 3.12% as computed by SFG. We therefore stand by our statement that taking the average across the estimates suggests that if anything the AER growth estimate is on the high side.

Simultaneous Estimation of the Cost of Equity, Reinvestment and ROE.

In relation to their simultaneous estimate of the cost of equity and growth SFG (2014d, p. 55) state:

“According to the average estimates over time for the cost of equity (10.6%), long-term growth (5.8%), and return on equity (19.3%), the estimated reinvestment rate is 30.2%, the estimated Price/EPS₀ is 15.4, and the estimated Price/EPS₁ is 14.5. 88 For the most recent period (1H14) the corresponding price/earnings ratios are Price/EPS₀ = 15.7 and Price/EPS₁ = 14.9.”

While this looks convincing, as will become evident, all is not as convincing as it seems. It does, however, provide a convenient means to demonstrate some of the issues in the simultaneous estimation approach that SFG propose. We stress that SFG base the price earnings formula on the basic one stage dividend growth model, whereas they implement their simultaneous estimates using a three stage DGM. Thus, their estimation is more complex in implementation than the analysis that follows. However, the simplified approach that the PE formulation provides nicely illustrates some of the issues in simultaneous estimation.

SFG (2014d, p. 55) assume the firm is in a constant growth state and develop a set of equations to show the relation between the PE, the dividend payout ratio/reinvestment rate and the return on equity investment (ROE). SFG’s equations are as follows:

$$Price = \frac{DPS_0 \times (1 + g)}{r_e - g} = \frac{DPS_1}{r_e - g}$$

$$\begin{aligned} \frac{Price}{EPS_0} &= \frac{DPS_0/EPS_0 \times (1 + g)}{r_e - g} = \frac{Dividend\ payout\ ratio \times (1 + g)}{r_e - g} \\ &= \frac{Dividend\ payout\ ratio \times (1 + Reinvestment\ rate \times ROE)}{r_e - Reinvestment\ rate \times ROE} \end{aligned}$$

$$\frac{Price}{EPS_1} = \frac{DPS_1/EPS_1 \times (1 + g)}{r_e - g} = \frac{Dividend\ payout\ ratio}{r_e - Reinvestment\ rate \times ROE}$$

SFG's PE equation can be rewritten to give the cost of equity as follows:

$$r_e = \frac{1 - Reinvestment\ Rate}{Price/EPS_1} + Reinvestment\ Rate \times ROE$$

Substituting SFG's estimates on the right hand side of the equation as below gives the cost of as 10.6%:

$$10.6\% = \frac{1 - 30.2\%}{14.5} + 30.2\% \times 19.3\%$$

What the above equation shows, however, is that for a given PE ratio, any desired r_e can be obtained by judicious choice of the reinvestment rate and ROE . For example, if we want r_e to be 20% this can be obtained by setting the ROE to 30% and the $Reinvestment\ Rate$ to 56.7%. Many other combinations of ROE and the reinvestment rate will give the same result, so there is plenty of flexibility in the choice made. The flexibility of choice is even greater if we also allow the PE to vary and therefore provides plenty of opportunity for gaming the system.

To be clear, we are not suggesting that SFG (2014d) are providing estimates that deliberately game the system. SFG constrain the choices available by requiring that their estimates meet certain criteria. As we have pointed out before (McKenzie and Partington, 2013b*), the result is that assumptions about the long term growth rate are replaced by assumptions about how the massive set of available choices should be filtered. Since the available set of choices is limitless, the exact result we get will also be determined by how coarse a grid we apply in initial selection of the choices that we allow to enter the filtering process.

Various filtering criteria are plausible. For example, we pointed out earlier the attraction of assuming that in equilibrium $NPV = 0$ and equivalently that $ROE = r_e$, which is consistent with a competitive equilibrium, value maximisation and the objective of regulation. Starting with SFG's estimates and their PE model above, but imposing the constraint (filter) that r_e and ROE have to be equal and iterating to a

solution gives an ROE and r_e of 6.9%.⁷ This result gives a considerably lower cost of equity than SFG's estimate, but gives exactly the same PE ratio. The point is that with simultaneous estimation, what you get will depend on the assumptions that underlie your filters. We would argue that the assumption underlying our filter is at least as plausible as SFG's and provides a result that explains the PE ratio just as well.

With reference to the reinvestment rate and the return on equity, SFG (2014d, p. 21) make the observation that:

“Any estimation method that assumes a constant input for the long term growth rate makes an implicit assumption about these two components.”

While these variables are important drivers of the growth rate, there is no requirement to make any particular assumption about the reinvestment rate and the return of equity in assuming a long term growth rate. However, if we wish to assume that these variables are the sole drivers of the growth rate, then we must assume that their product equals the long term growth rate and we must also make the restrictive assumption of no future equity issues and that the leverage ratio is held constant. As Lally (2013b) points out, in the presence of inflation, there can be problems in using the reinvestment rate times the return on equity to estimate the dividend growth rate, so we also need to assume that inflation is not a problem.

SFG (2014d, p. 49) report their estimates of a median long term return on equity of 16.9%, which is an increase on the initial return on equity of 16.1% together with a cost of equity of 10.9%. These results are puzzling. We are adjusting down to the long term growth rate, but the return on equity is going up. Therefore, for the growth rate to go down, the reinvestment rate must be going down - but cutting reinvestment in the face of a rising return on equity seems a little strange. The long term return on equity at 16.9% is above the cost of equity at 10.9%, which is a strange position to be assuming as an equilibrium. On these rates, investments in positive NPV opportunities are available and the question is why firms would not be investing more and bringing the return on equity down? It is therefore natural to question whether the return on equity has been overestimated.

We previously noted that the estimates of the return on equity seemed surprisingly high (McKenzie and Partington, 2013b*). On the basis of a comparison with the results of Kim (2011), we suggested overestimation of the return on equity. Kim reports much lower returns on investment and implicitly on equity than SFG. SFG's (2014f) response is that their results are consistent with the analysts' forecasts and with historic data and are not comparable to Kim's sample on the basis of the difference in time periods and firms' constituting the sample. This of course raises the question of the representativeness of the samples, which is an open question. The SFG sample is more restricted in both time and in requiring coverage of firms by analysts, but is also more current.

Since analysts only tend to cover large and liquid stocks, the SFG sample excludes smaller and less liquid firms paying dividends and it also excludes all firms that do not pay dividends.⁸ This latter group represent a large majority of firms by number, but a small minority by value. These firms by definition

⁷ The value of 6.9% equals the inverse of the PE ratio that is 1/14.5. This is no coincidence, but rather a general result. In an equilibrium where investments are zero NPV the cost of capital is the inverse of the price earnings ratio.

⁸ We note that any analysis based on analysts' forecasts, including the AER's is subject to these restrictions.

have a zero dividend yield and some will effectively have negative dividends in the form of new injections of equity capital. They probably have a currently low or negative ROE, a high reinvestment rate and a high expected growth rate, but also a higher risk of not surviving. It is too difficult to apply the DGM to this group and so it is not clear what effect their omission has. However, if we were to apply value weights in averaging DGM estimates across firms, the effect of these firms is unlikely to be material. This raises the question of whether, in SFG’s averaging of results across firms, the use of equally weighted averages, or value weighted averages, give substantively different answers.

Term Structure

There might be a term structure in equity rates of return. If such a term structure exists and is not accounted for in the cost of equity estimates from the DGM, then, as Lally (2013b) explains, the estimates can be unreliable. However, the existence of an equity term structure remains an open question in the research literature. Furthermore, even if we knew that there was a term structure, we would have the problem of estimating the cost of equity that was to apply to the more distant cash flows. It is a difficult enough problem estimating one cost of equity, without complicating that problem by requiring estimation of another cost of equity to apply at the end of the growth transition period. We therefore agree with SFG (2014d, p. 20) that if a term structure of equity was applied then:

“There is the risk that the regulated rate of return varies by substantial amounts over time because of estimation error, associated with whether a term structure exists and the assumption about the long term cost of equity.”

Consequently we do not recommend that an estimation technique involving an equity term structure be adopted. We do recommend that it be borne in mind that the existence of a term structure could materially change cost of equity estimates from the DGM.

