TRANSCRIPT OF PROCEEDINGS

AUSTRALIAN ENERGY REGULATOR

RATE OF RETURN INSTRUMENTS
CONCURRENT EVIDENCE SESSION 1 of 4

MATTERS DISCUSSED:
AER'S Industry Debt Index (EICSI), and
Equity Beta

FACILITATOR:
Ms Anna Brakey (Commissioner, ACCC)

AER STAFF PRESENT:
Mr Warwick Anderson (GM Network Pricing, AER)
Mr Jim Cox (Board Member, AER)
Mr Eric Groom (Board Member, AER)
Ms Catriona Lowe (Board Member, AER)
Mr Justin Oliver (Board Member, AER)
Ms Clare Savage (Board Member, AER)
Mr Esmond Smith (Senior Financial Advisor, AER)
Mr Jonathan Seymour (Assistant Director, AER)

EXPERTS PRESENT:
Dr Glenn Boyle
Dr Tom Hird
Mr Jim Hancock
Mr Dinesh Kumareswaran
Dr Martin Lally

RECORDED VIA VIDEOCONFERENCE

THURSDAY, 10 FEBRUARY 2022 AT 9.30AM

MS BRAKEY: Welcome everybody to the first session of the 2022 Rate of Return Instruments Concurrent Evidence Sessions, and welcome to all of those participating, the board members and the experts, and to all of those people who are observing as well.

I am Anna Brakey. I am one of the commissioners at the ACCC and I am facilitating this discussion. I am not participating in it; I am just the facilitator.

But first, I would like to acknowledge the traditional owners of country throughout Australia and recognise their continuing connection to land, water and community. We pay our respects to them and their cultures, elders past present and emerging, and finally I extend that respect to other Aboriginal and Torres Strait Islander people who are presenting today.

The purpose of this session is to assist the AER board to decide on the 2022 rate of return instrument by hearing the views of experts and having discussion with those experts, and listening to the discussion between experts as well.

We have all the AER board members participating in this session. I'm not sure, Clare, do you want to do a little intro or introduce the board members?

MS SAVAGE: Sure, yes. I'm Clare Savage. I'm the Chair of the Australian Energy Regulator. We have Jim Cox, who probably would be well-known to most of who with us, who is our deputy chair; Catriona Lowe; Justin Oliver and Eric Groom who are my board colleagues all here today.

MS BRAKEY: In December this year, the AER will publish the next rate of return instrument. This binding instrument will determine the allowed rate of return on capital in both electricity and gas networks for the following four-year period.

The rate of return is a significant driver of regulated revenue. Estimating the rate of return is complex and contentious. These sessions are a key element of the AER board's decision-making process in making the instrument.

If you want more information about these expert sessions, I recommend you go to the AER's website where it has published a series of papers on the process it is undertaking to develop the 2022 rate of return instrument. This will also provide details on how to further engage in the process, such as through submissions which are open at the moment and due to close on 11 March in advance of the AER making its draft instrument in June.

For those guests listening in to the session, that is the primary means for you to

engage in that process, through submissions. So you can raise any issues that are discussed today or in fact any other aspect of rate of return in your submissions.

For today, this morning's session will focus on the use of the AER's industry debt index and the assessment of equity beta for use in determining the return on equity. The experts that will be discussing the issue today with the board are Dinesh Kumareswaran, Tom Hird, Glenn Boyle, Martin Lally and Jim Hancock.

A reminder to experts that you have been asked here to assist the AER to make a responsible decision on the 2022 instrument. For these sessions, the AER would like to reinforce its expectations that your obligation is to assist the AER board in its decision-making process, and you should present the analysis and evidence that will assist the board in making that decision rather than positioning yourself in the role of the decision maker.

Given the scarcity of time in these sessions, the board has attempted to prioritise the issues and questions for discussion in the sessions, and the prioritisation was set out in the detailed agenda. As such, we are going to start the first session with a short presentation from Tom Hird and Martin Lally with their

thoughts on the AER's use of the index, after which we will call for other points that are genuinely additive from the other experts. We will then spend an hour discussing the index, focusing on the three main questions that are set out in the agenda, which are: The role that the index should take in the rate of return instrument; whether any outperformance on the return of debt requires an adjustment to the return on debt; and should the index be used to inform the term for the return on debt, and if so, how?

We will then move onto the issue of equity beta. We will also start out with short presentations by Glenn Boyle and Dinesh Kumareswaran, followed by any additional points by other experts.

The general discussion for equity beta is also scheduled to take about an hour and will focus on two main issues for the 2022 instrument: Should the AER continue to use the longest available estimates of beta for its comparator firms to set the equity beta; and if the AER moves to a five-year estimate of the return on equity, does this have implications for the period over which it measures beta? And should the AER place more reliance on the estimates of beta over the last five years?

We'll aim to finish this morning's session by about midday Sydney time, so unless any of the board members want to add anything now? I don't see anyone jumping up and down. I'll ask Tom Hird to start off with his views on the index, please.

DR HIRD: Thanks, Anna. I had a presentation which I think someone was going to bring up on the screen?

MR SEYMOUR: I think our preference was just to have people look on. I think all the board members should have all the details in front of them and we've sent it round to everyone who is registered through the event. So I think to avoid any technical issues, we'll just keep it in front of us.

DR HIRD: That's fine.

MS SAVAGE: It helps up to read your face as well and the other experts' faces as you go.

DR HIRD: Okay. Well, not if you're looking at my slides, it doesn't.

MS SAVAGE: We can do both. We're multitaskers at the best of times.

DR HIRD: I'm now talking to slide 2. Essentially, the point, a really important point is that before we start thinking about how the EICSI should be used, we have to define what the AER is trying to do.

In my view, there are two decisions the AER has to take. The first decision - and this is in relation to coming up with a sort of process for compensating for the cost of debt - the first decision the AER has to take is what is the benchmark efficient debt management strategy?

Having made that decision, the next decision is, how can the AER most accurately compensate for the cost of following that debt management strategy? So those are the two decisions. And I think that just to sort of give that some, you know, that's sort of conceptual but in practice to describe what the 2018 RoRI found was that the benchmark debt management strategy was the staggered issuance of 10-year debt, giving rise to a trailing average cost of debt and an assumption of 60 per cent gearing.

That was the 2018 RoRI answer to decision one, "What is the benchmark strategy?" The 2018 RoRI answer to question 2, which is how best to compensate for the cost of following that strategy, was a change in method. Previously, the AER was assuming that a broad BBB estimate of the cost of debt would be appropriate, would reflect the cost that NSPs incurred in following that strategy. In 2018, RoRI and the AER used NSP debt data - essentially a version of the EICSI, if you like - to determine that actually

that target credit rating should be raised from BBB to BBB+, and that would accurately compensate for the costs of the benchmark strategy.

That is the framework that I say is appropriate, or that the decisions that the AER should be trying to make.

If I move on now, talking to the third slide, where I'm describing the use of the EICSI or NSP debt data, I've got a nice table there which just says the two things you can use that data for is to inform decision one and to inform decision two. So long as you are using the data to inform those two things, then it is an entirely valid use of that data.

I've got a little cross against a use of the EICSI which I don't regard as valid. That is, I say, it would be invalid to attempt to target compensation to a particular construction of the EICSI. That is, if the debt management strategy underpinning the EICSI construction is different to the benchmark debt management strategy.

Essentially, I'm really trying to draw home here is that you can use the industry debt management data validly to determine a benchmark and then to determine the costs of implementing that benchmark, but that's the process through which the NSP data is to be used. And the reason I say it's invalid to instead determine a

benchmark and then target compensation to a construction of the EICSI that may not be the same as that benchmark is to illustrate with an extreme hypothetical example.

Imagine the average industry practice changed and it turned out to be that NSPs issued just one-year debt. So they are refinancing 100 per cent of their debt every year. In that case, there may well be, would likely be a large gap between the compensation paid to NSPs based on a 10-year tenor and that the measure of industry average costs such as the EICSI would reflect a one-year tenor.

So let's imagine there's this large gap and something has to be done. So I say in that situation, it would be entirely valid for the AER to look at that data and say, "Well, it's appropriate to reduce the benchmark tenor and in doing so reduce the gap between the compensation that's being provided and the estimate of the industry average costs."

But it would be invalid to retain a 10-year benchmark assumption and reduce the cost by just reducing the estimated yield on those 10-year debts so that they matched essentially the cost of a one-year instrument. That approach would involve essentially populating a 10-year trailing average with the cost of one-year instruments.

It would create an unhedgeable allowance, an inconsistent allocation of risks, and it would essentially ensure that compensation would never match efficient costs because you would be paying people the average of 10 years' worth of one-year debts when the evidence was that they were raising all of their debt rolling it all over in one year. So you have this trailing average that just didn't match the evidence of what businesses were actually doing.

That's the hypothetical. You know, that's the extreme example I've tried to use to explain why using the industry debt data to inform a benchmark and then to cost the benchmark avoids that internal inconsistency problem of allowing or targeting some measure of cost that is not consistent with the benchmark that you've set.

Now I think I'm just going to talk to slide 5 of my pack, where essentially I'm saying that I think actually the AER has historically performed a valid use; they have validly used NSP debt data to inform both those two decisions. And I think there was, you know, leading up to the most recent paper, the November 2021 final working paper, I was concerned that the AER was going down a path of invalidly using the EICSI, that is, to target compensation to the EICSI.

But it seems to me that the most recent

preliminary position is actually a valid use, which is to ask the question, we're using a BBB+ benchmark, and we are asking the question, "Do NSP, when adjusted for tenor and credit rating, do they issue debts at a cost that's similar to our assumed benchmark?" I think that's the right question.

I think there are other ways that the AER has gone about attempting to answer that question. But the way that the AER has attempted to answer that question is valid. I think there is not a lot of controversy at the moment in terms of where the AER is heading and where I think it should head.

