REVIEW OF RESPONSES TO *REVIEW OF SUBMISSIONS ON THE COST OF DEBT AND THE TAMRP FOR UCLL AND UBA SERVICES*

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CONTENTS

EXECUTIVE SUMMARY

This paper has reviewed recent submissions and cross-submissions to the Commerce Commission on the cost of capital for UCLL and UBA services, in response to an earlier report by me. I do not agree with most of the points raised. In respect of the points I do agree with, none if allowed for would change the conclusions in my earlier report. These latter points are as follows.

Firstly, both Grundy and CEG argue that the DRP data series used by me for assessing bankruptcy risk and output price volatility under various regulatory approaches to the cost of debt is too short, and the 2008 and 2009 values in my series (arising from interpolating over data for 2007 and 2010) are likely to underestimate the spike induced by the GSF. CEG also presents and uses a US series that is free of both problems. I consider that their series is superior in both respects, but use of it does not change the conclusions reached: that bankruptcy risk for firms who receive the prevailing rather than the trailing average DRP is not significant, and output price volatility is significantly lower using a trailing average cost of debt than doing otherwise.

Secondly, CEG argues that the survey evidence cited by Lally in estimating the TAMRP is unsatisfactory in various ways including the future period to which the survey question relates being unclear and the small number of respondents. I agree with these points but the practical consequences appear to be minimal, in the sense that a better survey does not seem to be available and disregarding it would not change the rounded estimate for the TAMRP of 7%.

Thirdly, in estimating the TAMRP using averages over foreign data, Network Strategies argues that medians should be used rather than means so as to dampen the effect of outliers. This is reasonable point but it does not change the rounded estimate for the TAMRP of 7%.

1. Introduction

The Commerce Commission (2014) has recently sought submissions on the cost of capital for UCLL and UBA services. Aspects of these submissions were reviewed by Lally (2014a), and an estimate of the TAMRP also offered. In response, submissions have been received from Grundy (2014), CEG (2014b), and Network Strategies (2014a), followed by cross submissions from CEG (2014c) and Network Strategies (2014b). This paper reviews these submissions and cross submissions.

2. Grundy Submissions

In respect of using foreign currency denominated bonds to estimate the DRP for Australian regulated businesses, Lally (2014a, section 2.1) argues that this is problematic because these bonds are not very liquid (because the holders of them typically hold till maturity) and therefore secondary market based estimates (from parties such as Bloomberg) would have low quality. In response, Grundy (2014, para 3) claims that Lally has not provided any evidence in support of the claim. However, Lally (2014a, section 2.1) cites the QTC (2012, Attachment 1, page 25) in support of the claim and the QTC (ibid) state that

"*It is typical in the US private placement market that bonds are acquired by insurance companies or other long-term financial investors and held until maturity. Trading volumes are relatively small and the market is not liquid. It would not be appropriate to rely on trading data, which is likely to amount to only indicative non-binding bid and offer quotes. It is more likely that only data for bonds, which have been issued during the period by Australian issuers can be relied upon*."

Since the QTC raises debt finance in both domestic and foreign markets (as per its website) it could reasonably be presumed to be knowledgeable about these matters.

In respect of using foreign currency denominated bonds to estimate the DRP for Australian regulated businesses, Lally (2014a, section 2.1) argues that this is also problematic because some firms are unable to obtain foreign debt finance. In response to this, Grundy (2014, para 11) argues that such firms will face a higher cost of debt than those who can borrow abroad and therefore applying to the first group of firms a cost of debt that applies to the second

group will underestimate the cost of debt of the first group. I agree but I have never argued that the cost of debt of the firms that can borrow abroad should be applied to those that can't. Instead, I have argued that the cost of debt for all firms should be estimated using data from only local public debt issues (Lally, 2014a, section 2.1). The consequence would be to correctly estimate the cost of debt for firms who can't borrow abroad and *overestimate* it for those who can borrow abroad.

In respect of using foreign currency denominated bonds to estimate the DRP for Australian regulated businesses, Lally (2014a, section 2.1) argues that this is also problematic because the buyers of such bonds (and therefore the secondary market data) come from a variety of markets.[1] If they are from the same market as the lender, the DRP estimated from secondary market data would tend to be similar to that based upon primary market data (absent any new information) and therefore may provide a poor estimate of the DRP of a local currency bond. Alternatively, if the buyers are from the local market, the DRP estimated from secondary market data could be a poor estimate of that for primary market data in the foreign market. To illustrate the problem Lally (ibid) considers the case of an Australian company that borrows in $US at 7.5% (after swapping into AUD), could borrow locally at 7.0%, and secondary market transactions on the foreign borrowing shortly after issue of the bonds were at 7.5%. In this case the secondary market data would overestimate the cost of local borrowing but accurately estimate the cost of foreign borrowing. By contrast, if the secondary market transactions were at 7.0%, then this data would correctly estimate the cost of local borrowing but underestimate the cost of foreign borrowing. In response to this, Grundy (2014, para 12) argues that a company would always seek out the cheapest form of debt and therefore would never borrow overseas at a higher rate than available locally. However, as noted in Lally (2014a, footnote 1), firms seek to diversify their sources of finance to protect themselves against highly adverse circumstances in a particular market at the time the firm seeks to borrow. For the same reason, firms stagger maturity dates and seek debt in advance of the maturity date of the debt in question. Thus, even if foreign borrowing for a particular term were more expensive than local borrowing, some level of foreign borrowing would still be observed. This is the general practice amongst firms. For example,

[1] This is apparent from examining the ownership composition of some of these foreign currency denominated bonds, as provided by Bloomberg.

Bhedin (2010, paras 19, 47, 51-52), who was the Group Treasurer for Vector at that point, attests to this policy and finishes with the following statement (ibid, para 52):

"*Vector seeks to maintain a diverse range of funding options and consistent with that policy will not necessarily fund at the cheapest available rate for a particular maturity*."

In respect of choosing a target credit rating for New Zealand suppliers of UCLL/UBA services, Lally (2014a, section 2.2) argues that placing sole or primary weight on the credit rating of Chorus (the only existing supplier of such services) will discourage Chorus from actions that raise it credit rating and weaken its incentives to maintain its rating (because such actions or inaction will raise Chorus's allowed WACC). In response to this, Grundy (2014, section 4) argues that the use of Chorus's own credit rating is necessary in order to ensure that its allowed WACC matches its actual WACC and that failure to do so will weaken Chorus's investment incentives (investing too much if the allowed WACC exceeds the true WACC and investing too little if the allowed WACC is below the true WACC). The merits of this argument depend upon whether the credit rating fall arises from inefficient behaviour by the firm (such as poor staggering of debt or failure to diversify across sources of debt), events beyond the firm's control (such as the GFC), or actions by the firm that reduce its credit rating whilst benefiting the firm elsewhere (to which Grundy explicitly refers). There is no merit to Grundy's argument if the firm is simply acting inefficiently, nor is there merit if the firm is trading-off a reduced credit rating against some benefit (because the incentive for the firm to do this is removed if the regulator matches its allowances to the firm's actions). By contrast there is merit in Grundy's argument if the fall in the firm's credit rating is beyond its control, and this highlights a trade-off faced by a regulator in choosing regulatory parameters: using the values for a firm ensures that events beyond its control are fully reflected in its allowed WACC and therefore optimises the investment incentives referred to by Grundy, but it also undercuts the incentives for firms to act efficiently over matters that are within its control. This trade-off is best addressed by choosing an allowed WACC that averages over a group of suitable comparators, thereby undercutting the incentive for the firm to act inefficiently but still achieving an allowed WACC that reflects events beyond a firm's control (because such events are likely to similarly affect the comparators).

Grundy (2014, paras 30-31) argues that the revenues allowed by the Commission, and by regulators in general, are too low because the regulator acts as if tax benefits from debt will

always be gained when this is not the case (due to non-debt tax shields and earnings shocks that reduce earnings before interest below interest). These claims are true in a classical tax regime, in which interest and equity returns are equally taxed at the personal level (and therefore debt generates a tax saving). They are not true in the New Zealand tax regime, in which the corporate tax benefit from debt is largely offset by the higher personal taxation on debt, and the cost of equity model used by the Commission (the simplified Brennan-Lally model) reflects this. Thus, although the corporate interest deductions are overstated for the reasons identified by Grundy, so too are the cost of equity reductions arising from the lower personal taxes on dividends relative to interest.

