IN THE COMPETITION APPEAL TRIBUNAL Case No.: 1380/1/12/21

Salisbury Square House
8 Salisbury Square
London EC4Y 8AP
(Remote Hearing)

Monday 1 November – Friday 19 November 2021

Before:
The Honourable Mr Justice Marcus Smith
Bridget Lucas QC
Professor David Ulph CBE

(Sitting as a Tribunal in England and Wales)

BETWEEN:

BGL (Holdings) Limited **Applicant**

v

Competition & Markets Authority **Respondent**

A P P E A R A N C E S

Daniel Beard QC and Alison Berridge (on behalf of BGL)
Marie Demetriou QC. Ben Lask and Michael Armitage (on behalf of the CMA)

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Friday, 12 November 2021

(10.33 am)

PROFESSOR JONATHAN BAKER (continued)

Cross-examination by MR BEARD (continued)

THE PRESIDENT: Good morning, Professor. I am so glad that you could make it without I hope too much bother.

A. Thank you. Thank you for your indulgence.

THE PRESIDENT: Mr Beard.

MR BEARD: Sir, members of the Tribunal, good morning. Professor Baker, good morning.

I want to pick up something that I left over in the discussions from yesterday. Could we just go to paragraph 42 of your report, so that is {A/7/13}.

Here you recognise that compliance has been imperfect with the wide MFNs. Then you say:

"But Ms Ralston's analyses of HIP pricing [this is retail pricing you are referring to, here, I think] across PCWs using Consumer Intelligence ... data likely overstate substantially the extent to which covered HIPs charged lower prices on platforms other than CTM. The CI data appear to be useful as a screen (which is how they were used by CTM), but they often do not permit an apples-to-apples comparison of individual price quotes across the platforms."

Then:

"Hence neither CTM nor Ms Ralston can be expected to identify reliably whether or not HIPs have engaged in differential pricing across PCWs in individual cases using CI data alone; they would also need information from providers ..."

Then you speculate about enforcement actions, but I just wanted to ask did you consider how that CI data had been used in industry in practice at all?

A. I recall seeing references to that some place, but I honestly do not know where I saw it, so I do not know precisely what I considered.

Q. Could we have transcript {Day4/25:1}, please. Oh, I am so sorry, it is the private session, my fault.

THE EPE OPERATOR: I do not have the private one.

THE PRESIDENT: We had this problem before, did we not?

MR BEARD: Yes, I thought it had been uploaded, I am sorry.

THE PRESIDENT: Not at all.

MR BEARD: I had it in my notes as Day 4 in private.

It is my notes that are flawed and thankfully we can have it, I apologise.

This is the transcript of the evidence given by Ms Glasgow. I do not know if you were here when that was going on?

A. No, I was not.

Q. Fine. Have you read this transcript, do you know?

A. Yes.
Q. Here I am picking up paragraph 7 in her statement which I do not think we need to go to, but I have quoted the relevant passage.
"So you are here saying that you engaged in a price monitoring process, and just to understand, you used the consumer intelligence data set to carry out that price monitoring, was that the key resource?
"Answer: Yes, we did not do anything internally, it was always externally with consumer intelligence."
Then I wanted to understand what the consumer intelligence data was, and so I put that question, you will see there. Then if you go to her answer:
"That is right ... I cannot remember the number of risks, but it puts a certain number of profiles each month, through various weeks in {Day4/26} the months and then providers a report to show where your prices are versus others."
If you read down to the bottom of the page. (Pause)
So what Ms Glasgow is explaining there is that everyone in the industry appears to use this consumer intelligence data, and essentially what it does is it puts through identical risks through these various PCW websites, and that is what all of these people use for price monitoring, all the PCWs use this for price

monitoring.

So the consumer intelligence data we are talking about is precisely apples and apples or it is robots and robots, is it not, in terms of the comparisons?

A. My recollection of reading this transcript that day is that there is perhaps a little bit later in the same section a comment by Ms Glasgow which I took to -- I understood as saying that she could not identify individual price differences without further enquiry, in much the same way that I understood that CTM acted, in what I was referring to in the section of the report. So I recall reading the piece of the transcript and understanding that the consumer intelligence data has information that is useful in a general way to understanding how prices might differ but was not -- but required further enquiry to drill down and understand the individual price differences.

