ETSA Utilities

ETSA Utilities ABN 13 332 330 749
a partnership of:
Spark Infrastructure SA (No. 1) Pty Ltd
ABN 54 091 142 380
Spark Infrastructure SA (No. 2) Pty Ltd
ABN 19 091 143 038
Spark Infrastructure SA (No. 3) Pty Ltd
ABN 50 091 142 362
each incorporated in Australia
CKI Utilities Development Limited
ABN 65 090 718 880
HEI Utilities Development Limited
ABN 82 090 718 951
each incorporated in The Bahamas

28 August 2009

By email

Mr Adam Petersen
Director – Network Regulation South
Australian Energy Regulator
Level 2, 19 Grenfell Street
Adelaide SA 5000

Email adam.petersen@aer.gov.au

Dear Adam

Re: Additional material submitted by ETSA Utilities in support of its regulatory proposal for the regulatory control period 1 July 2010 to 30 June 2015

We refer to our regulatory proposal and relevant attachments for the regulatory control period commencing 1 July 2010, lodged with the AER on 1 July 2009 (**regulatory proposal**). You have advised that the AER will be able to take into account in the draft decision the materials that ETSA Utilities lodges by 28 August 2009 and the attached materials are supplied in that regard. ETSA Utilities is providing these important materials in respect to our Proposal immediately they have become available.

The attached materials relate to the following two issues raised in the regulatory proposal concerning the establishment of the weighted average cost of capital:

- What is the utilisation of income tax imputation credits and there is persuasive evidence to depart from the parameter of 0.65 established in the recent WACC review; and
- the appropriate data sources to be used in estimating the cost of debt and the debt risk premium.

1 The appropriate level of gamma

In relation to the utilisation of income tax imputation credits, we provided as part of the revenue proposal:

- the material set out at Chapter 13 of the regulatory proposal;
- a report by Professor Robert Officer;
- a report by Mr Peter Feros; and
- a report by Associate Professor Christopher Skeels.

Associate Professor Skeels is one of the authors of the Beggs & Skeels (2006) study that was an important part of the AER's WACC review reasoning. In the WACC review decision, the AER undertook a comparison of the Beggs & Skeels dividend drop-off study and the study by SFG Consulting study of 2009 that replicates, updates and explores further the Beggs & Skeels work.

In the WACC review determination, the AER identified a number of points in relation to the SFG Consulting work that should be explored before reliance could be placed on it. Associate Professor Skeels has now undertaken a thorough independent review of the SFG Consulting work including a consideration and investigation of the points raised by the AER which is attached.

Associate Professor Skeels is a leading academic finance statistician who is involved in the marking of doctoral theses and editorial reviews of papers submitted to academic journals. He undertook the review of the SFG Consulting work in the manner and to the standards applied in those contexts. Consistent with those practices, he has had no direct face to face contact with SFG Consulting. He has, however, posed 9 detailed interrogatories to SFG Consulting in writing to which SFG has responded in detail.

The conclusions to be drawn from this work are:

- that on the face of the SFG Consulting report there were issues that should be interrogated including several of those identified by the AER;

- (setting aside for a moment the issue of the Cook's D exclusions undertaken by SFG Consulting) upon interrogation a small number of points do warrant correction in the SFG Consulting study but when they are taken into account the effect upon the conclusions is not material;

- in the course of interrogating the approach applied by SFG Consulting using the Cook's D statistic, a new, detailed review of 20 highly influential data points has been undertaken and a detailed interrogation of these has contributed significantly to the accumulated knowledge that had previously been established by SFG Consulting and Beggs & Skeels; and

- that the authors of both the Beggs & Skeels and SFG Consulting are now both of the view that the likely value for theta is between 0.23 and 0.57 and is more likely to be at the lower end of that range.

This material itself, especially combined with the material previously submitted by ETSA Utilities, is persuasive evidence to depart from 0.65. The work is also supportive of ETSA Utilities preferred approach of adopting a 0.5 in the current revenue proposal assessment process.

2 Averaging Bloomberg and CBASpectrum

In relation to the establishment of the benchmark cost of corporate debt, ETSA Utilities' revenue proposal is to use metrics drawn from an average of the Bloomberg and CBASpectrum services. In support of that proposal, ETSA Utilities submitted:

- Chapter 13 of the regulatory proposal; and

- an expert report prepared by Dr Tom Hird of CEG, on estimating the cost of debt.

Dr Tom Hird, in his initial report, reviewed both the Bloomberg and CBASpectrum estimates of yields on corporate bonds, and found that methodological concerns with both estimations meant that neither data source should be relied on as the sole source of estimates of corporate bond yields. Dr Hird suggested instead that an average of the yields from both sources be used to estimate the cost of debt and thus, the debt risk premium.

Since the lodgement of the regulatory proposal, Dr Hird has continued to interrogate the available material on Bloomberg and CBASpecturm and has further information to supply which reaffirms his confidence in his earlier work. This information is contained in the attached memorandum from Dr Hird.

Both Associate Professor Skeels and Dr Hird's reports are public documents, and may be published on the AER website. If you have any questions about any of the additional materials provided, please contact me on 08 8404 5694.

Yours sincerely

Eric Lindner
General Manager Regulation

Memorandum

To:	ETSA Utilities
From:	Tom Hird
Date:	28 August 2009
Subject:	**Data relevant to assessing the cost of debt**

1. The purpose of this memo is to provide to ETSA Utilities some new datasets, and associated analysis using those data sets, that are relevant to our previous June 2009 report *Estimating the cost of 10 year BBB+ debt.* In that report I examined, amongst other things, the adequacy of the Bloomberg and CBASpectrum fair value estimates (in particular for BBB+ bonds at 10 years to maturity). I concluded that one could not rely solely on Bloomberg fair values to estimate the benchmark cost of debt under the NER and that a superior approach would be to adopt an average of Bloomberg and CBASpectrum estimates.

2. Since writing that report I have continued to analyse these issues and new sources of information have been provided to me. I have also had the opportunity in an online environment to explore and interrogate the data sourced from Bloomberg. This memo provides a description of that data and some analysis using that data. For the reasons set out below, the evidence confirms the analysis in my previous report.

1. Data provided to ETSA

3. We have provided ETSA Utilities with data obtained from Bloomberg, CBASpectrum and UBS as listed below:

   - Bloomberg fair value yields for BBB and A rated bonds contained in “Bbg BBB & A & CGS curves – static”;
   - Bloomberg summary and contributor yields for BBB (BBB-, BBB and BBB+) bonds contained in “BBB bonds-static data”;
   - Bloomberg summary and contributor yields for A (A-, A and A+) bonds contained in “A bonds – cba list STATIC” and “A bonds-static data”;
   - CBASpectrum fair value yields for BBB and A rated bonds contained in “Spectrum As & BBBs curves”;
   - CBASpectrum yields for BBB+ bonds contained in “CBASpectrum BBB bonds”;
   - CBASpectrum yields for A (A-, A and A+) bonds contained in “CBASpectrum A bonds”; and
   - UBS yields for corporate bonds contained in the folder labelled “UBS”.

4. These data (and updates to them) are relevant to assessing ETSA Utilities’ cost of debt under the *National Electricity Rules*.

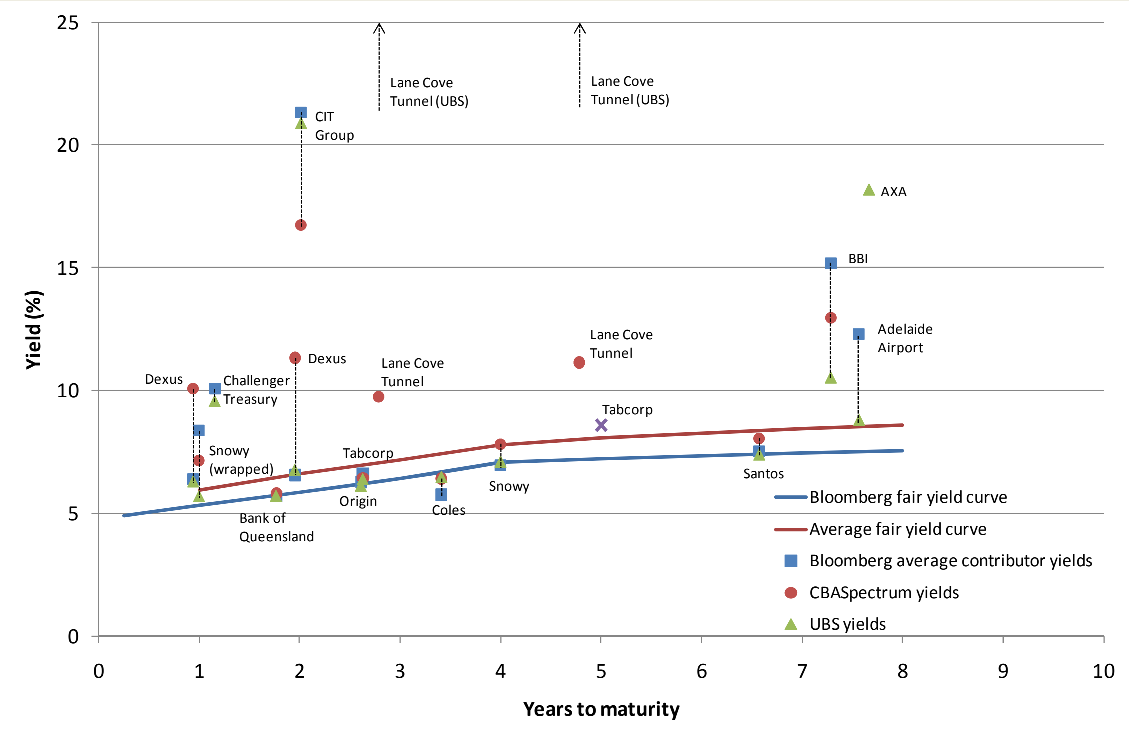
2. High level observations on the data

2.1. A lack of traded data

5. The contributor yields reported by Bloomberg do not appear to be based on actual traded prices or on prices at which individual banks stand ready to trade. This is clearly apparent from the wide differences in yields reported on the same bonds by different contributors to Bloomberg – if they were all reports of traded prices or indicative of a willingness to trade at that price then significant arbitrage opportunities would exist.

6. The diversity of yield estimates for BBB+ bonds can also be illustrated by comparing three different sources of yield estimates: UBS, CBASpectrum and the average of contributors to Bloomberg. The Tabcorp 5 year issue is included as the only observed actual traded price which is, notably, above the average of the Bloomberg and CBASpectrum fair value curves.

Figure 1: Yield estimates for BBB+ fixed coupon bonds (2nd February to 20 March 2009)



The UBS estimates of the yield on the Lane Cove Tunnel bonds are greater than 25% and are therefore only indicated with an arrow in the above chart. This evidence supports the views I expressed in my earlier report that there are few if any trades in the secondary corporate bond market (with the notable exception of Government guaranteed debt).

2.2. Bloomberg contributor prices are materially higher than both Bloomberg fair value estimates and Bloomberg BGNs

7. As described in my previous report, Bloomberg's methodology is proprietary and not entirely transparent. However, it is apparent that Bloomberg obtains estimates of

yields from a variety of contributors, including ABN Amro, AFMA, ANZ, Citigroup, NAB, UBS and Westpac (although in recent months only four of these have been regular contributors). For a subset of bonds, Bloomberg then produces its own daily estimate of the yields on individual bonds, known as the Bloomberg Generic Price (BGN). These are all included in the data provided.

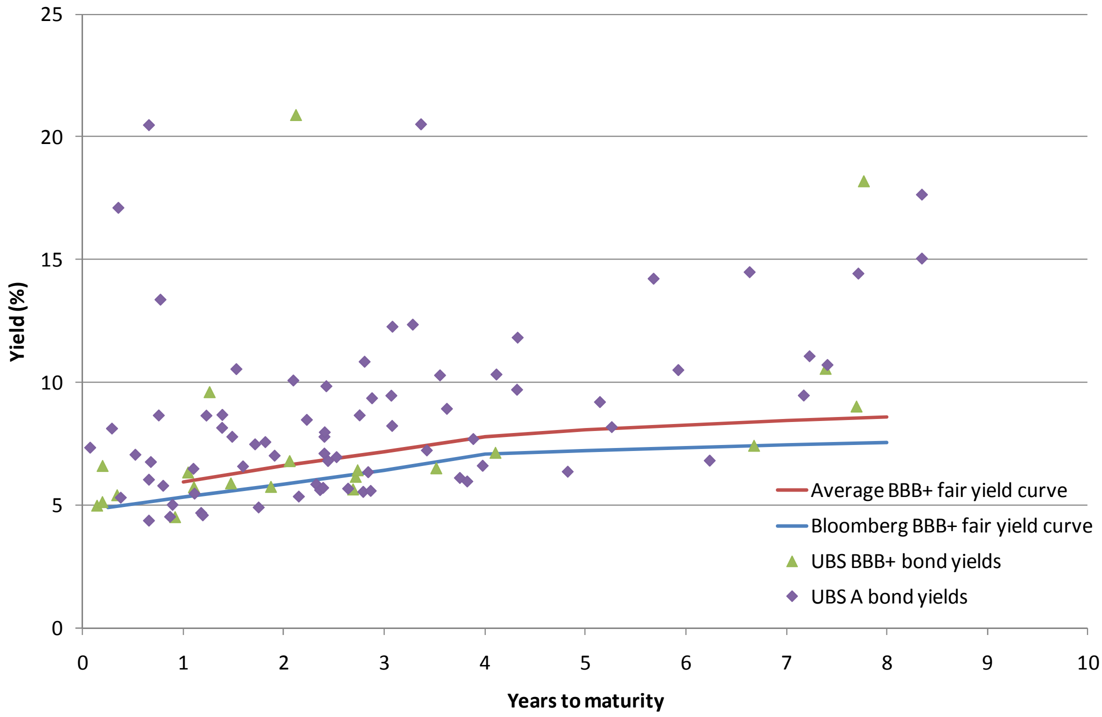
8. Bloomberg does not always publish a BGN for each bond on every day, and for some bonds Bloomberg may very rarely publish a BGN, even when contributor data is available. The BGN yield is not an average of the available contributor yields, and on any particular day may be beneath every contributor yield available on Bloomberg. Over the period from 2 February 2009 to 20 March 2009, the BGN yields for BBB+ bonds in Bloomberg's database were 5% below the average of contributor yields (for those bonds that had any BGN yields at all over this period).

