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SURREBUTTAL TESTIMONY

OF

ROGER A. MORIN

Before the Missouri Public Service Commission

On behalf of Union Electric Co.

d/b/a AmerenUE

Case No. ER-2008-0318

ROE Considerations

November 2008

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I. INTRODUCTION

Q. Please state your name.

A. Dr. Roger A. Morin.

Q. Are you the same Dr. Roger A. Morin who provided prefiled direct testimony and rebuttal testimony in this proceeding on behalf of Union Electric Company d/b/a AmerenUE ("UE" or "Company")?

A. Yes, I am.

Q. What is the purpose of your surrebuttal testimony?

A. This surrebuttal testimony responds to the rebuttal testimonies of Mr. Stephen G. Hill, on behalf of the Staff of the Missouri Public Service Commission and Mr. Michael P. Gorman on behalf of Missouri Industrial Energy Consumers.

Q. Can you describe how your surrebuttal testimony is organized?

A. My surrebuttal testimony is organized in two sections, corresponding to each of the aforementioned individuals.

Q. Do you have a general observation regarding both Mr. Hill's and Mr. Gorman's rebuttal testimonies?

A. Yes, I do. I was very surprised that both Mr. Hill and Mr. Gorman are totally silent on the ongoing financial crisis and its impact on utility cost of capital. Given the devastating impact of the crisis on stock prices, the rising costs of debt capital, the increase in discounted cash flow (DCF) estimates of return on equity (ROE) as a result of falling stock prices, and record high yield spreads, Messrs. Hill and Gorman should have revised their ROE recommendation upward.

II. REPLY TO MR. HILL

Q. Has Mr. Hill made any arguments in his rebuttal testimony that would cause you to alter your testimony and/or any of your rebuttal comments?

A. He has not. I shall now respond to Mr. Hill's arguments roughly in the same order he has presented them, first with respect to the Capital Asset Pricing Model (CAPM) methodology, second with respect to the Risk Premium methodology, and thirdly with respect to the DCF methodology.

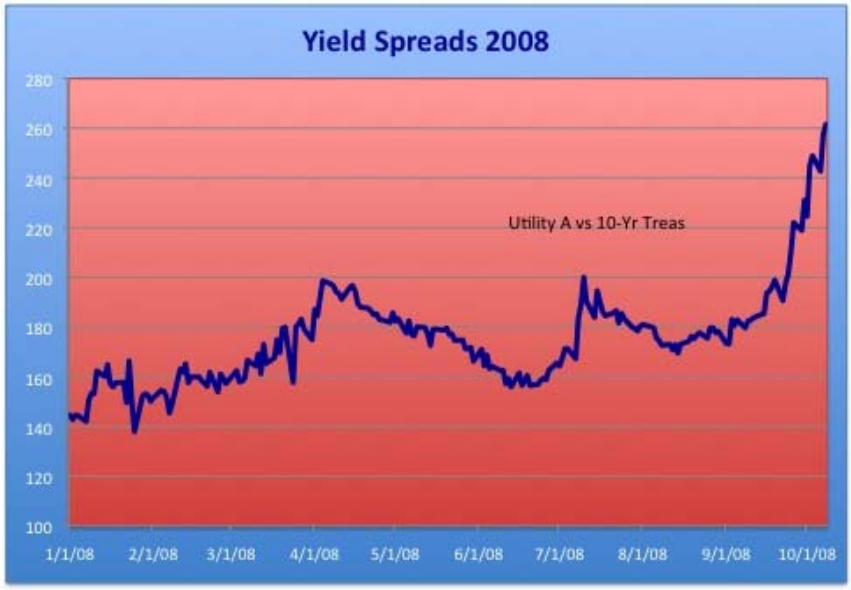
Q. Do you agree with Mr. Hill that interest rates have fallen since you prepared your direct testimony?

A. Yes, I do. On page 6 of his rebuttal testimony, Mr. Hill argues that interest rates have fallen by 15 basis points since I prepared my direct testimony, and that my CAPM estimates are therefore 15 basis points too high. While I agree that government interest rates have decreased slightly since I prepared my direct testimony, the cost of corporate debt and the cost of equity for electric utilities have increased, as evidenced by the DCF results for electric utilities that have increased significantly by some 100 basis points in response to lower stock prices (higher dividend yields) following the financial crisis.

Capital markets are currently in a state of turmoil. Borrowers are now forced to compete in a market virtually devoid of funds. As a result, the cost of money for corporations has increased, and new debt/stock issues are almost non-existent. Accessibility to the commercial paper market has become severely reduced, even for highly-rated companies. The debt markets have witnessed record high yield spreads (the incremental yield over Treasury rates needed to

issue debt) and a more severe differentiation between the spreads charged to companies with different credit ratings.

To illustrate, the chart below depicts the rising and record high spreads in recent months for utilities rated single A. Whereas utilities were borrowing money at some 180 basis points over Treasuries, the current spread is 260 basis points, an increase of 80 basis points, virtually the same upward increase as has been observed in the DCF estimates. In terms of market accessibility, the new issuance debt markets and the equity new issuance markets are all but closed. In a nutshell, there is a fundamental structural upward shift in risk aversion as capital markets are re-pricing risk, and capital has become, and will continue to be, more expensive for all market participants.



I therefore believe that my ROE recommendation is actually conservative in the current capital market environment. It would not be unreasonable to increase my ROE recommendation by at least another 25 basis points in light of the ongoing financial crisis.

Q. Do you agree with Mr. Hill that betas have fallen since you prepared your direct testimony?

A. Yes, I do. On page 7 of his rebuttal testimony, Mr. Hill points out that betas have fallen from the 0.87 level to the 0.80 level since I prepared my direct testimony in response to lower risk perceptions of the electric utility industry by investors, at least prior to the current financial crisis. However, I note that betas are estimated based on five-year historical periods and that the impact of the ongoing financial crisis is not yet captured in the five-year historical betas. As I mentioned above, there is a fundamental structural upward shift in risk aversion as capital markets are re-pricing risk, and capital has become, and will continue to be, more expensive for all market participants.

Q. Mr. Hill criticizes your market risk premium (MRP) estimate as inconsistent with the Ibbotson 2003 findings. Do you agree?

