TRANSCRIPT OF PROCEEDINGS

AUSTRALIAN ENERGY REGULATOR

RATE OF RETURN INSTRUMENTS
CONCURRENT EVIDENCE SESSION 3 of 4

MATTERS DISCUSSED:
Market Risk Premium

FACILITATOR:
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Mr Warwick Anderson (GM Network Pricing, AER)
Mr Jim Cox (Board Member, AER)
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Mr Justin Oliver (Board Member, AER)
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EXPERTS PRESENT:
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Dr Toby Brown
Mr Dinesh Kumareswaran
Dr Martin Lally
Dr Jonathan Mirrlees-Black
Professor Graham Partington

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MS BRAKEY: Welcome everyone to the third session and day 2 of the 2022 Rate of Return Instruments Concurrent Evidence Sessions. Welcome to all the people who are listening in as observers and to the AER board members and to the experts as well. I am Anna Brakey, for those of you who weren't watching in last week. I am one of the commissioners at the ACCC and I will be facilitating this session.

Before I do anything else, I would like to acknowledge the traditional owners of the country throughout Australia and recognise their continuing connection to the land, waters and community. We pay our respects to them, their cultures and elders past, present and emerging. And finally, I extend that respect to other Aboriginal and Torres Strait Islanders who are present today.

Last week we went through the purpose of the session and some of the kind of logistics. I won't repeat all that this morning, but I will say that the purpose of today is to assist the AER board to decide on the 2022 rate of return instrument by hearing from the experts, and we have all of the AER board members here today.

If you do want a little bit more information about these expert sessions then please go to the AER's website where they have published a series

of papers on the process that they are undertaking for the development of the 2022 rate of return instrument. I will just add though that there are submissions open at the moment. This is in particular for the people observing. So there are submissions open. Today it's just a discussion between the experts and then there is still a consultation process going on.

This morning's session will focus on the market risk premium, and the experts that will be discussing this with the board today are Graham Partington, Jonathan Mirrlees-Black, Martin Lally, Dinesh Kumareswaran, Toby Brown and Glenn Boyle. The experts have been asked here to assist the AER board to make the best possible decision on the instrument.

For our first session this morning we will do it slightly differently to the way that we did it last week. We will start the discussion with hearing from all of the experts one-by-one up front rather than having the two primary discussions that we had last week, and then we'll have a discussion after that first round.

So that first round will focus on two questions as set out in the agenda, their views on the weight of evidence about whether the MRP varies through time, and whether if they do think it does, how it varies. We will then move on to

a second part of this morning's session where we will get short presentations from Jonathan and Graham focused on the further three questions that are set out in the agenda: Whether the long-run estimate of historical excess returns is the best estimate of MRP; whether the AER can derive a better estimate by incorporating other information and if so, how; and what are the challenges the AER would need to overcome in giving weight to the other information. And then there will be another general discussion.

This morning's session is a little bit longer. It is scheduled to go for three hours, just recognising the importance of the MRP and the board's interest in the MRP. Having said all of that, does anybody on the screen or any of the active participants have any questions or anything that they wanted to go through?

No? Okay. Fantastic. We might move on with Graham kicking us off.

PROFESSOR PARTINGTON: I'm going to start with just a little bit of pedantry and I'm also going to tell you what I call "the Shiller story", because that helps us understand where this focus on time-varying returns has come from.

The pedantry first: Well, really there are two dimensions to the time availability of the market risk premium. We've got the current term

structure. That is, now looking forward, what market risk premium should we be applying to cash flows with different terms? And then we've got that term structure shifting through time.

Now, handling all of that is really rather hard. Too hard, in all probability. So we just tend to assume there's only one market risk premium, which is changing through time. That's okay, but if the AER does switch to a five-year term for the cost of equity, I rather suspect you might start to hear quite a lot about the term structure of the market risk premium.

Moving on to what Shiller's work was about, in 1981 he published his paper showing that price volatility was too great to be justified by the subsequent volatility in dividends. This of course delighted the people who were in behavioural finance because it was evidence that prices were irrational.

Shiller delighted the people who were in behavioural finance because it was right up the street of irrational asset pricing, but a key feature of the analysis was that we had a real constant discount rate and that became the focus of people who were in the efficient market rational asset pricing cap because of course Shiller's work was a real challenge to them. And their solution was, "Well, it's obvious that this

result is driven by the assumption of a constant discount rate. The volatility in prices that we observe must be due to time-varying discount rates. Then a search commenced for evidence of time-varying discount rates and indeed such evidence was found. And a lot of that work was done by a guy from Chicago called Cochrane who actually does very good work.

As a consequence of all that, the pendulum shifted. Originally it used to be the case that it was generally assumed that if the pricing's changed, it was because there was cash flow news, changes in expectations of cash flows. It then became the view that if prices changed, it was because of discount rate news, i.e. changes in discount rates or changes in the market risk premium.

Now, in my opinion the pendulum probably swung too far towards the time-varying discount rate explanation and the truth, like so many things, probably lies in the middle ground.

So that's how we got onto time-varying discount rates, but there's another story to be told about excess volatility, and that is that in fact dividends were the wrong measure of cash flow. Merton and Marsh, in a very nice paper, pointed out that the value of equity is fundamentally given by the present value of the

free cash flow to equity. Subject to the present value of dividends equalling the present value of the free cash flow, you can have any arbitrary stream of dividends that you like.

Now, as it turns out managers choose to set dividends so that they are a smooth version of the underlying free cash flow. So if you compare prices with a smooth version of the true value driver of prices, it's no surprise to find the prices are too volatile to be explained by volatility in dividends. I'd also point out that Shiller was probably mis-measuring his dividends since what you really need when you are doing valuations is the net dividend, and in Shiller's case that should have included share repurchases, which were a big deal in the USA. So the moral is: Take care when you are using dividends in your analysis of returns

I'm using up my time, so let's quickly move on to time variation in the market risk premium. Would we expect it? Well, I think yes, we would. Price of risk is likely to change through time; level of risk is likely to change through time. It is the product of those two things which drives the market risk premium. Risk aversion is likely to change for many reasons. For example, changes in the level of wealth or, to put it in popular terms, a change in the balance of fear

and greed.

It is also clear that if you look at realised stock market volatility or if you look at implied volatility from option prices, volatility changes through time. I should make the important point, however, that periods of high volatility are short-lived so they are not likely to drive long-lived variation in the market risk premium. On that basis, I say we should clearly expect time variation in the market risk premium.

What is the evidence? How does the market risk premium vary through time? If I had a predictable model of how the market risk premium is going to vary through time, frankly, I don't think I'd tell anybody. I'd keep it to myself. That's rational wealth maximising behaviour. What we can say about the evidence is that the predictive models tend to have poor outer sample performance and they suffer from specification error.

A colleague of mine, Min Zhou, wrote a very nice paper demonstrating where the specification error comes from you get spurious correlation, and also demonstrates that when you fix that spurious correlation, the significance of the predictor variables disappears.

So, bottom line, almost certainly the market

risk premium varies through time but I doubt that anybody can reliably tell you how it varies through time. And I think that's it.

MS BRAKEY: Thanks, Graham. We'll move onto Jonathan.

DR MIRRLEES-BLACK: Thank you, Anna. I have some slides which --

DR LALLY: Excuse me, Jonathan. Anna, could I just ask a clarifying question?

MS BRAKEY: Yes.

DR LALLY: You have put a number of questions to us. Is it the intention that each of us talk about that first question about variation through time and then after we've discussed that, then we will come back for a second round on the other questions that you've raised?

MS BRAKEY: There's kind of the two. There's whether it varies through time and if so, how. That's that session; that's the first part of this morning. And then the next three questions, we will deal with later.

DR LALLY: Understood. Thank you.

MS BRAKEY: Back to you, Jonathan.

MR MIRRLEES-BLACK: Thank you. I've got some slides. I'll move rapidly through them, but I will give you page numbers. I'm not going to go through them in full detail. If we start with page 5, as Graham was saying, academic opinion on the MRP

has shifted away from the view that it is stable.

Graham mentioned Cochrane, and in his address 2011 Presidential Address to the American Finance Association, he highlighted the shift in views and essentially while there is not unanimity, I think there is acceptance of time variation since them. He says, "Well, our view of the facts has changed 100 per cent since the 1970s," and he goes on the say, you know, dividend yields forecast returns, not dividend growth. I think that is important.

But I think that given that there is uncertainty about what the theory tells you, it is not conclusive, we have to look at the empirical evidence and see what it says. And for that, we can look at two types of evidence. We can look at ex-post equity returns versus safe rates, and we can look at forward looking returns based upon what market expectations are and the measures that we have of those.

Page 6 shows some charts of returns. I look at 10-year return periods, so each data point is a CAPM investment horizon in which you start off with the 10-year bond yield, which is a safe nominal return for that 10-year time period. Inflation is the growth rate of the consumer price index over that time period when we get to looking at inflationary returns. So this

approach, rather than looking at the annual returns, it reflects investor time horizons and the AER's 10-year term.

Now, the MRP on the right-hand chart is not stable. It is roughly stable but volatile around that until about 1970 and then it falls and it becomes more volatile. So we see from this clearly there is both structural variation for a number of reasons but there is also cyclical variation.

Moving to page 7, we can see that inflation has had a massive impact on these returns. There has been three periods over this sort of 100-year-or-so period of higher inflation which was probably unanticipated or at least partially unanticipated. And that's been associated with both lower equity and lower bond returns.

So in looking at comparing the MRP to inflation, the MRP is the blue line and the dotted green line is the inflation. And we can see that on the left-hand chart where bond returns are the blue line and equity returns are the grey one, real bond returns have risen with the fall in the MRP associated with that.

Now, with this chart, because I'm looking at 10-year time horizon rolling periods it doesn't include the very recent periods of very low interest rates. So we are missing some of that

evidence and of course we can't have that.

Now look at page 8. Historic MRP data is on this chart is the left-hand chart. It shows a weak negative relationship with real bond yields. But looking on the right-hand chart, we can see that there is real equity returns. There is a positive relationship between real equity returns and real bond yields based on this data and this method.

Importantly, though, and this is important for the considerations here, the increase is not one for one. So it's not that a 1 per cent increase in the real bond yield is associated with a 1 per cent increase in the real equity return. It's about 60 per cent on these numbers, and one can test and analyse seriously econometrically, but this is just looking at what the data is telling us from what we see.

So page 9, what else is going on? Well, we saw earlier that inflation has got a big impact on returns. This one we see that both the bond and equity returns - bond returns on the far right, equity returns in the middle - have a negative relationship with inflation. Historic MRP, no clear relationship with inflation. That's relevant in thinking about what are the drivers of the MRP and also is the MRP the right thing that we should be looking at?

What about the rest of the world? Here, I show the results of a study by Òscar Jordà and his team. It's a very detailed assessment over a long period of time of the returns on a range of assets from 16 developed economies. And we see a similar pattern to what we've seen in Australia.

The right hand chart is the risk premium, which is risky assets compared to the safe rate. Again, it looks at rolling periods. We see that it varies through time. There are structural changes; there are some cyclical changes as well. We see on the left hand chart that the risky return appears to be more stable than the MRP.

Now turn to page 11, where we move from historic evidence to looking at the forward evidence from the dividend discount model for Australia. This was work which was done by CEPA and published by the AER last year. We use several different dividend growth model specifications. Note that we are not assuming that dividends and discount rates are stable here in the future; it's just a measure of what the average dividend growth rate is expected to be and the average discount rate and what that is expected to be. Now, each of the specifications produce rather similar results and a rather strong negative relationship between the cost of equity and the risk free rate, and stronger in

the recent past.

Moving on to page 12, we show a similar chart to the US. And this is data again on the implied return on equity. And it's that the return on equity which was published by Professor Aswath Damodaran in New York. Here we show that on his data, there is a strong relationship between returns and the and the risk free rate for the last 30 years, but not for the whole period from 1961. So there was a structural change in the relationship. And there are many reasons potentially for that, but one could well be, given the importance of inflation to interest rates, that there had been a change in the way in which inflation expectations are determined, given the changes in monetary policies around that time.

So what do we conclude, returning to page 10? First of all, that there is cyclical variation, second that there is structural variation, there are structural changes to the economy and the way that monetary policy has developed, which is likely to have influenced that. But I think we can also say that the MRP looks like it is less stable than the return on equity, and more tentatively perhaps that equity returns is what investors form expectations about rather than necessarily the MRP, and that equity

returns have a closer relationship with some of those variables than the MRP.

One of the important conclusions of that is that if there isn't evidence of stability in the MRP - and there isn't - why should the tools for policy on MRP assume it? And what does this all mean? I think the AER should consider looking at MRP by estimating a return on equity or at least placing weight on measures which assume a more stable return on equity rather than the MRP, given the relationships we have shown.