Q. I see.

A. But I think I would need to see the remainder of the transcript in order to pick out the spot that I had in mind.

Q. I want to just deal with your sentence:

"The CI data appear to be useful as a screen ... but they often do not permit an apples-to-apples comparison ..."

If you are running the same risks by bots through all of these PCWs, that is precisely doing apples and apples or bots and bots, is it not, Professor Baker?

A. It is only if the question sets and the mapping are the same would that be the case, and I think that was the issue here, that you might get a different answer from one PCW than from the other for reasons like that that make the consumer intelligence data useful in a general way to understanding potentially general differences in price levels, but not useful for making specific comparisons by an individual provider. That was my understanding.

Q. Let me take it in stages. You are focusing on the outputs. When you consider apples and apples comparisons, what you are considering is are you comparing the same risks on the different platforms and that is precisely what consumer intelligence does, is it not?

A. I was speaking in my report about an apples-to-apples comparison of individual price quotes, and so I was thinking about the individual price quotes by HIP. If you are talking about general levels, then it is a reasonable basis for comparing prices sort of on average.

THE PRESIDENT: Professor, I think your concern or the

reason you are saying there might be an issue is that you have not been able to exclude the possibility of each PCW in respect of the same risk asking such different questions that the output, that is to say the premium that is quoted, is actually not particularly comparable. Is that a fair articulation of your issue?
A. That is a fair articulation, yes.
THE PRESIDENT: Did you do any empirical consideration of whether there was such a difference between the question sets asked by each PCW or is that simply something that you are raising as a potential concern that you would want to have bottomed out before you committed to apples and apples?
A. I did not do that kind of analysis. What I recall is I read the CMA's description in the Decision of how the consumer intelligence data was collected and used and what it meant, and that raised the concern that is reflected in my report.
MR BEARD: Just to be clear, you are not suggesting that the industry only used consumer intelligence data for general benchmarking, are you, because when it talks about price monitoring they are looking at the prices of specific HIPs. You are not suggesting otherwise, are you?
A. I think I understood your question so let me try this as

my answer. I understand that if -- that when the industry -- when say a PCW wanted to compare prices of individual HIPs across platforms it might well turn first and look at the consumer intelligence data, and then my understanding is that those comparisons might suggest the possibility of price differences in a direction that might concern a firm for whatever reason it is making that comparison, but that the firm could not be confident about that without further enquiry and would have needed to go to the firms and understand how they price specifically in order to make the precise comparison that it wanted to make.

Q. I think you are speculating about what the precise comparison would be, but I think you are also accepting that for the purpose of comparing individual prices for individual risks on individual HIPs this consumer intelligence data set does that and then you are saying if a disparity arises, which I think is the point that the Chairman was putting to you, further enquiries might be raised as to why that pricing disparity arose. Is that fair?

A. Yes, the way I put it is that it was useful as a screen and that further enquiry was required in order to understand whether it is actually a price difference or whether it is a -- whether what appears to be a price difference is actually an artefact of the way the questions were asked, for example, or -- and so not a difference in fact.

Q. When you say not a difference in fact, it is a difference in fact in relation to the price, is it not? It is the reason why that difference arises that the enquiry is made, is it not?

A. Yes, I mis-spoke. It is not an intended difference in price is what I should have said.

Q. You do not know whether it is intended or not, do you, because the point I am making is --

A. I am sorry, that is why the PCW would undertake the further enquiry, to see whether it was a price difference that the HIP, you know, intended to create or whether a price difference was one that was just apparent from the way the quoting was done but not intentionally different.

Q. They might change their question sets intentionally, might they not, so you could have intentional price differences created by the use of different question sets, could you not?

A. I think I recall seeing in the Decision perhaps a discussion of something like that, where the CMA said perhaps they found one example where that possibly was the case but that it seemed -- so that it would seem possible but not common or -- common practice in the industry.

Q. I am going to go back if I may to where I left off yesterday in relation to promotional deals. Just so you recall, we had been looking at table 9.2 just so you have it in mind {A/1/331}. You recall that. I am not going to ask you questions about that, I am moving on to the further versions, but I am just situating it so you have where we left off yesterday. Do you recall that table?