A. No, I do not. On page 8 of his rebuttal testimony, Mr. Hill argues that while I have used the Ibbotson MRP of 7.1%, a 2003 paper published by the same source – Ibbotson -- indicates a MRP of 5.9%. Mr. Hill fails to mention that in the 2008 edition of the Valuation Yearbook, Ibbotson (now Morningstar) in Appendix A (Table A-1 p. 2) calculates what they call "Long Horizon Equity Risk Premium" and arrive at 7.1% (for the period 1926-2007), the same estimate I have relied upon. As I discussed extensively in my rebuttal, Mr. Hill has selectively chosen published studies that purport to show that the historical MRP published by Morningstar is high. Mr. Hill's assessment of the research on the MRP is incomplete, inaccurate, and misleading.

Q. Is there a conflict of logic in your testimony about the application of the DCF method to the aggregate equity market?

A. No, not at all. On page 9 of his rebuttal testimony, lines 9-14, Mr. Hill claims that if the DCF is good enough to apply to the overall equity market, it is good enough to apply to utility stocks. I do not disagree with that position. I did indeed apply the DCF model to utility stocks – four out of eight results shown in my summary of results on page 68 of my direct testimony are DCF-based.

Nevertheless, one would think that the application of the DCF model to the market index as a whole, consisting of several hundred stocks, would provide at least as precise an estimate of the expected market return as the application of the DCF model to a handful of utility stocks would. The core assumptions of the DCF model are much more likely to be satisfied when applying the DCF model to a broad market aggregate than to a specific industry.

Q. Are historical returns autocorrelated?

A. No, they are not. Mr. Hill erroneously argues on page 12 of his rebuttal testimony- and without support - that consideration of the arithmetic mean is improper when there is negative autocorrelation in the historical return data.

An examination of historical market risk premiums reveals that the market risk premium is random with no observable pattern. To the extent that the estimated historical equity risk premium follows what is known in statistics as a random walk, one should expect the equity risk premium to remain at its historical mean. Therefore, the best estimate of the future risk premium is the historical mean.

Moreover, Morningstar - a resource on which Mr. Hill relies - finds no evidence that the market price of risk or the amount of risk in common stocks has changed over time:

> *Our own empirical evidence suggests that the yearly difference between the stock market total return and the U.S. Treasury bond income return in any particular year is random. . . .*
> (Morningstar, *Stocks Bonds Bills and Inflation, Valuation Edition 2008* at page 80).

In statistical parlance, there is no significant serial correlation in successive annual market risk premiums, that is, no trend. In short, Mr. Hill's claim of negative autocorrelation is unsupported.

Q. How do you respond to Mr. Hill's reference to a PowerPoint slide presented by Professor Marston to buttress his claim that the prospective market risk premium has declined relative to historical measures?

A. On pages 14-15 of his rebuttal testimony, Mr. Hill argues that the reference to the Harris-Marston research in my direct testimony on the magnitude of the prospective MRP, namely 7.2%, has been superseded by a PowerPoint slide in a presentation made by Professor Marston in 2007. Mr. Hill reproduces the slide on page 15 of his rebuttal testimony.

Reliance on a PowerPoint slide to support Mr. Hill's contention that the MRP has shrunk in recent years does not provide the kind of analysis that would allow this Commission to make a reasonable determination of the appropriate MRP. A PowerPoint slide is a highly questionable source of information in assessing an appropriate risk premium for a regulated utility and in gauging the academic state of the art in the field of finance. Moreover, I am not aware that the

Harris-Marston updated findings have been published in any peer-reviewed academic journal.

A. EMPIRICAL CAPM

Q. Please comment on Mr. Hill's assessment of the Empirical CAPM used in your testimony.

A. On pages 16-20 of his rebuttal testimony, Mr. Hill erroneously asserts that use of "adjusted" betas with an Empirical CAPM analysis "double-counts the effect of changing the slope of the capital market line." Contrary to such suggestion, the Empirical CAPM is not an adjustment (increase or decrease) in beta. Instead, the Empirical CAPM is a formal recognition of the fact that empirical evidence demonstrates that the observed risk-return tradeoff is flatter than predicted by the CAPM.

The Empirical CAPM and the use of adjusted betas comprise two separate features of asset pricing. Assuming *arguendo* a company's beta is estimated accurately, the CAPM will still understate the return for low-beta stocks. Furthermore, if a company's beta is understated, the Empirical CAPM will also understate the return for low-beta stocks. Both adjustments are necessary.

The graph on page 44 of my direct testimony demonstrates that the Empirical CAPM is a return (vertical axis) adjustment and not a beta (horizontal axis) adjustment. Moreover, the use of adjusted betas compensates for interest rate sensitivity of utility stocks not captured by unadjusted betas.

With respect to the empirical validity of the plain vanilla CAPM, empirical studies of the CAPM to determine to what extent security returns and

betas are related in the manner predicted by the CAPM have supported the conclusion that (i) beta is related to security returns, (ii) the risk-return tradeoff is positive, and (iii) the relationship is linear. The contradictory finding is that the risk-return tradeoff is not as steeply sloped as predicted by CAPM. In other words, low-beta securities earn returns somewhat higher than the CAPM would predict, and high-beta securities earn returns somewhat less the CAPM would predict.

In sum, a plain vanilla CAPM will understate the return required for low-beta securities and overstate the return required for high-beta securities. The Empirical CAPM refines the plain vanilla CAPM to account for this phenomenon.

B. HISTORICAL RISK PREMIUM

Q. How do you respond to Mr. Hill's criticism of your historical risk premium method?

A. On pages 20-23 of his rebuttal testimony, Mr. Hill criticizes my risk premium method by arguing that (i) because risk premium analyses look backward in time, they assume "past is prologue" and (ii) implicit in the use of an average historical return premium of equities over debt is the assumption that the risk premium is constant over time. Mr. Hill's first criticism is unwarranted. My direct testimony employs returns realized over long time periods rather than returns realized over more recent time periods. Realized returns can vary substantially from prospective returns anticipated by investors, especially when measured over shorter periods. A risk premium study should consider the longest possible period for which data are available. Short-run periods during which investors earn a

lower risk premium than they expected are offset by short-run periods during which investors earn a higher risk premium than they expected. Only over long periods will investor return expectations and realizations converge. The use of the entire study period in estimating the appropriate market risk premium minimizes subjective judgment and encompasses many diverse regimes of inflation, interest rate cycles, and economic cycles.