Moving on to slide 6, that's a sort of conceptual overview of what I think should be done. And just moving on to what the actual industry debt data is pointing to, the AER says that the industry debt data suggests that there has been, when adjusted for tenor and credit rating, a small outperformance of 4 basis points over the last seven and a half years.

My own estimates, I think using a different method but similar, result in actually a modest underperformance of 1 basis point over the last seven years and 3 basis points outperformance over six years and negative outperformance over one to four years. So it's a bit of a mix.

One point that I would note in terms of my own analysis is that the AER's measure of outperformance is not value-weighted and the AER does not present value-weighted estimates. I think the AER should present them. Even if the AER thinks that they are less relevant for whatever reason, I think presenting the information in that way would be useful. When I estimate a value weighted measure of this sort of tenor and credit rating matched out performance --

MS BRAKEY: Tom, could you make sure you go through quickly? You've used up about 10 minutes already.

DR HIRD: So there is always negative (indistinct).

The next question is, "Well, what is the benchmark strategy?" And the AER and I again reach similar answers looking at the data: The average WATMI is around 10 years. So the ultimate or final conclusion from that is what should the AER do with that NSP debt data? And it seems to me that that data is consistent with the AER having its settings currently appropriate.

MS BRAKEY: Thanks, Tom. Now we're off to Martin.

DR LALLY: Thanks, Anna. I have circulated a note. I trust people have that.

Looking at the first question that was

posed, what the role of the EICSI might be in the rate of return instrument, in preparing a paper for the AER in April last year, I suggested that the AER should decompose the difference between the EICSI estimate and the estimate it was obtaining from its current methodology into the three components term, credit rating and the rest, which is the so-called outperformance. And it seems that the AER has taken up that suggestion and done that, and the figure here that is referred to of 4 basis points is after matching the EICSI and the existing indexes on both credit rating and term.

The interesting question then is what do you do with this analysis? In so far as there is a difference, it's only 4 basis points after correcting for term and credit rating. If it were more substantial - and of course that possibility will arise as we move forward in the future. The EICSI may diverge from the results you are getting from the standard with the current methodology after correcting for term and credit rating for matching them. If there is a substantial difference, you'd want to try and understand why that's happened. Maybe there's an error somewhere, maybe there's an outlier that needs removing.

In addition, it may be the difference is

being driven by differences in the way that the EICSI and the current methodology are being constructed. An example of that was referred to by Tom: Value-weighting. Well, the EICSI isn't value-weighted whereas the RBA is value-weighted, Bloomberg is and Reuters are partly value weighted in the sense of removing small bonds.

So I think before you attach any significance to the 4 basis point figure or whatever it is, it's not just enough to match on term and credit rating. You've also got to match on methodology. And at the moment, it does not seem that that methodology's been matched on. I think that is important to do that.

After you've matched on methodology, well, what happens then? The EICSI data might be used to revise your values for term and credit rating. It looks as if credit rating they match on; it looks like they don't match on term. They did back in 2018 at 10 years, but it looks like the EICSI is now giving you lower values. That is an indicator that maybe you want to adjust your term down from 10 years to eight years, but you would need enough data to be confident of that.

As far as the outperformance is concerned, I stress outperformance is only interesting after you've cleaned up any differences in the methodology, which hasn't happened yet. If it's

not statistically significant or economically significant, I think we should just ignore it. 4 basis points, at the moment I don't know whether it's statistically significant, but it is not economically significant so I think you could ignore that.

But if you do get to a point, after matching on term and credit rating and cleaning up any differences in methodology, that you are getting an outperformance that is statistically significant and economically significant then it raises the question of whether you are going to adjust your existing numbers.

And that requires that you be pretty confident about the EICSI methodology. And confidence would require not just that methodology matches with RBA, et cetera, but the sample size has got to be large enough. I don't know how big the sample size is, but because you're using primary market debt, I suspect it's pretty small. It may be that if you can't get a sample size that is very large then the most that you will get out of this EICSI data is to give some indication to you about some possible problems: Is there something wrong with data, is there an error, or is there something that needs cleaning up? So, that's not a recommendation, but that's just some thoughts about what the AER

might do with this.

The other questions that are asked, the implications of outperformance and the use of the EICSI for estimating the term, I've covered them, I think, in answering the first question.

MS BRAKEY: Thanks, Martin. I'll first invite any questions from the board members. And sorry, I should have said earlier if any of the participants - the board members or the experts - want to say something, if you could just use the "raise hand" function and it helps me to direct traffic. Clare, straight to you.

MS SAVAGE: Thanks, Anna. Martin, I was just wondering whether or not would might wish to reflect on the extreme hypothetical that Tom presented and just talk us through how your analysis might apply to that?

DR LALLY: The extreme hypothetical being what?

MS SAVAGE: I think Tom was setting out a suggestion that you might have a situation where network businesses all began financing over one year and we obviously still have a 10-year benchmark. I was just wondering in that case how your analysis would apply?

DR LALLY: Yes, well, if the EICSI data indicates that they are all moving to one year that of course would be surprising. But it is just a hypothetical example. It might be a bit more

realistic if we said they were moving to six years.

That would be an indicator to you that you should reconsider in your current methodology using 10-year term and moving to six-year. We are currently somewhat in that position. The EICSI data is indicating at the current time that the average term is about eight years rather than 10.

But bear in mind my comment that the EICSI data is useful in this respect to the extent that the sample size is large enough. If you are only getting two observations per year, whereas from the existing methodology you're getting a far larger number of observations then that's not terribly useful. And I don't know what the sample size it. So the bottom line is that if the data is large enough and the EICSI data is indicating that they are moving to six years then you've got to think seriously about moving to six years as your benchmark.

MS SAVAGE: Just as a follow-up comment, one of the difference is obviously that you can make an adjustment for term, but one of the things that we've done in looking at decomposing the impact of the EICSI in terms of the term and raising and residual, as you suggested in your work, is that we are seeing that there is this sort of

residual, and the team are suggesting that that is linked to, in some way I guess, the difference between the primary and secondary markets.

Keeping in mind Tom's commentary, you're saying as long as you've got sufficient sample observations in the EICSI, you can have some confidence that there may be something that's been picked up, I guess, in the primary market that is not being accounted for in the secondary market?

DR LALLY: Yes.

MS BRAKEY: Tom, I noticed you put your hand up as Martin was talking. Did you want to engage on something he said in particular?

DR HIRD: I just thought it was useful for me to give my observation and perhaps Martin can observe that. I think we are aligned essentially, conceptually. I mean, there may be some implementation questions and some interpretations of the data, but I think conceptually we share a common view, which is that that if your NSP measured costs are different from your estimate of benchmark costs, there are two ways. That could be because some NSPs differ from the benchmark, in which case you might want to think about changing the benchmark. But you might not, because inevitably there will be different strategies amongst NSPs and you are never going

to - no individual NSP is going to have the average strategy.

But I just wanted to say that I think it's the case that Martin and I really share a view. And Martin's description of the methodologies is consistent with what I would describe as a valid approach to dealing with that.

MS BRAKEY: Thanks, Tom. It's great when we have agreement, so we'll cash in wherever we agree. Jim?

MR COX: No, thank you, Anna. I did notice perhaps slightly diverging views on term from Tom and Martin. Maybe it's not so significant because you have concerns about the methodology, but is there a different view on what the data are showing and how should we think about that?

DR HIRD: I think Martin's view is based on what the AER has published, which says a range of from eight to 10 to 11 years for the estimate of the weighted average term instruments or WATMI.

Martin said eight, but I think that somewhere in between those is the AER's view and the AER is not yet sure where exactly between those it is. My view, looking at the data, is that the most recent estimate is 9.5 years.

I think that is including all APA debt issuance and I think the AER has indicated that they may not be including all. But my view is

that it's pretty close to 10 years, the average. And certainly there are a number of NSPs that are above that in 10 years.

MS BRAKEY: Martin?

DR LALLY: Clearly, Tom has done some more detailed analysis on this. I am reliant entirely on the omnibus paper, the December 2021 paper which showed that the EICSI back in 2018 was at 10 years and has come down to eight. So Tom's more detailed analysis, I can't comment on that as I haven't seen it.

MS BRAKEY: I was wondering if any of the other experts wanted to weigh in? Glenn?

DR BOYLE: My only comment is that before worrying about how the EICSI might be used, we need to know whether this outperformance really exists. This is an issue that I think both Tom - although he got slightly cut off - and Martin certainly touched upon.

The average outperformance is 18 basis points, down 4 apparently after somehow controlling for term and credit rating. But we are not told the standard errors of these estimates, and given that the observations are overlapping, I suspect that it's not just sigma divided by root N we're talking about here. These standard errors could be quite high.

And so, I'm suspicious that 18, four,

whatever it is, it is probably not statistically significant. In fact, Tom's own alternative effort to tease, to make this outperformance, actually comes in with a negative point estimate. At that point, it is clearly not going to positive.

Initially, I think that is an essential first step. And until that is answered, it's pretty much - well, it's putting the horse before the cart, worrying about how you might use the data.

MS BRAKEY: Thanks, Glenn. Dinesh?

DR LALLY: Cart before horse, I think, Glenn.

DR BOYLE: Not with my horses.

MR KUMARESWARAN: I would just agree with what Glenn has said. And I would also agree with the conceptual framework that Tom and Martin have set out, that the EICSI data is probably most useful to the AER to inform what the benchmark should be, but the AER should be very cautious to understand what it's seeing in the data before acting to make adjustments to the allowances.

I circulated a slide that had some thoughts on it, and this might be useful to sort of demonstrate a couple of the points that we have just been talking about this morning.