Grundy (2014, paras 32-51) argues that a regulatory regime should allow for costs of financial distress (CFD), that the Commission fails to do so (along with regulators in general), and therefore it does not provide a sufficient revenue allowance. However, a substantial part of the CFD are directly borne by debt holders (including all CFD that arise after default and those arising from the possibility of shareholders adopting NPV negative projects because debt holders suffer even bigger value losses); debt holders then raise the promised yield on debt so as to provide ex-ante compensation to themselves. Since the Commission uses the promised yield on debt in determining revenue, it therefore allows for a substantial fraction of the CFD. In respect of the rest of the CFD, these are much less relevant to regulated businesses than unregulated businesses. For example, the failure of shareholders to adopt some NPV positive projects because debt holders would experience an even bigger value gain is a CFD but the disadvantage to the shareholders would be limited to the residue of the current regulatory cycle (at which point the cost of debt would be reassessed) and therefore the shareholders could minimise this wealth loss by deferring adoption of the project until near the end of the regulatory cycle. Furthermore, the Commission is likely to overcompensate shareholders because the promised yield on debt also includes an allowance for the value of the default option possessed by equity holders and its inclusion in the cost of debt used by a regulator is unwarranted (because it is a mere transfer between debt holders and equity holders and therefore does not affect the true WACC).[2] Consequently, defining the cost of debt as the promised yield is likely to give rise to an overstatement in the cost of debt and hence the WACC.

[2] The cost of debt comprises the risk free rate, an allowance for systematic risk, and expected default losses, and the latter arises from both the default option possessed by debt holders and some of the CFD.

To illustrate this point, suppose that an unlevered firm will deliver a payoff of \$155m or \$55m in one year with equal probability, investors are risk neutral, the risk free rate is 5%, and there are no taxes (personal or corporate).[3] The value of the firm is then \$100m, which equals the purchase price of the assets, and the cost of capital is the unlevered cost of equity, which is the risk free rate of 5%. Suppose now that the firm acquires some debt finance, promises a payment of \$60m to debt holders (principal plus interest), and there are no CFD, i.e., even in the presence of debt, the possible payoffs from the firm in one year are still \$155m or \$55m with equal probability. So, the value of the firm is still \$100m and the WACC now involves a weighted average of the costs of debt and equity, but it should still be 5%. However, given the default option possessed by equity holders, the payoff on the debt will be \$60m in the good state and only \$55m in the bad state, and therefore the value now of the debt will be

$$B = \frac{.5(\$55m) + .5(\$60m)}{1.05} = \$54.76m$$

So, a promise of \$60m will allow the firm to borrow \$54.76m, and the promised yield on debt will then be 9.57% comprising the risk free rate of 5% and compensation of 4.57% to debt holders for expected default losses, which arise purely from the default option possessed by equity holders because there are no CFD. Since the debt comprises 54.76% of firm value then the WACC defined using the promised yield on debt as the cost of debt will be

$$WACC = .4524ke + .5476kd = .4524(5\%) + .5476(9.57\%) = 7.50\%$$

The WACC is now too high, being 7.50% rather than 5%, because the cost of debt is wrongly defined as the promised yield. The implication for regulation is that the allowed WACC should be 5% rather than 7.50%. If the regulator allowed 7.50%, on the firm's asset base of \$100m, the regulator would then set a price or revenue cap so that the firms' expected payoffs in one year would be \$107.5m. The resulting value now of the firm would be as follows:

$$V_0 = \frac{\$107.5m}{1.05} = \$102.4m$$

[3] The example is intended only to illustrate the principle and not also the scale of the effect.

By contrast, the purchase price of the assets is only $100m. So, shareholders would have been gifted $2.4m through the regulator defining the cost of debt as the promised yield rather than the expected rate of return. Of course, it is not feasible for regulators to do otherwise. However, the result is that equity holders would be over compensated.

Lally (2014a, section 2.3) argues that the regulatory practice of estimating the average term for which firms issue debt followed by estimating the DRP for that term leads to an overestimate of the DRP because firms issue debt with a variety of terms to maturity, the spread in terms for bonds used in the estimation process is narrower, and the DRP is a concave function of term to maturity. In response to this, Grundy (2014, para 58) states that Lally provides no evidence in support of the claim that regulators estimate the DRP using bonds with less variation in times to maturity than the bonds issued by a firm. However, as noted in Lally (2014a, footnote 7):

"*I understand that the principal sources of debt for New Zealand corporates are bank loans, New Zealand public debt issues, and foreign bonds (public debt issues or private placements). I also understand that the term for bank loans typically does not exceed five years whilst the term for foreign bonds is typically at least ten years*."

Supporting evidence is provide by Bancorp (2010, page 15), and the range in terms to maturity is out to infinity (on perpetual bonds). By contrast, the range in terms to maturity on bonds used by the Commission is much narrower. For example, in its most recent determination (for information disclosure purposes), the DRP on a five-year EDB/GPB bond with a BBB+ rating was principally estimated from a bond with a residual term to maturity of 5.9 years (Commerce Commission, 2014, pp. 10-11) whilst the DRP on a five-year Airport-issued bond with a A- rating was principally estimated by linearly interpolating over bonds with residual terms to maturity of 3.3 and 5.5 years (Commerce Commission, 2014, pp. 20-21). In view of the significantly narrower range used by the Commission than in the set of bonds that firms have, the DRP estimated by the Commission will be biased upwards.

Grundy (2014, para 59) also states that Lally (2014a) provides no evidence in support of the claim that the DRP is a concave function of term to maturity over the relevant range of bond lives. However, as shown in Bancorp (2010, page 15), the relevant range is out to infinity

(for perpetual debt) and the DRP must be concave over this range or else the DRP on perpetual debt would be infinite (which is clearly not the case). Grundy also tests the concavity claim using RBA data over a relatively narrow range (five to ten years), and concludes that the DRP was a convex function of the time to maturity in 46 of the 114 months covered by the RBA dataset. However it follows that it was concave in the remaining cases, which constitute 60% of cases. So, even over this relatively narrow range in which a test for concavity would be weak (because a concave function would be so close to linear), Grundy's analysis supports concavity rather than contradicts it.

In respect of whether one should estimate the DRP using curve fitting or collecting a set of DRP estimates on suitable bonds with residual terms to maturity of approximately the desired term, followed by averaging over these estimates, Lally (2014a, section 2.3) argues that curve fitting suffers from the need to obtain DRP data over a wider range of maturities, which introduces a temptation to loosen data standards (by admitting lower quality data) with the likely result of biasing the estimate of the DRP of concern. In response to this, Grundy (2014, paras 65-66) argues that measurement errors in the lower quality data would be independent and therefore the larger number of observations might outweigh the inferior quality. However, Grundy's presumption that lower quality in data always involves measurement error is unwarranted. For example, suppose that subordinated bonds were included in order to increase the sample size (as done by Bloomberg and recommended by CEG, 2012, page 49, in an Australian regulatory case). Although these bonds have higher expected default losses than senior bonds over the same company, so long as credit ratings fully reflect all information relevant to bond pricing, then the DRP of a subordinated bond of a company that has a credit rating at the benchmark level (say BBB+) would be comparable with the DRP of a senior bond of another company with the same BBB+ credit rating. However, credit ratings do not reflect the expected recovery rate on bonds in the event of default (Chairmont Consulting, 2012, page 10) but DRPs do and subordinated bonds have unusually low recovery rates by virtue of being subordinated. In addition, subordinated bonds are relatively illiquid (Chairmont Consulting, 2012, pp. 12-13), which raises their DRP but does not lower their credit rating. Thus, subordinated bonds with a BBB+ credit rating could be expected to have unusually high DRPs for that rating category and therefore their DRPs would overestimate the overall DRP of a firm with a BBB+ credit rating. This type of problem is not measurement error, and therefore cannot be ameliorated by collecting even more data of

this type. The problem is bias (upward) and the larger the proportion of observations of this kind, the greater the bias.