Mr. Hill's second concern is also unwarranted. To the extent that historical risk premium estimates follow what is known in statistics as a "random walk," one should expect the equity risk premium to remain at its historical mean. In other words, the best estimate of the future risk premium is the mean historical risk premium. As explained in my direct testimony, I found no evidence that the market price of risk or the amount of risk in common stocks has changed over time. Because no significant serial correlation exists in successive market risk premiums from year to year, it is reasonable to assume that the mean historical risk premium will remain stable in the future.

Q. Are Mr. Hill's criticisms of your risk premium method supported by recent financial literature?

A. No. Mr. Hill attempts to support its critique by quoting an article published by Eugene Brigham in 1985 and an article published by Charles Phillips in 1993. *See* Mr. Hill's rebuttal testimony at pages 22, lines 17-21. Mr. Hill, however, neglects to mention more recent publications with respect to risk premium studies. Indeed, the most recent edition of Eugene Brigham's textbook *Financial*

Management: Theory and Practice[1] strongly recommends the use of risk premium studies similar to those used in my direct testimony. Furthermore, the most recent edition of Mr. Brigham's textbook describe the risk premium approach in much the same way as that applied in my direct testimony.

Q. Are risk premium methods widely used?

A. Yes. Risk premium analyses are widely used by analysts and investors, as well as cost of capital witnesses in regulatory proceedings. Most college-level corporate finance and/or investment management texts contain detailed conceptual and empirical discussion of the risk premium approach. Indeed, the risk premium method is typically recommended as one of the three leading methods of estimating the cost of capital.[2] Risk premium analysis techniques are also widespread in investment community reports. Professional certified financial analysts are well versed in the use of this method, and Mr. Hill's criticism is unwarranted.

C. DCF DIVIDEND YIELD

Q. Is Mr. Hill's criticism that you multiplied the spot dividend yield by one plus the expected growth rate (1 + g) warranted?

A. No. The plain vanilla annual DCF model ignores the time value of quarterly dividend payments and assumes dividends are paid once a year at the end of the year. Because the appropriate dividend to use in a DCF model is the prospective dividend for all companies that have positive growth rate forecasts, the dividend

[1] *See* Eugene Brigham & Michael Ehrhardt, *Financial Management: Theory and Practice*, (11th ed. 2005).

[2] *See, e.g.*, Eugene Brigham & Michael Ehrhardt, *Financial Management: Theory and Practice*, (11th ed. 2005).

for all companies should be increased by the (1 + g) factor. Multiplying the spot dividend yield by (1 + g) is actually a conservative attempt to capture the reality of quarterly dividend payments and understates the expected return on equity. Use of this method is conservative in the sense that the annual DCF model ignores the more frequent compounding of quarterly dividends.

Q. Does Mr. Hill multiply the spot dividend yield by one plus the expected growth rate (1 + g)?

A. Yes. Mr. Hill multiplies the spot dividend yield by one plus the expected growth rate (1 + g) for those companies expected to raise their quarterly dividends in the second quarter of calendar year 2008.

Q. Did you double-count the expected dividend yield for growth?

A. No. Contrary to assertions of Mr. Hill at page 26 of his rebuttal testimony, lines 4-12, I did not overstate the dividend yield by double-counting the dividend increase. This is because I used the "current dividend yield" as defined by Value Line in the Value Line Investment Analyzer software and then grossed up the current dividend yield to produce the expected dividend yield required by the DCF model.

D. DCF GROWTH FORECASTS

Q. Is reliance on analysts' earnings growth forecasts in the DCF model problematic?

A. No, it is not. On page 26 of his rebuttal testimony, lines 20-23, Mr. Hill erroneously asserts as follows with respect to my exclusive use of analysts' earnings growth forecasts in the DCF:

> *exclusive reliance on earnings growth, absent any examination of the underlying fundamentals of long-run growth, can lead to inaccurate equity cost estimates. For example, reliance on projected earnings growth in a situation in which projected earnings were expected to recover from reduced levels would include (in any DCF estimate) the assumption that equity returns will increase at the same exaggerated rate every five years into the indefinite future.*

In other words, the intermediate growth rate in dividends cannot equal the long-term growth rate when the dividend payout ratio is expected to change because projected dividend growth and earnings growth must adjust to the changing payout ratio. This "problem" is not unique to analysts' earnings growth forecasts and is also inherent in the use of historical growth rates to forecast growth rates.

Reliance on "near-term" dividend growth is improper because it is expected that energy utilities will continue to lower their dividend payout ratio over the next several years in response to increased business risk. Therefore, earnings and dividends are not expected to grow at the same rate in the future. Mr. Hill has conveniently supplied growth data on Table II of his rebuttal testimony, page 29. The growth rate data clearly demonstrate this phenomenon

because projected utility dividend growth rate forecasts (4.5%) are less than the earnings growth rate forecast (6.0%). As discussed in my direct testimony, I used consensus analysts' earnings growth forecasts in the DCF model to mitigate potential bias—an approach supported by empirical literature.

Q. Is your growth rate analysis "mechanistic in that it simply plugs selected projected data into a formula to produce a growth rate with no underlying analysis of either the historical or projected growth rate fundamentals," as Mr. Hill suggests?

A. No, it is not. Contrary to this statement on page 26 of Mr. Hill's rebuttal testimony, lines 13-16, my direct testimony devotes several pages (pages 53-58) to an analysis of historical growth rates and analysts' growth forecasts. Given this analysis, Mr. Hill's statement that I undertook "no underlying analysis of either the historical or projected growth rate fundamentals" is patently false.