In the final omnibus paper, the AER raised a particular concern about outperformance when the

benchmark spread exceeds 170 basis points. And there's a figure 10, which is in the final paper, which sort of demonstrates what the AER's concern is. On the y axis, we have the spread from the EICSI. And on the X axis, we have the benchmark spread. And you have a scatter plot there and a 45 degree line. So any of the points on the right-hand side of the 45 degree line indicate some outperformance, and points on the left-hand side indicate some underperformance.

And so, there isn't really enough detail in the omnibus paper to understand whether the 4 basis points of outperformance relates to all of the observations, but that's my assumption.

Now, if we are particularly concerned about the outperformance in excess of 170 basis points for the benchmark spread, what about the observations below 170 basis points? I haven't seen the raw data so I can only apply the eyeball test, but it looks to me like below 170 basis points there's some fairly material underperformance.

So if the AER thinks about using the EICSI data to make adjustments, does that apply that in situations where the benchmark spread is lower than 170 basis points, the allowance should be adjusted up? I don't think the AER should go down that route unless it's really confident that

it understands what the data is actually showing.

Martin also made a point that we have to understand the statistical significance of what we are saying. In fact, Glenn just made the same point. This goes to the number of observations. That region on the right-hand side of the chart where the benchmark spread exceeds 170 basis points, these are fairly unusual market conditions. These are things like pandemics, financial crises and things like that where the market is doing something a little bit strange.

And so, in those market conditions, are we confident that NSPs are issuing debt in the same way as they would when market conditions are more normal? My understanding from talking to people who work in these businesses is that in those unusual market conditions, businesses will defer some debt raising. These are very unusual. They don't happen very frequently, but they might defer some debt raising and only issue the debt that they absolutely have to refinance.

So in those observations on the right-hand side that the AER is concerned about, how many issuances are we actually talking about here? Are these a very small number of issuances, in which case is that outperformance really statistically significant?

MS BRAKEY: Thanks, Dinesh. James?

MR HANCOCK: Look, I had a question regarding the treatment of credit risk. Now, both the EICSI and the existing methods are trying to determine a credit risk premium. But if we think about the treatment of equity, we look at the expected return on equity. And if we have a similar treatment on debt then we look at the expected return on debt.

And so, we have a credit risk premium on debt, but we also have a default probability on debt that presumably offsets that credit risk premium. And so, if we are interested in looking at an expected return on debt then I think we need to allow for that.

One way of doing it is actually not having a credit risk premium and simply having something like a risk free rate. And if we are not looking at an expected return on debt, but are purely focussed on what are essentially the contractual terms on which debt is issued, why aren't we interested in the expected return on debt and why are we focusing on contracted returns with no allowance for default?

MS BRAKEY: Thanks, James. I guess I'll take that as a comment rather than a question at this stage, and perhaps the staff can come on a little bit later. Tom, did you want to add anything?

DR HIRD: I just wanted to make an observation on the

topic that Dinesh was raising, which I think was sort of in the spirit of the point he was making.

That chart which shows all or - well, not necessarily all, but the net outperformance occurring in periods where there were credit spreads, if I am understanding the AER analysis correctly, you can sort of see where those dots come from in the previous figure, which is figure 9.

So if I'm understanding the way this has been put together correctly, all of those observations are from essentially early 2015 to some point in 2016, so they are quite old. So they are from a specific period in time and that's quite a long time ago, and I think that would also be relevant to the question of how - I think it would be that makes those observations less reliable of a general trend than if we had just seen that 20 times in 20 different market circumstances. We have seen it happen once, essentially, in one set of market circumstances.

MS BRAKEY: I just wonder whether either Warwick or Esmond could kind of answer this question about the data.

MR SMITH: Sorry, Anna, it is Esmond here. In terms of what specifically? Obviously, Tom talked about the observations through time, and that's right. We are talking about - you know, I guess

that particular period, and there is only one period where we observed quite material outperformance. And so that flows into the average, that 4 basis point average outperformance.

I guess it's a matter of whether that is sufficient data to conclude that you should make an adjustment and whether you think that's going to happen through time in ongoing periods of disruption, I guess, which is what you're saying, isn't it, Tom?

DR HIRD: Correct.

MR SMITH: Was that the data question you were asking me about?

MS BRAKEY: Yeah, does that answer your question as well, Dinesh, from earlier?

MR KUMARESWARAN: I'm not sure that I followed Esmond's point. Can you just make that point again, please, Esmond?

MR SMITH: All I was saying is obviously we have a limited number of observations through time where our allowance has varied from the EICSI and there has been what appears to be material outperformance in periods, particularly in '15 and '16 where I think there were disruptions in the debt market due to what was happening in Europe that flowed through to here.

Ultimately it's a question of whether you

have enough data to conclude an adjustment is warranted for those periods. And Tom is saying whether they are going to occur again; that's a judgement call as to whether you think this is an ongoing cyclic thing that is going to occur that warrants an adjustment, whether it is material enough and so on. Is that clear enough, Dinesh?

MR KUMARESWARAN: Yes. I think those are relevant considerations. If we are talking about a temporary market disruption, should that be used firstly to inform what the benchmark should be? I would say no to that.

And if it's unlikely that those circumstances will arise again, I mean, if this is not indicative of an ongoing pattern of financing by the businesses, I'm not sure that there's a strong case to make an adjustment to the allowance either. And that's in both directions. So I'm talking about outperformance and underperformance here, it should apply symmetrically.

MS BRAKEY: Thanks, Dinesh. Eric?

MR GROOM: I guess my question is, to what extent is the efficient financing strategy constant through time? I think Tom characterised the question we've got as the first question is we need to come up with a benchmark cost of debt, and Tom has said there are two questions we answer there:

"What is an efficient strategy?", and, "How do we estimate an efficient strategy?"

What is coming through the WATMI data is that tenor varies through time and perhaps it is partly responsive to credit spreads. And so the difference, if you like, between the EICSI and the benchmark may come through because the EICSI is responsive to those dynamic changes in strategy or because they can borrow at a different cost to the benchmark cost of debt for a 10-year term.

We seem to be focused on the latter question, but I guess I would appreciate responses to the first question. Is the EICSI partly reflecting the impact of dynamic financing strategies that are responsive to market conditions, risk appetite of the businesses or their own financing requirements? And if so, what should we do about that?

DR HIRD: I'll respond. I think the EICSI - and we talk about "the EICSI", but I think there are many EICSIs depending on how we weight and what we include and what we exclude. But it will be measuring all changes in the debt management approaches. It will be measuring random changes, privatisations and, you know, just changes in policies that are not responsive to our market conditions. It will also be measuring any

responses to changes in the market conditions.

So I think yes, in part it will be reflecting some dynamic responses to market conditions. And I think if we could be confident enough to describe how the efficient benchmark strategy responds to changing market conditions, then we could write that into the benchmark strategy.

If we could and if were confident of how to do that, we could say, "Well, credit spreads are above 170. Debts issued this year are now seven years, not 10 years." That sort of approach.

But I think it's incredibly important that the regulator disciplines itself to answer that question. And if it's confident that it can answer that question, then change the benchmark or have a more sophisticated benchmark. If that's the only way that it can internally consistently deal with the issue that you are speculating about, Eric, I don't see any way that it can target the EICSI and leaving - you know, which it is targeting because it is saying, "We think firms are following a different strategy to the benchmark," while still leaving the benchmark there unchanged, because as soon as you do that, you are saying, "We are targeting compensation to a strategy that is different to the benchmark,

but we are populating the benchmark 10-year trailing average with this other estimate that is not consistent with it."

That is my key conceptual point, that unless you are using the NSP data to either change the benchmark or change the compensation targeted at the benchmark, you end up with internal inconsistency in your regulatory regime.

MS BRAKEY: Thanks, Tom. Did any other expert want to specifically address Eric's question?

DR LALLY: Yes, if I could. Because the EICSI is an index constructed from new issues, it may be that the reason why the EICSI has gone down from an average of 10 years to eight years since 2018 is because in recent times the firms that have issued debt, they are firms that have always tended to issue debt shorter than 10 years and they are just overly represented in the recent data.

That is why when you see the term on the EICSI going down from 10 to eight, it could just be an artefact of that. So you would want to have a longer period in which it's materially below 10 years before you would take any notice that of.

Now, that property of EICSI, that its current value is influenced by just whichever kinds of firms have recently issued debt, you

don't get that with the conventional indexes. They are secondary market stuff, and the data you get there is just whichever bonds trade.

So that's something to bear in mind, in attaching any significance to this figure of 10 years coming down to eight in the EICSI data. You've got to be cautious about that.

MS BRAKEY: Did any other expert want to weigh in? No? I'll move on to Clare then, who has been patiently waiting.

MS SAVAGE: So I've probably got two things. One of them probably takes us back a little bit and hopefully the other one takes us forward. But the first question, that takes us back a little bit, probably picks up Glenn and Dinesh's concerns a little bit more so we might need Esmond back on screen for a second because think there's a first order question: Is the data sufficient, or is the quality of the data sufficient for us to rely upon it? And I think both Glenn and Dinesh are raising questions about the number of observations and whether they are statistically significant. And Esmond, I think you spoke to the fact that we have a limited number of observations. But in the charts and analysis that we presented in the omnibus paper, did we test for statistical significance?

MR SMITH: Technology is a beautiful thing. I have

been chatting to other members of the team about that. And we will have to check this, but someone said the standard deviation around that 4 basis point outperformance was around the 15 basis point mark, so it was quite, quite large. We'll just have to check that, but yes, we don't have a lot of observations and it may be that the standard deviation is quite large.

MS SAVAGE: With that larger standard deviation, the sort of fluctuation numbers in Tom's numbers is not unexpected in terms of his analysis as well, then?

MR SMITH: Yes, probably that is right.

MS SAVAGE: That was my first question. I wanted to just get an answer, I guess, to Glenn and Dinesh's concerns. The second part of it was to say, "Look, in our omnibus paper we have looked at this problem in terms of the way Martin had suggested, which is term credit rating residual.