Imputation

SFG (2014d) reiterate their argument that there is an inconsistency between the cost of equity applied under the AER’s post-tax revenue model and the AER’s computation of the cost of equity under the dividend discount model. The cost of equity can be expressed as consisting solely of dividends and capital gains, or it can be expressed as consisting of dividends capital gains and imputation tax credits. The relation between these costs of equity can be expressed as:

$$r_{ie} = \frac{r_{xe}}{c}$$

Where r_{ie} is the imputation adjusted cost of equity which includes an allowance for the expected return from imputation credits, r_{xe} is the cost of equity excluding imputation credits (consisting of expected dividends and capital gains only) and c is the conversion factor. SFG’s analysis sets out to demonstrate that the implied value for c varies between the AER’s post-tax revenue model and the AER’s computation of the cost of equity under the dividend discount model. According to SFG’s (2014d) analysis, under the AER’s post tax revenue model the conversion factor is given by (p.28):

$$c = \frac{1 - \text{tax rate}}{1 - \text{tax rate} \times (1 - \gamma)}$$

Where tax rate is the corporate tax rate, γ is the value of imputation tax credits and $\text{tax rate} \times (1 - \gamma)$ is the effective corporate tax rate under imputation. The equation shows that the conversion factor is the ratio of the after tax cash flow assuming no imputation to the after tax cash flow after allowing for the

value of imputation tax credits. This is the same adjustment factor as derived in Officer (1994) and so has theoretical support. However, Officer's derivation was done in the context of a perpetuity and can, therefore, be problematic in the case of cash flows that are not perpetuities.

In its estimation of the cost of equity using the DGM the AER grosses up the dividend by the following factor to allow for the value of the imputation tax credits distributed with the dividend.

$$1 + \left(0.7 \times 0.75 \times \frac{0.3}{1 - 0.3} \right) = 1.225$$

This adjustment is based on the following formula:

$$\begin{aligned} & \textit{Dividends plus imputation credit value} \\ & = \textit{Cash dividend} \times \left(1 + \left(\theta \times \% \textit{ franked} \times \frac{\textit{tax rate}}{1 - \textit{tax rate}} \right) \right) \end{aligned}$$

Where θ is equal to the utilisation rate (value) of franking credits distributed. This is a well accepted formula for grossing up dividends to give the sum of dividends plus franking credits. Utilising this formula and a numerical example, SFG (2014d) demonstrate that under this approach the implied value of the conversion factor c , in the DGM, is initially time varying and of a different magnitude to that implied by the AER's post-tax revenue model.

We have previously concluded McKenzie and Partington (2013b*) that if SFG's characterisation of the AER's post tax revenue model is correct then they are correct in their conclusion of a difference in the implied value of the conversion factor c between the AER's post-tax revenue model and the AER's implementation of the dividend discount model. Whether SFG's characterisation of the AER's post tax revenue model is correct is an open question. Since we have not been asked to review the AER's post tax revenue model we cannot make a conclusive determination on the potential inconsistency.

Even were there an inconsistency, this does not imply that there is anything wrong with either set of calculations, merely that they have utilised different underlying models and assumptions. Just as it is no surprise to find that the CAPM and the dividend discount model provide different estimates of the cost of equity. It would be nice and tidy if the AER's post-tax revenue model and the AER's DGM approach reconciled to the same conversion factor c , but it would be surprising if they did so because the assumptions underlying the two models are different. The key question is whether any inconsistency will result in the network service providers being undercompensated. This will not necessarily be the case. As in Truong and Partington (2008) the solution to the inconsistency might be to recognise the value of undistributed credits, thus increasing the value of gamma. Also, as we have pointed out, the value of the DGM estimate of the cost of equity is likely to be upward biased, so adding a larger imputation adjustment would likely take the estimate further away from its true value.

SFG (2014d) report conversion factors in the DGM that are initially time varying. This suggests that there may not even be consistency in conversion factors between different versions of the DGM, say two stage versus three stage models, and possibly the conversion factors will be different across stocks. We suspect that this will be driven by patterns in assumed growth rates. As growth rates differ so the distribution of returns between dividend yield and capital gains will differ. Since in the DGM only the dividends are adjusted for imputation, the effect of imputation is only captured in the dividend yield and not in the capital gains. This might be argued to be appropriate if it is assumed that credits currently not distributed will never be distributed and hence are valueless, but not otherwise.

The point is that it is desirable that the whole set of assumptions, both explicit and implicit, are clearly laid out before any differences in conversion factors can be fully understood. A very similar inconsistency in the distribution of returns between dividends, capital gains, and imputation credits, was debated between Gray and Hall (2006, 2008) and Truong and Partington(2008). That debate shows that the task of uncovering the assumptions is not trivial and neither is achieving agreement on the answer. What Truong and Partington demonstrate is that when the adjustment $\frac{1 - \text{tax rate}}{1 - \text{tax rate} \times (1 - \gamma)}$ is used to gross up returns, the imputation scaling factor implicitly applies to both the dividend yield and the capital gain yield. This reflects the value of franking credits attaching to dividends and also attaching to the capital gains arising from profit retention. Whereas, the standard adjustment, as used by the AER, to gross up the value of dividends ignores capital gains and implicitly assumes that the value of retained credits is zero. To resolve the inconsistency therefore involves recognition of the value of retained imputation credits, which implies a higher value for gamma.

The alternative approach is to take the imputation adjustment out of the DGM. This has some advantages. First, making the imputation adjustment to the dividends adds another layer of complexity and assumptions to the model. Second, variables in the DGM, other than the current price, are supposed to be expected values. Therefore, the percentage franked and the value of θ should be expected values, as should the tax rate. The value of θ and particularly the percentage franked could also vary across firms. In the AER's DGM, the values for θ , percentage franked and the tax rate are predetermined and fixed across firms. The AER takes these values to be expectations and uses market wide values since the objective is to obtain an estimate of the market risk premium. This is a reasonable approach, but it adds another set of assumptions to the model. Thirdly, in Truong and Partington (2008) it was the formula used to the gross up dividends for franking credits that was identified as creating inconsistency.

If the imputation adjustment is dropped from the dividends, in the DGM, then the cost of equity derived from the DGM will give r_{xe} , the cost of equity excluding imputation credits. The question then becomes what conversion factor c to apply to get r_{ie} , the imputation adjusted cost of equity. One choice is given by:

$$c = \frac{1 - \text{tax rate}}{1 - \text{tax rate} \times (1 - \gamma)}$$

Assuming the SFG analysis is correct, the above adjustment has the advantage of imposing consistency between the adjustment implicit in the AER's post-tax revenue model and the explicit adjustment of the DGM. It is also consistent with the theory of Officer (1994). However, there is the disadvantage that Officer's model is derived assuming a level perpetuity. In the perpetuity framework the assumption is that all franking credits are distributed and hence valued. The adjustment is therefore inconsistent with assuming that retained franking credits have no value.

Questions

- a) If it is appropriate to limit the use of the DGM to informing the estimate of the market risk premium.

It is appropriate to restrict the use of DGM to informing the estimate of the market risk premium. While the DGM is probably the second most popular method of estimating the cost of equity, there is a risk of substantial error in the estimates of the cost of equity for individual firms. Averaging over many

firms across the market helps reduce the impact of the error. There is, however, a significant risk that the DGM will overestimate the cost of equity and hence also overestimate the market risk premium.

b) Assuming it is appropriate to use the DGM to estimate the regulated business cost of equity, is it appropriate to use the DGM to inform the systematic risk exposure of regulated firms?

Given the difficulties that we discuss in estimating the cost of equity using the DGM, and in particular the difficulty of estimating growth rates, we do not recommend the use of the DGM for direct estimation of the cost of equity for utilities. We have pointed out the problems for estimating growth that arise from financing dividends and the declining or even negative growth rates that could result. In this respect, we pointed to the evidence suggesting the risk of dividend cuts for utility dividends and, hence, negative growth rates for dividends.

c) If the use of the SFG DGM model will lead to a materially better cost of equity estimate for a benchmark regulated network service provider relative to the AER's foundation model approach and if the answer is yes, if you consider any extra computational intensity of the SFG DGM model (relative to the AER's DGM model) for this purpose is justified.

We are not convinced that the use of the SFG DGM model will lead to a materially better cost of equity than the AER's approach.

d) What you consider is a reasonable estimate of the long term growth rate that should be used in applying a DGM? In relation to this point, please consider:

- If a long term growth rate should be set to exceed the growth in GDP.

It seems most improbable that the long term growth rate will exceed the growth rate in GDP, the reverse is the more appropriate assumption.

- If it is reasonable to determine the long term growth rate from historical dividends or earning growth rates.

Historical data on dividends and earnings growth rates might be used to inform the reasonableness of current estimates. However, the reliability of the historical estimates for this purpose will depend on the representativeness of the samples used. Different samples can and do yield different estimates.

- If the long term growth rate can and should be determined as the product of the retention rate and the return on equity.