Mr. Hill continues on page 26 lines 16-17 to state that "*Dr. Morin, in his own published work, warns against this type of analysis*." This is a blatant example of Mr. Hill selectively citing materials out of context. The passage cited by Mr. Hill immediately precedes the following section of my book:

> *A note of caution is also necessary when dealing with historical growth rates and their use in the DCF model. Historical growth rates can be downward-biased by the impact of diversification and restructuring activities and by the impact of abnormal weather patterns in the case of energy utilities. Acquisitions, start-up expenses, and front-end capital investments associated with diversification and restructuring efforts, and unfavorable weather patterns can retard and dilute historical earnings growth, and such growth is not representative of a company's long-term growth potential. Therefore, caution must be exercised when applying any of the growth estimating techniques directly to recent historical utility company data.*

> *Given a dramatic change in a utility's operating environment, the need to be forward-looking is apparent. Historically-based measures of risk and growth can be downward-biased in assessing present circumstances . . . . The fundamental risks and growth prospects of electric utilities are also changing rapidly following the passage of the Energy Bill in 1993. These shifts in growth prospects take some time before they are fully reflected in the historical growth rates. Hence, backward-looking growth and statistical analysis may fail to fully reflect the fact that the risks and growth prospects of utilities have escalated, and may only provide limited evidence that the risk and the cost of capital to these utilities have increased. Of course, the converse may also be true under certain circumstances.*

Roger A. Morin, *Regulatory Finance: Utilities' Cost of Capital* at pages 237-38 (1st ed. 1994) (emphasis added).

Indeed, the same chapter contains an entire section that comprehensively discusses the hazards of relying on historical growth rates.

Q. What does the published academic literature say on the subject of analysts' growth rate forecasts in the DCF model?

A. As I discussed in my rebuttal testimony, published studies in the academic literature demonstrate that (i) analysts' growth rate forecasts are reasonable indicators of investor expectations and (ii) investors rely on such forecasts.

Q. Mr. Hill criticizes your DCF analysis because it relies on earnings growth projections and he believes that such forecasts are overly optimistic. How do you respond?

A. On page 27 of his rebuttal testimony, Mr. Hill denounces the use of financial analysts' earnings forecasts on the grounds that such forecasts are overly-optimistic. I disagree, at least for utility stocks. Using virtually all publicly available analyst earnings forecasts for a large sample of companies (over 23,000 individual forecasts by 100 analyst firms), Lys and Sohn show that stock returns

respond to individual analyst earnings forecasts, even when they are closely preceded by earnings forecasts made by other analysts or by corporate accounting disclosures.[3] Using actual and IBES data from 1982-1995, Easterwood and Nutt regress the analysts' forecast errors against either historical earnings changes or analysts' forecasting errors in the prior years.[4] Results show that analysts tend to under-react to negative earnings information, but overreact to positive earnings information.

The more recent studies provide evidence that analysts make biased forecasts and misinterpret the impact of new information.[5] For example, several studies in the early 1990s suggest that analysts either systematically underreact or overreact to new information. Easterwood and Nutt discriminate between these different reactions and reported that analysts underreact to negative information, but overreact to positive information. The recent studies do not necessarily contradict the earlier literature. The earlier research focused on whether analysts' earnings forecasts are better at forecasting future earnings than historical averages, whereas the recent literature investigates whether the analysts' earnings forecasts are unbiased estimates of future earnings. It is possible that even if the analysts' forecasts are biased, they are still closer to future earnings than the

[3] Thomas Lys & Sungkyu Sohn, "The Association Between Revisions of Financial Analysts' Earnings Forecasts and Security Price Changes," *Journal of Accounting and Economics* 13, 341-363 (1990).

[4] John Easterwood & Stacey Nutt, "Inefficiency in Analysts' Earnings Forecasts: Systematic Misreaction or Systematic Optimism?" *The Journal of Finance* 54: 1777-1797 (1999).

[5] Other relevant papers corroborating the superiority of analysts forecasts as predictors of future returns versus historical growth rates include: Dan Fried & Dov Givoly, "Financial Analysts Forecasts of Earnings: A Better Surrogate for Earning Expectations," *Journal of Accounting and Econometrics* 85-107 (1982); R. Charles Moyer, *et al*., "The Accuracy of Long-Term Earnings Forecasts in the Electric Utility Industry" *International Journal of Forecasting*, 1, 241-252 (1985); and David Gordon, "Choice Among Methods of Estimating Share Yield," *Journal of Portfolio Management* 15, 50-55 (1989).

historical averages, although this hypothesis has not been tested in the recent studies. One way to assess the concern that analysts' forecasts may be biased upward is to incorporate into the analysis the growth forecasts of independent research firms, such as Value Line, in addition to the analyst consensus forecast. Unlike investment banking firms and stock brokerage firms, independent research firms such as Value Line have no incentive to distort earnings growth estimates in order to bolster interest in common stocks.

Mr. Hill argues that analysts tend to forecast earnings growth rates that exceed those actually achieved and that this optimism biases the DCF results upward. The magnitude of the optimism bias for large rate-regulated companies in stable segments of an industry is likely to be very small. Empirically, the severity of the optimism problem is unclear for regulated utilities, if a problem exists at all. It is interesting to note that Value Line forecasts for utility companies made by independent analysts with no incentive for over- or understating growth forecasts are not materially different from those published by analysts in security firms with incentives not based on forecast accuracy, and may in fact be more robust.

Q. On page 31 of his rebuttal testimony, Mr. Hill states that you appear to de-emphasize your reliance on the DCF. Is Mr. Hill correct?

A. No, he is not. As previously stated, four of my cost of equity estimates out of eight, that is one half, are DCF-based. I would hardly characterize this procedure as de-emphasizing the DCF.

Q. Is Mr. Hill correct in his statement that a study of regulatory commission equity cost estimation methods in a NARUC survey found that nearly every commission lists DCF as a methodology on which it relied?

A. Yes, that is true, but what Mr. Hill failed to mention is that a vast majority of regulatory commissions do not rely **solely** on the DCF in setting the allowed rate of return on common equity. Instead, they utilize a variety of methods. Mr. Hill simply did not fairly represent the results of this survey. The principal finding of this survey is that the vast majority of regulators rely on all the evidence presented and do not necessarily subscribe to any one methodology.

E. MARKET-TO-BOOK (M/B) RATIOS

Q. Is Mr. Hill correct in his claims that there are inconsistencies in your published works regarding the DCF model and Market-to-Book ratios?