A. Yes.

Q. In her first report Ms Ralston questioned why the CMA had used this sort of crude headcount approach and not accounted for things like value or duration. She also argued that using a long time period was inappropriate, so as you know she recalculated that table, and you can see the results if we could go to {A/5/179}. If we pick it up at the bottom:

"Overall, Table 9.3 shows that, when one considers a shorter, more relevant time period of 24 months or less, the daily agreed value of promotional deals by HIPs with wide MFNs falls post-disapplication."

If we just go over the page {A/5/180}, there we have the table at 9.3. The numbers in these are all marked confidential. Quite how confidential they are I think is a matter of discussion, but we will treat them as such for the moment.

What you see there is the CMA approach listed at the top which is the 19 months pre and the 19 months post.

There it indicates that if you use that timeframe, then it suggests there is a positive change in the average daily agreed value of promotional deals, but if you use any shorter window you do not get a positive approach, and you know that the point that Ms Ralston was making was that the CMA's analysis is therefore extremely sensitive to the time series that you -- or the time period, I am sorry, that you use in relation to this comparison. You know that was her concern about it; correct?

A. I think I ought to clarify something that was not clear from the way in which you set up the question, if I could have the indulgence to do that.

The original table that you showed was -- I believe it was number of providers and number of promotions, it was something like that.

Q. Yes.

A. Ms Ralston raised the question whether counting the number of providers or the number of promotions fully represents the change in the level of promotions from before the wide MFNs were moved to after. She raised the concern that it is possible that duration of promotions differed and that if one looked at that one might get a different answer and she raised a concern about whether the value of the promotions might change.

So we are looking at this as -- and so then the way the conversation went after that was she -- I in my report and she in her reply looked at how the duration of the promotions changed, and my conclusion was it went up and then she had a different view and perhaps Mr Beard will want to explore the reasons for that.

Then we also wanted to look at the -- because she raised the question -- the value, so the difference in time periods that Mr Beard is pointing to here is concerned with measuring the difference in value, and what Ms Ralston's table shows is -- well, there is a difference in, perhaps Mr Beard will want to discuss, in how Ms Ralston's table and mine were computed, but the time period issue that Mr Beard is raising applies as I understand it solely to the value table which is the one that is on the screen.

Q. Let us go on, let us take your story through, Professor Baker, because I am going to go to the next sort of iterations of these tables.

In your report, you responded. At {A/7/18} you put in table 1.

This was a response by you setting out "Promotional Deals by Providers Subject to [wide] MFNs During and After the Relevant Period", and we see there effectively on the first two lines a replication of the table 9.2 that I took you to at the start, and then you have added in a number of promotional months.

What you are doing there is not just doing a straight headcount which was what the CMA had done. You are changing that and saying I am not just counting the number of deals but I am counting the number of months when there was a promotional deal operating. Is that correct?

A. That is correct.

Q. That is what we see on the bottom line. This is you saying, well, look, I can see the issue with just straight headcounts, but when we come to look at how long promotional deals are, I still get a shift between the relevant period and after the relevant period when I count the number of months when a promotional deal is active. That is what you are saying here, is it not?

A. That is correct.

Q. So this is still using the 19-month time series and we can argue about the scale of the numbers and the weight to be attached to this, but I think you attach some fairly significant weight to this and you describe it as a "substantial increase in promotional activity" in your report, and I can take you to it but it is paragraph (b)54, I am simply quoting it. That is correct?

A. That is fine.

Q. Just moving along, in her second report, as you have anticipated in the account you just gave, Ms Ralston then produced a revised version of this table. Could we go to {A/9/102}.

Here we have a table that says:

"Promotional deals by providers subject to wide MFNs during and after the Relevant Period."

We have a description of the active promotional deals and there is a slight difference here, is there not, because rather than being 5 and 9 in the top row, it is 6 and 9. That is right, is it not?

A. Yes, she has 6 actual in the relevant period.

Q. I know this is confidential, but I am going to do an anonymised question about that extra one, and I cannot see for the life of me why 6 and 9 can be confidential if 5 and 9 is not.