We do not recommend estimating the long term growth rate as the product of the reinvestment rate and the return on equity since we are not convinced that reliable long term estimates of these variables can be obtained. Furthermore the long term growth rate in dividends also depends on the financing choices that the firm makes, not just the retention rate and the reinvestment rate.

- Assuming the long term growth rate can be determined as the product of the retention rate and the return on equity, how these can and should be determined (e.g. endogenously using data as SFG do, using historical data, or some other means).

In the light of the answer to the question above, the current question is not applicable. We note however that we would not rely on the endogenous estimates of SFG.

Choosing a long term growth rate for use in the DGM and the period and pattern of transition to that rate is problematic, particularly at the level of the individual firm. This is a key reason why DGM estimates of the cost of equity are at risk of substantial error. As a market wide growth rate, a rate less than the rate of growth in GDP seems appropriate.

e) If a term structure can be incorporate in the DGM analysis?

We do not recommend incorporating a term structure into the DGM analysis because the existence of a term structure for the cost of equity remains an open question and the magnitude of the difference, if any, between the short-term and long-term cost of equity is not known.

f) What is the most appropriate method for adjusting dividends for the impact of imputation credits?

The formula that the AER uses is the standard formula for adjusting dividends for the impact of imputation credits. However, as discussed above this adjustment will differ from the Officer adjustment for returns. Resolution of this inconsistency revolves around either recognising that retained franking credits have value, or making a downward correction to the imputation adjusted cost of equity derived using the Officer formula.

Part A5: Review of the Use of Expert's Reports in Estimating the RoE

Expert reports provide one form of survey evidence on required rates of return and we have previously argued in favour of the use of survey evidence in assessing the market risk premium (see McKenzie and Partington, 2011). Therefore, we consider that expert's reports are one useful source of survey evidence when assessing the value of the market risk premium and the market return on equity. Indeed, discussing the merits of survey triangulation, we noted that the surveys of expert reports by KPMG (2005) and Bishop (2009) both supported a market risk premium of 6% (see McKenzie and Partington, 2012).

In our opinion, expert reports provide useful triangulation of evidence on required rates of return, but they are by no means the last word on such returns. The information in expert reports has to be interpreted with some care. In particular, we do not recommend relying on the overall or "headline" rate of return that the expert provides, but rather we recommend examining the components that go into this number. An important reason for this disaggregation is the role of the uplift factors that the expert's apply. As we argue below, the uplift factors in some, and perhaps most, cases may not be relevant to measuring the equilibrium expected return, which gives us the cost of equity. It is also the case that expert reports do not simply contain one estimate of the cost of equity, or cost of capital, but more usually present a range of estimates.

Incenta (2014b) reports that, without the uplift factors, the experts' average market cost of equity and the matching estimate using the AER's approach⁹ are close at 10.4% and 10.28% respectively. The difference of 13 basis points is not a substantive difference given the accuracy with which the cost of equity can be measured and the difference is unlikely to be statistically significant. Thus, any substantive difference between the AER's approach to estimating the current cost of equity and the expert's estimates of the current cost of equity comes down to the effect of the uplift factors.

Comparison of Purpose

The purpose of the AER in estimating a cost of equity is to obtain a rate of return that when applied to the regulated asset base will allow a cash flow that matches investors' equilibrium expected rate of return. The purpose of the expert report is to provide a valuation of a company, or an asset, and the cost of equity is calculated in order to provide a discount rate for the valuation.

It is self-evident that these are different purposes, what is less self-evident is that they are likely to result in different estimates of the cost of equity capital (or the WACC). Despite being different, they may well be correct for their purpose. The theory of DCF valuation is that the expected cash flow should be discounted at the investor's equilibrium expected rate of return. The practice of valuation is that the cash flow being discounted is often larger than the expected cash flow and consequently the discount rate being used is larger than the investor's equilibrium expected return. The reason for this is as follows. The problem for valuers is that they may have not taken into account all of the factors that might reduce the expected cash flow, and it might not cost be effective, or even feasible to undertake the extra analysis to work out the correct expected cash flow. To compensate for the overstatement of the cash flow, the discount rate used is higher than investor's equilibrium expected return. To put it

⁹ The matching estimate using the AER's approach is based on using the CAPM with the expert's estimate of beta, the prevailing risk free rate and the AER's market risk premium.

another way, in order to compensate for the overstatement of the cash flow, there is an overstatement of the discount rate. Viewed in this light, it would be no surprise to find that the discount rate used by experts is higher than the cost of capital used by the AER. Under these circumstances, we would also expect that most of the difference would be driven by the uplift factors used to top-up the discount rate.

Survey Considerations

The usual caveats applying to survey evidence generally also apply to surveys of experts' opinions as revealed by their reports. The results can be affected by i) framing, that is the context of the survey and the questions posed, ii) whether there might be reasons for respondents to provide other than unbiased responses, and iii) the choice of target population and the level of response, which goes to the representativeness of the sample and the possibility of non-response bias. We consider these issues below, and while we identify some problems, in our opinion expert reports can and should be used to inform estimates of the cost of capital.

Framing

Ernst and Young (2012) provide a description of the context as follows:

“The Corporations Act and the ASX Listing Rules specify the circumstances where an expert report must be issued to those shareholders who are affected by certain types of transactions (e.g. takeover bids, mergers/schemes, related party transactions, buy-backs, acquisitions / divestments, and others). Even where there is no requirement for an expert report under the Corporations Act or the ASX Listing Rules, the directors of a company may still voluntarily commission an expert report to assist security holders in making informed decisions in relation to certain proposed transactions (e.g. as part of assessing a bid from a party which is associated but not considered a ‘related party’ due to not meeting certain shareholding thresholds).” (paragraph 36, p. 7)

This makes it clear that the framing will vary from report to report. Thus, the cost of capital estimates and the inputs to those estimates may well vary from report to report as a consequence of framing. It would be expected that there are differences in the cost of capital estimates across expert reports (indeed, this is what we observe) and we anticipate that much of the variation between the reports will be in the uplift factors. Where the same expert uses substantially different inputs in their cost of capital calculation over a short period of time, then the difficulty is to distinguish between changes in the expert's underlying beliefs and changes induced by framing.

Unbiasedness

After discussing the nature of expert reports together with the law and regulations which impact on these reports, the Ernst and Young (2012) survey reaches the following conclusion:

“The cost of equity provided in independent expert reports is the evidence of expert capital market practitioners acting independently in accordance with defined standards of independence, and based on documented and explicitly justified analysis.” (paragraph 50, p.9)

With some caveats on the nature of independence as discussed later, this is a reasonable conclusion. The statement is an argument in favour of unbiased and transparent opinions. The advantage of expert reports is that valuation is the stock in trade of the experts and the rate of return used in the DCF

valuations that they provide is an important part of their toolkit. Their opinions on the rate of return are therefore expected to be informed opinions. Expertise, legal requirements and ethical behaviour on the part of expert valuers, increases the probability that expert reports would give unbiased estimates, but this is not guaranteed. Even when deliberate bias is eliminated, systematic errors in analysis can still give biased estimates. In this context, it is worthwhile to recall that security analysts are the experts in providing earnings forecasts, but it has been clearly demonstrated that their forecasts are upward biased.

Sample

The target population is all publically available expert reports containing estimates of the cost of capital. The relevant question here is whether such a population is a representative sample of the market. Do the relevant expert reports represent a random sample of companies in the market, or are they are weighted towards a specific industry or sector, such as mining, or are they weighted towards a particular size class, such as large firms?