9. The methodology employed by Bloomberg to estimate its fair value yield curves is not transparent. It appears that Bloomberg fits linear segments so that the fair yield curve fits through a subset of its BGN yields. However, it appears to exclude some BGN yields from its consideration in constructing this curve, such that its fair yield curve is, in turn, lower than the average of its BGN yields. Overall, the Bloomberg process results in the fair yield curve being 33% lower than the average of its contributor yields.

This evidence supports the views I expressed in my earlier report that Bloomberg fair value estimates appear low relative to available estimates for individual bond yields.

10. That Bloomberg's BBB and A rated fair value curves are fitted at the lower envelope of the available bond yield estimates can be seen by examining all UBS estimated yields for all bonds with a rating between BBB+ and A+ during the period 2nd February to 20 March 2009. It can be seen (as set out in Figure 2) that not only does the Bloomberg BBB fair value curve pass through the lower envelope of BBB+ estimated yields (represented by green triangles) but it also passes below the majority of estimated yields for higher rated bonds (bonds rated A- to A+ represented by purple diamonds).

Figure 2: UBS yield estimates for BBB+ to A+ fixed coupon bonds (2nd February to 20 March 2009)



11. The above figure also shows the average of CBASpectrum BBB+ and Bloomberg BBB fair value curves over the same period. As can be seen, this average fair value curve passes above the lower envelope of BBB+ estimated yields but passes below many BBB+ and A rated estimated yields – including below all but two of the 16 estimated yields for bond with more than 5 years to maturity.

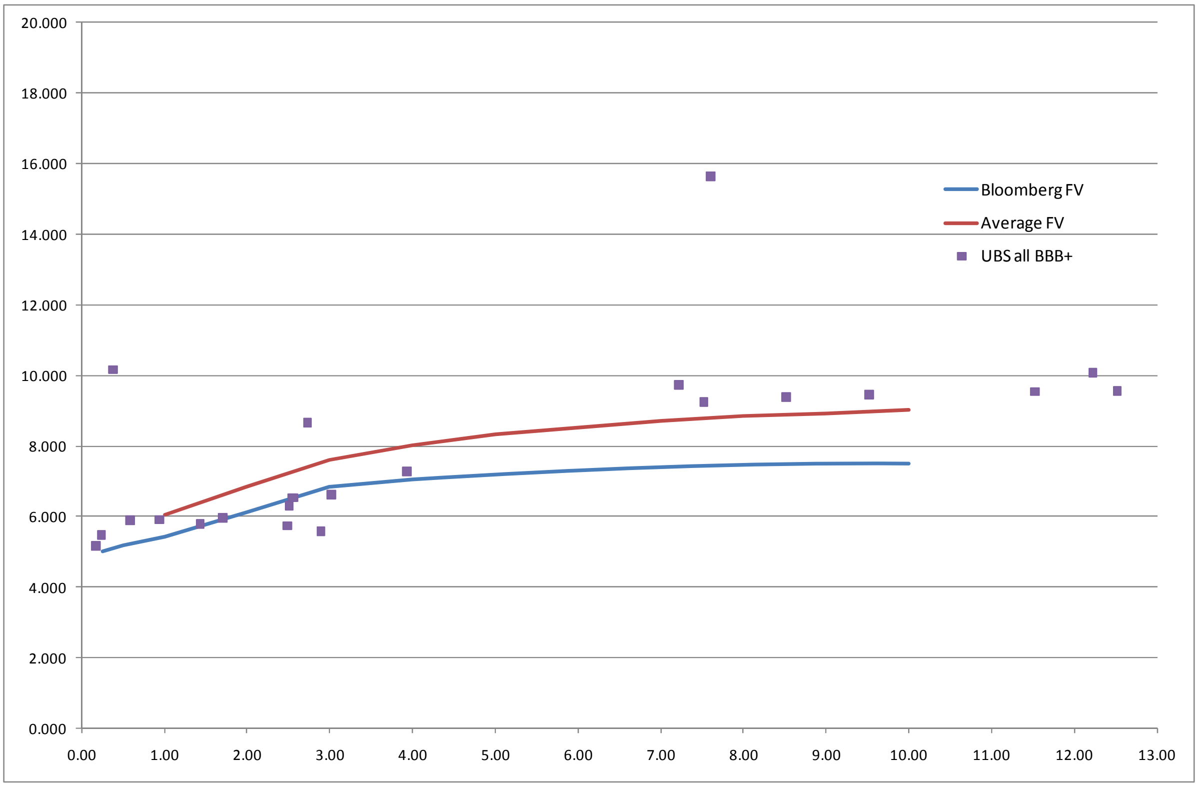
> This evidence supports the views I expressed in my earlier report that Bloomberg fair value estimates appear low relative to available estimates for individual bond yields – especially for bonds with long maturities.

12. Similarly, the UBS rate sheet also provides estimates of the equivalent fixed yield on BBB+ rated floating rate bonds – where these bonds pay a fixed coupon and a floating coupon that depends on the prevailing 3 month bank bill swap rate. As described in my previous report (section 4.2.1) the equivalent fixed yield on a floating rate bond can be estimated by adding the swap rate for that bond (ie, the swap rate from the current day until the maturity of that bond) to the yield implied by the fixed coupon component on the floating rate note.[1] The below chart plots the equivalent fixed rate yields on the Standard and Poor's BBB+ rated floating rate bonds in the UBS rate sheet against the Bloomberg/AER fair value curve for BBB+ bonds and the average of the CBASpectrum

[1] In the case of a bond issued at its face value (such as the Tabcorp bond discussed in section 4.2.1 of this report) the yield attributable to the fixed coupon and return of face value is simply the coupon on the bond. However, for a bond trading at a discount to its face value the yield attributable to the fixed coupon will be higher than the fixed coupon (and vice versa). The calculation of the yield attributable to the coupon on a bond trading at a price different from its face value is known as the "trading margin" and this is provided in column I of the "FRN's" sheet in the UBS rate sheets attached to this memorandum.

and Bloomberg fair value curves. The date chosen is the 20th of March 2009 which is the most recent version of the UBS rate sheets I have access to at the time of writing.

Figure 3: UBS Equivalent fixed yields on BBB+ floating rate bonds (20th March 2009)

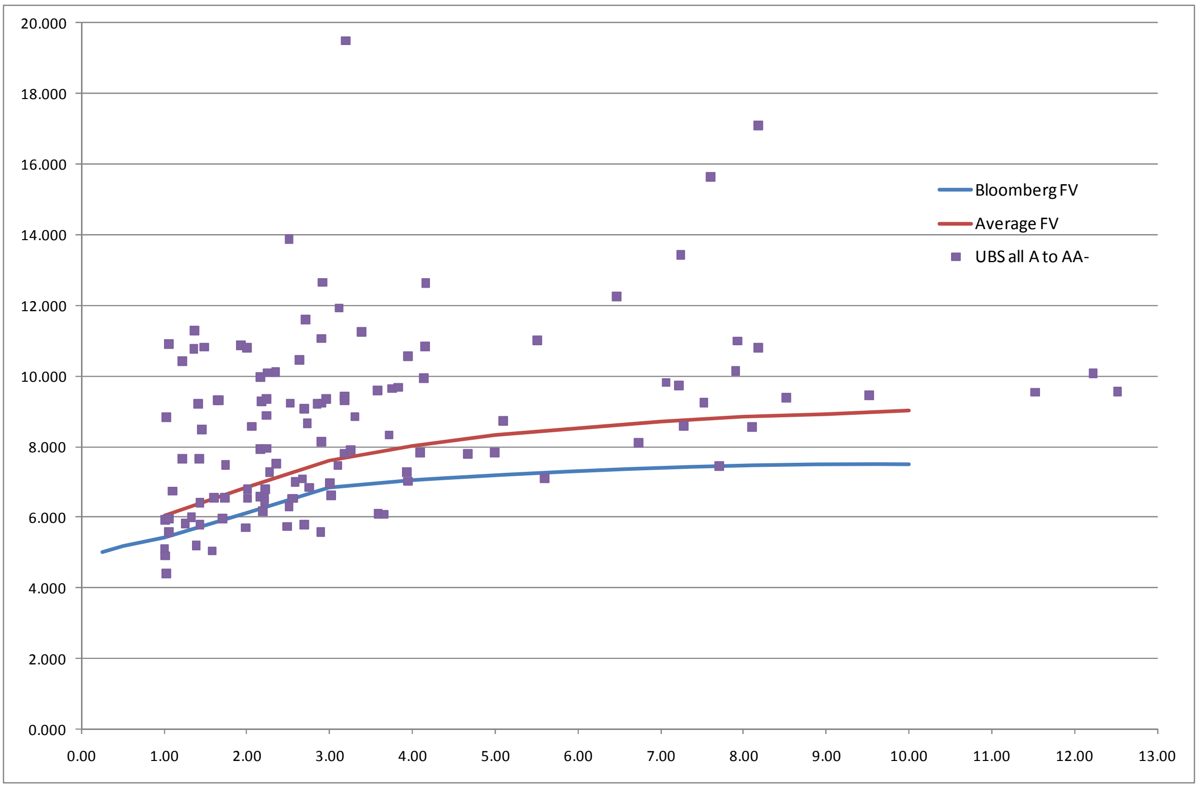


Source: UBS rate sheet 20th March 2009. Equivalent fixed yields calculated by adding trading margin to swap rate (columns I and N of the sheet "FRN's"). All yields reported on an equivalent semi-annual basis.

13. It can be seen that beyond 4 years to maturity all of the 10 estimated yields are above the Bloomberg fair value curve and are better described by the average of the Bloomberg and CBASpectrum fair value curves.

14. As has been done in Figure 2 above, it is possible to also include higher rated bonds in the analysis. The figure below includes all UBS estimates of the equivalent fixed rate on floating rate bonds for bonds with a BBB+ to AA- credit rating from Standard and Poor's.

Figure 4: UBS Equivalent fixed yields on BBB+ to AA- floating rate bonds (20th March 2009)



Source: UBS rate sheet 20th March 2009. Equivalent fixed yields calculated by adding trading margin to swap rate (columns I and N of the sheet "FRN's"). All yields reported on an equivalent semi-annual basis. Only yields on bonds with more than 1 year to maturity reported.

15. It can be seen that not only does the AER/Bloomberg fair value curve appear to be an underestimate of yields on longer dated BBB+ rated bonds but it is also an underestimate of yields on higher rated bonds. This floating rate bond data is consistent with the fixed coupon bond data described in Figure 2 above,

> This evidence supports the views I expressed in my earlier report that Bloomberg fair value estimates appear low relative to available estimates for individual bond yields – especially for bonds with long maturities.

16. That Bloomberg's BBB and A rated fair value curves were fitted at the lower envelope of the bonds for which Bloomberg published a BGN yield (which are not the full set of bonds for which pricing is available) can be seen in the below figures that also cover the period 2nd February to 20 March 2009. (Noting that the BGN yields represented in these graphs were themselves lower than the average of contributor prices.) Also included in the figure immediately below is the yield on Tabcorp's 5 year BBB+ rated bond issued on 24 March 2009 – where that yield is the only yield that represents a traded price rather than a yield estimate.

Figure 5: Bloomberg BGN yield estimates for BBB bonds (2nd February to 20 March 2009)