We need to consider whether the task is looking at structural returns or cyclical returns, and that comes back to Graham's point around term structure. And I think it is relevant that if a change is made, it's a result of change in the evidence and it's also a result of change in academic thinking. I'll leave it there.

MS BRAKEY: Thanks, Jonathan. So just to summarise your position, you do think it varies through time, and then the question of how does it vary through time, you're saying that you need to unpack whether you're after a structural or cyclical number? Is that how --

MR MIRRLEES-BLACK: That's true. But I would say yes, it varies through time. It's a reasonable assumption that equity returns are more stable

than the MRP. And essentially, I think it's better to make the assumption that - again, I mean, the MRP on average is higher when risk free rates are lower.

MS BRAKEY: You're saying there is a negative correlation, really?

MR MIRRLEES-BLACK: I think the evidence from forward-looking and some evidence from backward-looking shows that lower risk free rates are associated with higher risk premium.

MS BRAKEY: Thank you. Toby, I notice you've got your hand up. I was going to go around to all the experts, but did you want to interject now on something in particular?

DR BROWN: If I could just go real quick just to ask Jonathan a clarifying question. Just picking up on what you said at the end there, Jonathan, about equity returns being more stable than the MRP, would I be right to infer that that might push one in the direction of setting the equity return in the rate of return instrument at the beginning and then not updating the risk free rate at each revenue determination?

MR MIRRLEES-BLACK: No, I wouldn't say that. I'd say that - and we'll come to later, "Well, what's the best way of setting the estimate of the MRP?", and I think one should use a range of evidence for that. It's a question of how you construct

your forward-looking estimates of the MRP.

The use of the historic excess return makes the assumption that the MRP is the stable thing. If you're using that as an estimate, I think that's one way of doing it. But I think it makes sense to look at historic equity returns, real equity returns, as a metric for looking at the past as well as looking at excess returns. So I'm not saying you should keep it stable; you should look at the evidence on each occasion.

MS BRAKEY: Thanks, Jonathan. I think we'll move along to Martin now.

DR LALLY: Thanks, Anna. I don't think it's controversial that the MRP is extremely difficult to estimate. Graham, I think, has made that point very strongly and I agree with him. In Jonathan's analysis, the 10-year historic MRPs, or what he calls 10-year historic MRPs, they are in fact estimates, not the true values. So that variation we are seeing there may be completely a sampling error. The standard deviations on 10-year MRPs are enormous. So it's difficult to read too much into those 10-year historical averages.

What I think we can say is that the MRP is a reward for investors bearing risk, and that risk is measured defined by market volatility. And clearly market volatility moves through time.

Most particularly, it spikes in recessions. Therefore, one would expect the MRP would be higher during these recessions and then it would tend back towards a more normal level.

And that could easily explain why we see some evidence that the MRP is negatively correlated with the risk free rate. Risk free rates tend to be low in recessions due to the central bank policy. And that's the very time when you would expect MRPs to be high because volatility's high. So that negative relationship between the MRP and the risk free rate, not saying it's one-to-one, but the idea that there is a negative relationship is entirely plausible, driven by what happens during recessions.

A further point that's worth noting is that while these spikes in volatility could be expected to produce quite significant variations in the MRP, those would be relatively short-lived. So if volatility spikes for six months, the MRP spikes for six months. But the AER is interested in the MRP over a five-year period or maybe even a 10-year period. And that five- or 10-year period will incorporate within it some sort of average of the MRP for the next six months, the six months after that and so forth. So these volatility spikes may produce quite significant variations in the MRP, but

that's very transitory. The impact on the five- or 10-year MRP is much less.

A further fact which is happening or may be happening is that markets are clearly gradually integrating. 50 years ago an Australian investor didn't have much opportunity to invest outside Australia. They do now, and lots of people are doing it. So we would expect that we are moving from a world in which equity markets are domestically segmented to a world in which they are internationally integrated.

But it's plausible that the true MRP is lower under integration, under an international CAPM than it is under a domestic CAPM. So what may be happening is that gradually over time the MRP is declining as we move from a situation of completely segmented markets to completely integrated markets.

So I think you've got two things going on here: You've got fluctuations in the MRP, which very plausibly are being driven by fluctuations in market volatility; and secondly you have got potentially a slow, long-term decline in the true MRP. But measuring or observing these things is difficult. If we could estimate time variation in the MRP very accurately then we would know at any given point what the MRP was, and since that is the fundamental question we are interested in,

we wouldn't have be having the kind of problems we are here. We'd love to know what the MRP is for regulatory purposes, but it's tough. Estimates are very, very unreliable.

MS BRAKEY: Thanks. Martin. Do I summarise your position as you think that it does move through time but over a longer time period, and that it's quite difficult to - like, the answer to the first question, whether it varies through time, so you're saying it moves slowly through time?

DR LALLY: I'm saying it's doing two things. I suspect there's a very long, gradual decline over time and there is also short-term fluctuations that are driven by fluctuations in volatility which come from these periodic downturns.

MS BRAKEY: And how does it vary through time? You're saying that it's quite difficult to measure?

DR LALLY: Yes. Plausibly, it's fluctuating with volatility. But measuring it precisely? Well, that is simply impossible. We haven't got any technology that can estimate the MRP very reliably.

MS BRAKEY: Thank you. Glenn, we're going to you next.

DR BOYLE: Is it me, Anna, or is it Toby? On the list I'm looking at, it's Toby next.

MS BRAKEY: Sorry, it might be Toby. I might have the wrong list in front of me.

DR BOYLE: I'm happy to go, but Toby, if you are, go ahead.

DR BROWN: I am happy to go, so I'll go.

MS BRAKEY: Sorry, Toby.

DR BROWN: No, that's quite all right. I agree with a great deal of what's already been said, so I am going to sort of skip to a little bit of a summary of what I think, which is there's a lot of uncertainty here. But the MRP does vary over time; that's reasonably clear. But we just don't know a lot about how it varies. I think we can say that there is a negative correlation with the risk free rate, so that if the risk free rate is lowered at a particular point in time, the MRP is more likely to be higher at that point in time, and that correlation is less than one-for-one. So if the risk free rate has gone down by half a per cent, let's say, the MRP has probably gone up by less than half a per cent. And working out the precise details of the correlation is basically impossible. All of the confidence intervals on any estimation are going to be very wide. And who knows - the way that those two things vary together over time might also not be stable.

So that's what I think. Why does it matter?

I think it matters in two ways. First, how actually do you estimate the MRP, what's the best way of estimating it every four years? We're going to come on to talk about that later, but also I do think it's important for a second reason, and that's should we be fixing the MRP for the term of the rate of return instruments so we do it once up front, it's a number and then it stays the same, or should we have some mechanism for updating it during the term of the rate of return instrument? Obviously, if there was an updating process it would have to be mechanical without discretion, like the risk free rate is updated or has been in the past. And the reason to think about that second question is that if there is a correlation, then if you're fixing one MRP but allowing the risk free rate to vary then you're building in error. So I think both of those are interesting questions.

The next thing I'd like to mention is that there is some experience in North America of regulators trying to do a sort of an automatic update where you have a formula that effectively adjusts the allowed return on equity as observed risk free rates change. Those formulas have not had much success. They've worked for a while, but then they start to produce results. At the time of GFC there were formulas in place but

those sort of broke down completely. There is one in place in California, but that's sort of breaking. So they just don't work over an extended period.

The bottom line is that the MRP is not constant; it is affected by capital market conditions. So is the risk free rate, and those two things may well be moving in the opposite direction. But understanding or predicting exactly the correlations is not feasible.

MS BRAKEY: Thanks, Toby. So you do think it moves over time and you recognise that there's a negative correlation, that in actual fact the AER might build in error if there is that correlation and you fix the MRP but vary the risk free rate, but you're not really sure how to resolve the issue? Is that it?

DR BROWN: Well, actually, I've got a suggestion for how to resolve the issue, which is not to update the risk free rate, to essentially set the cost of equity at the beginning. Then that does potentially give rise to another issue because you've got the four years of the rate of return instrument and then you've got five years of the revenue determination so that a somebody, a network that comes in towards the end, those parameters are pretty - you know, the risk is tat they might be out-of-date and there is currently

no mechanism for those parameters to be updated until the next cycle. But there again I think there is a straightforward solution, and that's simply to, whatever the outcome of the rate of return instrument process is, to apply that immediately to all networks without waiting for the next determination. And I don't see why that couldn't be done in a rather straightforward way.

MS BRAKEY: Thanks, Toby. On to Glenn now.

DR BOYLE: Thank you, Anna. Well, I can really speed things up here because as is probably obvious from the slides I circulated: What Graham said, me too. So does the market risk premium vary through time? Well, absolutely. Unless somehow you can repeal the laws of arithmetic, the only way you can reconcile observed price movements is by invocation of a time-varying expected return. How does it vary through time? I have no idea. Nobody does. Anybody who actually does is sensibly keeping shtum about it and is making a fortune.

But if I could just touch very briefly on one point that Graham didn't mention. The very first question about, "What is you view on the weight of evidence about whether the MRP varies through time?", this is kind of a pedantic point in a sense but also keeping it in mind helps avoid confusion: There are actually two MRPs.

In an IID (independent and identically distributed) world where everything stays the same there is only one, but it is by definition constant. In the world we more likely live in, a non-IID world, then there are two MRPs. There's an unconditional MRP, which by definition is a constant and so doesn't vary through time and there is the conditional MRP, which by definition does vary through time.

I think most people, when they refer to the MRP, have the conditional version in mind but then talk about estimating it often as though it were the unconditional premium, via historical averaging. And this can cause confusion. So my answer as to whether the MRP varies through time, I'm saying there is a conditional MRP and it varies through time. How it varies through time, no idea.

MS BRAKEY: Thanks, Glenn. I don't think I need to summarise that point. I think you summarised it yourself. Dinesh?

MR KUMARESWARAN: Thanks, Anna. I've got a couple of slides so I'll just run through those very briefly. I essentially agree with everyone who has spoken before me that there are very good reasons to think that the prevailing MRP changes over time. What I'm referring to as the prevailing MRP is the conditional MRP that

Glenn's just talked about. I think there are very good reasons to think that that varies over time.

And on the second question, how does the MRP vary over time, I'm again with Glenn. I don't know precisely how the MRP varies over, time but I agree with what CEPA says on page 6 of their report that there is strong and convincing evidence that there is a negative relationship, for Australia anyway, between the market risk premium and risk free rate since at least the mid-1990s.

Let me elaborate on those two points. I want to emphasise a point that Graham made, that there are essentially two components to the market risk premium. I'm on slide 2. There is the market's assessment of the quantum of risk and secondly there is the price of risk. That is, the compensation required by a diversified investor for bearing each unit of risk.

Now, if you think that the MRP is constant, you would have to believe that neither of these things change over time, and I think that's just not consistent with the observed facts. The empirical literature on this topic is really focused on investigating the variables that might be correlated with changes in the market risk premium. The empirical literature takes for

granted that the MRP does change. So it's not investigating whether the MRP changes; it's how the MRP changes and what variables, conditioning variables, may be correlated with the changing MRP.

To the point about does a constant MRP fit with the observed facts, there's a paper by Harris and Marston, a couple of researchers from the University of Virginia, and they make the point that if you assume the constant market risk premium then what you would have to believe is that had the required return on equity changes one-for-one with government bond yields. And that just doesn't seem sensible or consistent with the observed facts.

So on slide 3 I have a picture that tries to demonstrate this point. The blue line is the yield on 10-year government bonds in Australia. And the orange line is the CAPM return on equity estimated by adding a fixed market risk premium to the blue line. This is the Harris and Martin point: You'd get a situation where the estimate of the required return on equity moves one-for-one with changes in the government bond yield.

Now, look at what happened during the period of the GFC. The estimate of the required return on equity, the estimate derived using that

method, fell by 15 per cent during the peak of the GFC. So the day after Lehman Brothers collapsed, the price of risk apparently went down, using this method. I don't think that makes any economic sense. And so, what Harris and Marston say, they have a couple of conclusions in their paper. They say that:

> *Shareholder required returns change by less than do long-term government interest rates. ... As a consequence, cost of equity estimates using a constant risk premium assumption are highly likely to underestimate (overestimate) required returns in low (high) interest rate environments.*

I think that's right. They also say that:

> *Improved practice would incorporate an estimate of the market risk premium that reflects current market conditions and the relationships among the equity risk premium, interest rates and key metrics of market risk.*

Again, I think that's right and consistent with what nearly all of the other participants have said, that if you want a good estimate of the market risk premium, you're best off by combining different pieces of evidence.