A. No. In his rebuttal testimony, on page 32, line 20 to page 33, line 2 and lines 8-9, Mr. Hill argues that the 1984 edition of my book did not criticize the ability of the DCF model to accurately estimate the cost of equity depending on the M/B ratio of utilities. Similarly, Mr. Hill asserts the following:

> *Dr. Morin's first text on the cost of capital, Utilities' Cost of Capital, was published in 1984, and was conceived and written during a time period for utilities in which interest rates were very high and market prices were generally below book value. …….There is no indication in Dr. Morin's 1984 text that when market prices are below book value (as they were at that time), the DCF overstates the cost of equity (as is now Dr. Morin's claim).*

Mr. Hill fails to recognize, however, that the ability of the DCF model to estimate the cost of equity accurately depending on the M/B ratio of utilities was simply not an issue for utilities a quarter century ago because utilities were

trading at market prices very close to book value. Similarly, it was not an important issue when Professor Gordon developed the DCF model in the mid-1960s. Instead of reaching back some 25 years, perhaps Mr. Hill should have consulted the 1994 and 2006 editions of my book,[6] each of which discusses at length the chronic inability of the DCF model to accurately estimate investor returns when Market-to-Book ratios deviate markedly from unity.

Q. Is Mr. Hill's contention that your views on the applicability of the DCF have changed since 1984 correct?

A. No. Mr. Hill has once more distorted my views and cited passages from my book out of context. Mr. Hill falsely asserts that there is no reference to the DCF understating the cost of equity in my 1984 text when Market-to-Book ratios are below one. In late 1984 when the book was published, M/B ratios were at nearly 1.0. Indeed, M/B ratios have been well above 1.0 for over twenty years.

The reference to the understatement of the cost of equity when M/B ratios are slightly below one referred to the dilutive effects of issuing stock below book value and the necessity of allowing for flotation cost.

Q. How do you respond to Mr. Hill's discussion of your numerical example regarding the reliability of DCF estimates?

A. On pages 35-36 of his rebuttal testimony, Mr. Hill reviews my numerical example and concludes that it does not show that the DCF understates the cost of equity when the M/B ratio exceeds 1.0. Mr. Hill appears to be confused on this subject. First, the allowed return of 10% is not assumed to be determined by the DCF, as

[6] *See* Roger A. Morin, *Regulatory Finance: Utilities' Cost of Capital*, chapter 10 (1st ed. 1994); Roger A. Morin, *The New Regulatory Finance: Utilities' Cost of Capital*, chapter 12 (1st ed. 2006).

claimed by Mr. Hill on page 36, line 10. Such an assumption would be circular. The allowed return of 10% is assumed to be determined exogenously by the CAPM or the Risk Premium method, for example.

The numerical example is quite simple despite Mr. Hill's attempts to confuse the issue. A stock is trading at $100 and the investor requires a 10% return, so that $10 of earnings are needed. But the regulatory body applies the 10% return to a $50 book value. So, there are only $5 of earnings available to the investor, and the realized return is only 5%. It is that simple.

To pursue the analogy provided by Mr. Hill at pages 36-37 of his rebuttal testimony, imagine a broker trying to sell to an investor with a return requirement of 10% a utility stock priced at $100 per share and whose M/B ratio is 2.0. The broker would say to the investor: "I've got a stock for you that's going to pay a 10% return on a $50 book value – in other words one share will get you $5 but each share has to drop from $100 to $50 in order for the price to drop to book value. Are you interested?" No rational investor would pay $100 for a stock that is going to drop to $50. Mr. Hill's position on M/B ratios defies logic.

Q. What does Mr. Hill's chart on page 39 of his rebuttal testimony reveal?

A. The referenced chart reveals that my recommended return is quite consistent with the M/B ratio. Mr. Hill plots the 2009 ROE against the M/B ratios for my comparable group of electric utilities. Referring to the chart, the implied return on equity is slightly above 11% because the average M/B ratio for my group is 1.80. A ROE of slightly above 11% is clearly within the range of returns on equity recommended in my direct testimony.

F. CAPM vs. DCF ASSUMPTIONS

Q. Is Mr. Hill correct that the assumptions underlying the CAPM are far more restrictive than those that support the DCF?

A. No. Mr. Hill's criticisms of the CAPM are overstated. On pages 41-43, Mr. Hill's rebuttal testimony contains a lengthy discussion of the CAPM paradigm of modern finance and describes the assumptions that enable the existence of the CAPM analysis are far more restrictive than those that support the DCF. Contrary to such assertions, the DCF model is at least as fragile as—if not more fragile than—the CAPM in view of the clear lack of realism of the assumptions underlying the DCF model relative to those underlying the CAPM.

The crucial assumptions of the general DCF model are:

1. Investors evaluate common stocks in the classical valuation framework and trade securities rationally at prices reflecting their perceptions of value.
2. Investors discount the expected cash flows at the same rate of return ("K") in every future period (assume a flat yield curve).
3. The discount rate, K, obtained from the fundamental DCF equation corresponds to that specific stream of future cash flows alone, and no other.

The crucial assumptions of the standard constant growth variation of the DCF model are:

Assumption #1. The three assumptions discussed in conjunction with the general DCF model still remain in force.

Assumption #2. The discount rate, K, must exceed the growth rate forecast, g.

Assumption #3. The growth rate forecast, g, is constant in every year to infinity and applies to dividend, earnings and book value.

Some, if not all, of these assumptions can be unrealistic in a given capital market environment. For example, the standard constant growth DCF model assumes a constant market valuation multiple (i.e., a constant Price-Earnings

ratio). In other words, the standard constant growth DCF model assumes that investors expect the ratio of market price to dividends (or earnings) in any given year to be the same as the current Price-Dividend (or earnings) ratio.

The DCF model is simply not equipped to deal with sudden surges in M/B and Price-Earnings ratios, as was experienced by several utility stocks in recent years.[7]

Many of the assumptions necessary for the DCF model are simply unrealistic. The constant growth form of the DCF requires future earnings per share, dividends per share, book value per share, and price per share to grow at the same constant rate. There is no evidence that these conditions actually prevail in the equity market.

As Mr. Hill itself admits at page 16 of his direct testimony, "*the DCF theory does not exactly 'track' reality. . . . payout ratios and expected equity returns do change over time*."