The reports used in the consultants' surveys were identified by using the CONNECT 4 data base. We would expect this database to have comprehensive coverage of expert reports, although not necessarily every report. Filtering of the CONNECT 4 expert reports was necessary in order to identify the expert reports that contained DCF valuations and cost of capital estimates that could be used in the survey analysis. This appears to have been done reasonably thoroughly and relative to other surveys on the market risk premium, we would expect the result was a much more complete "response". However errors in filtering are possible and this can change the results. For example in relation to its erroneous omission of three reports, Incenta (2014b) noted the following correction (p. 1-2):

"We agree with the AER that the additional firms it identified were inadvertently omitted and we have corrected an error with our interpretation of CFX (we assumed the entity is liable for company tax when, being a trust, it is not) and, in the process of further checking, identified an error in a formula and a slight inconsistency (albeit of minor significance). Having increased the sample of independent expert cost of equity estimates from 20 to 23, and corrected this error, we have revised down our estimate of the upward revision to cost of equity applied by independent experts, although many of our qualitative conclusions remain unchanged. Compared with the contemporaneous application of a mechanistic CAPM proposed to be applied by the AER (i.e. the 'spot' risk free rate and a market risk premium of **6.5 per cent**), and ignoring the effects of imputation credits, independent experts':

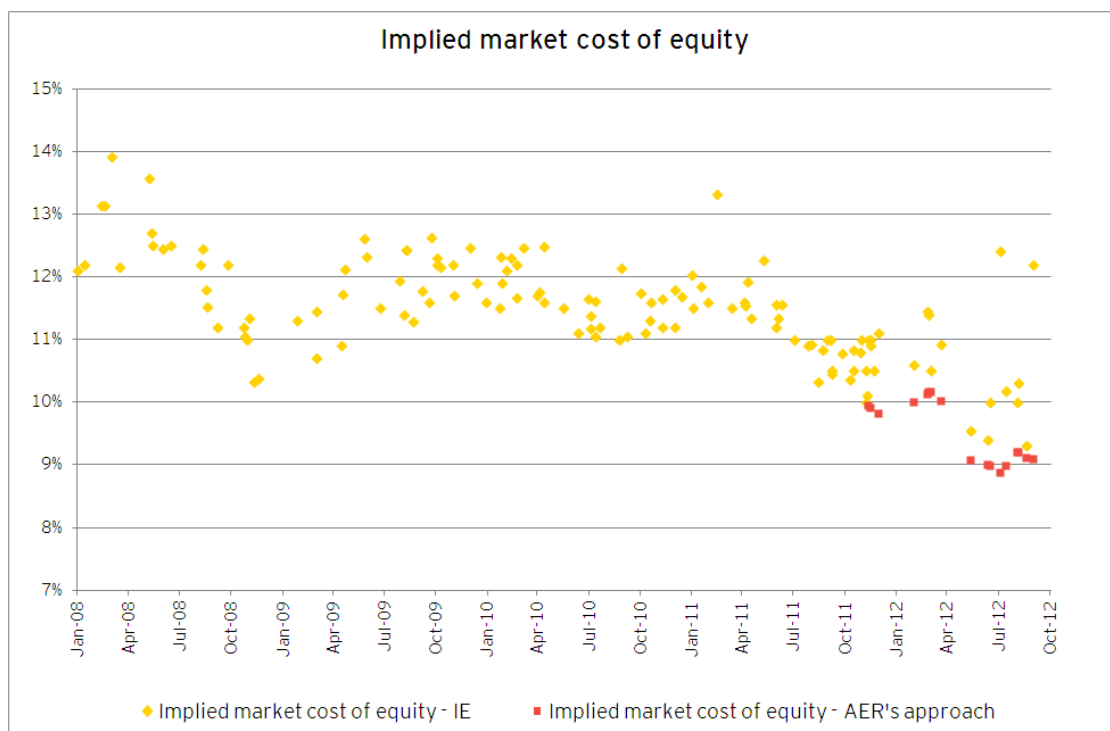
1. Return on the market – increased by **0.13 percentage points** for all 23 firms / projects ignoring any additional uplift ('alpha') (previously 0.20 percentage points),
2. Return on equity – increased by **1.7 percentage points** for all 23 firms / projects, i.e., after including the effect of any additional uplifts (previously 1.9 percentage points),
3. Return on equity for low beta firms – increased by **1.5 percentage points** for 5 low equity beta (averaging 0.76) firms (previously 2.8 percentage points), and
4. Return on equity for Envestra – increased by approximately **0.8 percentage points** relative to a mechanistic application of the AER's proposed approach (i.e. the same as previously)."

Particularly worthy of note is the substantial change in result for the return on equity for low beta

firms. This is due to the very small sample size involved, which makes average of these estimates very sensitive to sample composition. Therefore, sub-samples of expert reports with small sample sizes should not be relied upon. In particular, the Envestra valuation by Grant Samuel represents a sample of utility valuations with a sample size of one observation.

There are a reasonable number of sample observations in the time series - 132 in the case of the Ernst and Young (2012) survey. The Ernst and Young survey derives the market cost of equity implied by the information in each expert report. The time series evidence of Ernst and Young shows variation in implied cost of equity year by year and suggests a tendency for those rates to have declined over time (see Figure A5.1). This suggests that it would not be appropriate to compare the AER's current estimate of the cost of equity with experts' reports dated some years ago. Comparison with current reports would be appropriate. Variation through time, however, needs to be interpreted with caution given our comments about the size of year by year samples below and possible changes in the representativeness of the sample through time.

Figure A5.1: The Implied Market Cost of Equity



Source: Ernst and Young (2012), Figure 1.

Sample Size and Independence

Taken on a year by year basis the number of expert reports available to estimate the current cost of equity are not large. For example, Ernst and Young (2012) have 17 reports for their current estimate for 2012. Extending the sample period from October 2013 to 26 April 2014 gives 29 reports (SFG, 2013d). However, the number of independent observations is smaller than the sample sizes reported by the consultants. This is because the reports are concentrated in a relatively small number of advisory firms. For example, in the SFG (2013d) report, Deloitte provided 24% (seven reports) in the overall sample. Furthermore, in the SFG data, different projects being analysed in the same report are treated as separate observations. Of course there can be variation in the information across reports from the same advisory firm, but there is also repetition. When the same information is being repeated several

times, one opinion may be over-weighted. Whether this is appropriate depends on whether it is believed that multiple reports from one source are a consequence of higher expertise, credibility and reliability for that source.

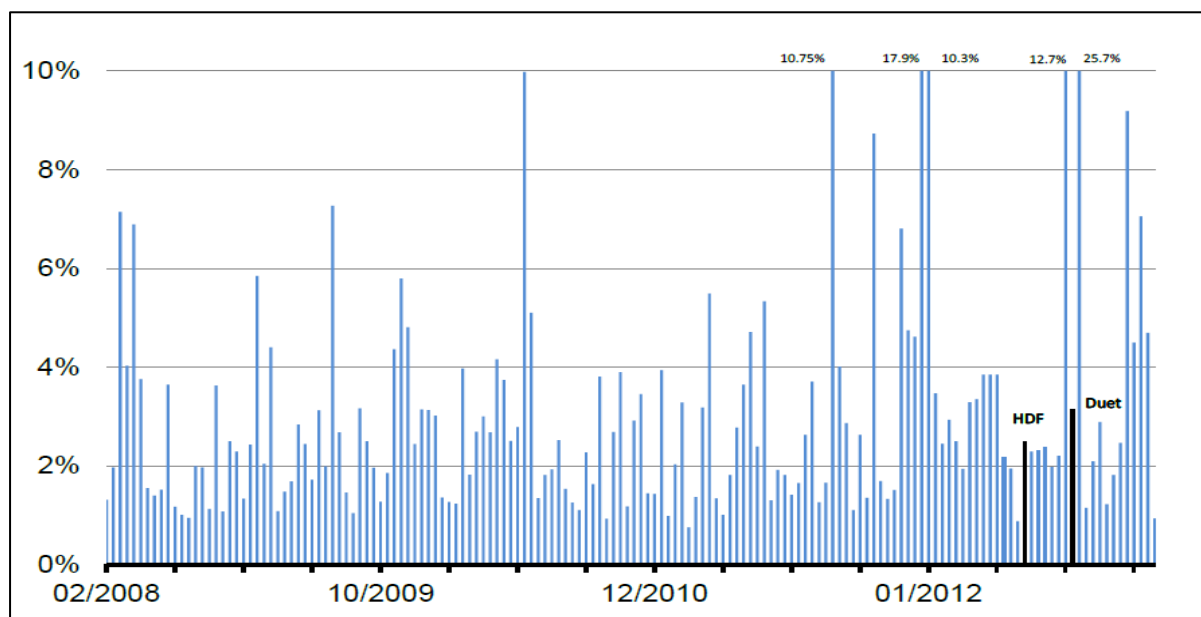
There is also another dimension to the dependence problem. Since there are relatively few expert firms involved, each firm is likely to have a very clear understanding of the cost of equity that other firms are using. Thus, there may be significant commonality (herding) in the cost of equity across reports by different firms.

Given the considerations of dependence (see above), the summary statistics computed for the current samples should be viewed with caution. It would be normal when considering whether there is a difference between samples of data (the expert reports and matched estimates under the AER approach) to perform significance tests. Given the small sample sizes of independent observations and the range of variability in the data, it is quite likely to be the case that observed differences based on current expert reports are not statistically significant.¹⁰ In our opinion this does not invalidate the use of expert reports as a basis for triangulation, but it does suggest caution in using them as a basis for substantial adjustment to the cost of capital.