Now, turning to the question of how does the prevailing market risk premium vary in Australia, again I have to emphasise that I don't know precisely how the market risk premium changes in Australia, the true market risk premium and how it is related to the risk free rate. But if you

look at the empirical evidence, it suggests at least that there is good evidence that there's a negative relationship between these two things. And the evidence that I have focused on here is from dividend growth models because, as the AER itself has explained in the past, the dividend growth model is probably the best way we have of getting a handle on what the prevailing market risk premium is.

So I've got three charts here, and there are many, many others that you can find that show a very similar thing. The first is from CEPA's paper. Jonathan didn't cover this one in his presentation, but it's a version of the DGM that uses forecasts of dividend yields. And you can see a negative relationship there between the market risk premium and the risk free rate.

The second chart is IPART's estimate of the prevailing market risk premium. IPART derives it estimate using six different methods, combining the estimates from six different methods, five of which are DGMs, differently specified DGMs. And again, you can see a negative relationship between the estimate of the market risk premium and the risk free rate.

And then the final chart is the calibrated DGM that the ENA has proposed, which is an extension of the three-stage dividend growth

model that the AER developed in 2013. Again, you can see a negative relationship. Over different time periods, but all telling a fairly consistent story of a negative relationship. Now, the slope of these lines are all different, so I don't think we can infer from that precisely what the true relationship is, but we can be fairly confident, I think, at least right now, the relationship is a negative one.

Now, one point I would like to make is that there sometimes seems to be a bit of a confusion when we talk about the dividend growth model that somehow there is an assumption of a negative relationship that goes into the model, so you start with an assumption of a negative relationship. That's not the case. What you have, the negative relationship is an outcome of the model. So you just put in the market data and then you observe what the model tells you the market data shows. And the market data would indicate a negative relationship.

I just want to pick up on a couple of points that Martin made. He mentioned that the market risk premium has a reward for bearing market volatility. And I think that's correct. One way we can understand why there might be a negative relationship is if we go back to slide 3, that picture that showed the GFC event. During that -

so I'm picking the financial crisis because it's an extreme event that sort of demonstrates a point. During the GFC, what you had was a spike in market volatility, just as Martin outlined.

And exactly during that period, what we saw was government bond yields fall very significantly. So the level of risk in the market went up and we observed that government bond yields went down. Why did that happen?

There's very good evidence that what occurred is a phenomenon called the "flight to safety" where investors observing an increase in market volatility - market risk - shifted their holdings away from risky assets in favour of or towards low-risk assets like government bonds. So that had the impact of pushing down government bond yields.

So what we can possibly infer from that is that when market risk went up, there was a substitution away from risky assets - pushed down government bond yields. At the same time, the level of risk went up and you would expect that that coincided with an increase in the market risk premium. That's one way we can conceptualise this relationship.

The second point Martin made that I want to pick up on is this idea of a long term decline over time in the market risk premium as markets

integrate. I think that's a reasonable story to tell, but I don't think it's entirely clear cut. One of the consequences of markets integrating is that investment opportunities open up to investors who previously didn't have these investment opportunities. Particularly in emerging economies.

And there's good evidence that those types of investors are more risk averse. For a range of reasons, they tend to be more risk averse investors than investors in established, developed economies. And so, it's not entirely clear that you have a persistent long-term decline in the market risk premium. You may do, but it may also be that the market integration that Martin talked about also increases the market risk premium. So I'll just leave it there and hand over to Anna.

MS BRAKEY: Thanks, Dinesh. That brings us to the end of the presentations. I was just wondering if the board members had questions at this point? No? Well, I think there was pretty strong support --

MS SAVAGE: I think Jim might have just put his hand up, Anna.

MS BRAKEY: Sorry.

MR COX: I'm sorry, Anna. I'm having trouble with my hand functions. I apologise. But I do have a

question.

One of the things that emerged in the discussion is the idea that there is a correlation, at least for some periods, a negative correlation between the risk free rate and the market risk premium, and that's argued on semi-empirical grounds.

Bearing in mind that we are regulators and we are going to have to set a market risk premium that will apply to determinations for a considerable period of time, is this the sort of correlation we can rely on or is it one that's likely to break down under the pressure of events? Since the argument is so empirical, I would just be interested in the views of experts on that question. I think it's one we'll need to think about.

MS BRAKEY: Who wants to jump in and answer that one? Perhaps either Jonathan, Toby or Dinesh who talked about the negative correlation? Okay, we've got Graham. Graham, did you want to jump in?

PROFESSOR PARTINGTON: I'd just point out this negative correlation may not exist at all. And indeed, if you look at the evidence, it's all over the place. Up, down, no relation, resume shifts.

Now, in the case of using the dividend

discount rate, the DGM model, it's no surprise at all to me if you find you've got a negative correlation. Let's just think about what happens. We're heading into a recession. Stock prices fall sharply. Management hold their dividend. What happens to the dividend yield? It goes up.

What happens to growth rates? Well, usually the long term growth rate won't be changed. And for various reasons that I'll be happy to discuss later, it's very probable that intermediate growth rates are not adjusted downwards enough. Consequently, what you will observe is an apparent expansion of the risk premium, the market risk premium.

Now, what's going on in a recession? Interest rates are going down both because of a declining demand to borrow and action by the monetary authorities. So what you see, interest rates low, estimated market risk premium - misestimated market risk premium, I would say, increasing. And vice versa when you move out of the recession.

So you may well get that negative correlation purely by virtue of sticky dividends when you the use the dividend growth model to estimate your market risk premium. So I wouldn't place a lot of weight on that evidence.

MS BRAKEY: Thanks, Graham. Martin?

DR LALLY: I agree entirely with the comments that Graham has made. I think that just reinforces my earlier point that the MRP is extremely difficult to estimate. I wouldn't place any reliance on the statistical relationships that have been found, partly for the reason Graham mentions and also partly because the confidence intervals on the estimates are so wide.

What I think the AER should do is to take account of a wide range of different estimators, some of which are sensitive to short term changes in the market risk premium. So, for example, the DGM would be in principle, and surveys would tend to do that as well.

So if the AER does put weight on a wide range of different estimators, by that very process it will be getting some relationship between the MRP estimate and current economic conditions, whatever that is, but not using a statistical relationship, a regression relationship between risk free rates and estimated MRPs.

MS BRAKEY: Thanks, Martin. Jonathan?

MR MIRRLEES-BLACK: Thank you. Of course Graham's assumptions around the way that corporate behaviour might work might have some element in terms of the conclusions you could draw from a

dividend discount model. And that's why I think it makes sense to look at alternative specifications of the models looking at discount rates.

In the work that we did for the AER we didn't just look at dividend discount models; we also looked at a specification where we looked at earnings yield, which of course takes away from the specific dividend policies of individual companies. We found the same result, which is that the expectations of the MRP implied by those numbers is that you have that same correlation.

So I think that the empirical evidence shows that there is something in that correlation even if you do alternative specifications, and I think that's one important point. And I think the converse is that we don't have the evidence of stability in the MRP, so let's not assume it. So we need to take the two propositions equally. It's not to say we should assume the MRP because we don't have evidence that it varies with the risk free rate. We have evidence that it varies with the risk free rate. I think the case is to prove the stability.

MS BRAKEY: Thanks, Jonathan. Glenn?

DR BOYLE: Just to address the Jim's question, I completely agree with Dinesh and Jonathan that there are really good plausible reasons for why

there might be a negative relationship between the MRP and the riskless interest rate. No problem there at all. But I would be hesitant, as Martin and Graham have pointed out, about drawing too much from these empirically estimated relationships. It's important to remember we can't actually observe, even ex-post, the true market risk premium. And these negative relationships have all been estimated using some model. Primarily the dividend growth model but, as Jonathan's pointed out, not limited to that.

The trouble is these are estimates. So they equal the true market risk premium plus an error. And all these negative slopes may be picking up is that the error is negatively correlated with the riskless interest rate. So it's very hard, I think, to infer anything very much from this evidence simply because we can't observe what it is we are trying to use as one of the moving parts.

MS BRAKEY: Thanks, Glenn. Clare, did you want to jump in now before I go to Toby and Dinesh?

MS SAVAGE: No, go to Toby and Dinesh and then I'll just make sure I understand where everyone's sitting.

MS BRAKEY: Right, okay. Thanks, Toby?

DR BROWN: Thanks. Yeah, so I just want to emphasise again that one issue is how do we estimate the

MRP, and of course that's very important. But under current practice, after the MRP has been estimated, for the next four years every time a determination comes around, the allowed return on equity is going to vary one-for-one with the risk free rate. And I think that's not really consistent with the evidence that we've been talking about.

MS BRAKEY: Thanks, Toby. Dinesh?

MR KUMARESWARAN: Yes, I just want to make a couple of points. The first is that I can't see why the AER needs to know the precise relationship between the market risk premium and the risk free rate. That's not necessarily a necessary condition in order to give some weight to something like the dividend growth model.

I guess the point that I was trying to make in my presentation is I don't know what the precise relationship is, but there is very good empirical evidence to suggest that right now there's a negative relationship. So I agree with Jonathan that the key question that the AER needs to ask itself is, "How convincing is the evidence that the market risk premium is constant?" And I think that evidence is very thin.

To Jim's question, it was essentially, "Well, if we believe there is currently a negative relationship, should the AER worry about

that relationship changing over the RoRI period?" Well, it's possible that the relationship might change, but the evidence suggests that it doesn't change overnight. So there are sort of regime shifts that occur, but you would have to go back more than 30 years to have a situation where the relationship flipped. So I don't think we should worry about that too much.

But even if that were the case, as I said in my presentation, you don't have to make an assumption about the negative relationship in order to apply the dividend growth model. It's an outcome of the model. So all you have to do is to run the models and see what relationship it's showing. So if it happens to show a positive relationship, well, that's fine. The most important thing is, do you think that the dividend growth model contributes some useful information to the estimation of the market risk premium?

MS BRAKEY: Thanks, Dinesh. Jonathan, did you want to just quickly add something before I go to Clare?

MR MIRRLEES-BLACK: Thank you. Very quickly, Graham's critique of the dividend growth model estimates is that it doesn't respond properly to recessions. Most sensible specifications of the dividend growth model will have a return to

normal. So you will explicitly look at what the expectations are through the cycle, through the recession. It will take precisely account of all that. So I think that that objection can be dismissed. And I think, as Dinesh was saying, we need to be driven by the empirical evidence. Rather than that we can't take anything out of the data, I think we can take things out of the data. And we certainly can't take it out of theory, so let's rely on the data.

MS BRAKEY: Okay, thank you. Clare?

MS SAVAGE: I apologise. The AER is in Senate Estimates at the moment, which is where I should normally be, so I'm a little bit two-minded at the moment. So I just want to make sure I've captured the thoughts of all of our experts today.

It seems to me that Graham and Glenn are both saying yes, it varies through time, but it's not clear that it varies necessarily with the risk free rate and it's actually not possible to estimate anyway or very difficult to estimate anyway. I think everyone else is saying yes, it varies with time, it's not necessarily fixed one-for-one with the risk free rate, it may be negative, but it's also impossible to estimate.

Whereas there's a variation in the strength of views in that latter camp, with Martin sort of

saying there might be some information that's relevant right through to I think Dinesh's view, which is more, "Yes, it's got a real role to play."

And I think in the next part of this, we'll talk about what role, if any. I think we will need to think about inflation, particularly given where we are at. And I think, Martin, you have got some quite interesting views on the role inflation can play in terms of some of these relationships.

And I think obviously the experts are here. It will be interesting to see whether the stakeholder groups continue to have the same position on a negative relationship if we are in a strongly growing environment for the risk free rate. That may be a different set of stakeholder presentations as we get through this year, but that's a side point.

But did I correctly capture the camps on the relationship to risk free rates? I think everyone's saying it's time-varying, but there's a difference of views as to whether there is a fixed relationship or not with the risk free rate. And I think everybody agrees it is impossible to estimate. But I just want to check I got the mix of views there.

MS BRAKEY: I think you probably did, Clare. I think

if there was kind of a little bit of a divide that I picked up, it was some people said, "Well, there is evidence. Let's rely on the evidence." There is really no evidence for a fixed MRP, so it's a question of whether you want to rely on the evidence that is there or go for a more theoretical or kind of a, "hands in the air, can't estimate this," type approach.

MS SAVAGE: When I was making notes last night, Anna, I was like, "We've got this awesome situation where we're trying to choose between doing something that seems to be wrong but stable or something that may be wrong and less stable but more right.

MS BRAKEY: Yeah. Okay. With that - Dinesh, you've got a hand up. Go, Dinesh, and then I'll quickly go to Eric.

MR KUMARESWARAN: Yes. I think, Clare, you characterised the positions pretty well. I just wanted to clarify one thing about my position. So my position is that the AER really doesn't need to know the precise relationship. It just needs to ask itself, "Does the dividend growth model" - I think that's why we're sort of talking about this relationship, because of the considerations about the dividend growth model. And I guess we'll come onto this in a moment, but all the AER needs to ask itself is, "Does the

dividend growth model contribute some useful information?"