Then you have the providers, and there you go up one in relation to providers and we will come back to the reason she has done that.

Then you have the promotional months, so this is mirroring your previous table that I just showed but with what Ms Ralston says is a relevant correction. That is right, is it not? I mean, I understand you do not like what Ms Ralston has done, but that is what is going on here, is it not?

A. Yes, Ms Ralston is attempting to do something similar to what I did, but she does it in a different way.

Q. She does not do it in a different way so much as -- well, we will come back to that. I am not going to argue about the semantics of that. We can deal with that when we deal with the addition or otherwise.

Then she counts the promotional days that are actually available when there were promotions on. She drills down below months and actually asks how many days was there a promotional deal on. That is the next row down. That is right, is it not?

A. Yes, that is right, and I take it we are looking at the first column in each of the two blocks.

Q. We are. I am just going to come to the potentials. It is actually easiest in many ways, the potential number of PDs active is dependent on the details in the note below, but let us pick it up with the providers. The potential number of providers, now, that again is not going to be a confidential number because the number of HIPs which were subject to wide MFNs during the relevant period is a number scattered throughout the report which is 32 of the 45 that sold through ComparetheMarket.

What it is saying is of the 32 HIPs with wide MFNs on CTM, you had four during the relevant period and seven afterwards.

Then the percentage on the third column is just a simple arithmetic percentage of those two numbers. That is correct, is it not?

A. Yes.

Q. Then in relation to promotional months she does the same thing. She counts up how many months there could be of promotions during the relevant period and then looks at the percentage of promotional months. So what she is doing there is asking, out of the capacity for HIPs to engage in promotional deals during the relevant period by month, how many months were the promotional deals active. That is correct, is it not?

A. In the first column where it says "Actual", yes.

Q. The actual is, but then the potential is the number of months where there could have been a promotional deal on a PCW during the relevant period. That is correct, is it not?

A. That is what Ms Ralston claims to have included there, yes.

Q. I see, claims to have included. Then promotional days, that is going to be the multiple of just the promotional months effectively. So it is the number of days. So this is just a more granular version of the promotional months. So it is saying how many potential promotional days could there be during the relevant period and how many of those days was there an actual promotion on. That is correct, is it not?

A. Yes, that is correct.

Q. Do you actually dispute the number of promotional months or promotional days that she is calculating here?

A. In the "Actual" column or the "Potential" column?

Q. In the "Potential" column.

A. In the "Potential" column I do not dispute the mathematical calculation.

Q. Thank you.

A. What I dispute is whether it actually reflects the potential promotions in a practical sense.

Q. I see. Can you just explain, then, what you see as the mathematical calculation going to those numbers just so that we are clear that we are all in agreement?

A. Well, I mean, I could read the note that follows.

Q. That is fine. You are not doing anything else. It is just at the bottom of the note:

"The potential number of promotional months is equal to 608: 19-month period ... [multiplied by] 32 insurers."

So you have 32 insurers who could put in a promotional deal in any month, you have a 19-month period, that is how you come up with the number, and that is the same with the promotional days.

So what she is saying there is, well, look, any one of the 32 could have a promotional deal on a PCW during that period, and you accept that that is a potential day or, in the relevant row, month in which a HIP could engage in a promotional deal, and that is true of each of the 32 of them; correct?

A. I think I understood what you asked there. So I accept that if you look at how many months there were and how many insurers there were and you multiply that, you get 608.

Q. Just to be clear, you have 608 insurer -- you have 608 months during which an insurer could -- any one of those 32 could engage in a promotional deal. That is correct, is it not?

A. Where there is a slot at the PCW that an insurer could have filled in -- physically filled I guess you would say.

Q. Yes, so those are the potential promotional months, are they not?

A. Well, by a definition of potential that Ms Ralston uses, yes, that is correct.

Q. You are suggesting there is somehow a constraint on those promotional months. You are not suggesting that there is any specific limitation that you have identified during the relevant period that limited any of those insurers, are you?
A. I am not sure I understand the question. My concern is that the practical number of potential months is much less because the -- really for two reasons. One is that the promotions largely do not make sense unless they can achieve a -- for the insurer and the PCW unless the insurer can -- the HIP can achieve a top five ranking. So they cannot all simultaneously promote in those promotional months and all simultaneously achieve a top five ranking. Also some of the PCWs insisted that if the promotion were to appear on their PCW that the HIP not simultaneously promote on some other PCW.