Another cause for caution is the effect of outliers. There are clearly some substantial outliers in the expert report data, as Figure A5.2 shows. This figure is taken from SFG (2013d) and shows the expert cost of equity less the corresponding estimate using the AER's approach. In a small sample the outliers can have a big effect on the averages. Clearly the observations that plot beyond the boundaries of the graph in Figure A5.2 are candidates as outliers and we would suggest that, although not necessarily outliers, even 4% or 5% above the AER estimate seems quite a big difference. We would anticipate that examination of such cases would reveal that substantial uplift factors were applied.

¹⁰ The sample size could be increased by considering the whole time series of observations, but this has the disadvantage that we are no longer considering current estimates and presumably the timeliness of the estimate is a key attraction of the experts' report. The more so given that the AER estimate has recently been increased.

Figure A5.2: Difference between costs of equity from expert reports and estimate from the AER approach



Source SFG (2013d, p.20), Figure 6

Adjustments Made in the Expert Reports

A summary of the adjustments made to the cost of capital in the expert reports are presented by SFG (2013d, paragraph 51, p12) as follows:

“All of the expert assessments in the 2012/13 sample group use the CAPM as the starting point when estimating the cost of equity capital. In none of these reports is the CAPM implemented mechanistically by adopting the contemporaneous government bond yield as the estimate of the risk free rate and adding a risk premium equal to the long-run historical average. The implementation of the CAPM varies across reports as follows:

- a) Some use an estimate of the risk-free rate that is in excess of the contemporaneous government bond yield;
- b) Some use an estimate of the required return on the market that implies a market risk premium in excess of the historical average of excess returns;
- c) Some apply a specific uplift factor to increase the estimate of the required return on equity.”

The Incenta (2014a) report provides examples of specific reasons for the adjustments in respect of the interest and market risk premium. The following statement from the Incenta report is typical of the reasons presented (p.9-10):

“For example, in its report on Consolidated Media Holdings Ltd, KPMG stated that it considered bond yields to be trading at historical lows, and that there is a ‘strongly inversely correlated’ relationship with the market risk premium. In these circumstances, KPMG considered it appropriate to either:

- Adopt a historical MRP as a proxy for the expected MRP and adjust the spot risk-free rate to take into account the relationship highlighted above; or
- Adopt the spot risk-free rate and adjust the MRP for the perceived additional risks attaching to equity investments implicit from historically low (or high as the case may be) risk-free rates to reflect the current investment environment and the inverse relationship between the two variables.

Similarly, Lonergan & Edwards stated that:

Had a higher risk free rate not been adopted [to compensate for the historically low risk free rate], in our view, it would be appropriate to adopt a correspondingly higher market risk premium.”

With regard to the issues of interest rates and an inverse relation with the market risk premium, it is not at all clear whether these views represent scientific fact or folklore. However, it is not worth discussing these issues extensively as, firstly, it is only the uplift factors which are currently creating an apparently material difference between the experts’ estimates and the AER approach and secondly, because we have provided extensive discussion of these issues elsewhere (see McKenzie and Partington, 2013a). Our previous argument, supported by relevant evidence, was that compared with the long sweep of history, Australian interest rates are not abnormally low¹¹ and that the relation between interest rates and the market risk premium whether positive or negative, or whether any relation exists at all, is an open question.

With respect to uplift factors, the justification offered in some cases is a size premium. With respect to small company size being the source of an uplift factor, the question is whether this is an adjustment that reflects a systematic size factor in the cost of capital, or whether this is just an ad-hoc adjustment for idiosyncratic cash flow effects. We can well believe that for small firms, valuation practitioners will make an ad-hoc discount rate adjustment to allow for the difficulty in estimation of the cash flow. However, it is also clear from the Incenta (2014) survey that some valuers view the size effect as systematic, although the evidence for this view appears to be largely based on US studies. How significant the small firm adjustments are in the context of determining the overall market required return is questionable, considering that the bulk of the market’s value comes from large firms. SFG’s (2014, p51) appeal to the Fama and French three factor model as motivation for the expert’s size adjustment sits uneasily with the Australian evidence that attempts to estimate this model result in an insignificant, or negative, size factor. Although it would appear that SFG would have us believe that these estimates come from low quality studies (SFG, 2014b, p. 24)

Incenta (2014a, p17 and 18) provided the following reasons for specific uplifts in the discount rate. In our opinion these reasons all relate to overstatement of the cash flow. These adjustments represent things that could go wrong and if they do the cash flow will be lower. Conceptually the correct way to account for such events is to work out the reduction in cash flow resulting from the event, weight this reduction in cash flow by the probability of the event occurring and reduce the overall cash flow by that amount to get the expected cash flow. This is difficult to do and the easy way out is not to do it, but rather to increase the discount rate instead. As a result, the cash flow being discounted overstates the expected cash flow and the discount rate needs to be increased to compensate for this.

¹¹ Furthermore, if it is argued that interest rates are expected to rise in the future, then the basic theory of interest rates (the expectations hypothesis) tells us that this will already have been factored into higher current long term interest rate.

“Grant Thornton explained the additional risk factors it applied to the Mt Boppy Project (2 per cent) and Marda Project (1 per cent) as requiring ‘a certain level of professional judgement’:

Specific risk premium represents the additional return an investor expects to receive to compensate for country, size and project related risks not reflected in the beta of the observable comparable companies.

In assessing the appropriate specific risk premium to be applied, we have considered the following:

- Uncertainty associated with the early stage nature of this asset;
- Risk associated with successfully converting mineral resources into ore reserves; and
- Economic viability of extending the life of the mine.

Asset stranding risk and legislative / regulatory risk were among the additional risks identified by Grant Thornton when it was assessing the cost of equity of Australian Power and Gas Limited (APK):

As detailed in the beta section below we have adopted a beta, which largely reflects the historical risk of the business. In selecting an additional risk premium we have the following specific additional risks

- We note that in recent years the average usage per customer has decreased slightly year on year due to higher energy costs and more focus on renewable energy sources...
- In FY13 APK experienced a significant increase in provision for bad debts
- There is no certainty that door-to-door selling will continue to be allowed in Australia going forward and this may have a material impact on APK, given it is currently a key marketing channel for the Company.
- Political uncertainty relating to the upcoming Australian elections and the impact these elections could have on clean energy legislation including carbon tax reform.

In addition to the size premium, which independent expert Leadenhall allowed for in its assessment of the cost of equity of Spencer Resources Limited, a number of other risk were allowed for:

In addition to the size premium selected above, we have selected a company specific risk premium of 2% to 3% for Bulletproof to account for potential negative events that have not been included in the forecast cash flows, in relation to:

- A lower rate of adoption of cloud computing than currently predicted;
- Significantly higher competition levels in the industry resulting in pricing pressure; and
- Potential alternative new technologies.”

Questions

- a) Whether you consider the use of expert reports (as set out in the Guideline) is reasonable for the purposes of informing the estimate of the allowed return on equity for a benchmark efficient entity with a similar degree of risk as the service provider.

We have repeatedly argued that opinion surveys can be used to inform estimates of the cost of equity. We therefore consider that the use of experts' opinions is reasonable for the purposes of informing an estimate of the allowed return on equity. In particular, they can inform an estimate of the market risk premium. However, the purposes of expert reports and the purpose of the AER differ and due to these different purposes, we would expect the cost of equity in experts' reports to be higher than that calculated by the AER. We expect that this difference will mainly be driven by the uplift factors. We also note that the scientific basis for the experts' adjustments to CAPM factors is open to question. Consideration should therefore be given to the components of the cost of equity in the experts' reports not just the "headline" rate. In particular careful consideration should be given to the role of the uplift factors. Given the variation across experts' reports it is sensible to ask whether any difference between the AER's estimate and the estimate of the expert reports is statistically significant. In answering this question, the lack of independence across observations drawn from expert reports needs to be considered.

- b) Whether you consider the use of expert reports for directly estimating the RoE is appropriate. In answering this question please take into account as part of your consideration the rationale for the creation of these reports and the methods they use to value firms.

We do not consider that expert reports should be used to directly estimate the cost of equity for regulated entities. This is because the sample size of reports for utilities is very small and the risk of idiosyncratic variation is high. Additionally, expert reports are likely to overestimate the cost of equity.

- c) If the use of experts reports to directly estimate the RoE would be expected to materially improve the allowed return on equity estimate for a benchmark regulated network service provider relative to the RoE estimated under the Foundation Model approach as set out in the guidelines.