We said term is something we are unlikely to adjust for because of the fact we are using a 10-year weighted average or trailing weighted average of debt, which would be hard to adjust for and keep NPV neutrality without some kind of transition.

So there isn't a proposal on the table at the moment for the AER to address the term issue, but we have talked a lot about term today. The

credit rating issue, I think we are saying we don't think is part of it. And I hear your comments, Tom about, well, think about this in the context of setting the benchmark.

We are still seeing this residual outperformance. We are looking at this question of primary versus secondary market and the influence that might be having. So I'm just wondering, could the experts take us a little bit into their thinking there and perhaps ask Glenn and Dinesh to suspend reality and pretend we've got a valid data set that you are comfortable is statistically significant, and then talk to us about how you see this question of the residual and what, if any, kind of application?

Now, in part I think, Dinesh, when you were talking about underperformance and overperformance, we talked in the paper about a cap to deal with overperformance. Would you see equally valid the suggestion of a floor or is that not a theatrical counterpart, I guess, in that context?

So I'm just interested, in having a bit more discussion on this question of residual because it is one of the live issues that we have got on the table. And Tom, particularly in your comments do you see a way in which you could adjust the benchmark by thinking about residual

or do you see those things as incompatible as well?

MS BRAKEY: I might go to Dinesh first, for that first question. Then I'll come to Tom. So I'll go Dinesh, Glenn then Tom.

MR KUMARESWARAN: I think that if we are convinced that there is some persistent outperformance or under performance then the appropriate course of action would be, as Martin has suggested, to diagnose what the source of that outperformance is and then adjust the benchmark.

I don't think the appropriate course of action is to make an arbitrary adjustment to the allowance or to apply a cap or a floor.

MS BRAKEY: Thanks, Glenn?

DR BOYLE: I basically agree. Importantly, it has to be either outperformance or underperformance. We can't have both because if we had both, they would even out and we would have zero. We would be back to that situation.

So if it transpired one or other of those did appear to be a robust phenomenon then I certainly don't favour a straight adjustment of the allowance. There would have to be some kind of adjustment to the benchmark.

But I accept that would be tricky in a time-consistent sense because every time you adjust that, particularly if it's the term, then

there is another transition period and, you know, this could happen at every five years. It could get messy. But that's the only feasible way I see of approaching that issue.

MS BRAKEY: Thanks, Glenn. Tom?

DR HIRD: In answering that question, and in listening to those previous answers, I just want to first make a point of terminology. Perfectly reasonably, people refer to the benchmark often to include both the debt management strategy and the credit rating estimate that we are using to sort of compensate for that.

I am using a slightly different terminology, so I am talking about decision one being to define an efficient debt management strategy and decision two being to define essentially a way of compensating for the cost of that debt management strategy, which currently is the BBB+ credit rating average across RBA, Bloomberg and Reuters.

So having said that, going back to your question, what if we did see persistent outperformance? I think you're asking me, "What would you say should be done?" Well, I think that depends on what the data says. If the data says that we can explain that persistent outperformance by virtue of NSPs issuing at lower credit spreads than your BBB+ estimates from Bloomberg, RBA and Reuters then we would change

that as our estimate of how to compensate for the cost of the benchmark strategy.

We wouldn't necessarily change the benchmark strategy unless there were separate reasons to do that. But if we found that NSPs were issuing at 50 basis points on average lower than the BBB+ benchmark, then we would need to think about how to change the BBB+ compensation method. One way would be to raise that credit rating to a higher level.

Another way would be to retain the BBB+; level and apply a sort of halo effect for NSPs. Which of those you would choose would depend on which probably most accurately reflected NSP costs over the period that you had the data, assuming again that you had enough data to make a reliable call on that.

MS SAVAGE: Tom, is that closer to where we were in the papers before the omnibus? So in the 2020 working papers?

DR HIRD: No, I think that's where you are now.

MS SAVAGE: You're suggesting that we use it to inform the blend?

DR HIRD: Yes, but that's --

MS SAVAGE: But that was our position in 2020.

DR HIRD: Yes, but the position previously wasn't based on matched tenor and credit rating analysis. That's where I - then you're picking

up differences in tenor as your explanation, which I say go to the benchmark. If you want to change that, go to the benchmark strategy and change it. But if you are appropriately matching to tenor and credit rating and there's still this halo, what do you do?

MS BRAKEY: Thanks. James?

MR HANCOCK: Clare, taking your proposition that we start from the point where we are confident that the EICSI is robust, I think if we start from that point then we are assuming that issues of tenor inconsistency or credit rating inconsistency have all been cleaned up and we have a sort of robust and comparable indicator.

And so then if I think about it, the primary differences between the Reuters-Thomson/Bloomberg/RBA and the EICSI relate to the securities that are in the samples.

And so the question then is, which of those samples is going to be more representative of the Australian networks? And so for the firms that are in the sample, presumably it does a very good job of capturing their circumstances because it's effectively sort of a population count of their debt raising costs.

But then there are also a number of other firms that are not in the sample because of their characteristics. And so the question is how

willing you are to assume that the experiences of firms that are in the EICSI, how robustly they translate across to the other network businesses. And I think that's the question that you need to come to grips with if you decide that you want to put the EICSI ahead of the alternative secondary market data sources.

MS BRAKEY: Thanks, James. Dinesh?

MR KUMARESWARAN: I'll just add something just to follow up to Jim's point. I think what he said is correct. So the EICSI, as I understand it, only reflects the data of the privately owned NSPs. And so there's a question about if you observe something with that data, should that result be extrapolated or used to set the allowance for all NSPs?

Equally, if the source of some outperformance or underperformance is being driven by a small subset of the NSPs, again is it appropriate to adjust the allowance for all NSPs on that basis?

I just wanted to also qualify my earlier answer to Clare with a couple of points. The first is I said that if we were convinced that there was consistent outperformance or underperformance, the appropriate course of action would be to adjust the benchmark.

Taking Tom's points on board, I think that

the decision to adjust the benchmark should also be weighed with the cost of doing that, because if we say, for example, that we are now convinced that the appropriate tenor is something less than 10 years, well, that has some implications for the trailing average allowance that would need to be implemented and so we would need a further transition and there are some costs associated with that. So I think the AER should weigh that before changing the benchmark.

The second point is that if we do as Martin has suggested and diagnose what the source of the problem is, the solution should reflect the problem. So if, for example, we are convinced that the source of some outperformance is that NSPs are issuing shorter tenor debt than the 10-year benchmark then I don't think the appropriate solution is to adjust the credit rating assumption because that's a more convenient way to fix the problem.

MS BRAKEY: Thank you, Dinesh. Do the board members have other questions that they would like to raise now? No? Okay. All right. Do the experts want to make any kind of final statements or comments on the index? Dinesh, is that an old hand or are you having another - that's an old hand? Okay. No further comments.

In that case, we might move on to the second

session which is about beta. I think we have got Glenn speaking, then Dinesh.

DR BOYLE: I'm up, am I, Anna? All right. I'll start my watch going so I don't go over five minutes.

MS BRAKEY: I'll give you a few minutes leeway. I just didn't want - I do think that the real value comes in the discussion. Not that there isn't value in your presentation.

DR BOYLE: No, no. I'll do my best.

MS BRAKEY: Thank you.

DR BOYLE: The important thing to remember about what we're trying to do in estimating beta is that beta is a description of how future returns will move together. Now, we can't observe this directly because it's in the future, so we have to estimate it from data. And as with all estimation, the best method depends on the underlying properties of the data that we are using to estimate.

So there are a few possible cases. The simplest possible cases is if those joint returns or returns generally are jointly IID [Independent and Identically Distributed]. In other words, the joint distribution of future returns just stays the same all the time. But if that's the case then beta is a constant. It never changes. And that is easy then, because just applying the

usual law of large numbers, et cetera, reasoning, then the best estimate we can come up with is the one that uses the longest possible time series of data. And in that case, the estimate we get will converge to the true beta.

Unfortunately, joint returns probably aren't IID. More likely is that the joint return distribution is stationary, but not IID. So there is a long run distribution to which the joint returns tend to converge to over time or return to over time, but in the short run, the distribution changes.

In this case, we have two betas. We have an unconditional or you can think of this as the kind of "long run beta", the beta to which beta tends to revert to or converges on. And then we also have the conditional or current beta. This is a beta that varies through time in a mean-reverting fashion reflecting current conditions, whatever they may be.

This can make a difference as to how we best estimate data. If the mean reversion just reflects mispricing then a rather clever paper by Jeremy Stein suggests the best thing we can do is just look through that, ignore it and basically pretend it doesn't exist and estimate the long run beta. That again indicates using the longest possible time series.

On the other hand, if the mean reversion reflects rational risk pricing as risk changes over time, or perceived risk and risk aversion changes over time and beta changes with it, then I say we may - and I just say "may" at this point - want to estimate beta using a shorter time series.

The final possibility is that the joint return and distribution is non-stationary. In other words, shocks to beta are permanent and there's no predictability from past data. Well, in that case we are stuffed, but it is very unlikely in the case of beta because the beta for the market portfolio by construction has to equal 1. So it is difficult for its individual components to wander off to plus or minus infinity. So for all practical purposes, we can rule out the non-stationary case.

Now going to the second page of those slides I sent through, going to question one, should we continue to use the longest available estimates of beta for our comparator firms to set the equity beta? Probably yes. In fact, I would say almost certainly yes. The optimal approach depends, as I have already suggested, on whether the beta is mean reverting or not, the source of that mean reversion if it is and also the speed of the mean reversion.

After all, if the reversion is very fast then the equity beta only diverges from the unconditional beta for very short periods of time, in which case you may as well just stick with the unconditional beta. The problem is that in the absence of robust evidence on these matters, it is pretty difficult to say what we should do.