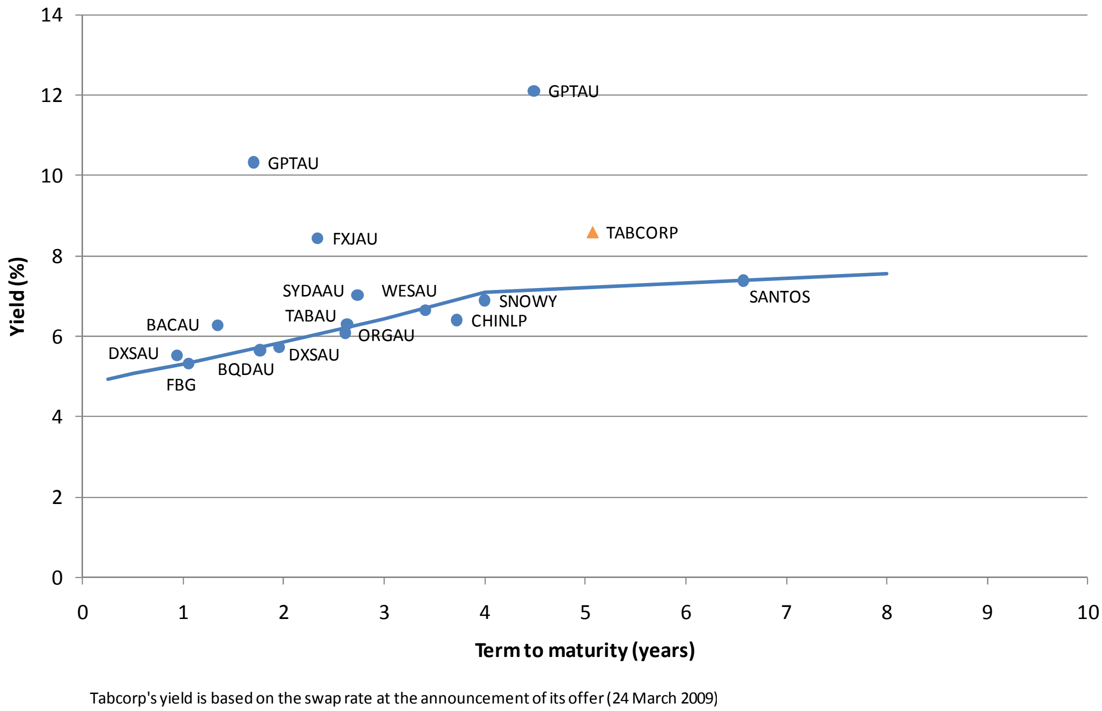
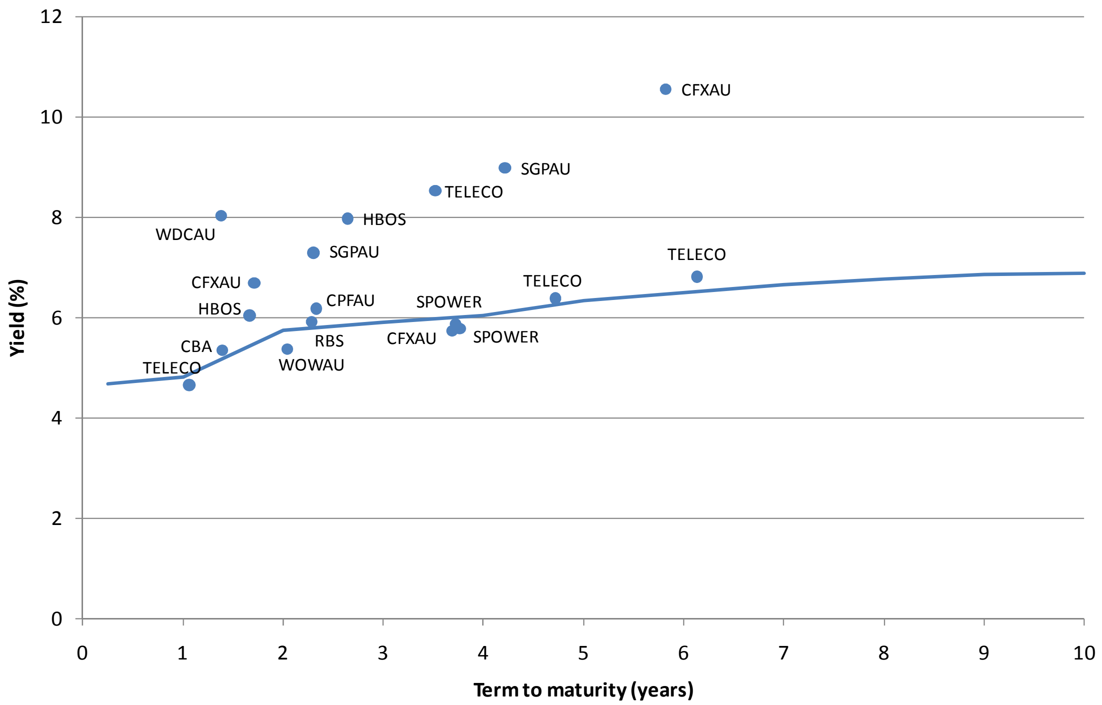


Figure 6: Bloomberg BGN yield estimates for A bonds (2nd February to 20 March 2009)

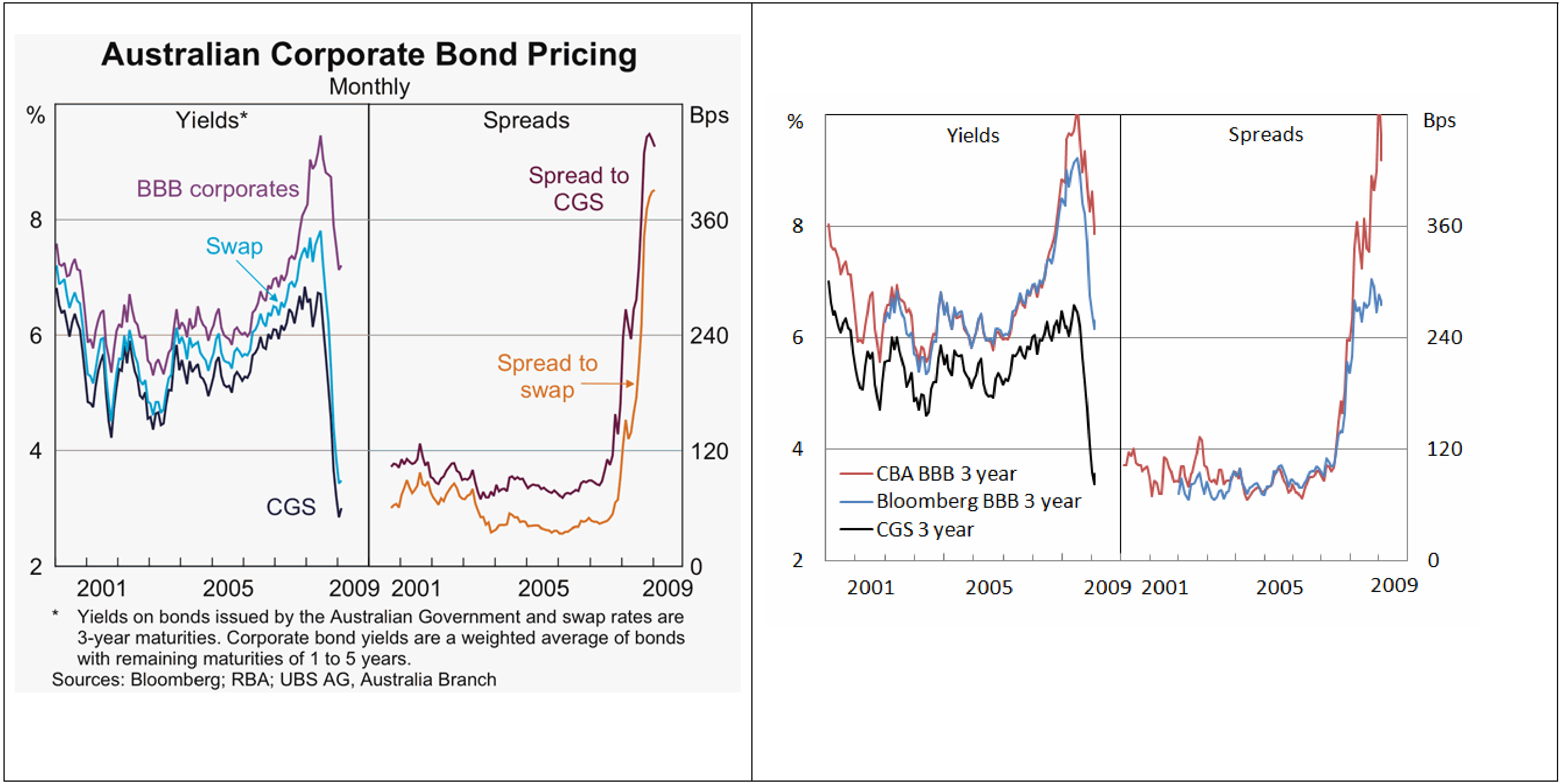


This evidence supports the views I expressed in my earlier report that Bloomberg fair value estimates appear low relative to available estimates for individual bond yields – especially for bonds with long maturities.

3. Consistency with RBA reported BBB yields

17. In the February 2009 Statement on Monetary Policy the RBA provides a graphical description of the change in yields and spread to CGS for BBB rated bonds with maturity between 1 and 5 years (median 3 years). The graph on the left hand side of the below Figure reproduces this graph. The graph on the right hand side also regenerates the same figure using both CBA Spectrum and Bloomberg fair value curves at 3 years to maturity.

Figure 7: Comparison of RBA 3 year BBB yields and spreads (left panel) to Bloomberg/CBASpectrum yields and spreads (right panel)



18. It can be seen from the previous figure that the Bloomberg BBB fair value curve has not behaved in the same manner as the BBB yields reported by the RBA. In particular, the RBA reported spreads to CGS increasing right up to the end of January 2009. This compares with Bloomberg which has effectively reported spreads to CGS as 'flat-lining' in the second half of 2008. By contrast, CBASpectrum has reported yields and changes in spreads that are more consistent with those reported by the RBA. The following more detailed observations can be made:

- The RBA is reporting yields on BBB bonds with 1 to 5 years maturity as being 7.2% at the end of January 2009. By contrast the Bloomberg fair value estimate for BBB bonds of 3 years maturity is only fractionally above 6%. CBASpectrum BBB 3 year fair value is fractionally below 8%. Thus, CBASpectrum fair value estimates are closer, if only marginally so, to the RBA estimates than Bloomberg. If we instead used CBASpectrum BBB+ fair value, the comparison would be materially more favourable to CBASpectrum. Given that BBB+ bonds are the most numerous of BBB bonds[2] it is reasonable to assume that both the RBA and Bloomberg BBB estimates are most heavily weighted to observations for BBB+ bonds.

- The Bloomberg BBB spread to CGS exhibits only mild increases from February 2008 onwards while both the RBA and CBASpectrum BBB spreads continue to increase right up to February 2009. This is a dramatic difference and suggests that the Bloomberg method had the effect of assuming BBB debt costs fell one for one with CGS yields. An increase in spreads of the order of magnitude depicted by the RBA was not reflected in Bloomberg fair value estimates.

19. As noted in our previous report, the Reserve Bank of Australia also separately reports a statistical table of monthly estimates of credit spreads to CGS for BBB to A rated bonds.[3] These demonstrate the same upward movement in spreads to CGS post August 2009 as reported in the RBA graphs above. However, the level of the spreads is higher (which likely reflects the inclusion of AFMA data as a source for this table but not for the above graphs).

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[2] For example, out of the 20 BBB bonds listed in the AER's UBS rate sheets as having 1 to 5 years maturity at the beginning of February 2009 9 were BBB+, 8 were BBB and only three were BBB-.

[3] http://www.rba.gov.au/Statistics/Bulletin/F03.pdf

4. Statistical tests of accuracy

20. In its NSW Final Decisions, the AER presented a test comparing the fair value estimates of CBASpectrum, Bloomberg and the average of CBASpectrum and Bloomberg against yield estimates. The results of this test supported a view that "Bloomberg fair yields are a better predictor of yield estimates than an average of Bloomberg and CBASpectrum fair yields or CBASpectrum fair yields alone".

4.1. Bonds included in the test

21. The four bonds included in the test were Tabcorp (3 year debt), Coles/Wesfarmers, Snowy and Santos.

4.2. Method of conducting the test

22. The tests, summarised at Tables 11.5 and 11.6 of the AER's Final Decision, examined the accuracy of the fair value curves in predicting the yield on the four bonds. Two sources of data for individual bond yields were used (Bloomberg BGN and CBASpectrum yield estimates).

23. The first test, using Bloomberg BGN yield estimates proceeded as follows:

- on each day over the period 2 February 2009 to 20 March 2009, for each bond the test calculated, respectively:
  - the difference between the Bloomberg fair value estimate for that bond on that day and the Bloomberg BGN yield estimates for that bond on that day;
  - the difference between the CBASpectrum fair value estimate for that bond on that day and the Bloomberg BGN yield estimates for that bond on that day; and
  - the difference between the average of the Bloomberg and CBASpectrum fair value estimates for that bond on that day and the Bloomberg BGN yield estimates for that bond on that day.
- on days where Bloomberg BGN yields were not reported for a particular bond, the above calculations were not made for that bond, but continued to be made for other bonds with BGN yields reported on that day;
- on each day over the period 2 February 2009 to 20 March 2009, the test calculated three estimates of 'closeness' between each of the fair value yield curves and the Bloomberg BGN yields by:

- taking a simple average of the differences (ie, positive or negative) between that yield curve and the BGN yield for bonds that reported a BGN on that day;
- taking a simple average of the absolute differences (ie, all expressed as positive numbers) between that yield curve and the BGN yield for bonds that reported a BGN on that day; and
- taking a simple average of the squared differences between that yield curve and the BGN yield for bonds that reported a BGN on that day.

▪ because not all of the four bonds reported BGNs on every day over the period from 2 February to 20 March, the averages as calculated above used data from one, two or three bonds. There were no days during the period where BGNs from all four bonds were reported. There was a single day (23 February 2009) in which no BGNs were reported. No averages were calculated on that day;

▪ the steps above give, on each day, three measures of the 'closeness' of each of the three fair value yield curves to the Bloomberg BGN yields (ie, nine estimates in total for each day). The test took a simple average of each of these series over the period from 2 February 2009 and 20 March 2009. The resulting nine summary statistics were reported in Table 11.5.

24. Exactly the same methodology was used with CBASpectrum yield estimates to generate the statistics in Table 11.6. The only difference of note in this calculation is that CBASpectrum reports yields for each of the four bonds in the test on every day of the period from 2 February 2009 to 20 March 2009.

4.3. Sensitivities tested by CEG

25. The test used only BBB+ bonds with maturities of 2 or more years as at 20 March 2009 that had both Bloomberg BGN yields and CBASpectrum yields reported over the period 2 February 2009 to 20 March 2009. These criteria eliminated all four bonds in the AER's sample. In our view there is useful information from a range of other sources, including:

   i. The equivalent fixed yield on the BBB+ Tabcorp floating rate bond issue which is the only known trade for a BBB+ bond in the period. Noting the Tabcorp bond issue was announced on 24th March 2009 and the fixed coupon margin finalised on the 1st April;

   ii. All UBS BBB+ estimated yields (including for three longer dated bonds than in the AER sample);

iii. The longest dated BBB+ yield estimate from CBASpectrum (BBI) which was also longer dated than any Bloomberg BGN yield estimate;

iv. Three shorter dated BBB+ bonds that all had more than 2 years to maturity at 2 February 2009 (being Origin, Dexus and CIT Group);

v. All estimated yields for BBB+ bonds that had less than two years to maturity at 2 March 2009; and

vi. All estimated yields by Bloomberg contributors for all BBB+ bonds where contributors provided estimated yields (including but not restricted to bonds with BGN pricing). This excluded, inter alia, contributor prices for BBI and Adelaide Airport (both of which are longer dated than the longest dated bond in the AER sample);

vii. Estimated yields on all bonds with higher ratings than BBB+. Noting that including A rated bonds to the very small BBB+ sample size of four bonds actually supports a higher fair value estimate rather than a lower fair value estimate – suggesting the presence of bias in the small sample of four BBB+ bonds.