We do not consider that the use of expert reports to directly estimate the cost of equity would materially improve the estimate of the allowed rate of return.

- d) Which expert report, if any, are appropriate comparators to use for informing or estimating the RoE for a benchmark regulated NSP. In answering this, please set out what you consider are desirable characteristics for comparator expert reports.

All expert reports are potentially appropriate comparators for informing estimates of the cost of equity. However, using objective criteria it is desirable to disregard outliers in the expert cost of capital estimates and it is also appropriate to place little weight on the uplift factors.

- e) If the level of adjustments undertaken in any expert reports you consider appropriate comparators (e.g. adjustment relative to the base WACC estimated using the SLCAPM) demonstrates that the expected adjustment to the SLCAPM under the Foundation Model approach (e.g. via using a beta towards the upper end of our estimated range for beta) is of an insufficient magnitude to take into

account any expected biases in SLCAPM used as the base model in the Foundation Model approach.

The most recent data suggests that there is little difference between the AER's estimate of the market cost of equity and the expert's market cost of equity if uplift factors are ignored. On this basis, it seems appropriate to conclude that the null hypothesis of no difference between the AER's estimate and the experts' estimate cannot be rejected.

Additional Issues to be Addressed.

In this section we discuss the additional issues that we were asked to address for the stage 2 report. The issues we were asked to address are formatted below as indented dot points, with our responses immediately following.

AER Consideration of Other Models

- Whether or not, in the absence of estimating the Black CAPM, Fama-French three factor model, and dividend growth model; there remains sufficient information on the relative merits of these models to assign to them the roles as set out in the AER's foundation model approach.

For the reasons discussed in this report we would not recommend the use of either empirical estimates of the zero beta return, or empirical estimates of the market risk premium from the Black model. This poses a problem, since the AER has accepted the proposition that utilities with betas less than one, will have a lower return under the Sharpe Lintner CAPM relative to the Black CAPM and therefore sees a need to adjust the return from the foundation model upwards. The AER's solution of adjusting the equity beta upwards is a reasonable compromise, but there is no objective basis to determine what that adjustment to beta should be. In particular, we do not consider that the consultants' estimates of the Black model provide a basis for assessment of the magnitude of the beta adjustment.

It is clear from the earlier discussion in this report that we would not recommend use of the Fama French three factor model, either theoretically or empirically, in determining, the regulated return. An appropriate course for the AER is therefore to take no action with respect to the Fama French model.

It is also clear from our earlier discussion in this report that we consider there is a case for the empirical use of the dividend growth model to inform the estimate of the regulatory return. However, we would not recommend this model as a primary determinant of that return. We also think there is greater risk of inaccuracy at the level of the individual firm, as opposed to the market as a whole. Therefore an appropriate course of action for the AER is to restrict the use of the dividend growth model to being a rough cross check on the market risk premium.

The Impact of Low Interest Rates

- The likelihood that the current yields on ten-year government bonds reflect an abnormal investment environment, and whether or not the foundation model approach is likely to produce a return on equity estimate that contributes to the rate of return objective in current market conditions. In considering this issue, the report should address the current evidence on any relationship between movements in the risk free rate and movements in equity risk premium.

In an earlier report, we commented on the claim of abnormally low interest rates and pointed out that low interest rates were commonplace when longer time series of interest rates were considered and that it was the high interest rates of recent decades that were anomalous. We queried whether there was a reversion to the prior low interest rate regime and whether low interest rates would become the new status quo. It increasingly seems that low interest rates will be with us for the foreseeable future. At the time of the recent RBA cut to the cash rate (February 2015), short term government securities were yielding of the order of two percent with ten year government bonds

yielding under three percent. In terms of history, we would need to go back to the early 1950's to observe these sorts of yields.

At the time of writing (late March 2015) the probability of a further cut to the cash rate, as reflected in cash rate futures, is of the order of 0.6 and the yield curve for Australian government bonds (see below) suggests that out to five years spot interest rates are expected to go lower. The yields out to five years are low even by longer term historical standards and it seems that the low bond rates will be with us for some time.

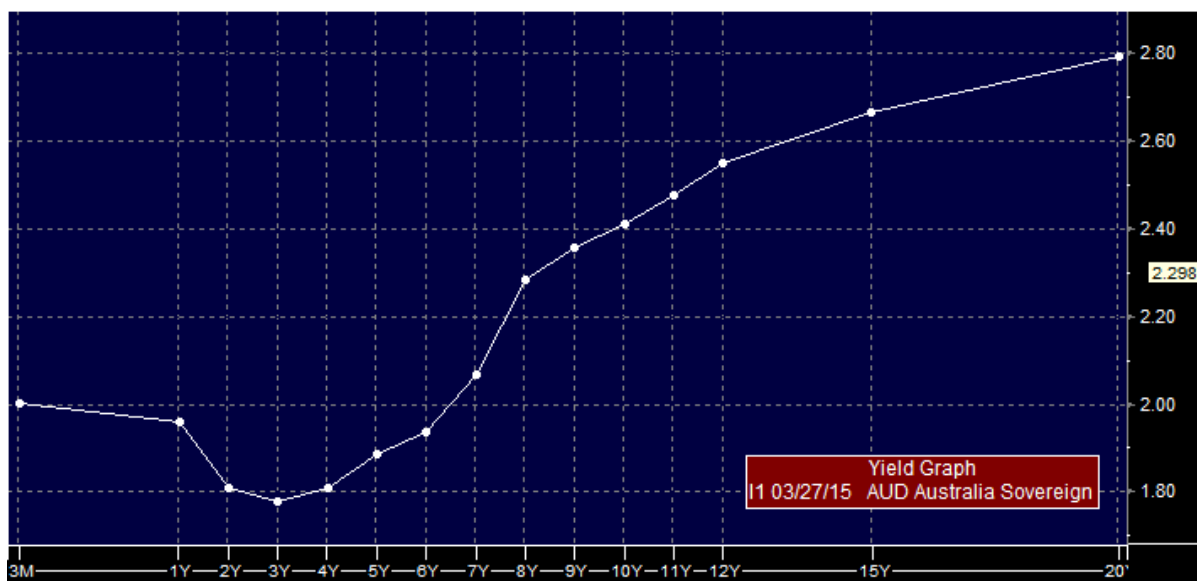


Figure 4: Current Yield Curve for Australian Government Securities

Source: Bloomberg 27 March 2015

The argument has been put that a “flight to quality” is the cause of the low yield on government securities. This may be true, but even if it is true, the yields are what they are. The fact that interest rates are low and are expected to remain low is not a compelling argument for increasing the benchmark risk free rate above the long term government bond rate. The low bond rates tell us that the required return for low risk assets is low.

Proponents of the “flight to quality” argument put the position that a flight to quality indicates increasing risk aversion and therefore an increasing risk premium. However, the behaviour of credit spreads, as we discuss below, does not suggest increasing risk aversion - if anything they suggest the reverse. There is also a widespread view that investor’s are engaged in a “search for yield”. This “search for yield” story has two versions. In both versions investors are taking on extra risk. The first version is that the low return on debt is causing investors to switch into shares with high dividend yields, resulting in a price premium for such shares. The second version is that in a search for higher yields investors are more willing to take on riskier investments. In other words, they are accepting a lower risk premium.

Clearly the proposition of a “flight to quality” increasing risk premiums is inconsistent with the proposition of a “search for yield” decreasing risk premiums. Our view is that some investors have

made a flight to quality, and some investors are engaged in a search for yield. It has not been demonstrated what the net effect on risk premiums is, but it is not axiomatic that a “flight to quality” means that both the interest rates and risk premiums used by the AER are biased downwards.

The proposition that as interest rates fall, investors demand compensation by increasing their risk premium, is often justified by the so-called Wright argument. That argument is that the equilibrium expected real return on equity is a constant. We have previously analysed this argument and found little support for it. The following statement by Fernandez (2013) rather nicely illustrates a key problem with the Wright argument, “Interest rates have a considerable bearing on share prices. Any investor’s experience shows that, in general, when interest rates fall significantly, share prices rise, and vice-versa.” We believe there are relatively few investors, or academics, who would disagree with this statement. The share prices rise because the required return falls.¹² This directly contradicts the Wright proposition that a fall in interest rates is offset by a rise in the required risk premium (and vice versa) in order to hold the real required return constant. If this were the case, then other things equal, reductions in interest rates would not be associated with rising share prices (and vice versa), since the benefit of the interest rate reduction would be offset by the rise in the risk premium.