Both of those things mean that as a practical matter one would never see 608 promotional months being filled with promotions the way the table is supposing implicitly.
Q. Let us deal with each of those, then.

Let us deal with the second one. You are saying a HIP will not promote on two PCWs simultaneously. Is that correct?
A. No, I am saying the second one was that some PCWs

required exclusivity and required a HIP to promote on them. I think, may I say the one that I remember?

Q. I am sorry, I think you had better not. You may have a key in front of you which will set out the --

THE PRESIDENT: No, I am going to allow the witness to name the name.

MR BEARD: I am grateful.

THE PRESIDENT: Because I think it is helpful. So, Professor, you go ahead.

A. Thank you, sir. My recollection is that MoneySupermarket had an exclusivity provision along the lines of what I was suggesting or at least required of its promoting HIPs that they not promote elsewhere.

MR BEARD: So you are saying if you were one of the 32 and you did a deal on MoneySupermarket during a month, you could not consistent with that do a deal on another PCW during that month; is that right?

A. That is right if MoneySupermarket in fact imposed the exclusivity requirement on the particular deal that we are talking about, yes.

Q. I understand. That does not actually change the number of months, does it, because Ms Ralston's figures are based on the idea that a HIP is only doing one potential deal on one PCW in any month, so that objection is not sound to the numbers, is it?

A. Yes, you are correct. She says, I see in the footnote --

Q. Thank you.

A. -- that each insurer can have only one active promotional deal at a time, so I guess it would depend on how the exclusivity provision is written, but if it is written that no simultaneous promotion was permitted, then you would be correct and I was wrong.

Q. Thank you. So that objection falls away. Then you are saying that the promotional deal only works where a PCW gets into a top five ranking, but you mean a top five ranking on any of the four PCWs presumably?

A. What I mean is that the PCW -- I am sorry, that the promotional deal is attractive when it permits the HIP to achieve a top five ranking on a particular PCW, and that would limit the number of PCWs that would -- I am sorry, the number of HIPs that would contract for promotions within a particular PCW.

Q. Your first objection is in fact assuming we take your proposition correctly, you should use effectively 20 HIPs rather than 32 as the number because that is five top five rankings for each of the four big PCWs; is that right?

Let me put it another way. Sorry, Professor Baker, you look slightly bemused, and it was a slightly elliptical question.

You are saying a potential promotional deal is only worthwhile if you can guarantee one of the top five slots. Now, we do not have evidence of that. We have evidence about the attractiveness of top five slots, but you have fairly said you have not looked at any of these deals and there is not evidence of that sort that you can specify or refer to in your report.

But let us assume your proposition. You are saying you have to hit one of the top five slots, but there are 20 top five slots on the four PCWs during any month, are there not?

A. It is correct that there are 20 top five slots across all four PCWs.

Q. On your basis, what you would say is rather than -- sir, I am concerned it is confidential, but the percentage figure on actual to relative, I find it difficult to understand why that should be confidential. I know it has been marked as confidential, but I do find it very difficult to understand.

THE PRESIDENT: No, again, I think we have been through this, and the concern in the yellowing is because it is an extrapolation from data that is confidential.

MR BEARD: Yes.

THE PRESIDENT: Ms Demetriou, am I on the right lines?

MS DEMETRIOU: I cannot see that it is, but I am going to take instructions on the point. No, I think it is --

MR BEARD: I am most grateful to the CMA.

THE PRESIDENT: Thank you very much.

MR BEARD: It is 3%, and I am going to do rough head maths here. You are saying it should move from 3% to around 5%, something of that sort?

A. That sounds in the ballpark.

Q. I am sorry, I am doing --

A. I would have to compute the actual number.

Q. -- 20 over 32 times -- sorry, 20 over 32 as the relevant proportion here, so if you go down by that much you would increase the -- you would decrease the number of potential providers. So you would do 608 times 20 over 32, but this is the head maths. I am talking roughly. It is not going to double, it is less than double, it is probably just under 5%, we can get a calculator out, but I think we agree on rough maths.