26. In each row of Table 1 we describe the impact on the test of including different bonds in the sample. In the column of Table 1 entitled “UBS” we describe the impact of sourcing yield estimates from UBS.

27. The overwhelming conclusion from these variations to sample size and sources for yield estimates is that the AER’s conclusion that Bloomberg fair value yields are a better predictor of yield estimates relies entirely on the specific sample of four BBB+ bonds used in the testing. That this is the case can be seen by simply eyeballing the figures supplied earlier in this memo. It can also be shown by examining the following variations to the chosen sample under each of which the conclusion that Bloomberg is a universally better predictor of bond yield estimates no longer holds:

- If the sample is restricted to only yield estimates from known trades of bonds (being the Tabcorp 5 year issue) (see row 1 of Table 1 below);
- If the only known bond trade (Tabcorp 5 years) is added to the four bond sample (see row 3 of Table 1 below);
- If the four bond sample is extended to include longer dated bonds (i.e., closer to 10 years) but not including the Tabcorp 5 year (see rows 4, 5 and 6 of Table 1 below). Note that Bloomberg does not report BGN’s for any longer dated bonds and this is why “NA” is listed in the Bloomberg BGN column in

these rows of the table. Similarly for CBASpectrum in relation to rows 5 and 6;

- If the four bond sample is restricted to only include bonds of greater than 4 years to maturity (see rows 7 and 8 of Table 1 below);
- If the four bond sample is extended backwards to include bonds with 2 years to maturity at the beginning of the AER averaging period instead of at the end (see row 9 of Table 1 below);
- In the entire sample of BBB+ bonds with yield estimates there are 16 bonds. It is necessary to remove the ten bonds with the highest reported spreads to fair value (i.e., nearly two thirds of the full sample) in order to arrive at a sample that gives the unanimous result that Bloomberg is a better predictor of fair value yields (i.e., that on all measures and for all data sources Bloomberg is a better predictor of yield estimates in the chosen sample) (see rows 10 to 19 of Table 1 below); and
- If all yield estimates on A rated bonds are included then there is a unanimous result that the average of Bloomberg and CBASpectrum (being the higher fair value estimate) is the best predictor (see row 20 of Table 1 below).

Table 1: Variations to the AER Chosen Sample of Bonds and Source for Yield estimates

Row	Category	Bonds included	Bloomberg BGN		CBASpectrum		UBS	
			BB FV	Avg FV	BB FV	Avg FV	BB FV	Avg FV
1	Actual trades	Tabcorp FRN	0	15	0	15	0	15
2	AER sample	Tabcorp, Coles, Snowy, Santos	15	0	11	4	15	0
3		As above + Tabcorp	6	9	0	15	15	0
4	Longer dated	AER sample + BBI	NA	NA	0	15	5	10
5	bonds	As above + Adelaide Airport	NA	NA	NA	NA	5	10
6		As above + Axa	NA	NA	NA	NA	1	14
7	> 4 year	Snowy, Santos	15	0	1	14	15	0
8	maturity	Snowy, Santos, Tabcorp FRN	14	1	0	15	5	10
9	Shorter dated bonds	AER sample + Origin, CIT and Dexus	11	4	NA	NA	0	15
		AER sample + CIT and Dexus	11	4	0	15	0	15
10	All bonds	All bonds + Tabcorp FRN	9	6	0	15	0	15
11		As above excl Tabcorp FRN	11	4	0	15	0	15
12		As above excl 2 Lane Cove Tun.	NA	NA	0	15	0	15
13		As above excl CIT	15	0	0	15	0	15
14		As above excl AXA	NA	NA	NA	NA	1	14
15		As above excl Dexus (08/02/11)	15	0	0	15	5	10
16		As above excl Dexus (04/02/10)	15	0	0	15	5	10
17		As above excl Challenger	NA	NA	NA	NA	8	7
18		As above excl BBI	NA	NA	4	11	15	0
19		As above excl Snowy (wrapped)	15	0	15	0	15	0
20	All A rated bonds		0	15	0	15	0	15

A Review of the SFG Dividend Drop-Off Study

A Report prepared for Gilbert and Tobin by

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28 August 2009

[1] The views expressed in this report are those of the author and do not necessarily reflect those of the University of Melbourne.

Executive Summary

I am a co-author of "Market Arbitrage of Cash Dividends and Franking Credits", published in *The Economic Record in 2006* (Volume 82 (258), 239-252). This paper was reviewed by Strategic Finance Group Consulting (SFG) in their 2008 and 2009 reports and by the Australian Energy Regulator (AER) in 2009.

In this report I review the Strategic Finance Group Consulting report entitled "The value of imputation credits as implied by the methodology of Beggs and Skeels (2006)", and the associated comments by the Australian Energy Regulator in "Electricity transmission and distribution network service providers: Review of the weighted average cost of capital (WACC) parameters" (AER (2009))[2]. In light of these reviews I formulated a series of questions that were given to SFG which they answered.

The main findings of my report are:

1. On the basis of my analysis of the SFG study I conclude that:
    - The SFG study chose not to adopt a scaling of some data that was used by Beggs and Skeels (2006) on the grounds that it would have little material impact. That said, SFG now provide results where this scaling is included which allows greater comparability with the results of Beggs and Skeels (2006).
    - The SFG study adopt a different approach to filtering out unreliable observations from the sample than do Beggs and Skeels (2006). The approach adopted lacks the economic justification of Beggs and Skeels (2006). This raised the question of the extent to which their results were contingent upon their choice of data filtering methodology. In response to questions along these lines, SFG have produced a new set of results where no observations identified as potentially

[2] The issues raised by the AER, which I review in this paper arise from SFG's 2009 report, "The value of imputation credits as implied by the methodology of Beggs and Skeels (2006)", which builds on an earlier report by SFG, produced in 2008, entitled "The impact of franking credits on the cost of capital of Australian firms". Hereafter, a reference to the "SFG study" is a reference to both of the reports listed above.

unreliable are excluded from the analysis in the absence of an economic justification. This make the results presented here (in Appendix I) much more credible than those in the SFG study.

- The extension of the post 1 July 2000 sub-sample by an additional 28 months of observations represents a significant contribution which should result in parameter estimates more accurately reflecting the true population values than does the sub-sample used by Beggs and Skeels (2006) which end in May 2004.

2. On reviewing AER (2009) I found the following:
   - Many of the criticisms raised by the AER were little more than allusions to potential problems with the SFG analysis. In some cases I found that these allusions were ill-founded and readily dismissed. In other instances the appropriate response was to rework the model and to actually establish whether the concern was valid or not. This latter class of concerns was incorporated into the questions posed to SFG. I found their responses to be convincing in as much as the potential problems were demonstrated to have little or no material impact upon the results.
   - The AER did raise some more substantial points that required attention from SFG. The most important of these were the issues of scaling and the use of Cook's D statistic for data filtering. SFG's response to the issue of filtering provides more reliable parameter estimates, which are presented in the appendix to this report. Furthermore, SFG's response to the issue of scaling demonstrates that their original results were not manifestly affected by the omission of scaling. The estimates obtained from the rescaled data are more comparable to those of Beggs and Skeels (2006) and are reported in the appendix to this report.
   - The other matter of substance raised by the AER was the use of incorrect corporate tax rates in the analysis by SFG. This was a mistake that simply had to be corrected for SFG's results to have any credibility at all. The results presented in Appendix I address this problem. It is seen that the error was relatively small and had little impact. Nevertheless, only the correct results are of interest and these are now available.

- The AER were concerned about the magnitudes of some of the estimated standard errors accompanying the estimates presented in the SFG study. One consequence of reworking their results to correct the corporate tax rates, to do a better job of data filtering, and to apply appropriate scaling is that the relevant estimated standard errors are smaller than seen in the SFG study, thereby mitigating the AER's cause for concern.

3. I find that the results presented in Appendix I constitute an empirically valid study of the dividend drop-off problem for Australia and that the SFG estimate of theta of 0.23 represents the most accurate estimate currently available.
4. It is clear that the more recent data used in the SFG results presented in Appendix I favour an estimate of theta that is lower than that of 0.57 which was obtained by Beggs and Skeels on the basis of less recent data. However, it might be argued that the minor methodological differences that remain between the methodology of Beggs and Skeels (2006) and that of SFG bias their estimate of theta downwards. (This is not a position to which I subscribe and I present it only in the garb of a devil's advocate.) Were such a position to be taken then, in my opinion, a compelling case can be made that the empirical evidence overwhelmingly supports the notion that the true value of theta lies between the SFG estimate of 0.23 and the Beggs and Skeels (2006) estimate of 0.57, and that in all probability it lies closer to 0.23 than 0.57.

Declaration

This report has been commissioned by Gilbert and Tobin ultimately for all six South Australian and Victorian electricity distribution businesses. I provide an independent review of the SFG study and the AER (2009) criticisms thereof. At no stage have I had any direct contact with SFG. Through Gilbert and Tobin, SFG have provided copies of their original SAS code, their data set, and copies of their results. At the conclusion of the first stage of this report I prepared a set of questions for SFG that were conveyed to them by Gilbert and Tobin. The response from SFG was conveyed to me through Gilbert and Tobin. The questions and SFG's responses appear as an Appendix to this document, together with the SAS code used to obtain the relevant results, the data used, and the results obtained.

C. Skeels

28 August 2009

1. Introduction

I provide (i) an independent assessment of “The value of imputation credits as implied by the methodology of Beggs and Skeels (2006)” a report prepared by SFG Consulting for ENA, APIA, and Grid Australia and submitted to the AER, and (ii) an evaluation of the AER’s assessment of the SFG study. These items are presented in the next two sections of this report. As a consequence of these reviews I posed a series of questions to SFG. These questions and the response from SFG appear as Appendix I. I discuss this response in Section 4 of this document. The fifth and final section of this report provides concluding remarks summarizing my findings.

2. The SFG Study

The SFG study is divided into seven main sections. The first two sections provide a context for the report and a survey of the recent literature on estimating the value of imputation credits, while the final section simply lists the references cited by the report. This review will focus on the four remaining sections which present (i) The AER approach (ii) a comparison of the SFG study and Beggs and Skeels (2006) results (iii) results based on an extended sample period and (iv) results based on a sample which excludes certain observations deemed to be “influential”.

The AER Approach

The AER states that “…the 2006 Beggs and Skeels study provides the most comprehensive, reliable and robust estimate of theta inferred from market prices in the post-2000 period. Accordingly the AER has placed significant weight on the 2001 – 2004 estimate of theta from this study, of 0.57” (AER 2009 p.328).

The essential feature of the AER approach is the use of a regression-based methodology focusing on the post 1 July 2000 period. The analysis of the SFG study adopts the same broad strategy. An important contribution by the SFG study is to extend the sample period to 30 September 2006; that used in Beggs and Skeels (2006) ended at 10 May 2004. By extending this period more information is available for use in estimation and, all things equal, one would expect the estimates obtained in the SFG study to more accurately reflect the true population values than do those provided by Beggs and Skeels (2006) on the basis of a smaller sample. Specifically, I believe that the SFG study estimates are of equal significance as those of Beggs and Skeels (2006).

A Comparison of SFG and Beggs and Skeels results

SFG argue that their report is more comprehensive than that of Beggs and Skeels because

(i) The SFG study employs a wider cross section of firms

(ii) The SFG study employs a “a longer and more recent data period” (SFG 2009, p.8).

I note in passing that the Beggs and Skeels (2006) data begins in 1986 while the SFG study data set begins in 1997, so the contribution of the SFG study is to bring more information to bear on the problems involved in estimating theta in the post-2000 sub-sample, the period of particular interest to the AER. Consequently, I would expect the SFG study estimate of theta for the post-2000 sub-sample to be a more accurate reflection of the population values than those of Beggs and Skeels (2006).

The SFG study follows the Beggs and Skeels (2006) using a Feasible Generalised Least Squares (FGLS) estimator to obtain coefficient estimates for the regression

$$\Delta P_i = \alpha_0 + \alpha_1 D_i + \alpha_2 F_i + \varepsilon_i \,, \tag{1}$$

where i is the index of the dividend event, $\Delta P_i = P_{c,i} - P_{x,i}$ denotes the difference between cum-dividend prices $P_{c,i}$ and ex-dividend prices $P_{x,i}$ (the so-called price drop-off), D_i is the cash dividend per share, and F_i is the corresponding franking credit. Here the weights in the FGLS estimator were obtained from an auxiliary regression of the squared residual from (1) on the gross dividend:

$$\ln \hat{\varepsilon}_i^2 = \lambda_0 + \lambda_1 W_i + \lambda_2 G_i + \lambda_3 P_{c,i} + u_i \,, \tag{2}$$

where W_i is the company size measured by market capitalization as a proportion of the All Ordinaries Index and $G_i = D_i + F_i$ is the gross dividend. I note in passing that Beggs and Skeels (2006, Footnote 6) scale $P_{c,i}$ in an attempt to further mitigate the effects of heteroskedasticty. There was no clear evidence that a similar scaling was performed in the SFG study.[3]

The empirical results of the SFG study are presented in Table 1 (SFG, 2009, p.8), which is reproduced below.

[3] This point was subsequently confirmed by SFG; see the SFG response to Question 2 in Appendix I, where the scaling is shown to have little effect, and the discussion in Section 4.