And so as a working approximation, it is probably best to assume that beta is approximately constant and use the longest possible available time series. I say this for a couple of reasons. The first reason is that this assumption covers several cases. It covers the case where beta is constant. It covers the case where mean reversion is reasonably fast. As I said, if it's fast, then there's not much divergence or divergence disappears quickly. And it covers the case where mean reversion doesn't reflect rational pricing factors.

The second reason I say that is that the one case it doesn't cover, where mean reversion is relatively slow and it reflects rational risk pricing, is that we don't really know how to deal with that. Estimating rational variation and expected returns over time is the Holy Grail of asset pricing theory. We haven't got an answer to that, so trying to estimate the conditional

beta reliably is pretty much impossible.

Moving quickly to the second question, if the AER moves to a five-year term for return on equity, does that follow that we should place more reliance on estimates of beta in the last five years? These two issues are largely but not completely independent. Obviously there is no implications for estimating beta if the best approach is simply to use the longest time series, in the three cases I outlined before.

If that's not the case, though, it is unfortunately not as simple as just saying, "Well, we want to estimate beta over the next five years, so we'll use the last five years of data." The amount of data you should use is going to depend on, for example, the speed of mean reversion. If reversion is very slow and so beta is very persistent, you might want to use much less than five years. You might want to just use the last year, for example.

The point I'm getting at here is that precision of estimates is kind of a second order issue here. What matters is their accuracy. How close can we get to the true beta? And there has been quite a lot of work done in the academic literature in the last few years on exactly this topic.

It seems to me that at the moment we are

kind of wandering around with very little information. What is needed is for some kind of horse race using the methodologies that have been adopted in recent years and, you know, stack up the one-year of data, two years of data, three years, five years, 10 years, all of the various candidates, and just see which of them actually most accurately estimates realised or subsequent beta.

Nothing may fall out of that, but at least it will give us some information. The point I'm making here is that this is a question that can't be answered conceptually; it can only be answered by data. It has to be data informed. And without that data and evidence emanating from that data, in my view it is pretty difficult to justify putting any weight on five-year beta estimates or indeed ten-year beta estimates or one-year beta estimates or whatever, because we could actually be making the problem worse. If using five-year beta estimates doesn't match up with rate of mean reversion then we would just be moving further away from the true beta. So without that kind of evidence from data, this is a difficult question to answer.

MS BRAKEY: Thanks, Glenn. Dinesh?

MR KUMARESWARAN: Thanks, Anna. I have got a set of slides which I hope everyone can see. Let me

start with the second question, because I basically agree with Glenn that I don't think the choice of whether the AER should put most weight on the last five years should be strongly related to whether the AER moves to a five-year return on equity.

I think what we want is the best statistical estimate of beta, and the weights to give to beta estimates derived using different time periods should reflect that objective. I don't think that the rationale for moving to a five-year estimate of the return on equity, such as there is one, is related to the selection of the historical period. I just don't think that that's the case.

Turning to the question of whether the AER should continue to use the longest available estimates, I think the AER should give some weight to estimates derived using different estimation periods. The reason for that is because we cannot observe actual betas; we have to estimate them. There is estimation error.

I don't think we can really determine whether true betas are constant or mean reverting or, if they are mean reverting, how fast they are mean reverting. I would also make a point that for exactly the same reason that the AER might put weight on estimates from different time

periods, the AER should also consider putting weight on domestic and foreign comparators. And I'll explain that point in a moment.

The AER has itself identified that there is a trade-off that it faces when it is considering whether to use estimates derived over a long period of history or the most recent five-year period. There are pros and cons, and I agree with that analysis.

Just to run through those pros and cons very briefly, when you are using very short run data, the estimates will tend to reflect prevailing market conditions more, but the problem is that you have a fewer number of observations. And so what you get are very statistically noisy estimates. I've got a chart here on slide 2 that presents rolling five-year betas for the three currently listed comparators that the AER looks at.

You can see from this how volatile these estimates are. A lot of this is driven by the fact that we are using quite a short estimation period. And you can see towards the end there, there is a big drop. That drop was the basically the change in the stock market in response to the emerging COVID crisis.

So the period of February and March 2020, stock markets dropped very substantially. The

returns for these companies didn't drop so much, and so beta estimates fell very substantially. Now, that one very influential event will remain in those beta estimates or continue to affect those beta estimates for the next five years until that rolls out of the sample. That is one of the problems with using very short run data to estimate betas. You can get very noisy estimates that are volatile over time and that can be influenced by very significant events, and the estimates can change materially for that reason.

Now, I think there is a particular problem that the AER faces in this five-year period in putting weight on the most recent evidence, and that is because most of the comparators - so the AER just uses domestic comparators to determine its beta estimate, and most of those have now been delisted, so we are now down to just three comparators.

And over the most recent five-year period, two of those comparators have been subject to takeovers. Their share prices have changed as a result of that takeover activity, and you would expect that that has affected their beta estimates as well. And we have also in the last five-year period had a large shock to the beta estimates due to the COVID crisis.

So do these events have anything to do with

what we expect the betas for these types of regulated businesses to be over the next five years? I don't think it does.

And then finally, we're down to three comparators. Two will be delisted soon and we only have one left, APA, and the AER has expressed some reservations about the reliability of that as a comparator. An additional problem with putting a lot of weight on the most recent five-year period is that we have a dwindling number of comparators for the AER to rely on.

Now turning to the pros and cons of using a long estimation period. Well, it is the basically the inverse of the pros and cons for using a short estimation period. The longer the estimation period, the less susceptible the estimates are to just random variation in stock returns. So if you did have something like another pandemic, it will influence the estimates but probably not so much. So you get improved statistical precision in the estimates.

The downside is that the further back in time you go, the less relevant the data becomes. And that might introduce bias into the estimates. Just to use an analogy, the AER has expressed some reluctance to use overseas estimates to compare betas. And the concern there seems to be that there are things about the either the

markets in which those comparators operate or the firms themselves that might introduce some bias into the estimates.

One of the reasons you might get some bias is because the composition of the market index against which those foreign beta estimates are measured might be very different to the Australian market. Well, you might get exactly the same problem if you use very long run data from the Australian market.

Here's a chart that is produced by the RBA which shows how the composition of the Australian stock index has changed over time. And you can see, even if you go back to the 1990s, how different the composition of the index has been. The same sorts of concerns that the AER has about using overseas data may also apply if you go back in time and use very long run data to estimate betas.

Now onto slide 5. What should the AER do when it is facing this trade-off? Martin, in his notes that were exchanged between the experts, set out a framework for thinking about this. There is an important insight from the statistics and forecasting literature which says it is possible to reduce the estimation error by combining two estimates as long as the errors associated with those estimates are not perfectly

correlated, even if one of the estimates is materially biased.

So you might have two estimates and you are convinced that one of them is biased, but you may do better by combining the estimates with the unbiassed one because of this - well, the reason for this, what's the intuition for this?

It is very similar to portfolio diversification. When you combine stocks into a portfolio, there is random variation in the returns of the stocks within the portfolio. But as long as the returns are not perfectly correlated, you'll get a reduction in the overall variation of the returns in the portfolio.

It is a similar sort of idea here. Glenn pointed out in his notes that it is not guaranteed that you will get a reduction in estimation error, and I agree with that. If the bias that is introduced by the additional estimate that you use is so large, it might overwhelm the benefits you get from a reduction in the variation in the errors.

That is true, but I think this is a valid thing for the AER to think about when it is considering what sort of time periods it should use. And I think my reading of the AER's preferred position is that this is the way the AER has been thinking about the issue. So there

are some pros and cons when using estimates from different time periods, but the AER thinks that giving different some weight to estimates from those different time periods will result in a better estimate. And I think that is probably right.

Some further thoughts on that: Does the AER have to use the longest estimation period possible rather than, say, 10 years? Well, I think that depends on how large the incremental gain in the statistical precision is relative to the increase in the potential bias that you might get by going back further in time.

I also think that if the AER wants to put weight on estimates from different time periods, it should be clear about what those weights are and why it thinks those weights are appropriate. I know that is a difficult task, but I think it is important to set that reasoning out.

The final point I would like to make is that the rationale for combining the estimates to reduce the scope for estimation error applies equally, I think, to the choice of comparators. Let me just explain that point very quickly.

When you are thinking about what data you should use for the beta estimation process, I think it is useful to think about two dimensions. One is the time dimension. So what

estimation period should we be using? Should we use the longest period available or the most recent five-year period to do the estimation? There is a trade-off there that we have just talked about.

And I think there is an analogous consideration, a second dimension. It is a cross-sectional one. So which comparator should we use when we do the estimation? Should we give all the weight to the domestic comparators or should we also consider foreign comparators?

You would think that the domestic comparators would produce less bias in the estimates because they are more comparable to the sort of businesses that the AER is regulating. But because the sample so small and getting smaller by the day, you get worse statistical precision in the estimates. Whereas if the AER were to also give some weight to the foreign comparators, you run the risk of introducing some bias in the estimates but you get greater statistical precision because you expand the sample, the comparator sample.

So what should the AER do? I think in relation to the time dimension, the AER should give some weight to estimates derived using different time periods to reduce the scope for estimation error. And likewise, it should also

think about giving some weight to domestic and foreign comparators for exactly the same reason.

I think these are both essentially the same problem that the AER is trying to solve, to minimise the estimation error associated with the estimates.

MS BRAKEY: Thanks, Dinesh. Any questions or points from board members, first of all? Jim?

MR COX: A couple of questions, I think, to both speakers. To Glenn, I'd like to hear him talk about the relevant points that Dinesh made, because that seems to me to be the counterargument.

To Dinesh, you suggest we should weight various sorts of evidence and you said that it was a difficult problem. What guidance could you give us as to how those weights could be derived, bearing in mind they have to withstand scrutiny from the public? I'd be interested in hearing on those two points.