In respect of the question of whether one should estimate the TAMRP exclusively using a version of the DGM, on the grounds that this is the best model for predicting market excess returns (as argued by CEG, 2014a, section 6), Lally (2014a, section 5) argues that the predictive power of a methodology in respect of market excess returns does not necessarily imply anything about the TAMRP because the predictive power may simply arise from market informational inefficiency. For example, if investors in aggregate believe that an appropriate TAMRP is 7% and price stocks on that basis but the best prediction is that the market return will exceed the tax-adjusted risk free rate by 15%, the additional 8% is not compensation for risk but a supernormal profit opportunity and therefore a regulator should estimate the TAMRP at 7% rather than 15%. Similarly, if the best prediction were that the market return would exceed the tax-adjusted risk free rate by only 1%, a regulator should again still estimate the TAMRP at 7% rather than 1%. In response to this, Grundy (2014, paras 69-71) seems to argue that the market *may* be informationally efficient and therefore the regulator's failure to use the best predictor will sometimes lead to a TAMRP estimate that is too low, in which case investors will not supply capital to the regulated firm. This is true but it is small compensation for all of the drawbacks identified by Lally (2014a, section 5) in CEG's use of this version of the DGM: that the DGM favoured by CEG (and presumably Grundy) does not correspond to the version for which the high predictive power is claimed, that neither model is tested on New Zealand data, that even researchers who find evidence of predictive ability in various models ascribe it to informational inefficiency rather than variation over time in the TAMRP, that the estimates arising from the model alleged by CEG (and presumably Grundy) to be the best predictor are so variable as to strongly suggest informational inefficiency, and that reliance upon the results of only one methodology is likely to involve a higher mean squared error in the estimate than reliance upon some average over various estimators. The fact that Grundy does not comment upon any of these points suggests that he does not dispute any of them.

Grundy (2014, para 72) also argues that a regulator's failure to use a methodology with good predictive ability requires that they "..be able to identify those times when the methodology is not accurate" and therefore that the "..regulator would have to believe that they were more skilled than professional money managers." The first claim is not correct; the Commission

does not oscillate between methodologies based upon perceptions of their predictive abilities at that time; it has used essentially the same methodology for estimating the TAMRP since it commenced making such estimates in 2001. In respect of the second claim, Grundy presents no evidence as to how professional money managers predict market returns. Even if he had, the evidence would be irrelevant if it involved predictions over a shorter period than that of concern to the Commission (the five-year regulatory cycle) and it would also be irrelevant if the success of the predictions was a manifestation of informational inefficiency rather than compensation for risk.

Grundy (2014, paras 73-83) argues that the CAPM used by the Commission assumes that all dividends have maximum imputation credits attached whilst the empirical estimates of the TAMRP presented in Lally (2014a) recognise that the average attachment rate is only 80% of the maximum. Grundy therefore proposes that the inconsistency be resolved by modifying the CAPM to recognise that the average attachment rate for credits is 80%. However a significant factor in the Commission choosing the model that it has is its widespread use in New Zealand and the support expressed for it amongst submissions received by the Commission since 2001. Consequently, if there were any inconsistency between the model and the data, the more appropriate resolution to the inconsistency would be to estimate the TAMRP using the same assumption of dividends having maximum imputation credits. Furthermore this is exactly what is done in equation (4) of Lally (2014a), and described in the paragraph that precedes that equation. Grundy's contrary belief presumably arises from the process used by Lally to obtain the market return exclusive of imputation credits. Because this is not reported by the NZSE but the 'gross' return (which includes the credits) is reported, the return exclusive of the credits is extracted from the gross return and, since the gross return includes imputation credits at the 80% rate, that 80% rate is used in deducting the credits from the gross return in order to obtain the return exclusive of the credits. So, Grundy has confused a process used to obtain the market return exclusive of the credits with the assumption used by Lally to estimate the TAMRP itself.

Grundy (2014, paras 84-86) also argues that some equity capital is supplied by foreigners, that they cannot use the credits, and therefore the model used by the Commission should also be adjusted to reflect that fact. Again, a significant factor in the Commission choosing the model that it has is its widespread use in New Zealand, which militates against Grundy's proposal. All models make assumptions that are to varying degrees not perfectly reflective of

the real world. Furthermore, if one were going to recognise the existence of foreign investors, it would not be sufficient to modify the model merely to recognise their impact on the value of imputation credits. It would also be necessary to modify the model to reflect the fact that equity markets were no longer completely segregated, and the result would be a model with parameters that were very difficult to estimate.

In respect of bankruptcy risk arising from regulatory setting of the allowed DRP in accordance with the rate prevailing at the beginning of the regulatory cycle whilst the DRP actually paid by a firm will be the trailing average, Lally (2014a, Appendix 1) concludes that the bankruptcy risk from this mis-match was trivial during the GFC. In response to this, Grundy (2014, section 10) argues that the DRP data presented by Lally is only annual observations and therefore may miss the spike in late 2008 and early 2009 arising from the GFC. This is a fair comment; as noted in Lally (2014a, Appendix 1), the data for January 2008 and January 2009 are interpolated from observations in January 2007 and January 2010. I therefore use Australian DRP data from Lally (2014b, Table 1), of 2% in January 2008 and 3% in January 2009. With this substitution, the maximum shortfall in the allowed DRP relative to the trailing average incurred by firms would have been 0.54% rather than the 0.32% shown in Lally (2014a), which represents 4.3% of the firm's NCF rather than the 2.5% in Lally (2014a). The bankruptcy risk is therefore still quite small. Grundy (2014, para 91) also refers to a DRP figure of 6% for US BBB rated bonds in January 2009. This figure is far too high for New Zealand because the US experienced a much more significant impact from the GFC than New Zealand did, involving the collapse, government takeover, bailout, or forced sale of several major financial institutions including Lehman Bros (collapse of the fourth largest US investment bank at the time and the biggest bankruptcy in US history), Freddie Mac and Fannie Mae (holders or guarantors of half of all US mortgages at the time and taken over by the US government), AIG (bailout of the largest underwriter of commercial insurance in the US at the time and the largest government bailout of a private entity in US history), Citigroup (bailout of the world's largest bank at the time), Wachovia (forced sale of the fourth largest US bank), and Washington Mutual (forced sale of the sixth largest US bank); these events had no parallel in New Zealand. However, even if the figure of 6% were substituted for the above figure of 3% for January 2009, the DRP shortfall (as defined above) would still be only 11% of the firm's NCF. Spikes in the DRP may be dramatic but, if they are short lived, they exert a much less pronounced impact on the (seven-year trailing average) paid by a firm. So, in summary, Grundy's point about the data used by Lally in 2008 and

2009 may be correct but allowance for it would not change the conclusion: regulatory setting of the DRP in accordance with the rate prevailing at the beginning of the regulatory cycle did not significantly raise the bankruptcy risk of regulated businesses during the GFC.

Grundy (2014, paras 93-94) also argues that Lally (2014a, Appendix 1) has unreasonably extrapolated the 2007 DRP of 1.3% back over the previous six years and the effect of doing so will also be to underestimate the bankruptcy risk during the GFC. However Lally (2014a, page 43) states that the DRP was stable in that pre-GFC period rather than merely extrapolates the 2007 figure back to the earlier years and it seems to me a sufficiently uncontroversial statement as not to require formal presentation of evidence. Nevertheless, Australian evidence is discussed in Lally (2014b, page 21), involving the Bloomberg BBB ten-year series from 2005-2011 (AER, 2011, Figure A.6) supplemented with data for regulated utilities provided by the QCA for the period 2000-2013. Collectively this data indicates that the Australian DRP was stable at about 1.3% from 2000-2007 and this supports the proposition that the New Zealand situation was similar.

In respect of volatility in the output prices of a regulated business under various approaches to regulatory setting of the allowed cost of debt, Lally (2014a, Appendix 2) examines this issue. In response, Grundy (2014, para 96) argues that there is some confusion in Lally (2014a, Appendix 2) between referring to use of a ten-year trailing average DRP when the data (drawn from Lally's Table 6) is in fact a seven-year trailing average. This is simply a typing error by Lally; the data are seven-year trailing averages and the reference to a ten-year trailing average should have been to a seven-year trailing average.

Grundy (2014, para 96) also seems to suggest that Lally's (2014a, Appendix 2) use of the five-year risk free rate in conjunction with the seven-year DRP to determine the allowed cost of debt is incorrect. In respect of Option B (in which the allowed cost of debt is the prevailing risk free rate plus the trailing average DRP) this is not correct; the five-year risk free rate prevailing at the beginning of the (five year) regulatory cycle is appropriate because firms are presumed under Option B to use interest rate swap contracts to convert the risk free rate component of seven-year debt into five-year debt. However, in respect of Option C (in which the allowed cost of debt is the seven-year trailing average), the seven-year trailing average for the seven-year risk-free rate should have been used rather than the five-year trailing average of the five-year risk-free rate. Correcting this error reduces the standard

deviation of output prices for Option C from \$0.92 to \$0.76. This increases the advantage of Option C over A and B, and therefore does not alter the conclusion that Option C is superior to A and B in this respect.