Table 1. Comparison with Beggs and Skeels (2006)

	Beggs & Skeels (2006)			SFG (2008)			SFG (2008) excl. influential 1%		
	Cash	Franking	N	Cash	Franking	N	Cash	Franking	N
Regression analysis ending 10 May 2004									
1 July 1985 –	0.465	0.752	910						
30 June 1988	(0.040)	(0.157)							
1 July 1988 –	0.646	0.450	546						
30 June 1990	(0.064)	(0.119)							
1 July 1990 –	0.765	0.376	236						
30 June 1991	(0.115)	(0.206)							
1 July 1991 –	0.861	0.201	1,669						
30 June 1997	(0.059)	(0.103)							
1 July 1997 –	0.795	0.418	573	0.773	0.361	710	0.871	0.142	699
30 June 1999	(0.099)	(0.186)		(0.270)	(0.645)		(0.087)	(0.184)	
1 July 1999 –	1.168	0.128	267	0.205	1.163	329	0.746	0.360	326
30 June 2000	(0.099)	(0.204)		(0.184)	(0.710)		(0.102)	(0.239)	
1 July 2000 –	0.800	0.572	1,310	0.895	0.526	1,389	0.945	0.190	1,378
10 May 2004	(0.052)	(0.121)		(0.227)	(0.541)		(0.059)	(0.136)	
			5,511	Adj-R^2	1.9%	2,428	Adj-R^2	24.1%	2,403
Regression analysis ending 30 September 2006									
1 July 1997 –				0.761	0.437	710	0.844	0.246	696
30 June 1999				(0.235)	(0.577)		(0.085)	(0.186)	
1 July 1999 –				0.100	0.369	329	0.797	0.224	326
30 June 2000				(0.077)	(0.388)		(0.102)	(0.240)	
1 July 2000 –				0.913	0.913	2,182	0.916	0.235	2,166
31 December 2006				(0.168)	(0.388)		(0.049)	(0.111)	
				Adj-R2	3.5%	3,221	Adj-R2	31.0%	3,188

In the first instance we will restrict attention to the first two columns of Table 1.In the first column SFG reproduce results from Beggs and Skeels (2006) and in the second column they report results based on a sample of 'large' firms, where 'large' firms have a market capitalization of at least 0.03% of the All Ordinaries Index, which was a necessary condition for inclusion in the sample used by Beggs and Skeels (2006).

For the most part the parameter estimates in these two columns are very similar although there are some differences. For example, for the sub-sample 1 July 2000 – 10 May 2004, The SFG study estimates the value of a dollar of cash dividends at 89.5 cents per dollar whereas Beggs and Skeels (2006) report an estimate of 80.0 cents per dollar. Similarly, the SFG study reports a corresponding estimate for imputation credits of 52.6 cents per dollar whereas the Beggs and Skeels estimate is 57.2 cents per dollar. Some of these differences may be explained by the failure of the SFG study to incorporate in their methodology the scaling of $P_{x,i}$ that was used by Beggs and Skeels (2006). This absence of scaling is unlikely to explain the differences observed for the 1999-2000 sub-sample.[4] The SFG study comments on the curious pattern in the estimates of the value of a dollar of cash dividends reported by Beggs and Skeels (2006) which peaks in this sub-sample but are silent about the fact that they report a trough in this estimate for the same sub-sample. While Beggs and Skeels (2006) report an implausible value of 1.168 for the peak

[4] Indeed, as was subsequently demonstrated in Appendix I, the scaling has little impact on the results.

value of a dollar of cash dividends, the SFG study reports an equally implausible value of 1.163 for the estimated value of a dollar of imputation credits.[5] Given the relatively small sample size for this particular sub-sample great caution should be taken in drawing any conclusions from this sub-sample.

With the exception of the 1999-2000 sub-sample, it is difficult to argue that there is a manifest difference between the results reported by SFG in the second column of Table 1 on page 9 and those reported by Beggs and Skeels (2006). The AER, in their discussion of the SFG study, focus on the difference in magnitude of the estimated standard error for the estimate of the franking credit drop-off ratio. I will return to this point in the sub-section discussing outliers/influential observations and also in the section assessing the AER's comments on the SFG study.

Extension of time period to include additional data

The SFG study extends the sample period to include an additional 28 months of data in the post-2000 sub-sample. The Beggs and Skeels (2006) study employs data to 10 May 2004 while SFG have data to 30 September 2006.[6] Results based on the extended sample can be found in the bottom panels of columns 2 and 3 of Table 1, respectively. The SFG study observes that "when more recent data is included but the estimation process remains unchanged in all other respects, the estimate of theta falls from 0.57 (as estimated by Beggs and Skeels) to 0.37" (SFG study, p.10). All statistical theory and, indeed, common sense indicates that the provision of additional data points makes available more information for estimation and so estimates obtained on the basis of a larger data set should provide a more accurate reflection of the underlying population. The proviso here is that there is nothing wrong with the data. I observe that the nature of the extra data points used by the SFG study in extending the data set is exactly the same as that used in the shorter data sets that finish at 10 May 2004. Consequently, the only reasonable conclusion to be drawn is that the extended data set should yield more accurate parameter estimates for the 1 July 2000 onwards sub-sample than does the shorter data set.

[5] These are the circled values in Table 1. In the response to Question 1, Appendix I this peculiar behaviour in the SFG study is shown to be largely driven by a single outlier, viz. Transurban.

[6] Table 1 includes a curious conflict in the claimed date at which the sample finishes, being variously cited as 30 September 2006 and 31 December 2006. This was clarified by SFG who confirm that the end date was 30 September 2006.

I note in passing that the results for the period 1999 – 2000 remain problematic even with the extended data set.[7]

Exclusion of outliers/influential observations

If I compare the sub-sample sizes used by Beggs and Skeels (2006) with those of the SFG study, and look at the comparable sub-samples in columns 1 and 2 of Table 1, I note that the sub-sample sizes used by the latter report are larger. Beggs and Skeels (2006) used a proprietary data set provided to them by CommSec under strict non-disclosure conditions.[8] As observed by SFG (2009, p.9), "Beggs and Skeels (2006) do not list the observations for which they were unable to obtain all of the required data items, so it is impossible to know exactly what sample they use." This statement is perfectly correct. In my opinion it is these differences in samples that are primarily responsible for differences in parameter estimates over the interval 1 July 1997 – 10 May 2004.

It is important to consider the source of differences between the samples used by the two studies. Beggs and Skeels (2006) excluded from their sample all observations for which any of the dividend payout, the corporate tax rate, the cum-dividend share price, the ex-dividend share price, or the market capitalization of the company were not known. In addition they restricted attention to 'large' firms for which the market capitalization was at least 0.03% of the All Ordinaries Index. Finally, Beggs and Skeels (2006) also excluded all observations that changed their basis for quotation within 5 days either side of the ex-dividend day and also removed all special dividend payments. The SFG study restricted their sample to large firms, in accord with the procedure of Beggs and Skeels (2006) but have not applied the other filters. Note, however, that the other filters fall into two distinct classes. The first class reflects shortcomings in the data available to Beggs and Skeels (2006) and the second class excludes observations for which there are economic grounds to believe that they may be unreliable. To the extent that the increased sub-sample sizes of the SFG study reflect the fact that they were able to find information that

[7] This is discussed by SFG in Appendix I
[8] The data set is described in Beggs and Skeels (2006, Appendix I).

Beggs and Skeels (2006) were missing, the data used by the SFG study are an improvement over those used by Beggs and Skeels (2006). To the extent that the SFG study data contains observations that Beggs and Skeels (2006) felt were economically unreliable then the larger samples are a concern. The problem is that, in the absence of the data used by Beggs and Skeels (2006), it is impossible to know whether the increased sample sizes were due to the inclusion of observations deemed unreliable by Beggs and Skeels (2006) or good observations that Beggs and Skeels (2006) simply did not have.[9]

Given the uncertainty about (i) the extent to which the sub-samples used by Beggs and Skeels (2006) and by the SFG study overlap and (ii) the extent to which the observations used by the SFG study that are not in the Beggs and Skeels (2006) data set are economically reliable it seems sensible to examine the robustness of the results to the chosen data set. One common statistical device for this is to use Cook's D statistic to determine those observations that are most influential in determining the values of the parameter estimates and then to explore those observations and decide whether they are reliable data points or not. The important point to recognize is that not all influential observations are unreliable. Moreover, not all unreliable observations are influential. However, it is clear that any influential observations that are unreliable should be removed from the analysis.

The SFG study adopts a strategy of excluding from the analysis 1% of observations which are most influential. The results obtained using this restricted sample are reported in the third column of Table 1. On the basis of these results, the SFG study erroneously argues that the Beggs and Skeels (2006) results are driven by outliers or influential observations. The reason this argument is wrong is simply because SFGdo not know whether or not the influential observations excluded from the analysis in this way were part of the data set used by Beggs and Skeels (2006) or not.

In comparing the results of Beggs and Skeels (2006), for the 1 July 2000 – 10 May 2004 sub-sample, with those presented by the SFG study in the third column of Table 1, it is clear that the

[9] In the results presented in Appendix I, SFG follow a middle path of using Cook's D statistic to identify potentially unreliable observations but only excluding those observations for which an economic justification can be found. This procedure is very similar in spirit to that of Beggs and Skeels (2006) and yields both plausible and credible results.

estimate of theta is sensitive to the presence of influential observations. Note that the two estimates of the cash drop-off ratio for this sub-sample, reported in columns 2 and 3 of Table 1 are not statistically significantly different from each other and so the excluded data points do not seem to exert much influence over these estimates.[10] It is interesting to note that the exclusion of just 3 influential observations from the sub-sample 1 July 1999 – 30 June 2000 has had a dramatic effect on the estimates there and it is difficult to draw any other conclusion than that, amongst the three excluded observations, there were some extremely unreliable observations.[11]

It is important to note that the deletion of an observation deemed to be influential in terms of the Cooks D metric is not mandatory. An observation that is influential in terms of leverage need not necessarily be an outlier. Moreover, even in the face of outliers, deletion of the offending observation is not obligatory. A number of alternative estimators are available to the researcher, the most commonly used of which is the Least Absolute Deviations estimator. The LAD, approach is given by

$$\sum_{i=1}^{N} \left|\varepsilon_i\right| = \sum_{i=1}^{N} \left|\Delta P_i - \Delta \widehat{P}_i\right| = \sum_{i=1}^{N} \left|\Delta P_i - \alpha_0 + \alpha_1 D_i + \alpha_2 F_i\right| \qquad (3)$$

The estimates are obtained by minimizing the sum of the absolute value of the residuals rather than the sum of the squared residuals as in OLS estimation. By focusing on minimizing the sum of the absolute values of the residuals rather than the sum of the squared residuals, the effect of the LAD estimator is to reduce the influence of outlying observations.

[10] To be clear on this point, the cash drop-off estimates of 0.895 and 0.945 are insignificantly different from each other but significantly different from zero in a statistically meaningful sense. However, the franking credit drop-off estimates are both statistically insignificantly different from each other and from zero.

[11] This has subsequently been determined to be shares related to Transurban; see the discussion in Appendix I in response to Question 1.

3. Criticisms of the SFG Study by the AER

In this section I explore the AER's findings with respect to the SFG study.[12] In what follows the AER's findings are presented in italics and my comments are presented in an upright font.

- *SFG's outputs (i.e. regression coefficients) as presented in its report to the AER (and including the p-values submitted later) were found to be replicable.*

  I concur.

- *Under all three methods employed by SFG (i.e. Beggs and Skeels, Hathaway and Officer, and ACG), the estimate of theta is highly sensitive to the sample selected.*

  This was also my experience during the writing of Beggs and Skeels (2006). There are two aspects to this issue. First, there is a question of which stocks to include in the study. In price drop-off studies, such as Beggs and Skeels (2006), it is essential that the price data is of a high quality. Prices which fully and instantaneously reflect all of the available information in the market are said to be efficient prices. In order for prices to be efficient it is necessary that sufficient trades occur for all of the available information to be revealed and so it is common practice to exclude from such studies stocks that are not traded with sufficient frequency or in sufficient volume to be thought of as having efficient prices. In the Beggs and Skeels (2006) study we followed the advice of the then manager of the CommSec Share Portfolio Database and chose a market capitalization of 0.03% of the All Ordinaries Index as the cut-off for particular stocks to be included in the study. Clearly this choice is arbitrary and, moreover, the results obtained are clearly sensitive to this choice. Other choices can quite reasonably lead to different outcomes. We note that the SFG study also models the price drop-off for large firms, using the same 0.03% market capitalization filter as Beggs and Skeels (2008).

[12] Specifically I am dealing with the material presented in AER (2009, pp. 438 – 441).

The second aspect of sensitivity to data stems from the fact that the appropriate data themselves are difficult to construct.[13] One of the important features of Beggs and Skeels (2006) was our access to the CommSec data. The type of data used in our study is difficult to assemble and, in particular, not all of the required data are available in readily accessible sources such as Bloomberg. Specifically, finding reliable cum-dividend and ex-dividend event prices on the dividend event date can be extremely difficult.[14] An important feature of the SFG study is that considerable attention has been devoted to the development of a 'clean' data set, i.e. a data set relatively free of unreliable or incomplete observations. Moreover this data set extends the available data set until the end of September 2006. Beggs and Skeels (2006) were constrained by a relatively short sample period after the 2000 legislative change and the additional information contained in the extended data set used in the SFG study should be beneficial in respect of more precisely estimating the effect of the 2000 legislative change.

The data set used by Beggs and Skeels (2006) extends back to the introduction of dividend imputation in 1986 whereas that used by SFG only starts in 1997. Note that when testing for a change in the franking credit drop-off as a consequence of the 2000 legislative change Beggs and Skeels (2006) only use data from 1998 onwards and so this difference in starting points for the different data sets will be immaterial.