Before we move on, I also wanted to say a couple of words about - well, in my presentation I had some charts that all seemed to show a negative relationship between the risk free rate and the market risk premium, but in the omnibus paper the AER explained that it had developed a version of the dividend growth model that apparently seemed to show a positive relationship.

I don't think that that particular specification that the AER's model has used is a sensible one, for the following reason: It seems that the main reason that the AER finds a positive relationship is because the AER has chosen a fairly unusual estimate of the long term growth rate for the economy. It's a key input to the dividend growth model. My understanding is that the estimate of the long term stable growth rate that the AER has used is the prevailing yield on 10-year government bonds, which is one of the most volatile economic variables you can imagine.

So let's just think about this for a moment. We're saying that a good estimate of the long-term stable growth rate for the economy is the prevailing yield on 10-year government bonds?

I don't think that's a sensible input to the dividend growth model.

I did check with some good macroeconomists, and they sent me an email with about a page of algebra that explained that there is some macroeconomic theory that suggests that the long-term risk free rate could be used as an estimate or a proxy for the long-term growth rate for the economy. But that's a long-term stable risk free rate, so something like 5 per cent that doesn't really change much over time. It's not the prevailing government bond yield.

So I think that exception to the rule that the AER has point to in the omnibus paper may be driven by an assumption or an input that's not appropriate. And to be fair to the AER, the omnibus paper did caveat that particular assumption quite carefully and say, "We don't think that this is necessarily a good estimate of the long-term growth rate." But I just wanted to put that particular model in some context.

MS BRAKEY: Jonathan, do you have a very short point on that? Because I'm trying to get to Eric. But did you want to address something related to what Dinesh just said?

MR MIRRLEES-BLACK: I just wanted to say that I agree with Dinesh in terms of long term growth. Real interest rates have structurally declined and now

expectations are that long term real interest rates will be substantially lower than real growth. I leave it there.

MS BRAKEY: Thank you. Eric?

MR GROOM: Thanks, Anna. I think one of the key issues or questions for me is whether there are structural changes happening rather than cyclical changes, because I think we have got to take a longer term view as to what's happening or what may be happening in terms of relationships between market risk premium and the risk free rate.

So I'm interested in understanding more about the extent to which there is evidence for structural changes, and if so, what they are and what may be the implications for that. That's a larger question. A more immediate question, if I could, what we're trying to do is not forecast future returns but come up with an estimate, a long-term expectations, I guess, for returns to investors.

And I guess the question was the testing, if you like, of different models seems to slip into whether they have good predictive power in terms of predicting future returns. What are the assumptions implicit in using a test of predictive power to test whether our expectation of estimations are reasonable or not?

MS BRAKEY: Eric, who would like first crack at that? Glenn?

DR BOYLE: Almost hopeless, Eric. Subsequently realised excess returns are - in the very long run, right, all these things, all the shocks, unanticipated shocks, will average out and they will converge on the unconditional mean. But in the short run, virtually all changes in returns are driven by things that weren't anticipated at the beginning of the period. So whether they are high or low reflects almost entirely those unanticipated shocks rather than whether expected returns at the beginning of the period were high or low. So no, you're not going to get very far that way.

MS BRAKEY: It seems as though there's general agreement with Glenn on that.

MR GROOM: I think that gets back to the point that we can say a model may have poor predictive power, but does that mean it's not reflective of current expectations? That's a challenge for us, I guess.

DR BOYLE: No, it doesn't mean that. But the problem is that we have no way of telling whether the model actually generates a good estimate of the ex-ante market risk premium. We can't even observe that ex-post. Right? So we can't get to five years later and say, "Oh, well. Now we can

see what the market risk premium was five years ago." Right?

Because if we could do that, then we could test all these models. But they are basically untestable because we can never observe the true market risk premium even with the benefit of hindsight. So the fact that a particular approach or model doesn't predict subsequently realised returns very well, has low predictive power, doesn't in and of itself mean that it's not a good model of the ex-ante market risk premium. It just means we can't tell.

MR GROOM: Yeah, which is a challenge for us.

DR BOYLE: Yes.

MR GROOM: If I could put one question? When we were discussing beta last week, Dinesh had a framework for considering the estimation of beta that talked about the choice, if you like, or the relevance of the saliency of the estimate and the stability of the estimate.

Can that framework be brought forward there to help guide us in this discussion today about - or should it be brought forward to help guide us in the discussion about alternative models for informing our estimate of expectations for the market risk premium? And this goes back to my question about structural changes through time.

MS BRAKEY: Dinesh, I might go to you there.

MR KUMARESWARAN: Yes. I mean, I guess what was underlying that framework was that you have different ways of estimating the same thing. And all of these different things are giving you some slightly different information. And so in a world of uncertainty, the best you can do is make use of all of the information available to you. That's essentially the point that I was trying to major and I think that's the point that Martin is making in his suggestion that we apply some weight to all of the available evidence.

Now, one way you could think about the task of estimating the market risk premium is as sort of a Bayesian approach. So you could start with a prior, and your prior being, "What is the long term market risk premium?" That is, what is the market premium over a very long period of time, or averaged over a long period of time? That's essentially the historical excess returns, the average of long term historical excess returns.

But then you have to ask yourself, "Well, what's the additional information that I have about the prevailing market risk premium?" And so in order to improve your overall estimate of the prevailing market risk premium, you'd start with that prior and then give some weight to some other evidence. So I think some other evidence that you might give weight to would be dividend

growth model estimates.

The AER says in its paper, the omnibus paper, that the dividend growth model has strong theoretical foundations. So it's not that it's theoretically flawed; the main objection seems to be concerns about the implementation of the model. So give the model, or the estimates derived using that model, appropriate weight to reflect your concerns about the limitations of that model. That's how I would think about it.

MS BRAKEY: Thanks, Dinesh. I might move on to Graham, and then I propose to have a short two- or three-minute break and we'll move on to the second part of the question. So, Graham?

PROFESSOR PARTINGTON: Just on the observation that it's always better to use more information, more information is not necessarily better if the bad information swamps the good. That's my first point.

And the second point is it is generally assumed that more information reduces uncertainty and often it does, but that also is not necessarily the case. More information can actually increase your uncertainty. So it doesn't follow at all that if there is more information, you should use it.

MS BRAKEY: Thank you very much. So what we might do is we might break till 10.50. So that's four

minutes time, just to give everybody a chance to get a drink or have a bathroom break or whatever. And then we will then restart with the second set of questions and Jonathan and Graham presenting on those. So we'll see you at 10.50.

SHORT BREAK

MS BRAKEY: Welcome back, everybody. We will now move onto the second set of questions on the market risk premium dealing with the long run estimate of historical returns and whether that's the best estimate of MRP, can the AER derive a better estimate by incorporating other information, and if so how, and what challenges would the AER need to overcome in giving weight to any other information? So we will kick off with Jonathan.

MR MIRRLEES-BLACK: Thank you very much, Anna. In the second part of the slides that have been circulated, we can start on page 15 where I have summarised - these are the AER's questions that you've asked us to address in this segment of this session. I won't repeat them. You can read those.

Moving to 16, I have questions around, well, is the long run estimate the best estimate of the MRP? And I put five questions. So firstly, does it measure the variable that we want to measure? This goes to Eric's point just at the end of -

before the break that it's measuring historic returns rather than expectations. So it doesn't directly measure expectations, although it might provide insights into what people might expect and so we need to factor that in.

Secondly, has there been a structural change? So therefore, will the future be like the past? And we say, well, there have been significant structural changes and obviously if we're are going to use it, we need to reflect that into future expectations.

The third questions is, is the AER providing through-the-cycle returns or is it taking account of some of the cyclical moves? Given what we've observed, which is that there can be significant changes which are quite long-lasting, even in cyclical returns, that's important to reflect in what MRP has said.

The fourth thing is, is the historic MRP the right metric to measure or should we be measuring total market returns? Is that a better measure of a structural economic variable? And I think that there's evidence that the real total market return is at least as good and weight should be placed potentially on both of those.

And then the fifth thing - I think this is important. Do investors exclusively use historic MRP to determine their own expectation of the

returns? I think the answer to that is no, rather straightforwardly.

So then the question is, are equity return market returns predictable over the medium term? It's not quite the same as expectations, which I think is what we're getting at. But I think some of this is relevant and we can say, "Well, there is evidence forecast that there are variables which are used which can provide some guidance around future expectations of return."

Shiller's work, which Graham's referred to, is well-known for his cyclically adjusted PE ratio. And that provides some guidance and predictive elements in terms of future returns. Yes, there are elements over out of long returns, but here we return back to Eric's point about what investors are expecting. And investors looking at capital market projections over the next 10 years or so, they do look at what market trends are.

And in general, if we look at the right-hand side, there is a relationship between valuations of markets and expectations of future returns. Where markets are cheap, they are more likely to go up further than when they are expensive. And even if the out-of-sample models are incorrect, that certainly guides investor expectations of what future returns will be, and I think that is

very relevant for the decisions that the AER is taking in terms of forward expectations of market returns.

So, page 18, can we derive better estimates, and if so, how? Dividend growth models - I won't go through the details - is one element. Capital market assumptions models is another. Not typically used in regulatory determinations. I'm not aware of that. The AER might look at some of the input data as conditional variables, but this is what real investors and portfolio advisers are using to construct estimates of forward-looking 10-year returns. They look at yield, they look at earnings growth and they look at valuation changes. Widely used in portfolio construction and widely used to frame investor expectations.

I think there is evidence around that, and you can find surveys of what those capital market assumption models are, rather than some of the direct surveys that AER has used. So there is evidence out there. How it is used is another matter, but there are method of getting to market expectations.

Moving to 19, we have spent a lot of time already today talking around dividend growth models and do they provide reliable estimates of equity return expectations. Caveats around models are well-known. Concerns of the AER are

set out in the omnibus paper or in the 2018 determination explanatory paper. There are a few objections here, which I've looked at. There is a perpetual model, but what I would say is that there are concerns around the estimates made for the dividend growth model. I think there are equally concerns about the historic equity return model which are hidden. There are assumptions which are made which are implicit in the historic equity return model. For example, there is a concern DGM produces imprecise estimates.

The historic equity return approach makes the assumption that the future will be like the past, but in those long term averages it gives a false precision of returns over the medium term. And I think that there is uncertainty over the long terms growth assumptions, which produces uncertainty over the dividend growth model, but the historic equity return approach, you're fixing the assumptions around what the past economic growth rate was, equity investment growth was, dividend structures. Expected returns should be conditional on some of the variables which are used to determine your expectations in the dividend growth model, and you can't vary those in the historic excess return approach.

Page 20, so conclusions on all of this,

historic excess returns are widely used as an estimate and one reason for this is that it appears to provide some certainty. But there is good information in the other measures and they do measure the variable of concern directly. Of course there is uncertainty and of course the AER needs to demonstrate a process that it's making good use of evidence and sensible judgement. But with this uncertainty, yes, in terms of the interests of consumers isn't it best that it makes use of this broader evidence and gets a decision that's roughly right rather than the spurious precision that can be there in the historic excess returns approach.

And I think it's important to state now it's a particularly important moment. Bond yields have probably reached a bottom and are beginning to rise, central banks are changing monetary policy, we've got bigger inflation risk. And so therefore, in that context I think it's really important that the AER should be able to take account of the full range of evidence, which includes dividend growth models and some of the other conditional variables which are used to inform capital markets assumptions. Thank you.

MS BRAKEY: Thanks, Jonathan. I think we're going to Graham now.

PROFESSOR PARTINGTON: Yeah, okay. Well, I did

circulate some slides. So let's start with the first one. The historic market return estimator is the default method, and it does have the advantage that at least the data is observable, the method is relatively objective and it's quite transparent. It's also clear that that's the method that has influenced the 6 per cent that practitioners in Australia commonly use.

So it has got some things going for it, but it's not all good. As Jonathan just said, we're assuming that history repeats itself or, as Glenn might put it, that we've either got a constant market risk premium or the market risk premium mean reverts. We also know that it's an imprecise measure over short horizons; you need lots of history in order to get a relatively low standard error.

And that poses a problem because what we are now doing, we are measuring the historic market risk premium over many different tax systems. For example, there was a time when Australia had no corporate tax. There was a time when it had not an imputation system, but a system that was very like an imputation system in its effects.

We are covering lots of crises, so I actually think - and I think Glenn agrees with this - covering the crisis is a good thing. You shouldn't take them out, because that is part of

the investor experience as it was and part of the investor experience as it is going to be in the future. Indeed there is literature which says if you want to test asset pricing models properly, you need to make sure you've got a good cross-section of crises in there.