A. Your approximation sounds reasonable to me.

Q. Thank you. So what we are doing is we are saying we are moving from 3% to 5%, but it is still a tiny number of potential -- it is a tiny number of actual promotional days as compared to potential days even taking into account your assumptions which we do not accept as evidentially sound. The point I want to put to you is

this gives us some context of the significance of promotional deals both before and after, does it not?
A. Well, let me -- so I accept your math, but let me add one other comment about this calculation which is that it is assuming -- what is called potential is assuming a lot of promotions, it is assuming effectively constant promotion by an insurer that -- or something close to it, given our issue about 20 versus 32, and an insurer that is contemplating constantly promoting is doing something -- you know, potentially doing something that is akin to lowering the base price, and with insurers, it appears in the -- from how I understand the findings that promotions are in general short term. There are some that are long, but the -- I think it was the median was two months, and so the -- and while there are examples of longer ones and examples of repeated promotions, I am not confident that 20 insurers would promote for 19 months, you know, two months at a time the way the "Potential" column is implicitly supposing either. But I do not dispute the math.
Q. You do not dispute the maths, but it does give us some sort of context. Now, I am not saying you cannot qualify it, and I understand the point you are making, we do not accept either your predicate or necessarily any scope of the contextualising, although we recognise

that there will be practical variabilities, but what I am saying here is you have a picture of something to do with the context here. You are looking overall at the scale of the promotional deals that were being undertaken before and after, and what you are seeing is first of all it is relatively small compared to the promotional deal scale even accepting your caveats. Second of all, it does not appear to be changing, does it?

A. The first point about relatively small, I mean, if you accept your math and if you think 5% is relatively small, you know, then it is 5%. The second point about how it is changing, the numbers for the "Actual" column during the relevant period of difference on Ms Ralston's report, then in mine, and so we would have to -- and so the change from during the wide MFN application and after is different in her report than in mine.

Q. That is because three months effectively of promotional deals were added by Ms Ralston into this table, was it not?

A. I believe what she did was added one provider in the "Actual" column and that would have added one promotional deal and three months in the "Actual" column.

Q. Yes, sorry, I was only looking at one row. You are perfectly right, Professor, to work it through. Then concomitant number of days.

That one HIP, which I am not going to refer to the name of because I think this may be sensitive, so if you have the key there I am going to refer to it as Legal & General, but you know --

A. Should I have a key?

Q. There should be -- I think it is probably amongst those papers. You should have something that looks like a list a bit like that. (Handed). Thank you so much. There is an infinite stock of these things, I am very impressed.

I am sorry, this is just to help us with confidentiality. I do not want to have to go into closed because this is a short point.

You see Legal & General that I am referring to?

A. Yes, I see Legal & General.

Q. That is the HIP that -- you recall that is the HIP we are discussing whether the promotional deals should be in or out. That is correct, is it not?

A. That is what we are discussing, yes.

Q. Your position is that Legal & General entered into the agreement to run a promotional deal prior to the relevant period, prior to the 19-month period, and, therefore, none of that time should be included within -- none of the time of that promotional deal should be counted within this analysis. That is correct, is it not?

A. Yes, I am doing a comparison in my table 1 of the number of promotions that were introduced during two time periods, and I am using "introduced" which makes sense after the relevant period because we want to understand the effect of the removal of wide MFNs on the number of promotions, and then in order to do a comparison of the same kind of thing, the prior column, the "Relevant period" column, in my table is also counting the number of promotions that are introduced.

The provider that Ms Ralston adds was promoting -- began the promotion before the 19-month period and that promotion extended for a few months into --

Q. Three months, yes.

A. -- the 19-month period, and so that was not a promotion that was introduced during the 19-month period.

Q. Understood.

A. So that is why I did not count it and she takes a different view.

Q. No, understood. Just to be clear, she only counts the months for that deal that are in the relevant period. She does not count the months prior to the relevant period.

A. She is counting the number of deals in place during the relevant period, and I am counting the number of deals introduced, and that is the comparison I wanted to make. She would want to make a different comparison.

Q. Understood, I understand, and we will make submissions on which is more appropriate in due course, but I just want to clarify your term "introduced".