MS BRAKEY: Glenn, I'll go to you first.

DR BOYLE: All right. Jim, the point of disagreement you are referring to is what?

MR COX: I think Dinesh is saying - you are arguing for the longest period in most circumstances, practically.

DR BOYLE: Yes.

MR COX: Dinesh was saying that introduces a lot of

irrelevant data because the world changes and is different from what it was 20 or 30 years ago, and that old data just simply isn't useful anymore. That, I think is the point of disagreement. It's probably exaggerated, but I think that was the point of disagreement.

DR BOYLE: No, no. That is exactly right. Well, the extent to which the world has changed, at least as far as beta is concerned, is an empirical question. So I would agree with Dinesh on that up to a point.

Perhaps rather than longest "possible" time series I should have used the word "feasible" rather than "possible", because if it turns out there is some major structural break in the time series, like the introduction of legislation that wasn't there before, then going back beyond that point might indeed bring about the kinds of problems that Dinesh is referring to.

So I don't think we are necessarily disagreeing there fundamentally, except to the extent that if there isn't any evidence of those kinds of breaks so that the world has fundamentally changed then again I'm just drawing on the law of large numbers. Use the longest possible time series of data.

I think the more fundamental disagreement between Dinesh and I, even though I didn't

actually mention this, was about the use of foreign comparators. I think the argument he is making here is based, because he referred to Martin's work, on something called mean squared error.

It is certainly true and mathematically true that if you add in betas from a different source that are uncorrelated with your original source then you will get a lower mean squared error, which is basically precision plus bias. But the thing is I could achieve that just by randomly drawing numbers out of the phone book. And I don't think anybody would suggest that we should do that as a method of estimating beta.

Here's another analogy: Suppose I wanted to estimate the size of Cocker Spaniels, the mean size of Cocker Spaniels. Right? So I would collect data on the size of Cocker Spaniels and I would take the arithmetical average, and that would be my estimate of the mean size of Cocker Spaniels. But I might not have many Cocker Spaniels, and so the standard error is high and it is not very precisely estimated.

But it turns out I have access to an unlimited amount of data on Great Danes, say, or alternatively Chihuahuas. Now, I could add in the Great Dane data, I have a lot of it, in with the Cocker Spaniels, and I could take the average

of that, and I would have a much more precisely estimated mean, but not a very sensible one because they are fundamentally and this case literally different beasts.

It is the same with domestic and foreign comparators or betas. Not perhaps to the same extent, but along the same lines. And the point I'm making here is that at the end of the day we need a number here. We're not testing statistical hypotheses. Hypothesis testing isn't the issue here. So precision, statistical precision, is very much a second order issue.

It is much better to have an accurate, as in unbiassed, estimate, even if it is imprecisely estimated, for the purposes that the AER are going to put it to because they need a single number. You don't need a range, you need an actual number. And so, it is important that that single number contain as little bias as possible. It may not be very precisely estimated, but that is better than having a precisely estimated, heavily biased number.

And so, that is why I don't like the use of foreign comparators and their betas. But even if you ignore all of that, there is then the issue of how you determine the appropriate weights. It is almost certainly not 1 over N. It is probably a lot smaller than that, even if your criterion

is minimising mean squared error.

I don't know how to determine those weights, and if you can't quantify something, you're really opening it up to abuse. We could set any number you like; everybody will be advocating for a number that best suits their purposes.

And so, even if you ignore all the in-principle problems with foreign comparators, I think there is a simple practical one: What is the weight you put on them? And if you can't quantify it then you go back to the in-principle problem and say the weight is zero. Have I answered your question, Jim? Probably? Thank you.

MS BRAKEY: Thanks, Jim. Dinesh?

MR KUMARESWARAN: To Jim's question to me, "How should the weights be derived?", I don't think there is a mathematical formula that you can apply to magically produce some weights. I think what the AER has to do is weigh the evidence and exercise some judgement. So that's why I say it is difficult to make this trade-off.

But I think that that judgement has to be made in the current context that we find ourselves in. So the AER is now in a situation where it is facing basically a null set of Cocker Spaniels, or rapidly getting to that point. So how do we estimate betas in that context? Well,

I think that the analogy that Glenn used between Cocker Spaniels and Chihuahuas is a little bit exaggerated, because I don't think that is exactly analogous to the situation that we find ourselves in.

Do we really think that energy networks in other countries are so different that they provide zero useful information? Particularly in a situation where we will soon find ourselves in a situation where we don't have any listed comparators or maybe only one listed comparator from which to estimate betas in Australia? I think in that context it is reasonable to move from giving zero weight to overseas comparators to some weight.

MS BRAKEY: Thanks, Dinesh. Martin?

MS SAVAGE: So you think international data is more like a Field Spaniel, which is probably quite close to a Cocker Spaniel?

MR KUMARESWARAN: Quite possibly.

MS BRAKEY: Martin?

DR LALLY: I'm surprised that Glenn favours choosing an estimator with zero bias rather than an estimator with the lowest mean square error. It is standard practice in the statistics literature to choose estimators with the minimum mean square error rather than minimum bias, so I'm surprised by that.

As far as species of dogs are concerned, Clare, I think you've nailed it that the comparison here is not Cocker Spaniels versus Great Danes, but Cocker Spaniels versus a similar species.

The third point I would make is that in circulating a note a couple of weeks ago which has triggered off all this issue about mean square error versus bias, the conclusion you get from that is that some weight on foreign estimates would be useful in conjunction with the local ones. There's no suggestion that weight would be 50-50, but some weight.

It appears Dinesh then extrapolates that idea to the question of how much historical data you should use, and he regards the five-year beta estimates and the 20-year estimates as being two competing estimators and we should put some weight on each of them.

I don't agree with that at all. The methodology that I referred to in my note of a week ago would say to you, in respect of the historical period to choose, not how much weight to put on five-year betas versus 20-year betas, it would say to you, "What historical period should I use that would minimise mean square error?"

That historical period might be five years,

it might be 20 years. Likely, it would be something between the two. So the pooling, if you like, or the compromise position, does not consist of putting some weight on the five and some on weight the 20; it involves choosing a historical period that minimises mean square error. And that might be, says 15 years.

So there is no pooling there. I think Dinesh, in extrapolating from foreign versus local to the historical period issue, that that extrapolation does not apply.

MS BRAKEY: Thanks, Martin. Eric, did you want to jump in on this point before I go to Tom? Or are you going to raise a different point?

MR GROOM: I'll come in after Tom, if that's okay.

MS BRAKEY: Yes, good. Tom, thank you?

DR HIRD: I just wanted to really ask the question, which maybe people can take up or not, but a lot of this discussion - the key issue is how much weight to give to foreign comparators. And the key question in that regard is how different are they likely to be to domestic comparators and what's the evidence that can be used to decide that question? And so I think that's the question.

I think, as I understand it, the evidence that's been put forward to date that Australian NSPs have lower equity betas than foreign

comparators is because you have got some measured estimates that are lower than in other jurisdictions. That seems to be the evidence.

I don't see very much compelling evidence that an Australian NSP is riskier than Vector. Just conceptually, if I was to ask the question, "Would I expect Vector in New Zealand to be more or less risky than AusNet in Australia?", my answer would be conceptually no. Then it comes back to this, that the betas are lower when we measure the betas.

My question is how reliable are those Australian betas, given the very small sample? How reliable is the mean of those? How reliably can we say that is lower than the mean for other jurisdictions? And that's just historically, right? That's a question - if we sort of looked at these numbers and we looked at them over some historical period in the US, New Zealand and Australia and said, "Well, they were lower in Australia over that period." We can make some statistical assumptions about the population we were drawing those Australian betas from and do some analysis.

And when I do that I get a very wide confidence interval for Australian betas, but it is wider than - it encompasses the mean from other countries. Putting that aside, that is a

historical estimate. And then we have to ask the question, "What does that tell us about the future? What confidence do we draw from that past to the future?"

There, Dinesh's point earlier is critical to think about international versus domestic comparators, which is that over the estimation period that the AER is using for the Australia betas, we had very different regimes for the Australian stock market markets. A mining sector boom, mining sector bust, global financial crisis and most recently a pandemic. All of those events are what we are - and some of them are shared in foreign jurisdictions and some of them not the same. They are peculiar to Australia.

If we are going to rely on those historical estimates to project forward for the Australian betas, which I think is the AER's current preliminary position, we are essentially saying that those things that affected the stock market in your estimation period, they are the things we expect to see again in the future.

I think that is almost certainly not true, and that further weakens the statistical relevance of having had lower bets in one period in Australia versus in other jurisdictions over a historical period.

MS BRAKEY: Thanks, Tom. James?

MR HANCOCK: I have a couple of comments, but I will for the moment confine myself to the issue of the use of international comparators. It seems to me that we would expect Australia to have lower betas than overseas countries. The reason for that is that the Australian economy and therefore the Australian stock market has a high representation of commodity producers that have very accentuated price cycles.

So there is a lot of volatility in the returns to those parts of the Australian economy. And that same degree of volatility is not present in economies that are fundamentally manufacturing and services based. So if it were the case that revenue streams in Australian utilities were about as risky as revenue streams in, say, the US or the UK then we could expect to have lower betas in Australia than in those overseas jurisdictions. And so before using international comparators, I would be wanting to consider explicit adjustments for the upward biases in those overseas comparator betas.

MS BRAKEY: Thanks, James. Tom, did you want to respond to that?

DR HIRD: Very quickly, just to make a point that in some periods in Australian history we have had the equity market dominated by resources sectors and in others less so. And the historical period

that we're using captures what actually was the case, and Dinesh presents a useful chart of that where that resources component of the capitalisation of the stock market was much higher in 1980 than in 2000, right? And indeed, much higher at the beginning of the current century than it is now.