And furthermore, the crises can make a big contribution to explaining levels of risk aversion and the magnitude in the MRP. We have also got structural differences. Obviously, the exchanges that existed 120 years ago - there will be one in Melbourne, there will be one in Sydney, there might have been one in Brisbane or Perth, there would have been a relatively small number of stocks, they would be heavily weighted to agriculture and mining - would look quite different to the exchange today. And there are some questions of data reliability at long horizons, the mystery of history, if you like.

In the Australian context, there's been some debate about dividend adjustments. And there's an interesting paper out of the RBA by Thomas Mathews (RDP 2019-04 "A History of Australian Equities") which suggests actually, if you construct your dividend series properly, the market risk premium in Australia, the historic risk premium, comes in at about 4 per cent.

A big problem with the historic average is

by definition you cannot capture changes that have occurred within the average. In other words, you can't pick up the ups and downs because you are averaging them out.

I tend to agree with Martin that it's likely that the average from history is going to overstate rather than understate the required rate of return. The integration argument, the argument that we are wealthier and therefore more likely to be less risk averse, the argument that it's easier to diversify so risk aversion is perhaps less of an issue, Siegel's argument that we had artificially depressed bond yields because of underestimation of inflation, survival analysis, and so on and so forth. There's lots of reasons that you can find in the literature why history might give you an overstatement of the market risk premium.

So it's not perfect, but in my book it's still the best option because I don't know of any way that you can reliably track changes in the MRP. That brings us on to slide 2, which is looking at one of the alternatives that you might use, which is the dividend growth model. So we get the implied cost of equity out of the dividend growth model.

Now, I usually give the dividend growth model a good kicking, but it's not all bad. It's

a well-developed theoretical model. And it has some current use. It gets some use in practice in estimating the market risk premium, and you can't entirely discount that. And applications of the DGM can be transparent. It may be a little bit contingent on which model you use and how you do it.

However, there is a big negative with the DGM and that is that it gives estimates that have substantial upwards bias. The first problem is what I call incurable optimism. When you look at DGM models, they almost invariably seem to be cases where we are trending down from an above average growth rate to get to the long term average. It's very rarely the other way round. So clearly you can't always be coming down from a high growth rate to the long term average. So that's the incurable optimism problem.

A practical problem is that it's well-known that analyst's forecasts of earnings and dividends are upward-biased. They are also sluggish to adjust. So, you know, the market changes, it takes some time for that to turn up in the analysts' forecast, and when it does turn up in the analysts' forecast, the earnings estimate or dividend estimate that you're going to get is too high. If the earnings or dividends forecast is too high, and if that forecast is too

high, you need a bigger discount rate to equate the current price to the higher cash flow.

The measurement of dividends often appears to be wrong. The cash flow to investors is given by the net dividend. That is, the dividend you receive less the dividend you don't receive, because you have participated in a dividend reinvestment plan so the cash never leaves the company, less of the capital that you've contributed to the firm, plus any share repurchases that take place.

Now, you might think, "Well, you know, are these just minor adjustments?" No, they are not. They are large, relative to the dividend. In fact, when I just recently looked at some data I was very surprised to find that recently on the ASX the capital raisings would have almost matched the magnitude of the total dividends. So if you don't use net dividends, you've got very substantial mismeasurement - overestimation, in fact - of your actual cash flows. So bias is a big problem.

There's another problem, and that is that you can get quite widely varying estimates. You regularly see this. It's not at all unusual to see dividend growth models which will give you a range of MRP estimates of 6 per cent. Is that useful to the regulator that, you know, you've

got a choice of numbers which vary by 6 per cent or sometimes more, maybe 8 per cent?

And then you've got the problem with sticky dividends. I have already described this problem. The point is that this problem is worst when you most want to detect changes in the market risk premium. That is, when the market is changing rapidly. I've pointed out how that can induce a negative correlation between the market risk premium implied by the dividend growth model and interest rates.

Jonathan defended against that by saying, "Well, I also use the earnings yield." We shouldn't get into a tit-for-tat debate here, but I will just point out that the earnings rate is a poor proxy for rates of return because it's a confounded by the payout ratio and the growth rate. And what happens to either historic or prospective earnings yields when markets fall? They go up. Historic earnings yield go up, obviously, because prices have fallen. The historic earnings don't change. Prospective earnings yields increase because prices have fallen, and because of the upward bias in analyst's forecasts, you haven't sufficiently reduced the earnings numbers so your market risk premium inflates in a similar way to the inflation if you use the dividend growth model.

On to the next slide. I'm still not walking alone here. I picked this particular quote because it's in a submission by networks and pipelines to the AER. Basically, this is commenting on the Bloomberg DGM, and the point they make is, "Look, it's too volatile to be of any use." And they more generally make the point in their paper that the dividend growth model is much more volatile than the historic market risk premium.

Now let's move on to the inverse relationship between the interest rate and the MRP, which is often called "the Wright approach" in Australia, which is the assumption that the return on the market is a constant. Right? Well, if we take that literally, there has to be a one-for-one offsetting movement in interest rates and the MRP. Interest rates go up 1 per cent, MRP goes down 1 per cent and vice versa.

Now, I find this fundamentally implausible, because if that was the case, you would find prices would respond very little to changes in interest rates because there should be no change in the discount rate. You'd also have to ask, are central banks wasting their time? Right? Good news for investors; interest rates have gone down. Bad news for the central bank; they've

inflated their risk premium. So the discount rate doesn't change, so changing interest rates doesn't change the incentive to invest.

So I think the one-for-one argument's fundamentally implausible. I think we are all in general agreement on that. And indeed, even Wright says that it's not one-for-one. Well, what is it? I've looked at the evidence on and off over the years. You can find an inverse relation, you can find a positive relation, you can find no relation, you can find regime shifts. There is no consistent reliable evidence for the direction of the relationship. And if it's not one-for-one, you also need the magnitude, right? If we can't get the direction, what chance have we got of getting the magnitude? You are wasting your time. And I won't waste any more time. Thank you.

MS BRAKEY: Did any of the experts want to add anything before I go to the board members for questions? Martin?

DR LALLY: Were we going to go around each of us?

MS BRAKEY: Not for this one. I think it was just Jonathan and Graham as the leads. But kick off with a comment.

DR LALLY: Okay. I agree with everything Graham has said about the dividend growth model. It reminds me of Churchill's famous comment about democracy,

that it was the worst form of government ever invented apart from all the others that have been tried. So just because a method's awful doesn't mean you throw it away. You have to look at the alternatives.

And the alternative that the AER seems to most strongly favour and has for a long time, the historical averaging methodology, I agree with Jonathan's point that there's a false precision to it. You get this number, and more or less any analyst who would go through the exercise would come up with much the same number, and they don't change much over time. So that must give enormous confidence to regulators that this is something they can hang their hat on.

But it is useful to look at the confidence interval on the estimate. The AER uses numbers from 1988. They favour most strongly numbers from 1988 to 2020. So that's 40-odd years. The 95 per cent confidence interval on your estimate of around about 6 per cent runs from 0 to 12 per cent. Now, that's just huge. That range dwarfs any sort of variation that the DGM is accused of.

And the AER may get some comfort from the fact that if you go back to 1988, you get 6.3 per cent. If you go back even longer, you get more or less the same result. Well, that's

just pure luck. Absolutely pure luck. Just to illustrate what actually is going on, I noted some numbers in my notes. If you go to the 15 West-European equity markets they would, we would expect, have pretty similar MRPs. And likely Australia would be somewhere around the same ballpark. But the historical average numbers going back to 1900 - so that's 120 years of data, not just 40 years of data - from Dimson, Marsh and Staunton, the results range from 3 per cent for Spain to 9.7 per cent for Austria.

It's just not plausible that Austria has an MRP three times Spain. So what that data is showing you is that most likely Spain was way too low and most likely Austria's number of 9.7 was way too high. So once you look at that kind of data, then the historical averaging technology with its 6 per cent number that doesn't change much from year to year, that's giving you a degree of comfort that just is not warranted.

So I would say to you the DGM is a terrible model; everything Graham says is right. But you are getting a false sense of security, as Jonathan says from the historical averaging model. And so what I would say to you is rather than ask yourself the question, "What's the best method?", we don't have to pick one. If we were picking one, we'd have to face this awful

conundrum. I would say to you, "All methods are imperfect. So choose a set of methods that you think are, for all their imperfections, worth putting weight on, and then equally weight those methods." And the set of methods that I would recommend is not only historical averaging and the dividend growth model, but this Wright estimator. And I would also strongly urge the AER to look at the results from foreign markets.

Otherwise, you know, if the regulator in Spain and Austria did exactly what the AER is doing, basically using historical averages, the regulator in Spain would be coming up with an MRP of 3 and the regulator in Austria would be coming up with 9.7. And that variation cannot possibly be reflective of the underlying differences in those markets.

Now, I should add that it's very fortunate that in that Dimson, Marsh and Staunton data Australia comes out at about in the middle. So whether you put some weight on the foreign data or not doesn't make much difference. But the general point is that for many regulators following the methodology that the AER would, the regulator in Spain or Austria, the results would just be preposterously low or preposterously high.

MS BRAKEY: Thanks, Martin. Dinesh?

MR KUMARESWARAN: I agree with most of what Martin has just said. I want to pick up on a couple of points that Graham made. Firstly, Graham has quite rightly pointed to some concerns and limitations with the dividend growth model. I think that in those circumstances, the appropriate course of action for the AER would be to see if we can improve the model to address those concerns. And as an example, the calibrated DGM proposed by the ENA is an honest attempt to try and address the two main concerns that the AER expressed in 2018 about the dividend growth model. And those are the same concerns that Graham outlined.

So some concern that the dividend growth model produced upwardly biased estimates, and secondly that there's uncertainty about how to choose the long-term growth rate in those models. There may be some other concerns as well, but I think the appropriate course of action would be to just be really clear about what those concerns are and then see if we can address those concerns. I don't think the AER should throw the baby out with the bath water, which is what Graham is suggesting, and just completely throw the DGM out.

Graham said that he thought it was implausible that the Wright method, which

required constant required return on equity and a one-for-one relationship between the market risk premium and the risk free rate, was implausible. I agree that that is an implausible assumption, but it is as implausible as assuming that market risk premium is fixed, which is currently what the AER is doing.

And then finally the historical excess returns approach, which is what Graham appears to favour, what does that actually mean? What does that estimate actually mean? It is an estimate of the risk premium that an investor can expect over the long run, over all sorts of market conditions. So pandemics, financial crises, booms, busts, dot com bubbles, everything. But the AER's task is to determine what the market cost of capital is over a future period, the prevailing market cost of capital. So I think that's why I agree with Martin that exclusive reliance on the historical excess returns estimate gives you a false sense of precision, because it's not really, except in very exceptional circumstances, going to give you a good estimate of the prevailing market risk premium.

MS BRAKEY: Thanks, Dinesh. Glenn?

DR BOYLE: I think it's important here to keep in mind the distinction between the conditional and

unconditional MRPs. We seem to be mixing them up a bit at times. In terms of the question that was asked here, you know, is the long run estimate of historical excess returns the best estimate of the MRP? Well, which MRP? If it's the unconditional MRP then if the excess returns distribution is stationary and ergodic, which can be tested, then the law of large numbers basically tells us that the sample average over a long time series converges to the unconditional mean, in this case the unconditional risk premium.

So there's not an assumption here about the future looking like the past, it's simply do the underlying excess return distributions, do they have the right statistical properties? If they do, then it follows that the best estimate you can get of the unconditional premium is the historical average.

Now, Martin points out quite rightly that there's a lot of noise associated with such an estimate and indeed over 30 years the confidence interval is from 0 to 12 per cent. Well, my response to that is that 30 years is far too short. The law of large numbers doesn't kick in over 30 years, and there's no corresponding small sample property that says, "If the distribution is such and such then you'll get a good estimate

of the current market risk premium and the conditional market risk premium using five years or 10 years or 30 years of data." There is no property of that kind. That really is just akin to taking a pick and a poke.

The only way historical averaging can be used is over a long time series. And in order to justify that, you need to test that series for stationarity and ergodicity. If it passes those tests, then that will give you a good estimate - or the best estimate we can get anyway - of the unconditional risk premium.

Now, of course you might say, "Well, that's not really what we want. We want the conditional risk premium, the one that reflects current conditions." That's certainly true, but I can still think of relationships why you might prefer to stick with the unconditional premium, and one is the obvious one that trying to go beyond that and estimate the conditional risk premium, as for the reasons that were outlined in the previous set of questions, is that it's not worth the candle. We don't know how to estimate the conditional risk premium with any precision whatsoever, and so trying to do so could just introduce more noise, or more particularly error, into the process than sticking with what will work in the long run.