Grundy (2014, para 99) also argues that the DRP data used in Lally (2014a, Appendix 2) is deficient in the ways described in Grundy (2014, section 10) and described above. As noted above, I accept that the interpolated figures for 2008 and 2009 may be too low. If the figures of 2% and 3% respectively are used instead (as discussed above), there is no material impact on the results, i.e., Options A and B have similar standard deviations whilst the standard deviation for C is about half of this.

Grundy (2014, para 100) also argues that the analysis in Lally (2014a, Appendix 2) considers the results for all possible five-year regulatory cycles and therefore does not reflect the experience that a consumer of the services of any particular regulated business would face. In respect of the GFC, this is true but conducting a separate analysis for each of the five possible five-year regulatory cycles would provide no more than two data points for each of them. The standard deviation that is estimated by Lally could be interpreted as that arising from the overall experience for consumers in several similar crises in which the commencement dates for the crises varied relative to the beginning of the regulatory cycle. Alternatively, in light of Grundy's point, one could compute the average absolute change in output prices across the data for each of the three options to generate a typical impact on the consumers of the firms under each of the three options. The results are very similar to those for the standard deviations: Options A and B are similar whilst C is significantly lower (superior).

Grundy (2014, paras 101-102) also argues that a large sample would show that Option C was superior to B. However, since the sample used by Lally (2014a, Appendix 2), which is the only available one, does show this, the point of Grundy's comment is not apparent.

Grundy (2014, paras 98, 101-102) also argues that a large sample would demonstrate that Option B was superior to A, with the degree depending upon the autocorrelation in the risk-free rate, the DRP and the sum. This is not correct. As shown in equation (18) of Lally (2014a), Option A uses the prevailing values for the risk-free rate and the DRP whilst Option B uses the prevailing risk-free rate and the trailing average DRP. Thus, the (negative)

correlation between the prevailing risk-free rate and the DRP *could* completely eliminate time-series variation in output prices under Option A. In this event, Option A would be superior to B. Thus, Grundy's belief that Option B is inherently superior is wrong. The issue of which is superior is purely an empirical issue, and the sample data used by Lally (2014a) indicates that these two options are similar.

3. CEG Submissions

CEG (2014a, section 4.2) appears to argue that the target credit rating for the supply of UCLL/UBA services currently supplied by Chorus should be based solely or primarily upon Chorus's credit rating (of BBB-) because Chorus is the only current provider in New Zealand. In response, Lally (2014a, section 2.2) critiques this as a cost-based approach that would undercut incentives for Chorus to act efficiently. In response, CEG (2014b, para 116) denies that they ever favoured *sole* recourse to Chorus's credit rating, but is equivocal on the question of whether Chorus's rating should nevertheless be the *primary* source of evidence. If CEG's view is that it should be the primary source of evidence, the incentive problems raised in Lally (2014a, section 2.2) remain.

In respect of using foreign currency denominated bonds to estimate the DRP for Australian regulated businesses, Lally (2014a, section 2.1) argues that this is problematic because these bonds are not very liquid (because the holders of them typically hold till maturity) and therefore secondary market based estimates (from parties such as Bloomberg) would have low quality. In response, CEG (2014b, paras 134-135) claims that Lally has not provided any evidence in support of the claim. The same point is raised by Grundy (2014, para 3) and is addressed in section 2 above. In brief, evidence from the QTC supports the claim that these bonds are not very liquid.

In respect of using foreign currency denominated bonds to estimate the DRP for Australian regulated businesses, Lally (2014a, section 2.1) also argues that this would raise the contentious question of whether to include bank debt. In response, CEG (2014b, paras 137-142) denies that the question is contentious but then proceeds to identify a series of difficulties arising from the inclusion of bank debt including the lack of secondary market data, the question of whether the interest rate on bank debt embodies a cross-subsidy from other services provided by banks, and the question of whether bank debt is even relevant on

the grounds that it finances working capital rather than the RAB. I agree with these points and they explain why the inclusion of bank debt *would* be contentious. So, despite denying that the question of including bank debt is contentious, CEG then proceed to explain why it is.

In respect of using foreign currency denominated bonds to estimate the DRP for Australian regulated businesses, Lally (2014a, section 2.1) argues that the DRPs paid by a given New Zealand borrower may differ between local and foreign borrowing because local and foreign lenders' perceptions of the default risk of New Zealand firms may be different, premiums for the relative illiquidity of the bonds may differ across markets, and the premiums for systematic risk are likely to be different (and also notes that the same points were raised by Davis, 2011, pp. 7-9). In response, CEG (2014b, paras 143-145) observes that Davis supports use of DRP estimates from foreign bonds. However it is not in dispute that there are a range of views on the merits of using DRP estimates from foreign bonds; the point of Lally's reference to Davis was to indicate that the proposition that DRPs paid by a given New Zealand borrower may differ between local and foreign borrowing was not controversial (although the average difference will tend to zero over time). Furthermore, Davis's comment appears within a section of his paper (ibid, pp. 5-10) that is devoted to arguing that foreign borrowing costs might differ from local borrowing costs and that there is no theoretical impediment to their inclusion in a cost of debt calculation. Davis does not comment upon any of the practical difficulties in doing so including low liquidity, the weight to be placed upon foreign debt, and the issue of whether to apply the same weight to firms who may not have access to foreign borrowing (due to their limited size and/or the lack of a credit rating). Because Davis does not comment upon these potential difficulties, his comment cannot be regarded as a strong endorsement of a regulator using foreign DRP estimates in setting an allowed cost of debt.

In seeking to estimate the DRP, Lally (2014a, section 2.3) argues that the regulatory practice of estimating the average term for which firms issue debt, followed by estimating the DRP for that term, leads to an overestimate of the DRP because firms issue debt with a wide variety of terms to maturity, the spread in the terms for bonds used in the estimation process is narrower, and the DRP is a concave function of term to maturity. In response to this, CEG (2014b, paras 153-157) argues that the DRP is instead convex and refers to the analysis by Grundy (2014) on this matter. Grundy's analysis has been discussed in the previous section

above. In brief, the limited evidence presented by Grundy contradicts rather than supports his claim.

CEG (2014b, para 157) also raises the question of whether I favour a debt term benchmark framed as a distribution rather than a single point, and favours the latter on the grounds of simplicity and little loss of accuracy. I agree. The point was only raised in Lally (2014a, page 11) to demonstrate that use of a single point benchmark in conjunction with using data with a range of terms to maturity around the benchmark was favourable to regulated businesses (rather than unfavourable as earlier suggested by CEG).

In respect of estimating the DRP by curve fitting, Lally (2014a, pp. 11-12) argues that this suffers from the need to choose amongst competing curve-fitting functions and also that there is no professional consensus on the best approach. In response to this point, CEG (2014b, paras 158-159) argues that the lack of consensus reflects "the richness of the literature in this area" and is not a concern. However, if there were a professional consensus that a particular model was best, a regulator could reasonably use it. In the absence of a professional consensus, a regulator could reasonably choose from amongst many models and the results could vary widely. I think this is clearly a disadvantage from curve fitting.

In respect of whether one should estimate the DRP using curve fitting or collecting a set of DRP estimates on suitable bonds with residual terms to maturity of approximately the desired term, followed by averaging over these estimates, Lally (2014a, section 2.3) argues that curve fitting suffers from the need to obtain DRP data over a wider range of maturities, which introduces a temptation to loosen data standards (by admitting lower quality data), with the likely result of biasing the estimate of the DRP of concern. In response to this, CEG (2014b, paras 160-161) argues that poor data quality is a problem for both methods. I agree that one could use data of the same quality in both exercises. However, improving data quality by filtering out lower quality data is time consuming, especially for curve fitting because the full set of potentially usable data is much larger and therefore the filtering effort required to achieve the same data quality used when averaging over bonds with residual terms to maturity of approximately the desired term is much greater.