- *In studying the program codes written by SFG, the AER identified a number of issues which may detract from the reliability of the results. For example:*
    - *It is common in the literature for the market return variable to be included as a control variable in assessing the dividend drop off ratio. Whilst the Beggs and Skeels study adjusted the daily observed ex-dividend share price for the aggregate movement in the market to account for the noise in the data associated with general market movements, it appears the SFG study did not make such adjustments.*

[13] Indeed, much of the feedback that I received at the time of its publication was in the form of enquiries seeking access to our data.
[14] The great benefit of using the CommSec data was that considerable resources had already been directed to 'cleaning' the data, i.e. filtering out incomplete or unreliable observations, as discussed in the previous section.

I concur with this observation.[15] Furthermore, the duration of the data sets differ, with the Beggs and Skeels (2006) study covering the period 1 April 1986 to 10 May 2004 whereas the SFG study covers the period 1 July 1997 to 30 September 2006. The important question, however, is the extent to which this matters. From Table 10.10 of AER (2009, p.440) we see that the actual estimates of various parameters are reasonably close to one another and so, in terms of formulating a best guess of the true parameter values,[16] it appears that the scaling does not have much impact. This is hardly surprising because the scale factor is simply 1+RI, where RI is the rate of return to the All Ordinaries Index over the ex-dividend day. For any given ex-dividend day, 1+RI will be close to 1 because RI is close to 0.[17] Hence, the impact of this scaling on the estimated coefficients will be minimal.[18]

In summary, there is nothing in the results to suggest that the difference in scaling between the two studies has any significant impact on the results obtained. Consequently, in my opinion, this reason does not constitute grounds to question the reliability of the SFG results.

- *The company tax rates applied by SFG over time do not appear to correspond with the official period over which the various tax rates apply (i.e. as reported by the ATO).*

  I concur.[19]

- *SFG's dividend drop off study is prone to the common problem of multi-collinearity in the regression model. However, consistent with the methodologies adopted in its study, it has attempted to deal with some of these issues – in particular through its use of the Beggs and Skeels methodology.*

[15] This scaling is subsequently used in obtaining the results presented in Appendix I, where it is seen not to make much difference to the results.
[16] The word 'estimation' is simply statistical jargon for forming a best guess.
[17] Not only is this an empirical 'stylized fact, it is consistent with the predictions of the theory of efficient markets which suggests that E(RI)=0 on any given day, implying that E[1+RI]=1.
[18] The impact of the scaling is addressed by SFG and is presented in Appendix I. In summary, the absence of scaling was confirmed to have minimal impact on the estimated coefficients.
[19] This error is corrected in the results presented in Appendix I.

Except for certain orthogonal designs, typically only observed in the experimental sciences, there is always some degree of multicollinearity present amongst explanatory variables in multiple regression models. Such multicollinearity does not materially impact upon the usefulness of these models nor does it materially impact upon the usefulness their results. The real issue that needs to be considered is the extent of the multicollinearity present which requires an understanding of the underlying problem.

By way of a simple analogy consider the songs of, say, Lennon and McCartney. It is straight-forward to measure the success of the songs but, without more information than just the measured success of the songs, it is impossible to know the individual contributions of Lennon and McCartney to that success. In multiple regression models, such as those under discussion, multicollinearity is a problem whereby the individual effects of each of the explanatory variables can be difficult to distinguish from each other even though we observe that, collectively, the explanatory variables do a reasonable job of explaining variability in the dependent variable.[20]

The question then is what circumstances might cause you to believe that multicollinearity is a problem. As per the discussion of the preceding paragraph the tell-tale signs are (i) the fitted model appears to have some ability to explain variability in the dependent variable and (ii) the estimated coefficients of the model are statistically insignificantly different from zero. Any estimated regression model presenting these symptoms presents a conundrum because, on the one hand, the model provides a reasonable fit to the dependent variable but, on the other hand, it seems that this reasonable fit is not caused by the explanatory variables in the model because the coefficients on these variables cannot be distinguished from zero. Clearly this is a contradiction because the explanatory power of the estimated model can only come from the explanatory variables and

[20] As an aside, just like the problem of separating the effects of Lennon from McCartney, the solution to the problem of multicollinearity is always to use more information to help isolate the individual effects of the various explanatory variables.

so they can't all be unrelated to the dependent variable (which is what a coefficient of zero implies).

An important caveat to the preceding discussion is that a single coefficient estimate being insignificantly different from zero is not symptomatic of multicollinearity. Indeed, a single coefficient being insignificantly different from zero simply implies that your best guess of the true parameter value is no different to zero.

If we turn to the results presented in AER (2009, Table 10.10) we see that the SFG study estimates of the regression coefficients for the franking credit drop-off are both insignificantly different from zero.[21] However, both of the estimated regression coefficients for the cash dividend drop-off are statistically significantly different from zero.[22] Consequently, we see no evidence of a conundrum whereby the estimated coefficients are statistically insignificantly different from zero even though the estimated model is doing a reasonable job of explaining the variability in the dependent variable. That is, in my opinion multicollinearity is not problematic in the SFG study. Specifically, because the estimated coefficient on the cash dividend is statistically significantly different from zero, the results from the SFG study unequivocally indicate that the cash-dividend is explaining the price drop-off of the share and that the franking credit is not obviously responsible for explaining any of the price drop-off stemming from the dividend event.

The AER also examined the results (and derivation thereof) reported by SFG in its latest report prepared for the JIA. Specifically the AER has explored the differences (if any) between the results from the SFG study and the Beggs and Skeels (2006) study. Table 10.10 presents the comparison of results as presented in the SFG's report for the JIA.

[21] To see this one need only calculate the relevant t statistics, which are clearly both less than unity in value – 0.526/0.541 and 0.369/0.388 – and so one would accept a null hypothesis that the true value of the franking credit drop-off was statistically insignificantly different from zero at any reasonable size.

[22] Here the relevant t-ratios are 0.895/0.227 and 0.913/0.168 and these clearly present compelling evidence that the true value of the cash dividend drop-off is statistically significantly different from zero at any reasonable size.

Table 10.10: SFG – comparison of results from Beggs and Skeels (2006) with SFG (2008) over the post July 2000 period

Period	Beggs and Skeels (2006)			SFG (2008)		
	Cash[(a)]	FC[(b)]	N[(c)]	Cash	FC	N[*]
1 July 2000 – 10 May 2004	0.800 (0.052)	0.572 (0.121)	1,310	0.895 (0.227)	0.526 (0.541)	1,389
1 July 2000 – 31 Dec 2006				0.913 (0.168)	0.369 (0.388)	2,182

Source: SFG, table 1 (extract).[1079]

Notes:

(a) Cash: Regression coefficient for the cash dividend drop-off

(b) FC: Regression coefficient for the cash dividend drop-off

(c) N: Number of observations in sample

Numbers in parentheses are standard errors

[1079] *SFG, op. cit., 1 February 2009 (e), p.8.*

The AER notes from table 10.10 that while theta estimates over the period 2001-04 are relatively close across the two studies (i.e. 0.572 and 0.526), the standard errors are markedly different (i.e. 0.121 and 0.541). On this basis the AER has explored the differences between these two studies, and found the following:

- *The Beggs and Skeels study has a smaller sample for most years in the sampling period,*

  This point was addressed in the previous section. The only way that a smaller sample size can be hailed as a good thing is if the excluded observations are deemed unreliable. As observed earlier, there is no reason to make such a presumption and good reasons to believe otherwise.

- *For each sampling year, the results from the Beggs and Skeels study generally have a lower standard deviation on the key variables (i.e. including dividends, imputation credits, cum-dividend price, ex-dividend price),*

  This observation is a consequence of the different samples used. The obvious question, however, is whether or not it matters. What is important is not the magnitudes of various sample statistics but rather how well these statistics reflect the underlying population. With larger sample sizes there is every reason to believe that your sample statistics will more accurately reflect the population. If this comes with larger standard deviations then so be it, the world is what it is.

- *SFG does not report the adoption of data filters which are reported by Beggs and Skeels as having been adopted in their study (e.g. the removal of special dividend events).*

  This is an important issue.[23] As discussed earlier, in the absence of the Beggs and Skeels (2006) data set as a reference one can never be sure how closely another data set corresponds to it. That said, the really important question is how much do the differences matter. The fact of the matter is that for most of the results it doesn't seem to matter very much. In cases where it does matter the question reduces to one of whose results to believe. In my opinion, as a matter of good statistical practice, one should go with the larger sample size unless there is reason to believe that there are problems with it. I have seen no compelling argument to believe that the larger sample sizes used by the SFG study should be dismissed as unreliable. By way of analogy we should know more about tomorrow's weather today than we did yesterday. If the two forecasts differ that doesn't make yesterday's forecast wrong, it was made conditional on the information available yesterday whereas the forecast made today incorporates today's information. The fact that there are differences between the estimates of Beggs and Skeels (2006) and SFG(2009) for the post 1 July 2000 sub-sample does not imply that either is wrong. Rather, it simply reflects the fact that the SFG results are conditioned on a more recent sample of observations than are those of Beggs and Skeels (2006).

On this basis, due to the differences in the data used and the sampling / filtering process undertaken across the two studies, the AER considers that the results from the two studies cannot be directly compared. Accordingly, the AER will continue to treat the SFG study and the Beggs and Skeels study as two separate and distinct studies.

[23] Note that SFG address this issue in Appendix I in their response to Question 7, where they state that these filters had been applied.

This is a curious position for the AER to take. The objective and broad strategies of the two studies are the same. The fact that there are some differences does not preclude a comparison. As outlined previously in this section, in my opinion the differences focused on by the AER are largely immaterial.

In order to examine the underlying reliability of the SFG results further (i.e. higher relative standard errors), the AER compared the SFG data set to data independently obtained from Bloomberg. Based on this analysis the AER notes a number of potential underlying shortcomings with the data used by SFG, including:

- *Stock price and dividend series are not consistent in terms of the company-specific basis of quotation, which is potentially a significant issue in cases when the total number of shares outstanding changes (e.g. stock split, bonus share issues),*[1080]

  These are exactly the sorts of observations that are likely to be captured by application of Cook's D statistic. Whether or not this is a problem is something that requires further investigation.[24]

- *It appears that firm-specific announcements made around the ex-dividend date (other than the dividend announcement itself) have not been appropriately controlled for in some cases,*

  This is a valid point, however, what remains unclear is the extent to which such observations are subsequently caught by the filter based on application of Cook's D statistic.[25]

- *Certain dividend-paying observations are excluded from the SFG data, without explanation.*

[1080] *For example, firms using share splits or bonus share issues in the past report artificially high share prices and high dividends quoted on the basis of smaller number of shares outstanding. These observations often have an excessive influence in a least squares regression, under which observations are weighted by their deviation from the sample mean.*

[24] This issue is considered by SFG in Appendix I.

[25] This is investigated further by SFG in Appendix I, where it is seen that approximately two-thirds of the observations identified using Cook's D statistic are readily seen to be unreliable on economic grounds.

> I do not see what this observation is based on. There are observations excluded through the use of Cook's D statistic that one may wish to debate but before either side of the debate could be established it would be necessary for both sides to go and examine the individual observations and to try to establish whether or not their exclusion could be justified on economic grounds.[26]

For these reasons the AER is less confident about the reliability of SFG's results due to the identified data problems (e.g. noise) and the sensitivity of its results to the sample selected. In a relative sense, the AER considers that higher confidence may be placed upon the Beggs and Skeels study, due to the reported data filters and the reported lower standard deviations of key variables compared with the SFG study.

There are issues raised here that are potentially of importance. However, the key word of the previous sentence is 'potentially'. To the extent that the problems occur then they are cause for concern. But to merely allude to the potential for problems does not establish that they are, in fact, problems. There are differences in the approach to filtering between Beggs and Skeels (2006) and the SFG study. In my opinion the former does a better job of it but that is a long way short of saying that the approach adopted by the SFG study is deficient, which is the position taken by the AER even though they have not actually established any such deficiency.[27]

The AER has also considered SFG's use of the Cook's D-statistic to exclude certain observations considered influential. While the AER considers the Cook's D-statistic can be useful to identify specific observations which have an undue influence on the estimation and fitting process, arbitrary exclusion of any observation that is diagnosed as being influential without examination of the underlying reasons is not justified. In addition, SFG's exclusion of the 'most influential 1 per cent' of observations appears arbitrary, and in fact none of the observations identified in the study seem to have a sufficiently high value for the Cook's D-statistic such as to even justify a conclusion that it is indeed influential. On these grounds the AER does not consider that SFG's application of the Cook's D-statistic is appropriate. Accordingly, the AER considers theta estimates generated using this approach are not sufficiently reliable.

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[26] This issue is pursued in Appendix I.

[27] The results presented by SFG in Appendix I, in response to Question 1, address this problem in a manner that, in my opinion, is convincing.

I agree with the AER on this point. I think that the use of Cook's D statistic in the SFG study is not ideal.[28] Specifically, the fact that an observation is influential does not mean that it is a bad observation that should be excluded from the analysis. An observation may be influential because it contains relatively more information than do other observations and so it is important to include it in the analysis. The AER themselves acknowledge that observations identified by Cook's D statistic should be examined to determine why they are influential. Therefore, it is somewhat surprising that the AER chose to hold as evidence against the SFG study information suggesting that the results are sensitive to influential observations rather than exploring why they are influential. In my opinion, this position is no more justifiable than is SFG's decision to arbitrarily exclude the observations. Moreover, if, as alluded to by the AER, the SFG study excludes observations that are not really influential then that should cause no concern at all. If they are not influential then their exclusion will have no significant impact on the estimated coefficients. When one takes into account that the sample sizes used by the SFG study, after the exclusion of observations on the basis of Cook's D statistic, are still larger than those used in Beggs and Skeels (2006) then this amounts to a very minor issue indeed.