We should be cautious about making inferences based on evidence from singular instances, but recently the RBA made a little anticipated cut to the cash rate (Tuesday, 3 February 2015). It is interesting to observe that following this rate cut, the market rose continuously for the rest of that week and the ASX 200 index ended the week up by a little over four percent. Rationally the market went up either because investors expected significant growth in company cash flows, or because their required return went down as a consequence of a lower interest rate. Given that the discussion at the time was about a slowing economy and reduced growth, a fall in required returns due to lower interest rates seems the more plausible explanation.

It is also difficult to reconcile arguments about increasing risk aversion, and demands for a greater risk premium as interest rates have fallen, with the observed behaviour of credit spreads. According to RBA data (see the graph below) credit spreads on BBB bonds rose to very high levels during the GFC, but had dropped very sharply by the end of 2009. Credit spreads then rose somewhat starting in 2011, due to concerns about sovereign defaults in Europe, but since mid 2012 credit spreads have been in a downward trend.

The general and very substantial decline in credit spreads since the GFC seems inconsistent with increasing risk aversion, if anything the data suggests the reverse. However, in our opinion decreasing risk aversion is unlikely to be a major explanation for the decline in credit spreads. Rather, credit spreads have mostly likely fallen because default risk has reduced and because of improvements in liquidity in the bond markets. If default risk is going down and bond market liquidity is improving, this seems an unlikely set of conditions for increasing risk aversion to be driving a higher market risk premium.

¹² To put it another way shares become more attractive relative to debt.

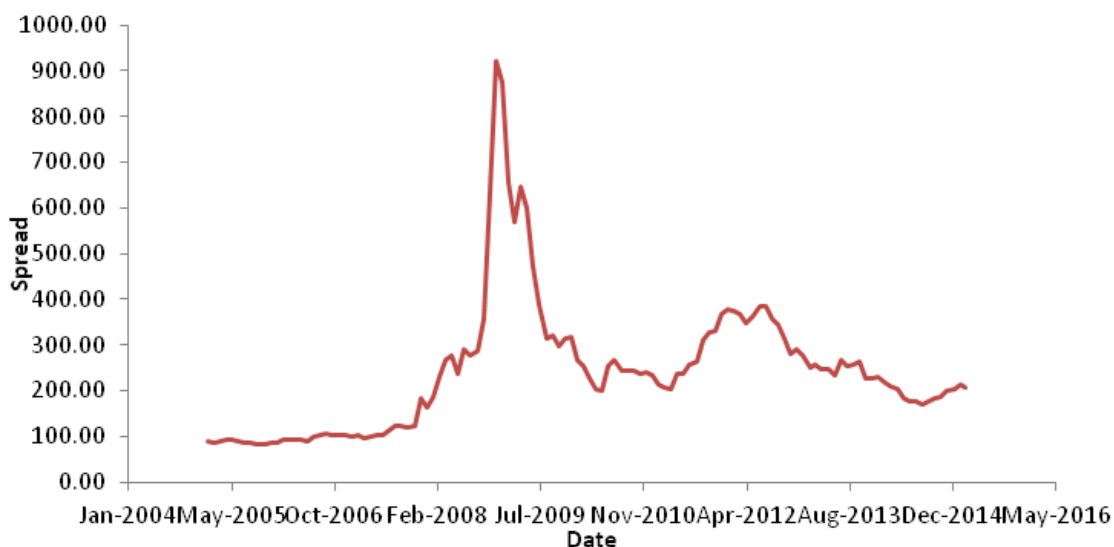


Figure 5: Spread of BBB bonds over 5 year Commonwealth Government Securities

Source: Reserve Bank of Australia

It is tempting to conclude that the falling credit spreads suggest a declining equity risk premium. However, as we have pointed out before, movements in the credit spread do not necessarily have direct parallels in movements of the equity risk premium, this is because the credit spread reflects promised returns and the equity risk premium is a measure of expected returns. Consequently, we would not infer that the equity risk premium has necessarily gone down because the credit spread has gone down. However, in relation to previous regulatory determinations, the regulated businesses and their consultants were arguing for a high equity risk premium because credit spreads were high as a consequence of the GFC. Given the very substantial decline in credits spreads from the GFC peak, logical consistency suggests that the regulated businesses and their consultants should now consider the evidence to be in favour of a falling risk premium. Our position is that we see no reason for the AER to adjust upwards either its measurement of the risk free rate or the market risk premium.

Re-levering Beta and the Use of Overseas Beta Estimates

- The considerations that should be had when examining both raw and re-levered equity beta estimates and the ability to draw collective inferences from both sets of estimates.

We have previously argued that the re-levering exercise is problematic. Various approaches to re-levering might be used depending on what theoretical position is taken on the effect of leverage on firm value/cost of capital and hence which adjustment formula should be used. For example, should an adjustment be made for corporate taxes and if so should it be the adjustment for a classical tax system, or for an imputation tax system? Should it be assumed that the debt beta is zero, or should it be assumed positive and if so what is the magnitude? These alternatives can have a material impact on the re-levered value of beta. Given that the process is problematic and also as we explain below unnecessary, we recommend not pursuing the re-levering of beta.

With respect to the use of betas estimated for overseas utilities, we consider that attempts to re-lever them to some assumed level of leverage in Australia are likely to be unreliable. This is because the problems of re-levering that we refer to above are compounded by institutional differences across countries. For example, evidence from Vietnam suggests that utility betas are very low. However, given

the difference in institutional, political and taxation regimes, we would not consider it appropriate to re-lever such estimates and apply them in Australia. Damodaran estimates the utility betas for developing countries at 0.14, but we would not suggest Australian utility betas were that low. Care also needs to be taken in utilising betas from the USA, which has different conditions to Australia (notably with respect to the tax regime). Consequently, we do not consider a re-levering exercise for overseas utilities be particularly useful. Furthermore, we would caution against the substantial weight that SFG applies to beta estimates from the USA in determining appropriate betas for Australian regulated businesses. The conclusion to be drawn from beta estimates for utilities in other markets is that they generally have relatively low betas. The appropriate conclusion from such evidence is that betas for utilities are generally lower than for average risk companies and a beta of less than one is appropriate for the Australian regulated businesses.

Given that the AER makes use of a plain vanilla WACC and thus makes the adjustment for taxes in the cash flow rather than the discount rate, the re-levering exercise is unnecessary. The theories of capital structure that underpin the re-levering exercise are either that changes of capital structure have no effect on the overall cost of capital, or that any effect is on the after tax cost of capital via the interest tax shield. Given that the AER allows for the tax shield benefit of debt in the cash flow the latter theory does not apply. Under the former theory the WACC is a constant independent of leverage. In other words, the plain vanilla WACC is a constant.

Once the WACC is calculated at any particular level of leverage, that cost of capital applies at all levels of leverage. Of course the cost of equity varies across different levels of leverage, but it is the overall cost of capital (WACC), not the cost of equity, that is the objective of the AER's determinations.

These principles can be illustrated using beta data from the USA, thus demonstrating why we do not need to re-lever such estimates. We take the data from Damodaran as cited in footnote 34 of SFG (2015b). Damodaran gives the utility industry beta as 0.59 at a debt to value leverage ratio of 38% and an unlevered equity beta (zero leverage) as 0.42. If, as SFG advocate, we can use the beta of US utilities as indicative of the risk of Australian utilities, then we can use the raw beta value to estimate the cost of equity. That cost of equity can then be used in the plain vanilla WACC calculated at the same leverage level as for the US utility. If re-levering is considered acceptable, then we can also use the unlevered beta to directly estimate the cost of equity and hence the WACC at 100% equity financing.¹³

For these purposes we utilise the AER's parameters for risk free rate of 3.55% and the market risk premium of 6.5% as per the Transgrid (2014) draft determination. Using the unlevered beta is easiest as plugging the unlevered beta into the CAPM gives the WACC at 100% equity:

$$\begin{aligned} WACC = r_e &= r_f + \beta(r_m - r_f) \\ &= 3.55\% + 0.42(6.5\%) \\ &= 6.28\% \end{aligned}$$

This comes in below the AER's allowed rate of return of 7.24%.

Alternatively we can compute the cost of equity using the raw beta at 38% leverage. This cost of equity is given by:

¹³ The unlevered equity beta is also equal to the asset beta.

$$\begin{aligned}
r_e &= r_f + \beta(r_m - r_f) \\
&= 3.55\% + 0.59(6.5\%) \\
&= 7.39\%
\end{aligned}$$

In order to calculate the WACC, we need the cost of debt. However, the cost of debt as assumed by the AER in the Transgrid determination is too high, as it reflects a higher level of leverage than 38%. We consider two alternatives for the cost of debt. First we take the debt to be AA grade since this is utility debt, which has generally been rated as having excellent characteristics with respect to business risk. With a 38% level of leverage, the debt would probably be rated as having modest to intermediate financial risk. Thus, the debt would likely be rated AA or A by Standard and Poors. According to Bloomberg data, AA Australian denominated corporate debt is currently yielding 3.79% for a ten year maturity.