To illustrate this point, in using the latter method to estimate the DRP on a five-year Airport-issued bond with an A- rating, the Commerce Commission (2014, pp. 20-21) considered 19

bonds, tested them against a set of criteria, and placed primary weight on two of them (with the highest perceived data quality). By contrast, the data used in curve-fitting exercises appears to be of lower quality. For example, Bloomberg's curves include callable and subordinated bonds whilst the Commission limits its data to plain vanilla bonds (defined to exclude both callable and subordinated debt: Commerce Commission, 2012, page 30). Callable bonds have higher DRPs than otherwise identical non-callable bonds because the firm's call right is disadvantageous to lenders and the higher DRP is compensation to them, but the call feature presumably does not affect the credit rating. Thus, recourse to callable bonds to estimate the DRP on a non-callable bond of the same credit rating would lead to overestimation of the DRP on the latter bond. In respect of subordinated bonds, these have higher expected default losses than senior bonds over the same company. Thus, so long as credit ratings fully reflect all information relevant to bond pricing, then the DRP of a subordinated bond of a company that has a credit rating at the benchmark level (say BBB+) would be comparable with the DRP of a senior bond of another company with the same BBB+ credit rating. However, credit ratings do not reflect the expected recovery rate on bonds in the event of default (Chairmont Consulting, 2012, page 10) but DRPs do and subordinated bonds have unusually low recovery rates by virtue of being subordinated. In addition, subordinated bonds are relatively illiquid (Chairmont Consulting, 2012, pp. 12-13), which raises their DRP but does not lower their credit rating. Thus, subordinated bonds with a BBB+ credit rating could be expected to have unusually high DRPs for that rating category and therefore their DRPs would overestimate the overall DRP of a firm with a BBB+ credit rating.

In respect of the best regulatory policy for setting the allowed cost of debt, Lally (2014a, section 3) critiques the views of Network Strategies (2014a), who favour using the risk free rate and the DRP prevailing at the beginning of the regulatory cycle for a term equal to the regulatory cycle. In response, CEG (2014b, paras 167-178) argues that Lally's critique of the views of Network Strategies also undercuts his own preferred approach (differing from Network Strategies only in favouring a DRP for the term that firms generally borrow). However, Lally's criticism of Network Strategies relates not to their preferred position but to their apparent belief that this approach satisfies the NPV = 0 test. Lally notes that both his own preferred position, and that of Network Strategies, do not satisfy the NPV = 0 test. However, this is only one of a number of considerations in choosing a regulatory policy.

In respect of the best regulatory policy for setting the allowed cost of debt, Lally (2014a, Appendix 1) argues that the bankruptcy risk arising from setting an allowed DRP equal to the rate prevailing at the beginning of the regulatory cycle whilst the firm pays the trailing average is trivial in the sense that the worst annual shortfall in the data set examined (2001-2014, which includes the GFC) is only $0.14 per $100 of RAB, which corresponds to about 2.5% of the firm's annual NCF. In response to this point, CEG (2014b, paras 181-190) argues that Lally's data set is too short, that a longer data set (US data from 1929-2014) generates a more significant maximum shortfall of $0.48 per $100 of RAB, that the cumulative shortfall over the 86 year period is 22% of the value of the debt portfolio, and that such a significant outcome is "likely".

I agree that a longer series is better and it is almost inevitable that a much longer data set will generate a maximum annual shortfall that is larger. However, even CEG's maximum annual shortfall (3.5 times as large as that in Lally, 2014a) would only constitute 9% of the firm's NCF in that year (3.5 times that of the 2.5% in Lally, 2014a). So, even in CEG's longer time series, the maximum bankruptcy risk is not significant. Furthermore, the cumulative shortfall over the 86 year period in CEG's data set of 22% of the value of the debt portfolio (of $44 per $100 of RAB) equates to $9.80, which is $0.11 per year over 86 years. Relative to the debt portfolio of $44, this is equivalent to an average shortfall of only 25 basis points per year. With an average interest rate of (say) 6%, comprising 2% for the DRP and 4% for the risk-free rate, this average shortfall would have been only about 4% of the interest payments. By expressing the aggregate shortfall over 86 years relative to the debt portfolio at any point in time, rather than relative to the interest payments over the same period, CEG have exaggerated the significance of the aggregate shortfall. Furthermore, CEG's (2014b, Figure 2) DRP data series declines over the entire period from 2.8% to 2.2%, because of the arbitrary choice of commencement date (1929), and this almost inevitably leads to the ten-year trailing average exceeding the prevailing values over the entire series, i.e., a shortfall. By contrast, had the DRP series risen over the period (because it commenced in, say, 1955), the result would have been reversed (see CEG, 2014b, Figure 2). Furthermore, the two most extreme events in CEG's data series (the maximum shortfall and the maximum excess) are associated with the Great Depression in the 1930s and the stagflation in the 1970s respectively. Events on this scale are unlikely to recur (because policy makers have learned from these experiences) and therefore the data series used by CEG is likely to overestimate future disparities. Furthermore, CEG use a ten-year trailing average rather than the seven-year

trailing average in Lally (2014a), which has the effect of magnifying any shortfalls or excesses. Finally, it should be emphasised that the DRP is a mean-reverting process and therefore oscillates around some long-term mean. Thus, the expected value for any randomly selected prevailing DRP is the long-term mean, as is the trailing average. In accordance with the law of large numbers, the average shortfall or excess per year will therefore tend to zero over time. In summary, neither the worst annual shortfall nor the cumulative shortfall in CEG's data series is substantial and they are likely to overestimate the future experience. Thus, rather than considering that this data raises grave concerns about a regulator using the DRP prevailing at the beginning of the regulatory cycle, I think it demonstrates that the bankruptcy risks arising from doing so would not be material.

CEG (2014b, paras 191-194) also argues that Lally (2014a, Appendix 1) has unreasonably extrapolated the 2007 DRP of 1.3% back over the previous six years and the effect of doing so will also be to underestimate the bankruptcy risk during the GFC. The same point has been raised by Grundy (2014), and addressed in section 2 above (page 14). In brief, Australian evidence from that six-year period indicates that the Australian DRP was stable at about 1.3% from 2000-2007 and this supports the proposition that the New Zealand situation was similar.

CEG (2014b, paras 195-197) also argues that the DRP data presented by Lally is only annual observations and therefore may miss the spike in late 2008 and early 2009 arising from the GFC. The same point has been raised by Grundy (2014), and addressed in section 2 above (page 13). In brief, the point may be correct in principle but allowance for it does not change the conclusion that regulatory setting of the DRP in accordance with the rate prevailing at the beginning of the regulatory cycle did not significantly raise the bankruptcy risk of regulated businesses during the GFC.

In respect of the best regulatory policy for setting the allowed cost of debt, Lally (2014a, section 2.4) argues that it should not give rise to undesirable incentives, most particularly in respect of capex and new entrants to the regulated sector, and that trailing averages (of either or both the risk-free rate and the DRP) suffer from this problem. Thus, if the trailing average DRP were used by the regulator and it were significantly below the current DRP, firms would be reluctant to engage in capex or to enter the regulated sector. Alternatively, if the trailing average DRP were above the current DRP, capex and new entrants would be unjustifiably

encouraged. In response to this point, CEG (2014b, paras 198-200) argues that a trailing average is superior to use of the prevailing rate because it "accurately compensates efficient debt raising costs over time." Presumably CEG is referring to the overall experience over the life of the asset. However, if the prevailing DRP were 3% and the trailing average were 1.5% (corresponding to the most extreme case in CEG's Figure 2), a firm initially compensated at the trailing average of 1.5% but initially paying 3% would be discouraged from engaging in capex at this time. A solution to this problem has been proposed by the QTC (2013, section 2), in which the prevailing rate is initially applied to both capex and new entrants, followed by gradually adjusting the rate towards the trailing average. However, this adds to the complexity of the regime, and therefore to the ease with which it can be understood. CEG (2014b, para 209) alludes to this and claims that it is a "mechanical exercise and does not involve any material complexity." However the issue is not whether the QTC's approach is mechanical but whether it is readily comprehensible and I do not think that it is.

In respect of the best regulatory policy for setting the allowed cost of debt, Lally (2014a, section 2.4) argues that, in so far as a change in regime occurs, any transitional process to the new regime used should be simple and minimise any one-off gains or losses experienced by firms as a result of the regime change. Furthermore, Lally argues that this criterion favours Option A over B, i.e., it favours use of the prevailing DRP rather than a trailing average. In response to this point, CEG (2014b, paras 202-206) argues that there is no present regime applied to Chorus for estimating the cost of debt (because prices for the UCLL/UBA services have been set to date by international benchmarking), and therefore this concern does not arise. However, this claim is not correct. If the Commission switches from price benchmarking to a regime in which a DRP is required, Option B immediately requires a trailing average DRP for the previous seven years and efforts to estimate this are likely to be even more contentious than estimating the current DRP. In view of this problem, a transitional process towards the trailing average might be adopted, as the AER (2013) has. Furthermore, CEG (2014a, para 278) sheds no light on this matter because they do not estimate a trailing average DRP or cost of debt. By contrast, Option A does not require historical DRP data. Accordingly, in respect of this criterion, Option A is favoured.