What else has come up? One point I guess I'd make is Martin said this confidence interval of 0 to 12 per cent is really wide, and far wider than anything you would get with, for example, the dividend growth model. Now, to me that's a slight confusion of confidence intervals associated with a single point estimate and the volatility of point estimates. With the dividend growth model you get a whole lot of different point estimates. That's the whole point of it; you get a different point estimate at each point in time.

But with the dividend growth model, that says the expected return or the market risk premium is a linear function of the dividend price ratio, where the slope is a bit more than one. How much more depends on what you choose the long-term growth rate to be. So that means that your dividend growth rate estimates are going to be proportional, or more than proportional, to whatever the dividend price ratio turns out to be.

Now, dividend price ratio, we know, varies a lot. I haven't got the figures in front of me, but I think in Australian data it's at least from 2 per cent to 7 per cent. So that's five percentage points right there. And so, it will vary more than that. And then associated with

each of those will be a confidence interval. We don't know what it is, because we don't know what the standard error is of our DGM estimates.

But what we can say is that the point estimates vary from 2 to 7, or proportionally from 2 to 7. The confidence intervals around those will vary by an awful lot more indeed. Even 0 to 12 looks quite good compared with that.

And the final thing I would say is that I'm not completely averse at all - don't get me wrong - to the use of the dividend growth model or survey methods or conditioning variables models. I think in the absence of anything better, they have probably got a role to play. They've got a role to play in estimating the conditional market risk premium.

But because we can't observe, as I said before, even ex-post, the true market risk premium, we've got no way of knowing how well these methods work. No way at all. And so, unlike with the historical averaging where we can appeal to the law of large numbers, with these numbers there's no corresponding thing we can hang our hat on.

And so, essentially using those models basically involves justification by faith alone. Now, if you are a Calvinist, that's probably fine. A regulator, probably not. So if you are

going to use these models, then here I agree with Martin. You should mix them up; you should use all of them and not rely on one of them.

MS BRAKEY: Thanks, Glenn. Everyone's making this sound like an impossible task, but we need to find a way through. Dinesh?

MR KUMARESWARAN: Just very quickly two points. The first is that Glenn says that there's no way of testing the reliability of the estimates of the conditional MRP derived using the dividend growth model. I agree with that. That's certainly true.

But exactly the same thing can be said about the capital asset pricing model. So we can never really know what true expected returns investors really require, but the AER chooses to use the capital asset pricing model. And it seems to me that the reason that the AER is using the CAPM is precisely the reasons that the dividend growth model would be useful to use. There are strong theoretical foundations to both of these frameworks. So that's the first point.

The second point is both Graham and Glenn have talked about the volatility of the estimates produced by the dividend growth model. Yes, the DGM estimates certainly can be volatile, but that's because they are reflecting prevailing market data.

We talked earlier about volatility in the market being an indicator of the risk premium. If you take plots of the volatility of the market, so the volatility index, and plot that against estimates of the dividend growth model, you get a much better relationship between DGM estimates and the volatility index than you do if you use just the historical returns.

So I'm not saying that you can get a precise estimate of the true market risk premium by using the dividend growth model, but I'm saying that the volatility that you see in the estimates isn't necessarily a bad thing. It may be actually be exactly what you want in a good estimator.

MS BRAKEY: Dinesh, when you were talking I just wondered whether you wanted to elaborate a little bit further on why you would use a CAPM and a dividend growth model, and what the kind of theoretical underpinnings of using both would be?

MR KUMARESWARAN: I was just drawing a parallel between the dividend growth model and the capital asset pricing model. The AER has explained that there are strong theoretical foundations for the capital asset pricing model. It's widely used, but we also know that there are problems with the capital asset pricing model.

There is empirical evidence that CAPM tends

to underestimate returns for low beta stocks and over estimate returns for high beta stocks. So there are some empirical problems identified with the capital asset pricing model. But the AER, notwithstanding those concerns, still uses the capital asset pricing model and I think that's appropriate, because the CAPM is a good theoretical framework. All I was saying was that the dividend growth model is exactly in the same boat, that there are strong theoretical underpinnings to the dividend growth model. Yes, there are implementation problems with it, but those implementation problems are not so catastrophic that we should just completely throw the DGM out.

MS BRAKEY: Okay, thank you. I might go to Glenn, and then I wonder whether Toby might come in with some views after Glenn.

DR BOYLE: Just very quickly on what was Dinesh's second point about the volatility of estimates produced by the DGM, I absolutely agree. If they really are picking up rational-based risk pricing movements, that's a good estimate. But of course we don't really know whether that's true or not, which is my point. And clearly the fact that they track observed movements in risk better than the historical average does, well that's true by definition because the historical average doesn't

try to do that.

I suppose my concern is more a practical one, is that as a regulator, would a regulator want highly volatile allowed rates of return which the highly volatile market risk premium would contribute to? Because that way you end up with high energy prices during one period, low the following, back to high the one after that and so on.

Now, networks won't like that; we know uncertainty decreases investment. And I'm pretty sure consumers wouldn't like it either because it makes it difficult to budget. So I guess I'm not really disagreeing with you at all, Dinesh. Subject or conditional on the assumption that the DGM is a good model, my complaint there is more, well, even if it is, is this the sort of thing a regulator would want to promote? I guess that's probably the best way I can think of putting it.

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

MR KUMARESWARAN: Yes, just on the point about should the regulator promote volatility in prices. I think what you'll find is that if you applied the dividend growth model, even if you gave some weight to the dividend growth model estimates, you'll actually get more stable prices because what you would find is - the suggestion is not to

use the dividend growth model to estimate the required return on equity for the businesses directly, it's to estimate the market risk premium. And if, as the data suggests, there's a negative relationship, what you'll find is that as the risk free rate goes down, the market risk premium will go up a bit, and as the risk free rate goes up, the market risk premium will go down a bit. And so, these two things will offset each other to some extent and the overall allowed return on equity will be more stable than if you gave 100 per cent weight to the historical excess returns alone.

So going back to the chart that I presented before, you can see how volatile the return on equity estimates were if you just add a fixed premium to the prevailing government bond yield. You won't get that sort of volatility in the allowed return on equity if you use something like the DGM.

MS BRAKEY: I might just quickly go to Graham. Toby, I am coming to you because I think you might have a proposal, but Graham, did you want to go?

PROFESSOR PARTINGTON: Yes. Okay, if we say, "Okay, there is possibly some information in the DGM so we should use it," which DGM should we use? Because there are lots of them. And which assumptions so we use. Right?

It's not that there's just a single number. There's not just a DGM which will give you a single number. There are lots of different DGMs and you can put lots of different assumptions into those models, and you will get very different results. So maybe the proposal should be if you use the DGM that you use the whole range of DGMs with a whole range of input assumptions?

MS BRAKEY: I think that's the IPART approach. But --

PROFESSOR PARTINGTON: And I don't mean a few; I mean lots.

MS BRAKEY: Right, okay. I think IPART's using six, maybe? Something like that? Five? Five. Toby?

DR BROWN: Thanks, Anna. I'm pretty convinced that we've got different models here and we should use them. I don't think it's a case of picking the right model or the best model and then and then forgetting about everything else.

I did just want to say something about what it means to use different models. We've heard that various people have said that we should "give weight to", and I think that's right. I think there should be an explicit commitment to a non-zero weight. There's some kind of bringing together of the results of different models, and that involves not putting a zero weight on any of

them. I'm not sure that it's sensible to pre-commit to exactly equal weighting on everything that you're going to look at, but none of the models should have a zero weight.

And sort of related to that, I do think it's a little bit different if we are kind of where we are right now in the rate of return instrument process and we are doing the best that we can to draw together these different models and all the different information, and the AER is going to use its best judgement to sort of bring all that information together.

That is a little bit different than what happens over the next four years as time passes, I think. And then if you were to implement some kind of updating of the MRP then of course you would have to specify exactly upfront what kind of models you were using, exactly how to parameterise them, where you were going to get the inputs from and what weights you were going to be using.

And that, I think, is pretty challenging, because it's hard to be sure that that's going to give you sensible results for the whole of the next four years. So I think that's the second best outcome.

Equally, given everything that we've been discussing about the way that the MRP and the

risk free rates are not independent of each other, I also don't think it's correct to update the risk free rates and assume that the MRP is fixed and that therefore the cost of equity moves one-for-one with the risk free rate.

MS BRAKEY: Thanks, Toby. Graham?

PROFESSOR PARTINGTON: Just a couple of points. One is a suggestion, and this is the suggestion about weighting. Picking the weightings is a fertile ground for debate and submissions to the AER, right? And often my observation of regulation around the world, what happens is it's all too hard and we degenerate to equal weights for everything, which I think is obviously suboptimal.

This is a suggestion; I don't know if it will work. It's not my suggestion, it's from my colleague Steve Satchell. And that is, maybe in thinking about the weighting it might be useful to look at fuzzy set theory. Okay? And that fuzzy set theory is about giving things weightings that do provide information, but it's not really at all clear where the boundary of the set lies.

So in the current context, we might well have something that we could say, "Okay," you know, take Glenn's point, "Here we've got something that is the unconditional estimator and

here we have other things which might give you an unconditional estimate, but it's kind of not clear. It's a bit fuzzy."

Normally, we would exclude those from the set because they are fuzzy. But with fuzzy set theory you include things in the set but you weight them differently. There is a whole mathematics behind this. Whether it would actually be any use, I don't know. But it's something that could be investigated. I had another point on Toby's comments but I've completely forgotten what it is now.

MS BRAKEY: Put your hand back up if you remember it. Jonathan?

MR MIRRLEES-BLACK: Thank you. I think that there's general acceptance that there are problems with the models, and it does seem that we can characterise Graham's position that we should just junk all this data. And I think that what I was saying in my presentation, which aligns with what Dinesh was saying, is we can with these alternative models improve the quality of the estimates made by overcoming the concerns that the AER has and by explicitly making assumptions. So I think that that's really important, that we don't throw out all that information.

But I think there's also a really important point, and it relates to what Graham was saying

on fuzzy sets. We know that there is uncertainty with each of these estimations. We know that, and we know that none of them is perfect. The result cannot be mechanical. So an estimate of what the market risk premium is, it cannot be mechanical.

And in the end, the AER has got to make its judgement in terms of the duty of the long-term interests of consumers. And given that it can't be mechanical, I don't think we can say there are fixed weights. I don't think we can say that there's an automatic formula in terms of weighting. We have to allow that the AER can form a judgement as to which of those estimates is the most appropriate at the time. Now, that might be hard, but that's because it is hard. But that's the regulator's job, to make those really tough judgements. And it might be wrong, but it's making the --

MS BRAKEY: But Jonathan, doesn't the legal framework kind of make that even more difficult, given in effect the AER can't apply discretion at the time of making a particular decision; it has to kind of apply the rate of return instrument without judgement, if you like?

MR MIRRLEES-BLACK: Yes, so we have to have a formula. You're absolutely right. We're now just saying, "at a point in time." We're saying,

"set a framework that applies for four years hence and have a mechanical approach to it." But in terms of what that judgement is on that day, what weights should be applied on that time when you are setting the forward weight for the next four years, which might then apply to network decisions for a long period hence, it's still judgement. It's not mechanical. And I think that that's - it's an expectation that judgement can be applied, I think, is important.

MS BRAKEY: Thanks, Jonathan. Martin?

DR LALLY: Let me pose a hypothetical scenario to the AER. Supposing that the historical averaging methodology had produced, as it had with Spain in the last 120 years, a figure of 3 per cent, but all other methods that could reasonably be thought to be useful - the DGM surveys, the Wright method, looking at foreign data - they were all suggesting 8? I don't think it's at all likely that in that kind of scenario the AER would stick with 3, for all this other evidence was pointing elsewhere.

And of course the AER is not oblivious to this other evidence. The AER knows that survey results are actually pretty much in line with the historical average result of around about 6. It knows that results from DGM and the Wright method and foreign data are not wildly different. And

that must give the AER a lot of comfort that the historical averaging number of 6 is about right.

So it kind of looks like the AER is in fact using this other information. It's just not doing it explicitly. And if the AER is using this other information in the sense that it's getting a lot of comfort from this other stuff that 6 looks about right, I would say to the AER, if you are gaining a lot of comfort from the fact that these other methods on average are pretty much like the historical averaging result, why not formalise that in the way that's been suggested here? Why not formalise it rather than doing what you're currently doing?

MS BRAKEY: Thanks, Martin. Clare?

MS SAVAGE: Thanks, Anna. I think we've probably got a fairly good grip on people's views about the usefulness or otherwise of the DGM. So it would be useful from my perspective if we could move a step forward into some of the practical application questions. And that doesn't presuppose a view from me at all that we would use the DGM, but I'd like to just explore in a little bit more detail if we were to go down, say, an option 2, option 3 approach as set out in our information paper, some of the things we might have regard to.