So I agree that there is the potential for these differences, but there isn't one Australian stock market. There is a series of different Australian stock markets through time. When we say, "We must use Australian data and a long period of Australian data," we are just projecting forward the past onto the future, and that is not necessarily a reliable position.

MS BRAKEY: Thanks. I'll wrap up this after, so Martin, did you want to make a very, very quick comment before I move to Eric?

DR LALLY: Jim makes a very good point that the composition of the Australian index may be quite different to other countries, in particular these resource stocks. If they have high betas and high weight then everything else must have low beta because the beta average is one in each market.

But there is a way of correcting for that. You can take betas from foreign countries, companies from foreign markets, and you can

re-estimate their betas with the market industry weights that are reflective of Australia. So that problem can be addressed.

MS BRAKEY: Thanks, Martin. Eric, I'll move onto you.

MR GROOM: Thanks. I think this has been a really interesting and useful discussion, because it has surfaced, if you like, a lot of implicit assumptions in the use of either long term data or international data. The framework may apply to when we talk about other parameters too in terms of cost of capital, in terms of thinking about how we take into account different sources of information in coming to a decision.

That seems to be really the nub of it, in a sense. Glenn put it in terms of weighting zero and one, and Dinesh is arguing more for a weight between zero and one to be given to different sources of data.

I guess there are two questions for me: If we were of a mind to consider international data, how would we determine how we weight that and what would be the issues in determining which countries to look at in compiling that international data?

If I look at, for example, the valuation for Spark - and I believe the valuer/expert for Ausnet took a similar approach - it was a very

modest and small list of international companies that they brought into the data set when they were assessing betas. And it struck me that if we were to do the same thing, wouldn't we be criticised for being arbitrary in terms of the data we took into account? So I guess I am looking for advice on that practical issue, if one were of a mind to go down that track.

MS BRAKEY: Thanks, Eric. Who wants to jump in and answer that first?

DR HIRD: I'll just say that you are going down that track irrespective of what you do. If you continue to give zero weight, that's making that decision. So we are in that world no matter what. It's complex technical sort of questions about how to determine those weights, but the first starting point is find some regulated businesses that have similar - that are mainly regulated. My starting point would be that they have the same risk unless there's analysis done to suggest that they don't. That would be how I approach the issue.

MS BRAKEY: Thanks, Tom. Martin?

DR LALLY: Same point as Tom.

MS BRAKEY: Okay. Dinesh?

MR KUMARESWARAN: Yes, I think that that's right. What I would suggest is that we start with a global industry classification, and start with a

large universe of potential comparators. Then we get rid of the comparators that have poor data or have insufficient data to contribute to the estimation process. They may be very illiquid stocks that are not suitable to include in the estimation process, so take those out.

Then I would go through and, as Tom suggested, check for what we consider to be the important characteristics for a suitable comparator for an Australian business. So if the AER thinks that it is important that we only look at regulated businesses, well, go through the list of comparators and take out the ones that have a large share of unregulated activities.

And you would have to apply some threshold, some sort of filter, some sort of mechanistic rule to do that, and that will require some judgement. There is no magic answer to that.

So set a threshold but be transparent about what the threshold is and then filter out the comparators. And then what you'll end up with is a set of comparators that are perhaps a few dozen. Then you go through those and you do a sense check for whether those look reasonable or not.

I would just point you to an exercise that the Commerce Commission in New Zealand did in 2016 when it set its input methodology's

examinations. It went through exactly the sort of exercise that I've just described now, and it ended up with a sample of something like 70-odd comparators.

And I would err on the side of using more comparators rather than less because I'm of the same mind as Tom, that I think the presumption should be that the comparator is of similar risk to the Australian DNSP or NSPs unless there is a good reason to think otherwise. So the burden of proof should be, or the rule should be, that we only take out comparators if we are convinced that they are not good comparators.

MS BRAKEY: Thanks, Dinesh. Glenn?

DR BOYLE: That's all very well and good, but as a practical matter what do you then do? You've got a bunch of foreign comparators and you estimate their betas. Maybe you average them and you combine them in some way or you potentially combine them in some way with the domestic confirmed betas.

But how does this help you in telling or indicating what weight you should put on them? Suppose foreign betas turn out to be basically indistinguishable from the domestic ones. Does that mean you put a lot of weight on them or no weight on them because they are not adding any information? It doesn't really matter, because

if they are the same then whatever weight you put on them will give the same answer.

On the other hand, what if they are a lot different? What if they are a lot higher than the domestic values or a lot lower? That could, as Tom has suggested, indicate that there is measurement error in the Australian betas and there is something wrong with them, or it could indicate that the Australian firms are just different to the average of these firms overseas, in which case you wouldn't put any weight on them at all.

And if it is that there is something wrong with the Australian firms, does that mean you put 100 per cent weight on the foreign betas? In other words, what I'm getting at is here is there is no easy translation from undertaking the sort of exercise that Dinesh is talking about to finding a weight to give to these foreign betas.

MS BRAKEY: Okay, thanks. I'll let Dinesh respond and then we'll go to Clare because I suspect she's going to change the topic again.

MR KUMARESWARAN: I agree with you, Glenn, that it is a difficult task to assign weights to these different types of estimates. But I think I heard you correctly when you said that if they are very different to the Australian businesses you should give the foreign businesses, or the

estimates derived using foreign businesses, a zero weight. I don't think that's correct.

I think you should only assign them a zero weight if they contribute no useful information at all. And I think the AER's task is to assess how much useful information they contribute. That is going to involve exercising some judgement and understanding the characteristics of the different samples. I don't think there is any way of getting away from that.

MS BRAKEY: Thanks, Dinesh. Clare?

MS SAVAGE: Thanks, Anna. I think you know me well already. I was going to say Eric likes to talk about the hedgehog and the fox. So I think we know that Glenn is a hedgehog and Dinesh is a fox when it comes to the use of samples.

We've talked a lot about international data and whether we should be using it in this context, and I think if we are going to go down that path, there are then questions about international CAPMs and international market risk premiums and let's not open that right now.

But what I wanted to just come back to was Martin made the statement before about what historical period minimises mean square error. So trying to focus this in a bit on this question around the period again. There is obviously a view there that rather than weighting different

periods or different sets of data, we should use the best possible period.

I'm just interested in whether the other experts have a view on Martin's comment there? And if I have mischaracterised what you've said, Martin, sing out. But I thought you were basically saying to choose a period rather than a weighting.

MS BRAKEY: Thank you. Who wants to jump in on that first? James, quick with the trigger?

MR HANCOCK: Yes. I think that we should use as long a data set as we've got that we think is robust. So perhaps that is the sort of period that is published in the AER's work to date.

Picking up on Glenn's comments, I think that to use recent data, you have to ask why you are using it, the more recent data period. Firstly, as Glenn said, you could be assuming that the data are non-stationary. Now, we don't really have evidence that this is the case. It could be that the betas cycle, but even if they do, we don't really know what to make of that. Because if, for example, let's take a very simple case where the cycle was eight years. Then, we would have a four-year period when the betas are below average and then we expect in the next period that the betas are above average. So if that is the case, we don't want to be taking the last

four years. In fact, we want to be reversing them.

So we just don't know how to use this sort of variation in the data. And so, I feel that we shouldn't use it and we should go for a long period to acknowledge that we don't really know the data generating process well. If someone comes up with something that can demonstrate a better understanding of it, then fine. Then, we consider it.

Secondly, the point has been made of combining estimators. So combining long estimators and, say, a five-year period estimator. I would caution on the idea that there is much increase in precision to be had from that, both those estimators are using the same underlying sample. So they are really just applying different weights to the same underlying sample. So in the five-year one, you apply a zero weight to everything that is older than five years. In the long period, you are using - putting a smaller weight on the most recent five years, but you don't actually have a lot of new sort of sample points. You are simply applying different weights to them.

So my overall impression, given all of these problems, is that going for a long period is probably the approach that exposes you least to

measurement error.

MS BRAKEY: Thanks. Tom?

DR HIRD: I think I generally agree with what James just said, but I just put the caveat in place that with any historical estimate of the equity beta, what you are measuring is the relationship between the firm in question and the stock market in that period. And then if you are going to take that equity beta estimate and apply it in a forward-looking basis, you are basically saying that the shocks to the economy in that period are representative of what the shocks we expect to hit the economy in the future.

And so that is a way of answering - you know, that is a relevant consideration when deciding how long of an estimate to use and also whether you think the diversity in markets assists of coming up with some sort of aggregation of the different types of shocks that might exist that might apply in the future.

MS BRAKEY: Thanks, Tom. Glenn?

DR BOYLE: I missed part of Clare's question. It broke up. But I think I got the gist of it, which was about Martin's suggestion for minimising mean square error, et cetera. That actually is one example of the recent academic work I referred to in terms of estimating accuracy of different candidates for beta.

However, there is a slight problem in that once you get out to the long estimation periods or long data periods, you don't have very many independent observations. Particularly for 15 years, how many independent 15-year periods do we have in the data? I would suggest not very many. So the objective of minimising mean square error, of there are other ways of doing it, has its limitations. It works well if we are dealing with short estimation periods, not so well if we are dealing with longer ones.

Just very briefly, I may be misquoting Tom here. He has expressed some concerns about using a long series of data because it involves projecting the past onto the future. Well, that's basically - if we can't do that, we can't do anything. If you are saying that you can't do that, you are basically assuming that the underlying distribution is non-stationary, because that is when there is no predictive power from the past. So while I understand what Tom is getting at, we basically have to assume that there is some predictive power in the past or we are nowhere.

MS BRAKEY: Thanks, Glenn. Tom, did you want to respond?

DR HIRD: I to some extent agree that if you can't use the past to predict the future then we are in

a difficult place. But I think it is important to say that we can look back and look at what happened in those periods and ask the question, "Are they likely?" Do we think that that exact mix is likely?