In respect of the best regulatory policy for setting the allowed cost of debt, Lally (2014a, section 2.4) discusses an option (C) that involves the trailing average cost of debt over a period matching the average term for which benchmark firms borrow. In discussing

implementation issues, Lally (ibid) refers to an implementation difficulty with this approach arising from the fact that benchmark firms (similar but unregulated firms) would be likely to couple interest rate swap contracts with their borrowing arrangements, thereby reducing the effective term of the risk-free rate component of their cost of debt, that a regulator could not mirror this in their allowance for the cost of debt because these swap contracts would be unobservable by a regulator, and that this problem undercuts the rationale for using Option C. In response to this, CEG (2014b, paras 210-215) argues that the definition of Option C does *not* involve the regulator acting in this way and therefore the alleged implementation problem is not present.

Prima facie, CEG raises a reasonable point. There are in fact two versions of Option C, and the discussion of them in Lally (2014a, section 2.4) fails to make this quite clear. So, to be precise, I define Option C-1 to involve the trailing average cost of debt over a period matching the average term for which benchmark firms borrow, with the borrowing term defined in the literal sense of the contractual term of the debt. This Option C-1 does not experience the implementation difficulty referred to above, consistent with CEG's claim. However, the basis for choosing the period to which the trailing average relates would be to match the debt costs allowed by the regulator to those actually experienced by a benchmark firm, and such a regulatory policy would fail to do so because benchmark firms would be likely to use interest rate swap contracts to shorten the effective term of the risk-free rate component of the cost of debt. The consequence of this regulatory policy would then be to grant an excessive cost of debt allowance to regulated firms. Similarly, if unregulated firms could choose between two possible opex levels, and chose the lower-cost level in order to optimally trade-off cost against reliability, a regulator might choose the higher cost level but if it did so on the basis that it was replicating the actions of benchmark firms the claim would be false and this would undercut the merit in its choice. Equally, if unregulated firms chose the more expensive opex level but also entered into some kind of derivative contract that had the effect of essentially converting their choice to the less expensive opex level, a regulator could again not justify choosing the higher cost level (without the derivative contract) on the basis that it was replicating the actions of the unregulated firms.

A variant of Option C-1, which I label as Option C-2, would involve the trailing average cost of debt over a period matching the average term for which benchmark firms borrow but with the debt term defined as the effective term of the debt; in respect of the DRP, this is the

contractual term for the debt and in respect of the risk-free rate component it is the contractual term adjusted for the effect of the interest rate swap contracts. So, for example, if benchmark firms borrow for ten years and swap the risk-free rate component into three year debt, the effective term of the debt is ten years for the DRP and three years for the risk-free rate. This option *does* have the implementation difficulty referred to in Lally (2014a), i.e., the swap contracts are unobservable and therefore this option can't be implemented. So, amongst these two variants of Option C, only Option C-2 could be implemented leading to a cost of debt allowance that was for a longer effective term (in respect of the risk free rate component) than used by benchmark firms, and therefore an excessive cost of debt allowance would be granted.

In respect of the best regulatory policy for setting the allowed cost of debt, Lally (2014a, section 2.4) examines three options and argues (in Appendix 2) that Option C (a trailing average cost of debt) has the lowest volatility in output prices whilst Option A (regulatory use of the prevailing risk free rate and DRP) and Option B (regulatory use of the prevailing risk-free rate and the trailing average DRP) have similar volatility. In response to this, CEG (2014b, paras 216-217) argues that the data used by Lally involve assuming (without any evidence) that the DRP values for 2001-2006 are equal to that in 2007. The same point has been raised by Grundy (2014), and addressed in section 2 above (page 14). In brief, Australian evidence from that six-year period indicates that the Australian DRP was stable at about 1.3% from 2000-2007 and this supports the proposition that the New Zealand situation was similar.

In respect of the best regulatory policy for setting the allowed cost of debt, Lally (2014a, section 2.4) examines three options and all three require an assessment of the average term for which benchmark firms borrow (in the contractual sense). Conflicting evidence on this matter appears in CEG (2014a, pp. 48-50), relating to telecommunications firms worldwide and suggesting ten years, and the Commerce Commission (2010, pp. 449-451), relating to New Zealand firms and suggesting seven years. Lally (2014a, section 2.4) favoured the New Zealand evidence and therefore the figure of seven years. In response, CEG (2014b, section 5.4) acknowledges the greater relevance of local data but argues that the industry is also relevant and that this factor supports the ten year figure. In particular, the data presented by CEG (2014a, Table 9) is for telecommunications firms whilst that presented by the Commerce Commission (2010, Table H4) is for regulated firms in general. However, CEG

(2014a, Table 10) presents results for a range of industries for both New Zealand and three foreign markets, and the average New Zealand result (over those industries, which don't include telecommunications) is eight years and easily the lowest across the four markets. This suggests that New Zealand firms tend to have low debt terms regardless of industry, and therefore that CEG's foreign data on telecommunications firms is unsuitable for New Zealand.

CEG (2014b, section 5.4) also argues that the low debt term in the Commission's data reflects the small size of the firms, that a provider of UBA and UCLL services would be larger, and therefore that the Commission's New Zealand data would underestimate the appropriate debt term for a provider of UBA and UCLL services. However the Commission's data is value-weighted and therefore largely addresses this concern.

CEG (2014b, section 5.4) also argues that privately-owned firms tend to have longer debt terms, and that taking account of this would raise the estimated debt term arising from the Commission's data. However, CEG provide no evidence in support of either claim. So, in summary, CEG's claims in respect of the debt term of benchmark firms are not supported.

In respect of estimating the TAMRP, CEG (2014a, section 6.3) estimates it using a DGM in which the expected market return is estimated out to infinity and the prevailing five (or ten) year risk-free rate (net of the tax effect) then deducted. This yields an estimate of the TAMRP of 8.2% (7.9%) using the five (ten) year risk-free rate. In response, Lally (2014a, section 6.4) argues that such estimates are likely to be too high because of the inconsistent terms for the last two parameters, because changes over time in the risk free rate and the TAMRP are less than perfectly offsetting, and because the risk free rate is unusually low at the present time. In response, CEG (2014b, section 6.1) claims that the inconsistency in terms used by them actually leads to an underestimate of the TAMRP for the next five (or ten) years providing that the long-run TAMRP is 7%. This claim can be rebutted with a contrary example, as follows.

Suppose that the current ten year risk free rate is 4.6% (as in Lally, 2014a, section 6.4), which implies a tax-adjusted rate of 3.3%, the TAMRP for the next ten years is 7.2%, and therefore that the current market cost of equity over the next ten years is 10.5%. Since the risk free rate is low, the rate expected in ten years should be higher and I assume it equals the long-term

average of (about) 6%, implying a tax-adjusted rate of 4.3%.[4] In addition, since the risk free rate is expected to rise, the MRP might be expected to fall, and I therefore assume it is expected to fall to its long-term average of 7% (consistent with the scenario posited by CEG), implying a long-run market cost of equity of 11.3% (applicable from year 10). In addition, I assume an expected growth rate in dividends of 5%. Letting D denote the dividends in the most recent year, it follows that the current value of equities is as follows:

$$S = \frac{D(1.05)}{1.105} + ... + \frac{D(1.05)^{10}}{(1.105)^{10}} + \frac{E(S_{10})}{(1.105)^{10}}$$

$$= \frac{D(1.05)}{.105-.05}\left[1-\left(\frac{1.05}{1.105}\right)^{10}\right] + \frac{\left[\frac{D(1.05)^{11}}{.113-.05}\right]}{(1.105)^{10}}$$

Per \$1 of current dividends D, the current equity value is then \$17.64. By contrast, if one assumed a constant value for the market cost of equity capital k (along with a constant growth rate g) then the estimate of k would satisfy the following equation:

$$S = \frac{D(1+g)}{1+k} + \frac{D(1+g)^2}{(1+k)^2} + ... = \frac{D(1+g)}{k-g}$$

Solving this equation for k then yields

$$k = \frac{D}{S}(1+g) + g$$

Substituting the equity value of \$17.64 above (per \$1 of current dividends) into the last equation along with the expected growth rate of 5% yields an estimated cost of equity of 11%. Deduction of the current tax-adjusted risk free rate of 3.3% then yields an estimated TAMRP of 7.7%, which exceeds the true value for the next ten years of 7.2%. So, this example contradicts CEG's claim that their estimate for the TAMRP of 7.9% is likely to be too low as a result of estimating the market cost of equity out to infinity and deducting the current tax-adjusted ten-year risk free rate.