And some of the experts have kind of spoken

to weightings or regulatory judgement, but I had some very specific questions that I just wanted to throw out there and my board colleagues may also have some. So I just wanted to put them on the table, and it might be quickest if I just put them on the table and then people can respond to them as they wish, and maybe my colleagues might want to add to them.

I think the information paper makes a bit of the fact that DGMs can give a negative estimate of the market risk premium. So I'm interested in expert views on that and whether they would agree with that and whether it's problematic. I'm interested in views about whether a stable market risk premium is more desirable than an unstable one with the potential for volatility.

Dinesh, I heard your comments about the fact that if it's varying with the risk free rate return then it would be more stable, but noting how would we actually vary it through the four years with the risk free rate. So I'm interested to understand if you think it should vary with the risk free rate, how could that be done formulaically to produce that level of stability.

I am very interested, Martin, particularly in your views about unexpected inflation and the views of other experts, and what implications that might have for the reliability of historical

excess returns. And I particularly call that out given where we are currently at in terms of the RBA's inflation expectations and where the central bank or where the US inflation is currently at.

I'm interested in thoughts on a starting point for the historical excess returns, and I note, I think, Martin, you were effectively saying, "Have we really chosen a time period that gives us an answer that sort of then looks like the other information?" So I'm interested in if there is a view about the time period.

I'm interested in any thoughts on the RBA's new series and any views other experts other than Dinesh might have on that. And not that I'm not interested in your view, Dinesh. You've already spoken about it, about the calibrated DGM that the ENA has proposed.

So they are the kind of things I'm interested in. I don't know whether my board colleagues might like to add to them, and then whether experts wish to sort of come ask and speak.

MS BRAKEY: Jim?

MR COX: I mean, there are a couple of issues I'd like to hear a bit more, and I think we've had a good go on DGM versus historical excess returns and which is the least worst model. But I'm

interested in Martin's idea, supported by other experts, that we should look at a range of information. I guess I would be interested in views on what is the boundary of information that we should consider? How reliable does the information have to be before it is worth considering, or is there another consideration? And I think a related point gets back to the sorts of things we were talking about last week, so reliability versus precision and so on. But I suppose a related point, he mentioned the role of surveys as useful information. We actually haven't talked much about surveys this morning, but it would be interesting to have your views on surveys and why you think there might be useful information in making this difficult decision. So they are a couple of extra things I'd like to hear about as well.

MS BRAKEY: Thanks, Clare and Jim. I think that's given us a very long list of questions to deal with. And I just wonder what the best way to go through it is?

MS SAVAGE: It might be worth seeing if Eric or Catriona or Justin had anything. And I'm happy for experts to pick the bits they're interested in. I don't need to hear comprehensively from everyone. But I just - they are the kinds of things that I've been thinking about.

MS BRAKEY: All right. In that case, we might make it a little free for all. Catriona?

MS LOWE: Thanks, Anna. I was also interested in the question around the stability point that Clare has raised, so I'm happy at this stage to hear what the responses are and if there's an even time to follow up.

MS BRAKEY: Thanks. And Eric or Justin? No? Okay.

MR GROOM: Sorry, Anna. I guess it really picks up on the question 6 that Clare put forward: I'd be interested in the approach that RBA and the Bank of England say they use in looking at dividend growth models, where they state all the problems with DGMs but essentially come to a view that they'll look at the changes in levels rather than the absolute values and look through short-term changes, which all sounds fine but how would you implement that in practice if one were to say that that's sensible advice?

MS BRAKEY: And Justin?

MR OLIVER: Look, probably not. Nothing more for me to add, thank you.

MS BRAKEY: Which of the experts would like to start somewhere on that laundry list? Martin?

DR LALLY: Quickly having taken a note of some of these things, the first point: The DGM estimate was negative. I presume that was back when interest rates were very high, sort of risk free

rate 5 per cent, the estimate of the expected return 14 per cent, so your MRP is minus one.

I would have to see the details of that to see whether in fact that's just sort of some error in the model or not, but if you are accessing information from half a dozen different models - and my view is you then take the median result, not the mean. The mean can be dragged all over the place by a very extreme result such as a negative number - if you get a freakish number from one of those six methods, it's not going to materially alter the median. So I don't think you have to worry about freakish results from one model if you've got a wide range of models.

The question about unexpected inflation, I believe that's a reference to the historical averaging method as a result of all this unexpected inflation in the 20th century having produced an overestimate. And Graham gave a very comprehensive list of things that might cause the historical averaging method to have overestimated true value. I concur with all of that from Graham, and it's just one more reason why you don't want to rely on just one method, because they're all imperfect.

What is the set of methods that should be used? In my view, it's historical averaging,

DGM, surveys, the Wright method and use of foreign data from all of them.

And finally, a question about stability. Unquestionably, the historical averaging methodology gives you stability. From regulatory point to regulatory point, it's not going to vary much from 6 per cent. But my experience in doing these kind of exercises for regulators where I come up with estimates for half a dozen different method and then take the median estimate, my experience is that median doesn't change a heck of a lot as well. So if regulators are concerned that by moving away exclusively from historical averaging they would be exposed to horrendous variations from regulatory reset to regulatory reset, I do not think the historical experience that I've had in doing this kind of thing would lead to that kind of fear.

MS BRAKEY: Thanks, Martin. Toby?

DR BROWN: Thanks. One thing I'd like to say is that the types of implementation I've seen of DGM models in North America, there are different ways of implementing the models and a model that seems to work well at a certain point in time doesn't always stand the test of time. Some regulators have tried to be very sort of prescriptive about the model specification, and that sort of worked for a time and then it becomes clear that it

doesn't work for one reason or another.

So I think it is difficult and dangerous, perhaps, to be very specific about the model you are using, particularly if you are using only one model. I quite like what Martin said about having several models and using a median approach, and that that's less likely to be pulled around.

That being said, what does that mean in practice for the AER and for the rate of return instrument? I think if you do your best and use the different models and weight them together and come up with a number for the MRP, and then that number is fixed for the next four years and therefore a determination that you do in sort of three and a half years' time is going to use today's MRP as good as you can get it, and then there's sort of this follow-up risk free rate in three and a half years' time, that's the worst that I think we could do.

An improvement would be to have a formulaic way of updating the MRP in three and a half years' time so that it's not inconsistent with whatever the spot risk free rate is in three and a half years' time, but the best way of dealing with all this is set the MRP now, set the risk free rate now and then apply that immediately to everybody. And then in four years' time or five

years' time, just repeat. Do the same thing again.

And I don't see any difficulty with, in effect, setting the rate of return parameters for everybody in the rate of return instrument process and on a completely different timetable setting all the other building blocks. I don't see any difficulty and I think it solves some of the problems that we're confronting now.

MS SAVAGE: So just to clarify that, Toby, the risk free rate does change as we make each determination because we take the rate at that time. So are suggesting that we would fix that?

DR BROWN: I'm suggesting, in essence, that you should take the rate of return out of its determination process so that each determination would deal with - well, it would deal with everything else. So OpEx, CapEx and so on. And the determination would sort of have a placeholder for whatever rate of return parameters come out of this process, out of the rate of return instrument.

And those parameters would feed through into the calculations of maximum allowed revenue and so on in an automatic way. And therefore, at the time of the determination there would be no need to touch any of the rate of return parameters - risk free rate, MRP, nothing - because that would

be dealt with separately in this process. And there would be a mismatch of the timetables, but I don't see any difficulty with that.

MS SAVAGE: So we would just be updating every four years instead of every five?

DR BROWN: Well, I don't think there's anything magic about four years in an economic or financial sense, or five years for that matter. But I think the key point is I don't see any difficulty with investigating OpEx and CapEx and the other building blocks sort of one-by-one as each network comes in for its determination, but doing the rate of return generically on a different timetable because I don't think there's any interaction between the rate of return building block and any of the other ones, or at least all of those interactions are just arithmetic that can be dealt with through the existing formulas.

MS BRAKEY: Thanks. Eric, did you want to ask something directly of Toby there before I go to Dinesh?

MR GROOM: Yeah, it's a clarification. If I could use some practical examples, Toby, I think what you mean is: We come up with the rate of return instrument for 2022. That applies to - at each reset we make from 2022 that would be applied. So would it apply about to a reset in 2023 and a

reset in 2024? That is, just as we enter into this new regime?

DR BROWN: I see. Yeah, there would be (indistinct).

MR GROOM: So they get a fixed rate of return on equity, which would be the sum of your fixed RFR and your fixed MRP. 2026, we come up with a new rate of return instrument with a new fixed approach and that would immediately apply to all standing determinations. That is, the determination made in 2025 would have a rate of return on equity that gets replaced by the 2026 return instrument. And then you'd walk forward where everyone would change their return on equity with the commencement of each return on greater return instrument - irrespective of when the actual determination was made for the reset?

DR BROWN: Yeah, I confess I haven't really thought too much about the sort of transition. But yes, what you said is exactly right.

MS BRAKEY: Thanks, Eric and Toby. Dinesh?

MR KUMARESWARAN: Thanks, Anna. Just to Toby's point about essentially the RoRI fixing the allowed return for the whole RoRI period and then it's just reset, and so every determination within the RoRI period just gets the same allowed return.

I'm not really sure what the objective of that is other than to avoid the problem of updating the estimates over the RoRI period. It

seems to me that under that approach you would be moving further away from the market cost of capital that would prevail over the RoRI period. At least at the moment you have the risk free rate updating within the period. So I'm not really sure what the sort of practical benefit of that particular suggestion is.

Just moving on to a few points that Clare and others raised. So this concern about the negative estimate of the market risk premium, I think Martin's correct that those circumstances would only occur if the prevailing government bond yield was very high. 15 per cent, I think Martin said. We haven't had that for a very, very long time. And it it's very unlikely over the next RoRI period to get a situation like that.

If you go to slide 4 of the pack that I presented, you've got those DGM estimates, all those charts. But none of those DGM estimates come close to being negative. So I think this is not really a material concern. I don't think that's a really serious problem.

Clare had a question about how the MRP estimate could be varied formulaically over the period. One way you could do that is at the start of the RoRI period you estimate your market risk premium and you use a number of methods to

do that. So historical excess returns, DGM, whatever. And implicit within each of those methods is a relationship between the risk free rate and the market risk premium. Then over the RoRI period you could just formulaically apply that same relationship for each determination. So the risk free rate would change and the market risk premium would change in a formulaic way consistent with the relationship implied by each of the methods that you've used.

Now, I got the sense from the omnibus paper that the AER is reluctant to make a determination about the relationship between the market risk premium and the risk free rate because it's very hard, as Jonathan said, to determine what that relationship is. We're all saying that.

And I think that's correct, but the AER is not avoiding making at least an implicit statement about that relationship. Whatever way you determine the market risk premium at the start of the RoRI period, you are implicitly making a statement about the relationship between the risk free rate and the market risk premium.

So if, for example, you put 100 per cent weight on the historical excess returns at the start of the RoRI period, implicitly you're saying that there's no relationship between the risk free rate and the market risk premium.

MS SAVAGE: I just want to make clear, are you suggesting that if we were to go down this path and we come up with a number and let's pretend we've look at all the bits of available evidence and we think it's 6, and then looking at each of those pieces of evidence we see a vaguely positive or negative correlation with the risk free rate, are you suggesting that we would at the beginning of the RoRI period say that it is - I'm just making up something up, right, but let's pretend that it's a 0.6 or something.

And then as the risk free rate rises, we would adjust the market risk premium by a negative relationship or 0.6 through the period? Or are you suggesting that at each determination there may be an agreed - and I'm not sure if this is even legal - but an agreed set of models that we use that we would update in the same way we update the risk free rate? I just wasn't quite sure what you were saying.

MR KUMARESWARAN: Well, my understanding of what the AER proposes to do is to set a fixed return on equity allowance for each determination for the duration of the five-year determination. But those determinations will occur at different points within the RoRI period. So if that's the approach that you intend to follow then the task is then to estimate at the start of each

determination within the RoRI period what the appropriate market risk premium is. Now, you will have an estimate of the market risk premium at the start of the RoRI period.

Now, let's suppose just for argument's sake that that estimate was formed by giving 50 per cent weight to historical excess returns and 50 per cent to a DGM estimate. And suppose that DGM implied that there was, for every 1 per cent increase in the risk free-rate there would be 0.5 per cent reduction in the market risk premium.

You observe that relationship or at least recognise that that's the relationship implied by the DGM, and then you would update the DGM estimate using that relationship for each the start of each determination within the RoRI period. Similarly, you would update the historical excess returns using exactly the same method that was used to establish the historical excess returns estimate at the start of the RoRI period.