In summary, based on its detailed analysis, the AER has concerns over the quality of the market data used in the SFG study, and the robustness of its regression results. The AER's concerns in this regard also relate to the methodology employed, the sampling selection and the filtering process undertaken by SFG. Moreover, while the AER has not re-run its own dividend drop-off study completely, in the process of correcting some of the identified deficiencies in the SFG study, the AER notes the re-estimated values of theta are highly variable.[1081]

[1081] *In particular, once some of the identified discrepancies in the SFG study are corrected by the AER, the point estimate for theta ranges from -0.23 to 0.47.*

Given these concerns, and the likely material impact on the results, the AER does not consider that the SFG study provides persuasive evidence regarding the value of imputation credits. Accordingly, while the AER has given full consideration to the SFG study, it has placed limited weight on theta estimates generated by the SFG study for the purposes of this final decision.

[28] SFG's use of Cook's D statistic in Appendix I is, to my mind, far superior to its use in the SFG study. In Appendix I points identified by Cook's D statistic are only excluded if an economic justification for such an exclusion is found.

In light of my analysis presented above it is difficult to see how the AER has reached its final position on the results of the SFG study. The SFG study does not exactly replicate the methodology of Beggs and Skeels (2006) nor is the data set used in the analysis identical to that used by Beggs and Skeels (2006). However, in my opinion, these differences are of little consequence in assessing the relative merits of the results. Sometimes the differences work in favour of one report over the other, sometimes they work in the other direction.

4. Further Explorations With SFG

In light of questions raised in my review of the SFG study (see Section 4) coupled with the inconclusive nature of many of the issues raised by the AER it seemed sensible to seek some clarification from SFG about these matters. To this end a series of questions was communicated to SFG. The questions and the SFG response to these questions can both be found in Appendix I. In this section I review this response and discuss its implications for my earlier observations. I shall step through the questions and answers in turn.

> ***Question 1:*** *In respect of the data points excluded from the analysis on the basis of Cook's D statistic, it would be useful if these data were identified and investigated to determine what, if anything, was unusual about them.*

This was clearly one of the most important questions asked. The exclusion of influential observations on the basis of Cook's D statistic alone was difficult to justify. By searching for economic justifications for the exclusion of observations SFG have been able to obtain similar results to those obtained in the SFG study without the spectre of the statistical black box that was their use of Cook's D statistic clouding the issue. Indeed, the results that they obtain are even better. If one compares the final line of results presented in column 3 of the Table of results for this question with the corresponding results from Table 1 of the SFG study we see that the franking credit drop-off ratio is now significantly different from zero with an estimated standard error of 0.082 as compared to 0.111 previously. Similarly, for the period ending in May 2004 this estimated standard error reduces from 0.136 to 0.106. At the same time we are also seeing a substantial increase in the adjusted R^2 for the estimated models.

> ***Question 2:*** *Beggs and Skeels (2006) scale their data using a factor of (1+RI), where RI is the rate of return to the All Ordinaries Index over the ex-dividend day.*

- *I can find no evidence of such a scaling of data in the SFG Consulting SAS code provided to me by GT. Was there any such scaling of the data in the construction of data set that accompanied the SAS code provided by SFG?*
- *If the scaling has not been employed, what would be the impact on the magnitudes of the coefficient estimates and the corresponding standard errors of estimate if the data were scaled?*

The example provided by SFG illustrates just how small the scaling factor is in practice, which is why its impact on the results is so small. For this reason it is clear that the omission of this scaling factor in the SFG study was a minor issue. Nevertheless, it is better to establish this with certainty (which is now done) rather than simply speculate about it as had been the case before the response by SFG to this question. In any event, it should also be remembered that Beggs and Skeels (2006) explicitly acknowledged that this scaling was an imperfect way of dealing with noise and so the SFG study had some grounds to question its use.

Question 3: *In Table 1 of SFG (2009), over the period starting 1 July 1997 and continuing through to the end of the period of analysis, it is the case that the estimated coefficient for cash dividend drop-off is always larger in magnitude than the estimated coefficient on the franking credit drop-off except in the middle column of the table, where this relationship is reversed in the 1 July 1999 – 30 June 2000 sub-sample. This reversal does not occur in either the Beggs and Skeels (2006) results or in the SFG (2009) results in the third column of the table. Is there an explanation for this reversal?*

SFG now present compelling economic justifications for why certain observations should be excluded from the analysis. This makes their results much more credible and their results presented in Appendix I represent a substantial improvement over those in the SFG study.

Question 4: *In Table 1 of SFG (2009) why does the addition of observations covering the period 11 May 2004 – 31 December 2006 have so much impact on the parameter* *estimates for the period 1 July 1999 – 30 June 2000 and why is this effect restricted to the middle column?*

> The response of SFG identifies a potential shortcoming of the Beggs and Skeels (2006) formulation if the data are not properly filtered. The path is not through equation (1) but rather through equation (2). In any event, now that SFG have seriously addressed the issue of finding economic justifications for the exclusion of observations this area of concern has become a non-issue.

Question 5: *In the second panel of results presented in Table 1 of SFG (2009), what is the true sample end date as both 30 September 2006 and 31 December 2006 are reported?*

> The issue of the end date of the sample is now completely resolved.

Question 6: *As an alternative to excluding influential observations, how would adopting an estimation technique that is more robust to the presence of outliers than are least squares-based estimators affect the results? For example, it would be interesting to see the impact of re-estimating the model using a Least Absolute Deviations estimator.*

> The decision not to explore LAD estimation is not unreasonable. The question was posed as an alternative way of thinking about unreliable observations but the correct solution was always to properly interrogate those observations thought to be unreliable. SFG have now done this and so the problem becomes moot.

Question 7: *Beggs and Skeels (2006) applied a series of filters to their data set, as described in Appendix II of their paper. Specifically:*

- *All observations where the dividend payment, the corporate tax rate, the cum-dividend share price or the ex-dividend share price was not known were removed.*
- *All cases where the market capitalization of a company was not reported, or where the weight of market capitalization in the All Ordinaries index was less that 0.03 percent were eliminated.*

- *The data set was screened for any companies that changed their basis for quotation within 5 days either side of the ex-dividend day and any 'special' dividend payments were also removed, where special dividends are an irregular distribution of excess cash reserves.*

Which, if any of these filters have been applied as part of the analysis reported in SFG (2009)? How would the application of all of these filters impact upon the results obtained? Do these filters exclude from the analysis all of those data points excluded through the use of Cook's D statistic?

SFG observe that the filters used by Beggs and Skeels (2006) were, in fact, used in the SFG study and that exclusion on the basis of Cook's D statistic was an additional level of filtering. I return to my previous observation that SFG are now interrogating the data economically, as well as statistically, making their new results much more credible than their earlier results.

Question 8: *The AER (2009) notes differences in the magnitudes of the reported standard errors of estimate between Beggs and Skeels (2006) and those provided in the middle column of SFG (2009) for the 1 July 2000 – 10 May 2004 sub-sample. I note that there is not a corresponding difference between the results of Beggs and Skeels (2006) and those of the third column of SFG (2009). Would applying the filters discussed above materially impact upon the disparities between the two sets of standard errors of estimate?*

It is clear that the results in the second column of Table 1 were provided in the SFG study as a basis of comparison but were not their preferred results. That the AER chose to focus its attention on these results was unfortunate. Still, it was somewhat understandable given the questions that existed surrounding the use of Cook's D statistic. Now that these questions have been resolved, so that Cook's D statistic is used in a more sensible and justifiable way, there is no reason to consider any of SFG's results other than those provided in column 3 of the table appearing in the response to Question 1 in Appendix I.

Question 9: *The AER (2009) raised a concern about the company tax rates applied by SFG (2009) not corresponding to the reported ATO tax rates. The SAS code provided by SFG employs the following tax rates (denoted by t):*

01 July 1996 – 30 June 2001:	*t=0.36;*
01 July 2001 – 30 June 2002:	*t=0.34;*
On or after 01 July 2002:	*t=0.30;*

Please comment on any differences between these rates and the ATO rates and what, if any, are the empirical implications of any differences.

> There was an error with the corporate tax rates and it has now been resolved. The observation that it had minimal impact is no substitute for correcting the mistake, which has now been done.

5. Concluding Remarks

The arguments presented by the AER against the results presented by the SFG study are, in my opinion, unconvincing. For the most part the AER's arguments are nothing more than allusions to potential problems in SFG's analysis, problems whose existence can readily be determined one way or the other; for example, problems arising from multicollinearity in the data. For the most part, my analysis suggests that these problems do not exist; for example, the absence of evidence of multicollinearity affecting SFG's results. Although there are some minor issues that one may wish to take with the SFG analysis it is difficult to see how the AER came to a conclusion that they would likely have had a 'material impact on the results.

Of course, the most complete response in this case is to actually investigate those aspects of the SFG study that were causes of concern. This has now been done through (i) the questions presented in Appendix I and (ii) the SFG response presented in Appendix I. I find that the results now presented by SFG are quite convincing. This leads me to consider that their estimate of theta of 0.23 is the best such estimate currently available for Australia. It might be argued that their methodology does not perfectly replicate that of Beggs and Skeels (2006) and that the remaining differences may downwardly bias the estimates provided by SFG in Appendix I. I am not one who shares that view as I think that their analysis is now compelling. However, if one was to take that view then I think that a very strong case could be made for the true value of theta to lie somewhere between the SFG estimate of 0.23 and the Beggs and Skeels (2006) estimate of 0.57, and in all probability to lie towards the lower end of that range. Any higher value for theta seems completely implausible, both in terms of the empirical evidence presented and in terms of the theoretical arguments underpinning them.

References

Australian Energy Regulator (2009). Final Decision – Electricity transmission and distribution network service providers: Review of the weighted average cost of capital (WACC) parameters.

Beggs, D.J. and C.L. Skeels (2006). Market Arbitrage of Cash Dividends and Franking Credits.*The Economic Record*, 82 (258), 239-252.

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Appendix I: The response by SFG to questions raised by this report

Note: Supplied with the answers to these questions are three additional outputs of the model which we have prepared to illustrate particular points made in the answers. These are:

- Model A which amends only the tax rates applicable in 2000-01 and 2001-02 which we have done in response to Question 9;
- Model B which includes the amendments above and also scales the data for the changes in the All Ordinaries Index which we have done in response to Question 2; and
- Model C which removes 20 data points which we have done in response to Question 1.

The code and output files from each of these are provided with these answers.

Question 1: *In respect of the data points excluded from the analysis on the basis of Cook's D statistic, it would be useful if these data were identified and investigated to determine what, if anything, was unusual about them.*

Answer:

We used the Cook's D influence statistic to identify the most influential 1 per cent of observations (33 observations), and repeated our analysis excluding these data points. Of these 33 data points, 16 correspond to the period subsequent to 1 July 2000, three correspond to the period 1 July 1999 – 30 June 2000 and 14 correspond to the period 1 July 1997 – 30 June 2000. The data points excluded are listed in the table below.

Code	Date	Code	Date	Code	Date
AGG	21-Feb-00	CHB	22-Jun-06	CHO	9-Sep-03
BIL	8-Aug-01	FPA	6-Jun-03	AQP	11-Sep-06
CPU	25-Sep-97	BSO	21-Dec-98	SMS	29-Oct-97
PSN	10-Sep-98	AMW	6-Nov-97	TPI	27-Mar-06
BSO	22-Jun-98	CSL	18-Sep-06	AGG	4-Aug-00
AGG	20-Aug-01	PSN	18-Mar-02	PDG	26-Feb-01

Code	Date	Code	Date	Code	Date
AGG	17-Feb-03	AHA	26-Mar-98	CIN	8-May-01
MRL	3-Sep-01	MAY	8-Oct-98	HAN	4-Apr-05
HSM	10-Nov-00	AMW	17-Jul-97	COH	24-Feb-00
TCL	15-Sep-97	PSN	26-Mar-98	CSL	29-Oct-97
AMW	21-Jan-98	STV	8-Mar-00	SEV	8-Oct-98

A data point could be influential because of an error in the data which generates an extreme share price or dividend change. Or it could be a valid data point but merely has a large amount of influence on the coefficient estimates. In an event study (a dividend drop-off study is just one type of event study in which we measure the share price reaction) each share price movement will be affected to some degree by other price relevant information released on the same date. For example, on the same day that the stock trades ex-dividend, the company could announce the acquisition of a competitor. In the individual case it would be difficult to infer whether the share price movement is due to the ex-dividend event, or the market's reaction to the other price-relevant information.

However, if the sample is large enough we would expect observations in which the share price was positively affected by contemporaneous information to be countered by observations in which the share price was negatively impacted by contemporaneous information.

Despite this expectation, if a handful of observations are extreme, the sample may not be large enough for this noise to be filtered out of the analysis. Hence, our technique was to determine whether a very small proportion of data points had a material impact on the analysis. It turns out that this was the case, as evidenced by the substantial increase in explanatory power (R-squared statistic), lower standard errors and stability of coefficients over time, once this very small number of data points is removed. When we wrote our report we did not separately investigate these influential data points, preferring to present our analysis both including and excluding these data points, and express our preference for the filtered sample on the basis that the excluded data points have a high chance of containing data errors.

Subsequent to the AER's review, we identified that 11 of the 33 excluded data points are likely to have been affected by contemporaneous information, so should not be relied upon. In particular, in four instances, the ex-dividend date corresponded to the date at which shareholders were eligible to participate in new share issues. These issues substantially increase the number of shares on issue, resulting in large share price falls on the ex-dividend date. The remaining seven data points are likely to have been affected by price-sensitive information released on or around the announcement date. The table below lists the 11 data points which we have identified are likely to have been affected by contemporaneous price-sensitive information.