[4] Since the introduction of a moving-average inflation target in New Zealand December 1992, the ten-year risk-free rate has averaged 6.12% (data from Table B2 on the Reserve Bank website; www.rbnz.govt.nz).

In respect of estimating the TAMRP, CEG (2014a, section 6.3) estimates it using a DGM and they judge the DGM to be the best method on the basis of tests by Li et al (2013) on US data from 1977-2011. In response, Lally (2014a, section 5) notes that Li et al's analysis is performed on US rather than New Zealand data, that CEG do not test either their version of the DGM or Li et al's on New Zealand data, and that CEG are implicitly assuming that their model would generate the best results using New Zealand data merely because Li et al's model does so using US data for 1977-2011. In response, CEG (2014b, section 6.2) argues that this is an "unreasonable hurdle" to apply, that I have not cited any contrary literature, and that I have not applied this test to other estimates of the MRP that I draw upon. However, I have not argued that a predictive model must be tested on New Zealand data, only that it has not been and therefore that the model is less persuasive here than it would otherwise be. Furthermore, and in respect of contrary literature, I am not aware of any but the fact that nobody has tested the model on New Zealand data does not constitute support for it. Furthermore, and in respect of testing the predictive accuracy over market returns of other estimates of the TAMRP, I have not done so because I do not think the test very useful; if it reveals predictive accuracy, this may simply reflect market informational efficiency (and this view is even shared by proponents of the approach CEG has taken, as noted in Lally, 2014a, section 5).

In respect of the question of whether one should estimate the TAMRP exclusively using a version of the DGM, on the grounds that this is the best model for predicting market returns (as argued by CEG, 2014a, section 6), Lally (2014a, section 5) argues that the predictive power of a methodology does not necessarily imply anything about the TAMRP because the predictive power may simply arise from market informational inefficiency. For example, if investors in aggregate believe that an appropriate TAMRP is 7% and price stocks on that basis but the best prediction is that the market return will exceed the tax-adjusted risk free rate by 15%, the additional 8% is not compensation for risk but a supernormal profit opportunity and therefore a regulator should estimate the TAMRP at 7% rather than 15%. Similarly, if the best prediction were that the market return would exceed the tax-adjusted risk free rate by only 1%, a regulator should again still estimate the TAMRP at 7% rather than 1%. In response to this, CEG (2014b, section 6.3) argues that if the predictive power of the DGM is good and the regulator sets the TAMRP at a lower rate then the "typical potential investor will not supply capital to the regulated firm." Implicit in this claim is the assumption

that the predictive power of the DGM represents a good estimate of the TAMRP rather than simply informational inefficiency. Subject to the assumption, CEG's point is correct but it is small compensation for all of the drawbacks identified by Lally (2014a, section 5) in the use of this version of the DGM: that the DGM favoured by CEG does not correspond to the version for which the high predictive power is claimed, that neither model is tested on New Zealand data, that even researchers who find evidence of predictive ability in various models ascribe it to informational inefficiency rather than variation over time in the TAMRP, that the estimates arising from the model alleged by CEG to be the best predictor are so variable as to strongly suggest informational inefficiency, and that reliance upon the results of only one methodology is likely to involve a higher mean squared error in the estimate than reliance upon some average over various estimators. The fact that CEG does not comment upon any of these points suggests that they do not dispute any of them.

CEG (2014b, para 251) also argues that a regulator's failure to use a methodology with good predictive ability requires that they "..be able to identify times when the methodology is not accurate" and therefore that the "..regulator would in fact have to believe that he/she was more skilled than the money managers whose beliefs are reflected in market prices." The first claim is not correct; the Commission does not oscillate between methodologies based upon perceptions of their predictive abilities at that time; it has used essentially the same methodology for estimating the TAMRP since it commenced making such estimates in 2001. In respect of the second claim, CEG presents no evidence as to how these money managers predict market returns. Even if they had, the evidence would be irrelevant if it involved predictions over a shorter period than that of concern to the Commission (the five-year regulatory cycle) and it would also be irrelevant if the success of the predictions was a manifestation of informational inefficiency rather than compensation for risk.

In respect of the question of whether one should estimate the TAMRP exclusively using a version of the DGM, on the grounds that this is the best model for predicting market returns (as argued by CEG, 2014a, section 6), Lally (2014a, section 5) notes that the best model referred to by CEG (that of Li et al, 2013) generates estimates of the MRP that range from 0% to 15% over the period 1981-1983, from 2.6% to 9.4% over the period 2000-2002, and from 4.4% to 12.6% over the period 2007-2009 (Li et al, 2013, Figure 1), and also argues that such variations are implausibly high, and therefore that they would give rise to considerable unwarranted variation in output prices. In response to this point, CEG (2014b, section 6.4)

claims that the more important point concerns variations in the estimated market cost of equity and argues that its estimated market cost of equity is no more variable over time than that arising from the Commission's approach (CEG, 2014b, Figure 4). However, I do not agree that volatility in the estimated market cost of equity is an appropriate criterion for choosing an approach to estimating the TAMRP; variations in output prices are not a concern if they arise from variation in the observed risk-free rate and/or the true TAMRP but estimates of the TAMRP that are too variable to be plausible are undesirable even if they dampen volatility in output prices. Furthermore, CEG does not comment upon the wide range in results from Li et al (2013), as described above, despite relying upon this paper to support the claim that the DGM is the best approach to estimating the TAMRP. The lack of comment suggests that even CEG accept that the range in results from Li et al (2013) is implausibly large, that this damages the credibility of the estimates, and therefore damages the credibility of CEG's sole reliance upon a similar methodology.

In respect of the question of whether one should estimate the TAMRP exclusively using a version of the DGM, Lally (2014a, section 5) favours use of estimates from a variety of methodologies on the grounds that this produces an estimator with a lower MSE. In response to this, CEG (2014b, para 258) argues that Lally places too much emphasis on historical averages. However, of the five estimators used by Lally (2014a, section 6), only two rely entirely on historical averages (Ibbotson and Siegel version 1), two are entirely forward looking (DGM and survey evidence), and one has information of both types (Siegel version 2). Thus, in effect, 50% weight has been given to historical averages and 50% to forward-looking approaches.

CEG (2014b, para 259) also argues that Lally (2014a) does not provide any evidence that historical averages are good predictors of the TAMRP whilst applying that standard to DGM estimates. However, Lally (2014a) does not require that DGM estimates (or any other type) be good predictors of the TAMRP because the TAMRP is not observable. Instead, the predictive power issue was raised by CEG (2014a), who asserted that their DGM estimator was a good predictor. However, the most that can be said of it is that it is a good predictor of market excess returns rather than the TAMRP (and these are different phenomena).

CEG (2014b, para 261) also argues that Lally's (2014a, section 6) approach does not use information from five different estimates but instead places "full weight" on the median

estimate. This claim is not correct; the only circumstance in which one estimator receives full weight is that in which there is only one estimator and the fact that a median of five estimator will be one of the five estimators does not imply that it has received 100% weight. Furthermore, for both five and ten-year terms, the mean is almost identical to the median (6.8% v 6.7% for five years and 7% v 6.9% for ten years). Thus, use of the mean instead of the median would not have changed the (rounded) result of 7%.

CEG (2014b, para 263) also argues that Lally's (2014a, section 6) approach does not use information from five different estimates but instead places "almost full weight" on estimates that involve historical averaging. This claim contradicts CEG's claim described in the previous paragraph. It is also incorrect; as noted above, the five estimators used by Lally (2014a, section 6) are equally split between historical averaging and forward-looking estimators.

Lally (2014a, section 5) argues that an even better goal than choosing an estimator with minimal MSE for the TAMRP over the next regulatory cycle would be to choose an estimator with minimal MSE for the TAMRP over the *life* of the regulated assets. In response to this, CEG (2014b, para 264) argues that such an approach is inconsistent with Lally's criticism of CEG's DGM for estimating the market cost of equity out to infinity. However, Lally's criticism of CEG's DGM is not that it estimates the market cost of equity out to infinity but that it inconsistently deducts the current five or ten-year risk free rate (Lally, 2014a, section 6.4).