Q. Are the CAPM assumptions restrictive relative to those that underlie the DCF model?

A. No. The CAPM model assumptions are not nearly as restrictive as the DCF model assumptions. The CAPM can be considered a special case of the broader Arbitrage Pricing Model, which has far less restrictive assumptions than the CAPM. The Arbitrage Pricing Model requires only two major assumptions:

(i) that security returns are linear functions of several economic factors, and

[7] *See* Roger A. Morin, *The New Regulatory Finance*, chapter 8 (1st ed. 2006) for a discussion of the realism of the DCF assumptions; *see also* Roger A. Morin, *The New Regulatory Finance*, chapter 5 (1st ed. 2006) for a discussion of the assumptions underlying the CAPM.

(ii) that no profitable arbitrage opportunities exist since investors are able to eliminate such opportunities through riskless arbitrage transactions.

The more minor assumptions required by the Arbitrage Pricing Model are (i) that investors are self-interested; (ii) that investors are risk averse; (iii) that investors can diversify company-specific risks by holding large portfolios; and (iv) that enough investors possess similar expectations to trigger the arbitrage process.

The Arbitrage Pricing Model relationship asserts that the return on any risky security is equal to the risk-free rate plus a linear combination of risk premiums. Each risk premium is the expected return in excess of the risk-free rate associated with an asset that has a systematic risk with respect to that factor only. The CAPM is a special case of the Arbitrage Pricing Model in which the market portfolio is the sole factor influencing security prices. Under this circumstance, the Arbitrage Pricing Model collapses into the CAPM, with the beta coefficient transformed into the traditional security beta.

In sum, Mr. Hill's discussion of the list of assumptions that underlie the CAPM is vastly overstated and should be ignored.

Q. Is Mr. Hill's assertion that the CAPM is not a special case of the Arbitrage Pricing Model correct?

A. No. Contrary to the assertion of Mr. Hill at page 41 of his rebuttal testimony, footnote 33, the CAPM can be considered a special case of the broader Arbitrage Pricing Model, which has far less restrictive assumptions than the CAPM.

The person who developed the Arbitrage Pricing Model, Professor Steve Ross, refers to the one-factor Arbitrage Pricing Model equation as follows: *"the equation is identical to that of the CAPM."*[8] Another advanced graduate corporate finance textbook states in a chapter on the CAPM and Arbitrage Pricing Model that "*the CAPM may be viewed as special case of the APM [Arbitrage Pricing Model] when the market rate of return is assumed to be the single relevant factor.*"[9] In conclusion, Mr. Hill's views with respect to the Arbitrage Pricing Model are incorrect—the CAPM is indeed a special case of the Arbitrage Pricing Model.

Q. Is Mr. Hill correct that you did not alert the Commission to the dangers of relying on the CAPM?

A. No. Contrary to Mr. Hill's statement at page 40 of his rebuttal testimony, lines 1-2, the following question and answer from page 31 of my direct testimony discusses the dangers of solely relying on the CAPM:

> ***Q. DO THE ASSUMPTIONS UNDERLYING THE CAPM REQUIRE THAT THE MODEL BE TREATED WITH CAUTION?***
>
> *A. Yes, as was the case with the DCF model, the assumptions underlying any model in the social sciences, including the CAPM, are stringent. Moreover, the empirical validity of the CAPM has been the subject of intense research and controversy in recent years. Although the CAPM provides useful evidence, it also must be complemented by other methodologies as well.*

[8] Stephen Ross, *et al.*, *Corporate Finance* (6th ed. 2003).

[9] Thomas Copeland, *et al.*, *Financial Theory and Corporate Policy*, 219 (3d ed. 1992)

G. BETA AND SECURITY RETURNS

Q. Do you agree with Mr. Hill's criticism of your CAPM analysis?

A. No. On page 43 of his rebuttal testimony, Mr. Hill selectively chooses a 1992 study by Fama and French that questions the importance of beta in explaining observed returns. Generally, financial theory has shown that beta is a sufficient risk measure for diversified investors, and most of the empirical literature has confirmed its importance in determining expected return. There is a notable exception—the one selectively chosen by Mr. Hill. In the cited article, the authors found little explanatory power in the relationship between *realized* returns and beta, but the CAPM specifies a relationship between *expected* returns and beta.

Moreover, Mr. Hill neglects the fertile academic literature published in journals on this subject since the publication of the Fama and French results in 1992. Since the publication of the Fama and French paper in 1992, the CAPM and its primary risk measure (beta) have received renewed support. In a 1993 paper, Chan and Lakonishok[10] found a strong relationship between beta and return for the years of their study. In a prominent paper in the same journal, Fischer Black[11] also refuted the conclusions of Fama and French and stated that "beta is alive and well." In March 1995, Kothari, Shanken, and Sloan[12] demonstrated that beta receives statistically significant return compensation when betas are estimated from time-series regressions of annual portfolio returns on the

[10] Louis K.C. Chan & Josef Lakonishok, "Are Reports of Beta's Death Premature?" *Journal of Portfolio Management*, 51-62 (Summer 1993).
[11] Fischer Black, "Beta and Return," *Journal of Portfolio Management,* 8-18 (Summer 1993).
[12] S.P. Kothari, *et al*., "Another Look at the Cross-Section of Expected Stock Returns," *Journal of Finance* Vol. 50, No. 1 (1995).

annual return on an equally weighted market index. In a December 1995 paper, Kim[13] found that, once corrected for the errors in variables problem, there was more support for the role of beta. In yet another 1996 paper, Jagannathan & Wang[14] showed that when betas are allowed to vary over the business cycle, the empirical support of the CAPM is very strong. Fama and French themselves revisited the issue in 1994 and proposed a three-factor model for security returns that included beta as a factor. In their annual survey of capital market returns, Morningstar (formerly Ibbotson Associates) compares Fama-French results with CAPM results and determines that the results, for large-capitalization companies, are virtually indistinguishable.[15] Finally, Nobel Price winning economist William Sharpe refuted the Fama-French criticism in "Revisiting the CAPM," *Dow Jones Asset Manager* (May-June 1998).