The WACC is then given by:

$$\begin{aligned}
WACC &= \frac{D}{V} \times r_d + \frac{E}{V} \times r_e \\
&= 38\% \times 3.79\% + 62\% \times 7.39\% \\
&= 6.02\%
\end{aligned}$$

This is reasonably close to the 6.28% WACC calculated for 100% equity above (as it should be) and the two calculations would be approximately equal if we increased the cost of debt by 70 basis points to 4.49%. This latter cost is close to the current yield that Bloomberg reports for BBB debt with a seven year maturity. However, 4.49% is substantially below the AER's trailing cost of debt of 6.67%. Substituting the AER's trailing cost of debt into the WACC formula gives:

$$\begin{aligned}
WACC &= \frac{D}{V} \times r_d + \frac{E}{V} \times r_e \\
&= 38\% \times 6.79\% + 62\% \times 7.39\% \\
&= 7.16\%
\end{aligned}$$

This result is again below the AER's allowed return of 7.24%. On the basis of these calculations we find no evidence that the USA data suggests that the AER's allowed return is too low. We also note that Damodaran calculates the WACC for USA utilities at a low 4.07%, driven in part by current very low interest rates in the USA. While these calculations do give some comfort to the AER, we reiterate our earlier caution that too much weight should not be given to inter-country comparisons and overseas betas.

Excluding Data from the Mining Boom

- The advantages and disadvantages of excluding the mining boom time period from equity beta estimates, and the implications any mining boom exclusion may have for the estimation of other cost of equity parameters such as the market risk premium.

Our general view is that it is undesirable to delete periods of history from data analysis because of so called abnormal conditions. We have previously recommended not excluding data unless it is clearly erroneous. The danger is ad-hoc deletions, where what is abnormal may lie in the eyes and prejudices of the beholder. It is quite reasonable to subject estimates, such as those for equity betas, to robustness checks by examining the sensitivity of the estimates to particular events, or different periods. However, as a basis for obtaining a definitive estimate of beta, we would not advocate deleting the mining boom from the estimation period.

There are three particular difficulties with deleting the mining boom. First, mining booms are a regular feature of Australian equity markets rather than abnormal one off events. In other words they are part of what is normal. Second deletion of the most recent mining boom would involve deletion of a substantial period of data and third the data deleted would constitute much of the recent data. Throwing away a large quantity of data, particularly recent data is generally regarded as undesirable. Such action reduces the timeliness of the estimate and by reducing sample size, increases the standard error of the estimate.

Risk of Disruptive Technology

- The extent to which risks associated with disruptive energy technologies should be considered in setting a return on capital allowance.

There are two issues to address here, whether any effect is material and whether any adjustment should be applied to the discount rate. Let us be clear in this discussion that the discounted cash flow method of valuation developed from finance theory, is that the expected cash flow should be discounted at a discount rate that reflects the systematic (undiversifiable) risk of the cash flow.

It is clear that disruptive technology may affect the value of the firm. The effect on value is due to changes in the expected cash flow of the firm and the effect is a downward adjustment of expected cash flow.

Disruptive technology can change the expected cash flow and as a consequence the value of the regulated business changes, but not the discount rate (cost of capital). The discount rate does not change because the effect is not systematic. These effects are firm/industry specific and can be diversified away in an investor's portfolio. This is easily seen in the case of disruptive technology, put crudely the diversified investor holds stocks in both utilities and the firms implementing the disruptive technology.¹⁴ This diversifies away the risk. Since the effect is diversifiable it is not a systematic risk and it is not appropriate to make an adjustment to the cost of capital.

The appropriate way to adjust to for disruptive technology is therefore to adjust the cash flow. To the extent that the result of disruptive technology is stranded assets, then the effective economic life of the asset is reduced and/or its residual value is less than originally assumed. Consequently, one way to allow for the impact on cash flow is to increase the regulatory depreciation allowance. However, it is very important that if this compensation is offered ex-ante it should be clearly agreed that no compensation will be offered ex-post if assets are stranded. That is no lump sum compensation should be available on exit. The question is what should be the magnitude of cash flow compensation, if any, and is it material?

Disruptive technology, such as increasing use of home solar systems clearly poses a risk to the regulated businesses, but it is difficult to be definitive about the materiality of such effects on firm value, but some evidence may be available. For stocks listed on the ASX there is a continuous disclosure requirement. Material impacts a on a firm's value are required to be communicated to the market as soon the firm becomes aware of them. A natural question, therefore, is whether listed utilities have made any announcements to the market about diminution in firm value arising from disruptive

¹⁴ More generally the investor holds shares with returns that are less than perfectly correlated.

technology? If there is such disclosure, what is the magnitude of the effect disclosed? Similarly, do such statements about value diminution appear in publically released company briefings to financial analysts? If there is no such disclosure the logical conclusion is either that the effect is not considered material by the firms, or alternatively that the firms have breached the disclosure requirements.

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Appendix Curriculum Vitae

CURRICULUM VITAE

PERSONAL

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NSW 2006
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Academic Qualifications: B.Sc. (Hons) Economics/Forestry, University of Wales, 1971
MEc. (Hons) by thesis, Macquarie University, 1983.

Employment

My current position is Associate Professor of Finance and Chair of the Finance Discipline at the University of Sydney. I was also head of the postgraduate research program in finance. Concurrent with my position at the University of Sydney I was also the Education Director for the Capital Markets Co-operative Research Centre PhD program. In a career stretching back more than thirty years I have held Associate Professorships in finance at The University of Technology Sydney and The University of British Columbia. I have also held academic positions at Macquarie University and the University of Bangor I have had extensive teaching and research responsibilities in finance and accounting as well as being head, or deputy head of University Departments and Schools. I have been a major force in the design of several undergraduate and masters degrees in finance and also PhD programs.

I have written in excess of thirty consulting and expert witness reports covering topics such as valuation, the cost of capital, the value of imputation tax credits, and the market risk premium.

Awards and Major Research Grants

Awards

2012 Bangor University: Honorary Visiting Senior Research Fellow title extended for the period 2013-2016.

2010 Finance and Corporate Governance Conference, the GARP (Global Association of Risk Professionals) Prize for Quantitative Finance/Risk Management/Derivative Instruments.

2009 Asian Finance Association Conference, the CFA (Chartered Financial Analyst) Prize Asian Investments.

2009 Bangor University: Honorary Visiting Senior Research Fellow title for 2009-2012.

2008: PhD students name their rock group after me "The Partingtons"

2001: Manuscript award for the best paper: Education Notes, *Accounting Research Journal*, 2000.

2000: Peter Brownell Manuscript Award. Awarded by the Accounting Association of Australia and New Zealand for the best paper in *Accounting and Finance*, 1999

1985: Butterworths Travelling Fellowship

Major Research Grants

2014-2016 Centre for international Financial Regulation, Measuring Market Quality: Current Limitations and New Metrics

2007-2014: National Co-operative Research Centre Scheme, grant for the Capital Markets Cooperative Research Centre. \$98 million (\$49 million in cash and matching in kind contributions.) About three million dollars per year (\$21 million over the term of the grant) of the cash was under my management to run the scholarship and education program.

2000-2003: Australian Research Council, industry linked grant, *Intangibles, Valuation and Dividend Imputation* (\$667,000).

1985-1988: Australian Research Grants Scheme, *The Determinants and Consequences of Dividend Policy*, (\$30,000).

PUBLICATIONS

Books

R. Brealey, S. Myers, G. Partington and D. Robinson, 2000, *Principles of Corporate Finance*, Australian

Edition, McGraw-Hill (1st printing 2000, 2nd printing 2000.)

C.A. Martin, J. McKinnon, R. Hines, G. Harrison and G. Partington, 1983, *An Introduction to Accounting*, McGraw-Hill (1st edition, 1983, 2nd edition, 1988, 3rd edition 1990.)

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G. Partington, Discussion of an International Analysis of Dividend Payment Behaviour, 2009, *Journal of*

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L. Hodgkinson and G. Partington, 2008, The Motivation for Takeovers in the UK, *Journal of Business Finance and Accounting*, **35:1-2**, pp. 102-126

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