And because you gave 50-50 weight to each of those two estimates, you would apply the same 50-50 weights to those updated estimates and there would be your new estimate of the market risk premium. So that's a very stylised, abstract way of conceptualising what I'm

suggesting, but that's essentially it. Does that answer your question?

To Martin's point about using a median of the estimates from different methods, I don't think that is a sound thing to do, because if you use a median, your final point estimate is determined essentially by one or at most two estimates, two pieces of evidence, and you essentially end up throwing out all of the other evidence. I think that if you are going to use multiple pieces of evidence, it would be better to use information properly from all of the estimates. So that would involve taking a mean, not a median.

And finally, the point about the stability of the estimates. I think the AER should consider what do we really want to be stable here? Is it individual parameters within the CAPM or is it the overall return on equity allowance? Returning to my earlier point, yes, the DGM will produce more volatile estimates of the market risk premium but when combined with a volatile estimate of the risk free rate, you will tend to get more stable estimates of the allowed return on equity. I think I've never really understood this issue about the concern about the volatility in the estimates of the market risk premium, because it seems on the one hand we want

extreme stability in one of the parameters that's used in the CAPM, the market risk premium, but we are very happy to have extreme volatility in another parameter, which is the risk free rate.

And I've never understood why those two things can coexist. I think that the overall objective should be to produce the best estimate of the market cost of capital, and I think we should use the best techniques to estimate each of the parameters in the CAPM that delivers that estimate.

MS BRAKEY: Thanks, Dinesh. Jonathan?

MR MIRRLEES-BLACK: Thank you. I won't address all of the points. We have had some discussion around and there was a suggestion from Toby, commented on by Dinesh: Should we be fixing a cost of equity across the instrument or should we be fixing the MRP across the instrument? This was actually - there was a proposal around this at the last expert session for the 2018 estimate. And it does feel like it's quite straightforward to say we could either have a zero weight on changes or fix the MRP, fix the cost of equity, or go somewhere in between formulaically.

I think at that point it's quite straightforward to have a weighting between the too. Even if we can't be sure of what the difference is between the two, we can still have

a weighting between fixing the cost of equity or the MRP.

So to your point, Clare, around unexpected inflation, I think it's important if we're doing that, that we fix the real cost of equity rather than fixing the nominal cost of equity in particular at the current moment where we're seeing increased inflation risks, whether or not they materialise. So a consideration of ensuring that discussions around the real cost of equity enter into this, I think, is important.

The second point I'll make is we have had a lot of this query about should we be using - if we are using a wide range of models, I think it is right to use a wide range of models. That allows the board to take into account all the appropriate information. But should we be using a median, should we be using a mean or should we be using something else? I think it's something else.

When I've been in these situations where I've been taking data from a range of models and then you have to update it and you've fixed yourself to either using a median or mean and you ask yourself the question, "What do I really think the number is in this circumstance?", it does depend on what those numbers are, it does depend on the strength of your belief around the

quality of the evidence around those particular individual estimates, what's driving those, what are the judgements that are taken into account in those.

And so, I'd say this it's not a fixed median, it's not a fixed mean. It's a question of a judgement about where the strength of the evidence lies. It might be hard to formalise that in a fuzzy set methodology that Graham's proposing, but I think that it's quite possible to make clear reasoning in words as to why the board has taken a particular judgement on those parameters and to where that evidence takes. And I do favour, as Martin was saying, being clear about how implicit information is being used.

MS BRAKEY: Thanks, Jonathan. Do any of the other experts want to answer any of the questions? Otherwise I might come back and start addressing a couple of those questions a bit more specifically. Dinesh?

MR KUMARESWARAN: I just wanted to pick up on one thing that Jonathan has said. The AER's current framework does in fact fix, effectively, a real return on equity allowance over the period because of the deduction of inflation from the depreciation allowance. So effectively, that is what is currently happening. And the investor gets compensated for actual inflation under the

AER's framework.

MS BRAKEY: Thanks for that clarification, Dinesh. I wondered whether anyone wanted to - sorry, I've got Glenn. Glenn, did you want to say something?

DR BOYLE: Yeah, if I can. First, I think the point Toby raises is a very important one although the solution is not so obvious. After all, if we think there is a conditional market risk premium that varies through time and we set it at date T, then it's not very time consistent to then say three and a half years later we actually think it's the same and that we should apply the same market risk premium.

If it's time-varying then it will have changed, and it could have changed a lot. So as I say, the solution is not too obvious, but I think I would lean towards the one advocated by Dinesh, although I would use a different formula. Basically, I would take the historical premium and I'd say give that some weight, whatever it is, and then use a combination forecast from all the conditional models, the DGM, surveys, et cetera, et cetera, et cetera, give that the remaining weight, and then basically you've got a formula.

Once you've set the weights, you've got a formula. And so you can apply that at the time that the RoRI is set and then you can just update

it each time there's a new network who comes up for renewal.

I guess I would use changes in the conditional models rather than their levels. Their levels seem a bit suspect to me and we don't really know enough about them, but changes should net that out. They should at least be moving in the right direction, even if they don't have the level right. And so basically what I'm saying is you start with a history bit and then you look to see whether the conditional ones are above or below their long-term mean - sorry, moving above their long-term mean or moving below their long-term mean, and you adjust the historical one accordingly. That would be a relatively simple formula to implement.

Somebody asked a question - I think it might have been Clare - about the right starting period for the historical time period. Well, that's easy. As far back as you can go - as a starting point. Then you have to test for stationarity and ergodicity. If it passes that test, that's what you should use. If it doesn't pass that test and you have to - basically, it doesn't until you get down to 30 years or something of data, that's not long enough.

That would be one thing I'd definitely recommend against, is using a historical average

based on a short period of data. You've got to test to see whether the long series actually has the right statistical properties, and then you use that.

MS SAVAGE: So you thought was 30 years wasn't long enough?

DR BOYLE: No, no. I mean, as Martin's pointed out, the confidence intervals go from zero to 12. You can't even reject the hypothesis that it's zero over 30 years. So no, you need much more than that. The problem is if you go back a lot further, you might find - I don't know until we do it - that the relevant statistical properties don't apply. Maybe there are structural shifts that actually show up in the data and the distribution is no longer stationary, let alone ergodic. But I don't know. It might be that in fact, despite all these things we think have changed over the last hundred years, it's not showing up in the data. In which case, you can use all of it. But 30 years? I mean, that's just not cutting the mustard.

And a last point, quickly. Graham raised the point about finding weights via fuzzy sets. I hadn't thought of that. That's probably true. It would be quite tricky though, but the basic idea traces back much more simply to quite a famous paper by Bates and Granger about combining

forecasts.

And at least in their framework, they actually give specific formulae for calculating the weights. Now, I don't think you can take these two literally and you'd have to - and also the conditions for applying these combinations are fairly stringent, but at least that would give you a starting point to think about for how to calculate these weights. I don't know much about fuzzy sets, so I can't help much with that.

MS BRAKEY: Thanks, Glenn. I might just put together one of Clare's questions and one of Jim's questions, which is do the experts have any views on the calibrated DGM, ENA's calibrated DGM, and any views on surveys? Martin?

DR LALLY: I don't know the details about ENA's calibrated DGM, so I can't comment on that until I see that. As far as surveys are concerned, they are rather like what the regulator's trying to do. The regulator is trying to look at all the available information and come up with a view about what the appropriate MRP is for some future period. Well, that's what survey respondents are doing as well. So I think that's a very, very valuable piece of information.

Of course, it does depend on a number of things: Just who is the survey being conducted amongst and how many of them are there, and is

there some reasonable degree of stability in the composition of the survey respondents over time. But in principle, I think surveys are very valuable because they are doing fundamentally the same thing that any regulator is doing.

MS BRAKEY: Thanks. Graham?

PROFESSOR PARTINGTON: Well, I pretty much endorse what Martin has just said. You know, handle surveys with care but they do contain useful information. Probably the most useful information comes from people who actually use the MRP in the decisions that they are making. The problem is if you just restrict yourself to that set, you are very likely just to get a result that says 6 per cent because of the stability in the expert valuer's MRP estimates.

You may find that some people will quote, I think, papers by Greenwood and Shiller that actually you shouldn't use survey evidence - or at least, this is their interpretation of the Greenwood-Shiller results - because the surveys provide poor forecasts. Well, we've already discussed that, you know, forecasting returns is actually a mug's game.

But there's another very interesting feature to that research. And Shiller carried on with it. And that is that what came out of these surveys actually seemed to drive subsequent

behaviour. For example, the investments that people undertook. So, you know, you can be highly critical of surveys, but if you want to find out about what people expect and what is going to drive their behaviour, then surveys it seems to me are a pretty important piece of evidence.

MS BRAKEY: Thanks, Graham. Glenn?

DR BOYLE: Me too, on surveys. I view them with considerable suspicion in isolation, but they do represent a somewhat independent estimator of the conditional market risk premium. So I would use them in combination with other estimators of the conditional market risk premium. There, they very definitely have a role to play. As for the calibrated DGM, like Martin I'm not sufficiently on top of the details of the ENA one to comment specifically on that. But just a point Graham made earlier about the suggestion that perhaps you should use lots of different DGMs, a combination forecast that way. I'm a little leery of that, for the simple reason that they are likely to have many features in common, and so they are not really independent pieces of information. So my preference would be to choose one and include that in a combination forecast along with surveys and conditioning variables models and so on, and that way.

MS BRAKEY: Thank you, Glenn. Jonathan?

MR MIRRLEES-BLACK: Thank you. Firstly, on calibrated DGM, and I think as we said in a CEPA paper, it can be difficult with DGMs, on an individual DGM, to look at an absolute number. But changes do contain perhaps more valuable information. And in that spirit I think that the calibrated DGM has some merit because you can be using it to find changes through time, but with an underlying approach which ensures that on average the expected growth is in line with what one might believe about returns over a long period of time. So the calibrated DGM is worth exploring further.

Second, surveys. And I echo and support what's been said by the other experts on surveys. But one thing I would add is there's a wide number of capital markets expectations which give expectations of portfolio constructors, and those advising pension funds and others on what expected returns will be. That's a source of information which doesn't seem to be used by regulators, and I think it might be worthy of investigation.

MS BRAKEY: Thanks, Jonathan. So I'm about to hand over to Dinesh for the final comment of this session. I was going to say, I'm quite surprised at how much agreement there is on the value of

surveys. So over to you, Dinesh.

MR KUMARESWARAN: Well, I'm just about to burst your bubble, Anna. Surveys can be interesting, but the problem with surveys, as the AER itself has explained on a number of occasions, is they are only as reliable as the survey design. So if the questions that are put to respondents are not well-designed, you might not get very useful information from the surveys. It's also difficult to interpret the results from the surveys, because it's not clear whether the respondents have answered the question that's been put to them faithfully.

I think that's one of the problems with surveys. I'm not discounting them completely, but I think that there are some serious practical problems with surveys. And I think you just have to be very careful about how you use them.

Just a tongue-in-cheek comment. I find it curious that Martin and Glenn both think it's a good idea to ask economics professors, who are typically the respondents to these surveys, what the market risk premium is, but last week you told us that we should completely ignore the term that investors in these assets are actually using. I find that a bit - but there's a serious point underlying that comment.

One of the other problems with surveys is

that the people responding to these questions often don't have any skin in the game. They are just asked for opinions. These surveys go out to economics and finance professors and they are asked, "What do you think the market risk premium is at the moment?" They are not actually using that in any - for an evaluation exercise to invest in a particular asset. They're just expressing an opinion about something. And I think investors actually have a pretty strong incentive to try and get the cost of capital estimate right. So the second reservation that I have about the surveys is that the people responding to them typically don't have skin in the game like investors do.

MS BRAKEY: Dinesh, can I challenge you on that to say if you were to include some of the investors in the survey, would they have the incentive to provide you with a certain number?

MR KUMARESWARAN: Perhaps. I mean, there are surveys that actually ask investors or corporations. So they are targeted at corporations. They are saying, "Well, what market risk premium are you using in your evaluation exercise?" Perhaps those sorts of surveys might be a bit more useful.

MS BRAKEY: Okay, thank you. Thanks. Look, I think we might draw to a close this session. It has

been a very interesting session and quite wide-ranging. We will be coming back this afternoon to talk about cross checks and the overall rate of return, so we will be reconvening at 2 o'clock Sydney time. Clare, anything you want to say before we wrap up?

MS SAVAGE: No. I think it's been a very useful session. I'm not sure - I think it's probably highlighted the challenge of the regulatory task in terms of what we need to do here, but very grateful for the input that has been provided to our thinking.

MS BRAKEY: Thank you very much. And we will see most of you, I would imagine, at 2 o'clock. Thanks.

THE SESSION CONCLUDED AT 12.28PM

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