Q. Do you agree with Mr. Hill's final conclusion that investors' current risk premium expectations are *considerably lower* than that indicated by long-term averages of historical returns data?

A. No, absolutely not. First, one would think that the recent and ongoing financial crisis would dissuade anyone from the notion that risk premiums are diminishing. Can Mr. Hill seriously contend that investors will demand less of a risk premium today than in recent years? Second, I cite a passage from a review of the literature conducted by Mehra and Prescott (cited on page 12 of Mr. Hill's rebuttal

[13] Dongcheol Kim, "The Errors in the Variables Problem in the Cross-Section of Expected Stock Returns," *Journal of Finance* Vol. 50, No. 5 (1995).
[14] Ravi Jagannathan & Zhenyu Wang, "The Conditional CAPM and the Cross-Section of Expected Returns," *Journal of Finance* Vol. 51, No. 1 (1996).
[15] Ibbotson Associates, *Stocks, Bonds, Bills, and Inflation 2005 Yearbook: Valuation Edition*, 146-147 (2005).

testimony) that concluded that historical and forward-looking market risk premiums are very similar.

There are two revealing passages from Mehra and Prescott's more recent review of the MRP literature:

> *Even if the conditional equity premium given current market conditions is small, and there appears to be general consensus that it is, this in itself does not imply that it was obvious either that the historical premium was too high or that the equity premium has diminished.*
> *In the absence of this [knowledge of the future], and based on what we currently know, we can make the following claim: over the long horizon the equity premium is likely to be similar to what it has been in the past and the returns to investment in equity will continue to substantially dominate that in T-bills for investors with a long planning horizon.*

H. FLOTATION COSTS

Q. What allowance for flotation costs does Mr. Hill make with respect to his recommended return on equity for UE?

A. Mr. Hill fails to include any allowance whatsoever for flotation costs in his recommended return on equity for UE. Mr. Hill's DCF estimates are therefore downward-biased by approximately 30 basis points as a result of that omission. As I discussed extensively in my rebuttal, Mr. Hill's five reasons as to why he fails to include an allowance for flotation costs are spurious and unconvincing.

Q. Do you agree with Mr. Hill's criticism of your comparable group?

A. No, I do not. On page 5 of his rebuttal testimony, Mr. Hill argues that the risk of my second group of electric utilities is not comparable to my first group of electric utilities. I disagree, for both groups had almost identical betas of 0.87 when I prepared my direct testimony.

II. REPLY TO MR. GORMAN

Q. Has Mr. Gorman made any arguments in his rebuttal testimony that would cause you to alter your testimony and/or any of your rebuttal comments?

A. He has not.

Q. How do you respond to Mr. Gorman's criticisms of your DCF analysis?

A. Mr. Gorman expresses two concerns with my DCF analysis on page 13 of his rebuttal testimony. First, Mr. Gorman criticizes the fact that I have used Value Line growth projections that are provided by a single analyst. Although this is correct, I have also relied on analysts' growth projections. Moreover, Value Line projections are relevant; Value Line is the largest and most widely circulated independent investment advisory service, and influences the expectations of a large number of institutional and individual investors.

Second, Mr. Gorman argues that my growth estimates are not sustainable because they exceed Gross Domestic Product (GDP) growth rates. As discussed in my rebuttal testimony, I disagree with this argument. My growth rates fall in the range of 5.8% to 7.5%, which compare favorably to long-term GDP growth rates of 6.0%, and not the 4.5%- 5.0% intermediate GDP growth rates cited by Mr. Gorman.

Q. In light of the devastating impact of the current financial crisis on stock prices, did Mr. Gorman update his DCF estimates?

A. No, he did not. Had he done so, his DCF estimates would increase by some 100 basis points (1%).

A. EMPIRICAL CAPM

Q. Please comment on Mr. Gorman's assessment of the empirical CAPM used in your testimony.

A. On page 9 of his rebuttal testimony, lines 7-8, Mr. Gorman asserts, without support, that the Empirical CAPM analysis significantly overstates a utility company-specific risk premium for use in a risk premium analysis. Mr. Gorman offers no argument, foundation, or literature references to buttress this claim. Mr. Gorman also argues that the ECAPM results in double-counting because of the use of adjusted Value Line betas. As discussed above, the Empirical CAPM is not an adjustment (increase or decrease) in beta. Instead, the Empirical CAPM formally recognizes extensive empirical evidence demonstrating that the observed risk-return tradeoff is flatter than predicted by the CAPM.

Empirical studies of the CAPM to determine to what extent security returns and betas are related in the manner predicted by the CAPM have supported the conclusion that (i) beta is related to security returns, (ii) the risk-return tradeoff is positive, and (iii) the relationship is linear. The contradictory finding is that the risk-return tradeoff is not as steeply sloped as predicted by CAPM. In other words, low-beta securities earn returns somewhat higher than the CAPM would predict, and high-beta securities earn returns somewhat less the CAPM would predict.

I was astonished by Mr. Gorman's statement on page 16 of his rebuttal testimony that the ECAPM is not based on sound academic principles and is not supported by the academic community. There have been countless empirical tests

of the CAPM in the finance literature to determine to what extent security returns and betas are related in the manner predicted by the CAPM. The results of the tests support the idea that beta is related to security returns, that the risk-return tradeoff is positive, and that the relationship is linear. The contradictory finding is that the risk-return tradeoff is not as steeply sloped as the predicted CAPM. I was surprised that Mr. Gorman was unaware of this important strand of financial literature, for this finding is one of the most well-known results in that literature. One only has to look at the vast published literature on the subject. The lengthy bibliography on the ECAPM in Appendix A of my direct testimony should dispel any doubt that the ECAPM is a not supported by the academic literature.

In sum, a plain vanilla CAPM will understate the return required for low-beta securities and overstate the return required for high-beta securities. The Empirical CAPM refines the plain vanilla CAPM to account for this phenomenon.