I think if your process was to use a five-year equity beta and you just measured one that covered the global financial crisis, and you were measuring that in 2012, you would have a good reason to think, "Well, okay. That is giving a lot of weight to that particular set of market shocks, but is that consistent with what my best estimate is going forward?"

So I totally agree that you can use the past, but you have to be aware that the past is unlikely to be like the future. And where that is most important and most practical is this question of - well, I'll give you an example of why that's important, or potentially important.

We are all aware of the transition away from gas, that gas businesses are likely to have a limited remaining life, especially gas distribution. There is going to be a change in the energy sector.

Now, that change isn't in the past. It may be that you theorise that that won't have any effect on estimated betas, but that is a theory and you could theorise that it will. Closing

your eyes to say, "Well, we don't have any Australian data on that and it is too hard to look at international data," I think that would be a mistake because the future isn't going to be the same as the past. With international data, if we don't have Australian data, we've got one observation, then we are not going to be able to - you know? So there is going to be information out there that we need to find.

MS BRAKEY: Thanks, Tom. Clare?

MS SAVAGE: This might be a question of detail that's less relevant. I guess it's just something I was curious about. We talk about excluding the GFC in some of the datasets that we are looking at.

I was just looking at the chart that Dinesh had there. Given the prevalence of the banking sector at that point in the composition of market capitalisation and the protection the banks received from government through the GFC, does that have a bearing on the relative equity betas we would observe through that period? And is that perhaps one of the reasons why you would exclude the GFC? Or not, really?

DR HIRD: I mean, I would respond to that just to say conceptually, yes. That, as a large shock, is going to affect the measured betas. I have done work in the past that says in that period financial stocks had the highest estimated betas.

And as Martin points out, everyone else has lower estimated betas as a result of that.

And equally, in the lead-up to the global financial crisis the resources sector had very high betas. It was growing very fast and dragging the market up with it at very high estimated betas and everyone else as a result, by definition, had low betas. And those events, you can't just think, "We'll just take 10 years. That's the longest period." You really have to think, "What is in that 10 years? And what's likely to be driving the estimated betas?"

MS BRAKEY: Thanks, Tom. Glenn?

DR BOYLE: But there will always be unanticipated shocks in the future. It doesn't have to be another GFC, but something else could generate a similar kind of impact on financial markets or indeed even betas. So I think excluding something because it seems extreme at the time from a long series of data is often throwing out the most useful information. So I would be a bit wary about doing that.

Another thing I think we need to keep in mind here is that estimating first moments means like the market risk premium is very different from estimating second moments like covariances. As no doubt we'll get into when we get to the market risk premium, estimating means just

requires a long time series of data.

Second moments, you can get a lot of an idea of what's going on and a more precise estimate simply by increasing the frequency of the data over a shorter period, because it is how things move together. So a shock like the GFC will have a big impact on prices and generate negative returns, but they all tend to go down together. It won't necessarily have anything like the same impact on betas.

So it's kind of important just to keep that in mind; this idea of throwing out or reducing time series simply because you're concerned about particular events is less of an issue. I agree it would be a big issue if you were estimating a mean. Estimating a beta or covariance is a bit less of an issue.

MS BRAKEY: Thanks. I'll go back to the board members now. Are there other issues that you want to raise? In that case, we still have 25 minutes left. I just wonder if the experts have any - sorry, Eric?

MR GROOM: I thought I might just come back to this issue of gas and stranding, because I think it is quite an important issue going forward. That is, to what extent is a stranding risk expected to be reflected in betas? I guess that comes back to the extent to which it will show up as a

systematic risk or not?

MS BRAKEY: That is exactly where I was going to take the conversation, Eric.

MR GROOM: If that were the case, how could that be reflected in the beta given the absence of data from a prior period where the stranding risk wasn't so apparent as it is becoming?

MS BRAKEY: Thanks, Eric. I think that is a really interesting question. Does anyone want to have first crack at that?

DR BOYLE: Well, I'll walk into the lion's den. I think stranding is potentially a real issue, but I don't think it is accommodated via beta. To the extent that it is and does actually turn out to be a systematic risk, then yes. How we would estimate that, I'm not sure.

But I'm not convinced that it is a systematic risk. It may still be a risk that requires compensation for various reasons due to market imperfections, but trying to do so via beta assumes that its risk premium is the same as that of systematic, in other words, the market risk premium - which could be the case by sheer chance, but in general is unlikely to be the case. So my at least preliminary view is that I'm pretty doubtful about allowing for systematic risk via an adjustment in beta. I think it needs to be calculated and adjusted separately.

MS BRAKEY: Thanks, Glenn. Dinesh?

MR KUMARESWARAN: I think Glenn is spot on there. I don't think there is any reliable way, firstly, to determine if stranding is a systematic risk for these gas networks. And secondly, if it were a systematic risk, how you would measure that given that we haven't seen it in the historical record. I just don't think there is any way of doing that.

I have seen some arguments made that you might compare beta estimates for gas businesses and electricity businesses historically and try and discern something about the difference in risk between those types of businesses. I just think that the beta estimates are so noisy that you can't make out anything useful by doing that sort of comparison. I think the more appropriate course of action for the AER would be to deal with stranding risk through something like accelerating depreciation or some other regulatory mechanism. I don't think trying to fit it into the rate or return allowance is appropriate.

MS BRAKEY: Thanks, Dinesh. Tom?

DR HIRD: I would agree with both Glenn and Dinesh that stranding risk is primarily a non-systematic risk, so that is where the focus of any regulatory action and compensation needs to take

place. But the question is, is it only a non-systematic risk? Is there a systematic component to it? And it is plausible that there is.

How could stranding risk drive systematic risk? Well, it depends on the investors' view of likely cost in the future of - if you just think about now, could an investor be looking at a gas business and asking themselves, "Does this stranding risk raises my systematic risk assessment of that?" Well, if they think that sudden action on climate change is going to be bad for the market, say, and part of that sudden action on climate change is going to be bad for the gas network then there is a systematic component to that.

If, actually, worse than expected climate change is bad for the market and also is bad for gas networks as it triggers policy changes then there is a systematic component to that. Now, I am purely speculating about that, but the question is if we don't have the domestic Australian gas distribution businesses in our sample, for example, or we have one transmission business then we are going to have trouble trying to tease that out.

Now, maybe we won't be able to tease that out using international comparators. Maybe it

won't show up at all; the betas are too noisy. But I'm pointing out that would be an advantage from including international comparators, because you could examine exactly that issue.

MS BRAKEY: Thanks, Tom. James?

MR HANCOCK: I acknowledge the stranding issue is an issue that seems to be on the mind of networks. It seems to me that trying to deal with it with a beta, as the previous speakers have said, is a very sort of distant and tangential way of dealing with it.

And the other thing perhaps to note as well is, thinking about what the correlation might be, it is possible that if we have a strongly growing economy, that the appetite to move away from fossil fuels will actually be stronger than if we have a weakly growing economy in which people sort of feel that their living standards aren't rising enough.

So it might be the case that the transition away from fossil fuels and the stranding to some, at least on that ground, does not have a strong correlation with the business cycle. Maybe a strong business cycle actually accelerates stranding and then a weaker business cycle would slow it down. And that would have implications for its beta impact.

MS BRAKEY: Thanks, James. It was nice to have

something that there is a fair bit of agreement on there. Any other issues that the board members want to raise? No? Are there any other comments that the experts would like to make? Martin?

DR LALLY: Yes. There's been a number of mentions in the AER's reports about the idea that if you use foreign beta estimates then necessarily you must be adopting an international CAPM. And I don't think that's right at all. Clearly, the Australian regulator is using a domestic CAPM. The betas are defined against the Australian market index. And if that set is sufficiently small, you might want to use foreign beta estimates, but those foreign beta estimates, for example from the United States, would be defined against the US index.

In both cases, you're using beta estimates defined against their local market index. That is not an international CAPM. An international CAPM would involve, amongst other things, using betas defined against an international market index. So the issue - and I have raised the issue of the merits of an international CAPM. They are quite distinct from the question of whether you would supplement domestic beta estimates injected into a domestic CAPM with foreign beta estimates each defined against its

own market portfolio. These are completely distinct issues.

MS BRAKEY: Thanks, Martin. Did anybody want to comment on that? Dinesh?

MR KUMARESWARAN: I agree with Martin on that. He is correct.

MS BRAKEY: Thanks, Dinesh. That was nice and brief. Glenn?

DR BOYLE: I also agree with that. I do want to take issue with one thing Martin said a little earlier, though, when he claimed that all of statistics is involved with minimising mean squared error and that my emphasis on bias was misplaced. It depends what kind of statistical exercise you are undertaking. If it is hypothesis testing then Martin is correct. But that is not what we are doing here.

As I emphasised before, it is the point estimate of beta that matters. The AER can only use one number. It can't use a confidence interval or range; it can only use one number. And so that number needs to be the closest to what the true number is. And that is the bias category. Precision and mean squared error are second order here.

I would also like to say, just as the very last thing, that also I agree with Tom's suggestion that one possible justification for

foreign firm betas would be if it could tease out the effect of stranding on beta. I'm not quite sure yet how that could be accomplished, but if that were true then that would be a useful utilisation of foreign betas.

MS BRAKEY: Thank you, Glenn. With that, I think we can probably have an early mark. Thank you everybody for participating in this session.

I should have mentioned at the very start that we are recording this session and the purpose of recording the session is to provide a transcript that the AER staff will work up and circulate in the next week or two, I would imagine. That's the reason that we are recording today's sessions.

So I think we reconvene this afternoon at 1.30 for the term of the rate of return and the weighted trailing average. So we'll see you all just shortly before 1.30.

THE SESSION CONCLUDED AT 11.47AM