Code	Date	Reason
BIL	8-Aug-01	Bonus issue (3.1287 for 1) associated with dual listing.
CPU	25-Sep-97	Bonus issue (3 for 1).
PSN	10-Sep-98	Interim results announced on cum-dividend date.
AGG	20-Aug-01	Native title determination announced on day after ex-dividend date.
MRL	3-Sep-01	Bonus issue (4 for 5).
CSL	18-Sep-06	Merger announcements either side of ex-dividend date
AHA	26-Mar-98	New power plant agreement announced on ex-dividend date.
SMS	29-Oct-97	New contract announced on day after ex-dividend date.
TPI	27-Mar-06	Merger announced on ex-dividend date.
AGG	4-Aug-00	New gold mine officially opened with release of production information.
CIN	8-May-01	Issue of shares in AHL (1 for 1) in exchange for CIN shares

While conducting this analysis of individual data points we also excluded nine data points comprising Transurban stapled securities, each of which comprised one share in Transurban City Link Ltd, one unit in the Transurban City Link Unit Trust and 499 Equity Infrastructure Bonds. These nine data points had cum-dividend prices which ranged from $1,045 to $2,005, respectively which is approximately 10 – 20 times the next highest cum-dividend price in the data set which is $110.40. As the form of the analysis is in dollars per share, these extreme high priced securities have a substantial influence on coefficient estimates. None of these data points appear in the period post July 2000 but they contribute to the instability of coefficient estimates over time and the standard errors of the estimates.

We repeated our analysis after excluding these 20 data points. As discussed below in respect of questions 2 and 8, we now also account for the market movement on the ex-dividend date and correct for incorrect corporate tax rates which affected two years of analysis. After incorporating these changes, we summarise our results in the table below.

	Beggs&Skeels (2006)			All observations			Excl. 20 contaminated points		
	Cash	Franking	N	Cash	Franking	N	Cash	Franking	N
Regression analysis ending 10 May 2004									
1 Jul 85 –	0.465	0.752	910						
30 Jun 88	(0.040)	(0.157)							
1 Jul 88 –	0.646	0.450	546						
30 Jun 90	(0.064)	(0.119)							
1 Jul 90 –	0.765	0.376	239						
30 Jun 91	(0.115)	(0.206)							
1 Jul 91 –	0.861	0.201	1,669						
30 Jun 97	(0.059)	(0.103)							
1 Jul 97 –	0.795	0.418	573	0.904	0.287	710	0.901	0.295	698
30 Jun 99	(0.099)	(0.186)		(0.289)	(0.676)		(0.084)	(0.180)	
1 Jul 99 –	1.168	0.128	267	0.311	1.061	329	0.827	0.358	328
30 Jun 00	(0.099)	(0.204)		(0.228)	(0.402)		(0.106)	(0.236)	
1 Jul 00 –	0.800	0.572	1,310	0.998	0.402	1,391	1.015	0.129	1,386
10 May 04	(0.052)	(0.121)		(0.251)	(0.610)		(0.038)	(0.106)	
			5,511	Adj-R^2	2.1%	2,430	Adj-R^2	37.0%	2,412
Regression analysis ending 30 September 2006									
1 Jul 97 –				0.936	0.289	710	0.931	0.240	698
30 Jun 99				(0.269)	(0.643)		(0.074)	(0.171)	
1 Jul 99 –				0.166	1.434	329	0.827	0.358	328
30 Jun 00				(0.118)	(0.646)		(0.109)	(0.241)	
1 Jul 00 –				1.040	0.260	2,182	0.983	0.230	2,175
30 Sep 06				(0.181)	(0.428)		(0.031)	(0.082)	
				Adj-R^2	3.9%	3,221	Adj-R^2	44.5%	3,201

We observe that the estimated value for imputation credits dividends in the period from 1 July 2000 – 30 September 2006 is 0.26 using all observations and 0.23 if the data points discussed above are excluded. The corresponding estimates for the value of cash dividends are 1.04 and 0.98, respectively. With the exclusion of the data points discussed above, we observe considerably greater stability in coefficient estimates over time, and substantial increases in explanatory power, with the adjusted R-squared statistic increasing from 3.9 to 44.5 per cent.

Question 2: *Beggs and Skeels (2006) scale their data using a factor of (1+RI), where RI is the rate of return to the All Ordinaries Index over the ex-dividend day.*

- *I can find no evidence of such a scaling of data in the SFG Consulting SAS code provided to me by GT. Was there any such scaling of the data in the construction of data set that accompanied the SAS code provided by SFG?*

- *If the scaling has not been employed, what would be the impact on the magnitudes of the coefficient estimates and the corresponding standard errors of estimate if the data were scaled?*

Answer:

In our earlier paper, we did not scale our ex-dividend share prices by (1 + the return on the All Ordinaries Index) as we did not expect this to have a material impact on the coefficient estimates and standard errors. The objective of a dividend drop-off study is to estimate the price of a stock including and excluding the attached dividend. Our proxies for these two values are share prices observed on the cum- and ex-dividend dates. This is an imperfect proxy because in the intervening 24 hours, price-sensitive information may be conveyed to the market which affects the price we would expect to observe excluding the dividend. The scaling factor is used to remove the impact of market movements on the expected share price, under the expectation that each stock is affected by the same degree to market-wide information.

This market-wide information is sometimes positive news and sometimes negative news, so the average market return on any given day is expected to be approximately zero. For example, if the annual market return is 12 per cent and there are 260 trading days in a year, the expected return on any given day is 0.04 per cent, computed as (1.12)^(1/260)-1. Provided the ex-dividend events occur with equal probability on days in which the market rises or falls, the coefficient estimates should be unchanged. In theory, the standard errors would be expected to fall, under the assumption that this scaling reduces the noise in data associated with overall market fluctuations.

In the results presented in the table above, we have now incorporated this scaling adjustment. As noted above, we prepared Model B which when compared with Model A differed only in that the scaling was undertaken. This change did not materially affect the results.

Question 3: *In Table 1 of SFG (2009), over the period starting 1 July 1997 and continuing through to the end of the period of analysis, it is the case that the estimated coefficient for cash dividend drop-off is always larger in magnitude than the estimated coefficient on the franking credit drop-off except in the middle column of the table, where this relationship is reversed in the 1 July 1999 – 30 June 2000 sub-sample. This reversal does not occur in either the Beggs and Skeels (2006) results or in the SFG (2009) results in the third column of the table.Is there an explanation for this reversal?*

Answer:

The one-year period from 1 July 1999 to 30 June 2000 contains just 329 observations out of 3,221 (or 10% of data points) so the coefficient estimates from this period are relatively more susceptible to influential observations. Once we excluded the most influential 1 per cent of data points, the estimated value for cash dividends increases from 0.100 to 0.797, and the estimated value for imputation credits decreases from 1.439 to 0.224.

In the analysis presented above, there is one data point excluded from the period 1 July 1999 to 30 June 2000, relating to the Transurban stapled security, which had a cum-dividend price of $2,005. With this exclusion we observe the estimated value for cash dividends increase from 0.166 to 0.827, and the estimated value for imputation credits decrease from 1.434 to 0.358.

This illustrates an important point we have made throughout our analysis - that coefficient estimates from regression analysis need to be interpreted jointly. In sub-samples where the estimated value for imputation credits is high, the estimated value for cash dividends is low, and vice versa. In theory, the market's estimate of the value of cash dividends should be independent of its valuation of imputation credits. However, when a stock paying a fully-franked dividend has a high drop-off ratio, this will increase the model's estimated value for imputation credits, and also lower its estimated value for cash dividends, and vice versa.

Question 4: *In Table 1 of SFG (2009) why does the addition of observations covering the period 11 May 2004 – 31 December 2006 have so much impact on the parameter estimates for the period 1 July 1999 – 30 June 2000 and why is this effect restricted to the middle column?*

Answer:

The form of the regression model estimates the coefficients for all time periods jointly, applying weights to observations throughout the three time periods which fluctuate according to the characteristics of the entire data set. This means that when we add additional data points to the third time period, the weights placed on observations in the earlier time periods are affected, and the coefficient estimates are subsequently altered. This has considerably less impact when the most influential 1 per cent of observations are excluded (or just removing the Transurban data point), because with this cleaner sample, the coefficients are already more stable and have relatively lower standard errors than the full sample.

Question 5: *In the second panel of results presented in Table 1 of SFG (2009), what is the true sample end date as both 30 September 2006 and 31 December 2006 are reported?*

Answer:

30 September 2006.

Question 6: *As an alternative to excluding influential observations, how would adopting an estimation technique that is more robust to the presence of outliers than are least squares-based estimators affect the results? For example, it would be interesting to see the impact of re-estimating the model using a Least Absolute Deviations estimator.*

Answer:

We would not recommend the use of the Least Absolute Deviation technique as a preferred method for dealing with influential observations, because the basis for excluding 1 per cent of data points was not merely that they were influential. The data points were excluded because they had a high probability of being invalid, implying that they should carry zero weight in the analysis, rather than the reduced weight which would be implied by a Least Absolute Deviation approach.

We understand that the question that arises with the use of our exclusion technique is not that we have excluded some data points which are invalid. Rather, what can only be of potential concern is that we may have also excluded a handful of valid data points, but which merely showed up as being highly influential.

However, our response is that we are excluding only a very small proportion of the sample, and achieving a large improvement in explanatory power and error minimisation. In other words, we want to draw conclusions from a sample of over 3,000 data points. By keeping a handful of influential data points we would end up drawing conclusions primarily based on a fraction of the sample, and that fraction is the sub-sample most likely to be affected by invalid data.

In the analysis presented above we have excluded data points only if we could identify specific price-sensitive information released on or around the ex-dividend date, or in one instance (ie Transurban) a security with an extremely high price. With these exclusions the standard errors are considerably reduced, explanatory power increases substantially and the coefficient estimates are relatively stable across time.

Question 7: *Beggs and Skeels (2006) applied a series of filters to their data set, as described in Appendix II of their paper. Specifically:*

- *All observations where the dividend payment, the corporate tax rate, the cum-dividend share price or the ex-dividend share price was not known were removed.*

- *All cases where the market capitalization of a company was not reported, or where the weight of market capitalization in the All Ordinaries index was less that 0.03 percent were eliminated.*
- *The data set was screened for any companies that changed their basis for quotation within 5 days either side of the ex-dividend day and any 'special' dividend payments were also removed, where special dividends are an irregular distribution of excess cash reserves.*

Which, if any of these filters have been applied as part of the analysis reported in SFG (2009)?How would the application of all of these filters impact upon the results obtained? Do these filters exclude from the analysis all of those data points excluded through the use of Cook's D statistic?

Answer:

We excluded observations in which:

- the market capitalisation was less than 0.03 per cent of the All Ordinaries Index, where market capitalisation was measured at the end of the month in which the shares traded ex-dividend.
- the share prices or dividends were unknown.

We did not exclude any data points for lack of corporate tax rate data. However, please refer to the discussion of the corporate tax rate under point 9 below.

The application of the filters do not exclude from the analysis all of the data points excluded through the use of the Cook's D statistic. All the results reported in columns two and three of our February 2009 report and the table above apply the filters described in this question and the differences between the second and third columns in the February 2009 table results from the application of the Cook's D statistic.

Question 8: *The AER (2009) notes differences in the magnitudes of the reported standard errors of estimate between Beggs and Skeels (2006) and those provided in the middle column of SFG (2009) for the 1 July 2000 – 10 May 2004 sub-sample. I note that there is not a corresponding difference between the results of Beggs and Skeels (2006) and those of the third column of SFG (2009). Would applying the filters discussed above materially impact upon the disparities between the two sets of standard errors of estimate?*

Answer:

The third column reported in our analysis is the more reliable sample, because of the exclusion of the most influential 1 per cent of observations. The standard errors are considerably lower for this sample and hence are closer in magnitude to those reported in Beggs and Skeels (2006).

Question 9: *The AER (2009) raised a concern about the company tax rates applied by SFG (2009) not corresponding to the reported ATO tax rates. The SAS code provided by SFG employs the following tax rates (denoted by t):*

01 July 1996 – 30 June 2001:	*t=0.36;*
01 July 2001 – 30 June 2002:	*t=0.34;*
On or after 01 July 2002:	*t=0.30;*

Please comment on any differences between these rates and the ATO rates and what, if any, are the empirical implications of any differences.

Answer:

Our analysis assumes the following corporate tax rates, which are marginally different from the actual tax rates and which are in error. This is an error on our part but one which is unlikely to have a material impact on the results. The differences are as follows:

2000-01: We used a company tax rate of 36% when the actual company tax rate was 34%.

2001-02: We used a company tax rate of 34% when the actual company tax rate was 30%.

The company tax rate was reduced from 36% to 34% and then 30% over this two year period. Our code had the timing of this adjustment incorrect by a one year in each instance.

The analysis presented above uses the correct tax rates. This change would not have materially affected our earlier conclusions.

	Model A. Tax estimates corrected				Model B. Market adjustment included			
	All observations		Excl influential 1%		All observations		Excl influential 1%	
	Coeff	Std Error	Coeff	Std Error	Coeff	Std Error	Coeff	Std Error
Large firms (Market cap > 0.03% of All Ords)								
Intercept	0.001	0.009	-0.007	0.003	-0.002	0.010	-0.007	0.003
Cash - Period 5	0.763	0.238	0.843	0.085	0.936	0.269	0.888	0.091
Cash - Period 6	0.101	0.079	0.796	0.102	0.166	0.118	0.900	0.106
Cash - Period 7	0.933	0.172	0.912	0.050	1.040	0.181	0.969	0.050
Franking credit - Period 5	0.436	0.583	0.246	0.186	0.289	0.643	0.221	0.197
Franking credits - Period 6	1.442	0.570	0.224	0.240	1.434	0.646	0.098	0.254
Franking credits - Period 7	0.336	0.412	0.251	0.116	0.260	0.428	0.234	0.117
N	3221		3188		3221		3188	
Adjusted R squared	3.4%		30.9%		3.9%		34.0%	

Model C. Excluding 20 datapoints and not excluding on the basis of the Cook's D statistic presented in response to Question 1.