Q. Does Mr. Gorman object to your CAPM analysis?

A. Yes. Mr. Gorman argues that an updated utility beta would reduce my CAPM return estimates. I agree that utility betas have declined from 0.87 to about 0.81 since the filing of my direct testimony. However, as was the case earlier with Mr. Hill, historical beta estimates do not capture the heightened volatility of current capital markets

Q. Does Mr. Gorman have any other objections to your CAPM analysis?

A. Yes. Mr. Gorman further argues that my MRP estimate lies at the high end of a 6.2% – 7.1% range that he deems adequate. Mr. Gorman bases this argument (page 5 lines 1-4) on the use of the New York Stock Exchange (NYSE) index as

the market index rather than the S&P 500 Index. According to Mr. Gorman, the MRP would be 6.8% and not 7.1% if one were to use the NYSE index as the market index and 6.35% if one were to use only the largest companies included in the NYSE.

There are two problems with Mr. Gorman's argument. First, the argument of using more restrictive market indices defeats the purpose of defining an index that is broadly representative of the equity market. An appropriate market index should capture as broad a cross-section of the equity market as possible, which the S&P 500 Index does.

Second, the more weight you give to large capitalization companies, the smaller the risk premium. Investment risk increases as company size diminishes all else remaining constant. The size phenomenon is well documented in the finance literature. Small-cap and medium-cap companies have very different returns than large ones and, on average, those returns have been higher.

Mr. Gorman fails to point out that Ibbotson / Morningstar in Appendix A (Table A-1 p. 2) calculates what they call "Long Horizon Equity Risk Premium" and arrive at 7.1% (for the period 1926-2007).

B. HISTORICAL RISK PREMIUM

Q. Why does your historical risk premium analysis exclude data from 2006 and 2007?

A. Mr. Gorman is correct that my historical risk premium analysis of the electric utility industry excludes 2006-2007 data. My historical risk premium analysis for the electric utility industry stops in 2005 because the annual Moody's Public

Utility Manual from which the data were drawn was discontinued following the acquisition of Moody's by Mergent. In any event, adding two years of data to a 75-year study is unlikely to have a significant impact, if any, on the average result for the overall period. As a matter of fact, given the rising authorized risk premiums over the past decade and given the fact that the current utility risk premium exceeds the historical average, the addition of data for 2006-2008 would raise the historical risk premium. Thus, Mr. Gorman's argument regarding the exclusion of 2006-2007 data is without merit, all the more so given the devastating effect of the ongoing financial crisis.

Q. Do you agree with Mr. Gorman's criticism that the historical achieved risk premium is the result of declining interest rates over the last 20 years?

A. No. Mr. Gorman's assertion on page 11 of his rebuttal testimony, lines 6-11, that the risk premium is overstated because it is the result of declining interest rates is erroneous. Declining interest rates are associated with rising bond prices and high achieved bond returns, which, in turn, reduce the risk premium between utility stocks and bonds. Moreover, the lengthy historical period used in my risk premium study, 1931-2006, is long enough to smooth out short-term aberrations and encompass several business and interest rate cycles. In short, the facts deflate Mr. Gorman's criticism.

Q. Did Mr. Gorman substantiate his claim that the use of average annual return data instead of year-end data would produce different results in your historical risk premium analysis?

A. No. On page 11 of his rebuttal testimony, lines 12-22, Mr. Gorman criticizes my historical risk premium analysis because I have used December to December as an annual time period and asserts that I should have used different months, say July to July. Mr. Gorman, however, provides no empirical evidence to substantiate this assertion. Indeed, it is standard practice when performing historical risk premium studies to employ consistent calendar year stock price data because the investor is assumed to purchase the stock at the same time every calendar year, usually year-end. This procedure maintains consistency with the bond return calculation and maintains the investor-holding period at a consistent one-year interval.

C. FLOTATION COSTS

Q. Why does Mr. Gorman fail to include any allowance for flotation costs in its recommended return on equity for UE?

A. Mr. Gorman recognizes the legitimacy of common stock issuance costs but objects to a flotation cost adjustment on the grounds that that it should be based only on known and measurable common stock expenses (pages 15-16).

To base a flotation cost allowance on a one-company sample, although company-specific, would not provide a sufficiently reliable statistical and economic basis to infer a utility's appropriate flotation cost allowance. Although it may be conceptually correct to rely on the particular company circumstances in quantifying

the flotation cost allowance, it is not a practical alternative. The flotation cost allowance is a weighted average cost factor designed to capture the average cost of various equity vintages and types of equity capital raised by the company.

As an additional practical matter, the market pressure effect is difficult to measure accurately for a specific issue. This is because one must disentangle the downward effect on stock price resulting from the increased supply of stock from the effect of general movement in the stock market. One must also measure the actual stock price following a common stock issue in relation to a hypothetical benchmark price without the issue over some arbitrary period. This can be performed more reliably and more rigorously using a sample of utility stock offerings.

Mr. Gorman also argues that UE is not a publicly-traded company and infers that UE's flotation costs are born by the parent, and not UE itself. This objection is unfounded because the parent-subsidiary relationship does not eliminate the costs of a new issue, but merely transfers them to the parent. Fair treatment must consider that if the utility subsidiary had gone to the capital marketplace directly, flotation costs would have been incurred.

Q. Does that conclude your surrebuttal testimony?

A. Yes, it does.

BEFORE THE PUBLIC SERVICE COMMISSION
OF THE STATE OF MISSOURI

In the Matter of Union Electric Company d/b/a AmerenUE for Authority to File Tariffs Increasing Rates for Electric Service Provided to Customers in the Company's Missouri Service Area.	)))))	Case No. ER-2008-0318

AFFIDAVIT OF ROGER A. MORIN

STATE OF GEORGIA)
) **ss**
CITY OF ATLANTA)

Roger A. Morin, being first duly sworn on his oath, states:

1. My name is Roger A. Morin. I work in Atlanta, Georgia, and I am employed by Georgia State University.

2. Attached hereto and made a part hereof for all purposes is my Surrebuttal Testimony on behalf of Union Electric Company d/b/a AmerenUE consisting of 33 pages, all of which have been prepared in written form for introduction into evidence in the above-referenced docket.

3. I hereby swear and affirm that my answers contained in the attached testimony to the questions therein propounded are true and correct.

Roger A. Morin

Subscribed and sworn to before me this 5th day of November, 2008.

Notary Public
MICHAEL R. CROWELL
A Commissioner of the Supreme
Court of Nova Scotia

My commission expires: N/A