In seeking to estimate the TAMRP, Lally (2014a, section 6.5) draws upon survey evidence from Fernandez et al (2013). CEG (2014b, section 6.5.2) argues that this survey evidence is unsatisfactory in various ways including the future period to which the survey question relates being unclear and the small number of respondents. I agree with these points but the practical consequences appear to be minimal, in the sense that a better survey does not seem to be available and disregarding it would not materially change the median results in Lally (2014a, Table 4, Table 5).

4. Network Strategies Submissions

Network Strategies (2014a, section 2.3) notes that the current TSLRIC process requires use of the costs that would be faced by a new entrant, and argues that this precludes use of a trailing average DRP or cost of debt. However, the definition of TSLRIC in the 2001 Telecommunications Act refers to "forward-looking costs in the long run", and this is rather equivocal (although general practice in applying TSLRIC is to define costs as those of a new entrant). Furthermore, the general practice in applying TSLRIC is to define costs as those of an efficient operator, and a trailing average DRP certainly corresponds to the DRP that would be incurred by an efficiently operating firm (which would use staggered borrowing). So, I do not think that a trailing average DRP or total cost of debt is clearly precluded by the wording of the Act. Accordingly, I think that the merits or otherwise of a trailing average should be assessed against the criteria presented in Lally (2014a, section 2.4), and this suggests that regulatory use of the prevailing cost of debt is superior to use of a trailing average. In any event, even if "forward-looking" were interpreted to preclude a trailing average DRP or total cost of debt, this would merely reinforce the conclusion already reached by me.

In estimating the TAMRP, Lally (2014a, section 6) uses averages over foreign data. In response, Network Strategies (2014a, section 2.5) argues that medians should be used instead so as to dampen the effect of outliers. This is reasonable point. However, it does not change the rounded estimate of 7% for the TAMRP.

In estimating the TAMRP for a five-year period, Lally (2014a, section 6.7) makes an adjustment for the difference between five and ten year risk free rates of .08%. Network Strategies (2014a, section 2.5) claims that a differential of 0.8% was mistakenly used instead in the calculations. This is correct. However, as noted by Network Strategies, the impact of this error is trivial.

In estimating the TAMRP using the Ibbotson methodology, Lally (2014a, section 6.2) updates an estimate for 1931-2002 by using data for 2003-2013, by combining the two estimates with weights proportional to the number of years underlying each estimate. In response, Network Strategies (2014a, section 2.5) argues that this "introduces a bias in favour of older data." I do not agree. The first estimate, using data from 1931-2002, equally weights each annual observation and the process for updating it (weighting in proportion to the number of years) ensures that this equal weighting of annual observations is preserved. Thus, there is no bias in favour of the older data.

In estimating the TAMRP for a five-year period, Lally (2014a, section 6.7) draws upon US data from 1953-1985 on the differential between five and ten year rates in order to estimate the time series of New Zealand five-year risk-free rates prior to 1985. The implicit assumption here is that the differential will be similar across the two markets. In response, Network Strategies (2014a, section 2.5) argues that there is no firm basis for this assumption, and that only New Zealand data should be used or that data from other foreign markets should be used. However, as noted in Lally (2014a, section 6.7), New Zealand five-year risk-free rate data is not available prior to 1985. Thus, if only New Zealand data were to be used, it would be necessary to extrapolate the New Zealand differential since 1985 to the earlier period. Since the New Zealand differential since 1985 is 0.07%, compared to the US differential of 0.08% pre 1985, the results would be virtually identical to those obtained by Lally (2014a, section 6.7). Furthermore, and in respect of using data from foreign markets other than the US, Network Strategies' belief that there is no firm basis for using the US data would presumably apply equally to any other choice of foreign markets. Furthermore, in using foreign data, a crucial requirement is that both five and ten year rates are available for a protracted period prior to 1985 and the US satisfies this requirement. Network Strategies does not identify any other country that does.

5. Cross-Submissions

Network Strategies (2014a, section 2.3) notes that the current TSLRIC process requires use of the costs that would be faced by a new entrant, and argues that this precludes use of a trailing average DRP or cost of debt. In response, CEG (2014c, section 2.1) argues that a new entrant would take several years to build a national telecommunications network and therefore would undertake borrowing progressively over that period, leading to a trailing average being incurred. I have no view on the time taken to build such a network, and therefore on the merits of CEG's argument. However, at best, CEG's argument merely rebuts the view that a TSLRIC framework implies that the current cost of debt would have to be used rather than a trailing average. Since I favour use of the current cost for different reasons, this issue is either irrelevant to my conclusions or it reinforces them.

CEG (2014b, Figure 2) use US DRP data from 1929-2014 to examine variations between the prevailing and trailing average DRP. In response, Network Strategies (2014b, section 4)

argues that this data is unsuitable because it is unlikely that the efficient debt practices of firms in the 1930s would be the same as those today. However this claim about debt practices in the 1930s is irrelevant to the merits of the data because its use by CEG is not premised on that claim; its validity requires only that the volatility in the data itself be indicative of future volatility.

In relation to this CEG analysis, Network Strategies (2014b, section 4) also argues that the results from CEG's analysis will depend upon the choice of the first year, and refer to 1932 instead of 1929. This is true, but second order. Whether the data series commences in 1929 or 1932, the DRP will still be higher than at the end of the series (2014), and therefore a trailing average will on average exceed the prevailing rate. The key point is whether the DRP at the commencement date is above or below that at the end of the series, as discussed in section 3 above.

Grundy (2014, paras 32-51) argues that a regulatory regime should allow for costs of financial distress (CFD), that the Commission fails to do so, and therefore does not provide a sufficient revenue allowance. In response, Network Strategies (2014b, section 5) argues that allowance for such costs is not necessary for an efficient operator (as per TSLRIC), that it should not in any event rely on one estimate from Grundy, and that Grundy does not explain how this would be implemented. The first of these claims is wrong (even an efficient operator is exposed to the risk of financial distress) and the other two are second order. The principal problem with Grundy's claim is discussed in section 2 above. In brief, a substantial part of the CFD are already reflected in the promised yield on debt, the rest are much less relevant to regulated businesses than unregulated businesses, and the Commission is likely to overcompensate shareholders because the promised yield on debt also includes an allowance for the value of the default option possessed by equity holders and its inclusion in the cost of debt used by a regulator is unwarranted (because it is a mere transfer between debt holders and equity holders and therefore does not affect the true WACC).

Network Strategies (2014b, section 6) argues that CEG's DGM estimate of the TAMRP based upon an estimate of the market cost of equity out to infinity is inconsistent with CEG's belief that the TAMRP does change over time. This is a reasonable point but the chief problem in CEG's estimate of the market cost of equity out to infinity is that it deducts the current five or ten year tax-adjusted risk-free rate from it, and this inconsistency in terms is

likely to induce an overestimate in the TAMRP under present conditions (as discussed in section 3 above).

6. Conclusions

This paper has reviewed recent submissions and cross-submissions to the Commerce Commission on the cost of capital for UCLL and UBA services, in response to an earlier report by me. I do not agree with most of the points raised. In respect of the points I do agree with, none if allowed for would change the conclusions in my earlier report. These latter points are as follows.

Firstly, both Grundy and CEG argue that the DRP data series used by me for assessing bankruptcy risk and output price volatility under various regulatory approaches to the cost of debt is too short, and the 2008 and 2009 values in my series (arising from interpolating over data for 2007 and 2010) are likely to underestimate the spike induced by the GSF. CEG also presents and uses a US series that is free of both problems. I consider that their series is superior in both respects, but use of it does not change the conclusions reached: that bankruptcy risk for firms who receive the prevailing rather than the trailing average DRP is not significant, and output price volatility is significantly lower using a trailing average cost of debt than doing otherwise.

Secondly, CEG argues that the survey evidence cited by Lally in estimating the TAMRP is unsatisfactory in various ways including the future period to which the survey question relates being unclear and the small number of respondents. I agree with these points but the practical consequences appear to be minimal, in the sense that a better survey does not seem to be available and disregarding it would not change the rounded estimate for the TAMRP of 7%.

Thirdly, in estimating the TAMRP using averages over foreign data, Network Strategies argues that medians should be used rather than means so as to dampen the effect of outliers. This is reasonable point but it does not change the rounded estimate for the TAMRP of 7%.

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