Now, you have already mentioned that the general approach in relation to promotional deals or the median approach, because some can be very short, is we think about them potentially as being around two months long, but some can run longer.

What I wanted to clarify is whether or not you had looked at in relation to deals introduced beforehand that you excluded, and indeed deals that you included because they were entered into during the period, were subject to renewal points along the way. Did you look at that?

A. My understanding is that the data does not generally permit -- you know, the way that -- I am sorry, the way the promotional data set was constructed, you cannot tell which were renewed routinely, and so there may have been ones in the relevant period that were renewed for -- and so were treated as being multiple promotional months, and there may have been some after that were that way too, and that the table that I used, you know -- so that I can -- I guess so I do not know which ones in general were renewed.

Q. Understood. I think you are accepting there you do not know but you would accept that if you had evidence that they were being renewed during the period then you would count the period from the renewal as being, as you put it, introduction, a new introduction. I know it is a continuum of a promotional deal conceptually, but as a matter of your approach you must treat that as a new introduction; yes?

A. As a matter of the approach, I would want to treat all such deals the same way. That is I would either want to take the view that it is an introduction, that the renewal data is an introduction, and then treat it that way for deals that were renewed after the relevant period and deals that were renewed during the relevant period, or I would want to treat it as not an introduction and do it the same way in both the relevant period and after the relevant period.

Q. Right, I understand.

A. There would be -- I mean, I could -- ideally I would want to do it both ways and see if it made any difference.

Q. Right.

A. But I would want to do it consistently, and that is the main point.

Q. I completely understand your consistency point. I do not think you actually have a dispute between us and you and Ms Ralston and you on consistency. Inconsistent approaches would obviously flaw the data. So it is a question of consistency. What you have done here is said: the consistent approach I want to take is when it is introduced is the relevant time. When I effectively load all of a promotional deal, even if it gets renewed multiple times, I am going to treat that as either in or out of the relevant period. That is correct, is it not?

A. I am sorry, would you please repeat the question?

Q. Well, let us do it by example. Towards the end of the relevant period a deal is entered into with another insurer. This is Aviva (Quote Me Happy), if that helps you to recall.

A. Yes.

Q. That one was a deal that, as Ms Demetriou put it to Ms Ralston, was repeatedly renewed every -- I think it was every three months. Every three months.

A. Excuse me, did you say during or after the relevant period?

Q. During. So it started during the relevant period and then was renewed on a three-monthly basis?

A. I am trying to remember whether -- I recognise the provider name as one that -- where there was this issue, but I am just trying to remember whether it was during or after, but if you are confident --

Q. I am sorry, I think I am wrong, it is after the period, I apologise, Professor Baker, my question was flawed. It is in the after period, and that is actually relevant for these purposes, and I am so sorry. It was Aviva (Quote Me happy) in the after period, but it was renewed on a three-monthly basis. The way you treated it was to say, well, if it got renewed four times, for example, I will effectively include 12 months of promotional deal even though we know that a renewal came after three months, and in those circumstances the renewal came during a period where there was no wide MFN.

Now, the point I want to put to you is that the approach you are adopting at the end of the period you are considering, the after period, by doing that systematically skews the number of promotional deal days during the after period, because it is bringing them forward, and conversely by excluding any deals that were entered into before the relevant period even if they were renewed during the relevant period, you are again systematically biasing the relevant period days and months for promotional deals, are you not?

A. That was a complicated question.
Q. It is a complicated question. It has two parts. Do you want me to break it down?
A. Why do I not attempt an answer and then you can let me know if I have missed something.
Q. Yes.
A. The important point is to be consistent in the treatment of these deals, and my understanding is I think you are right -- well, I will accept that we know that they were renewals, which I think, as you say, after the -- and during the after the relevant period.
    I think the important point, the concern is that there are deals during the relevant period that might also have been renewals and it is not possible to tell when and whether because of the way the data is recorded.
    What I believe I recall is that the data set records that some of the providers -- that each of the providers might have chosen a slightly different format for replying to the same information request and that at least in some of them you would -- and perhaps all of them, I do not recall for sure -- that you could see -- you would just see in each month when the deal was, when there was a promotion, and given that promotions are typically -- you know, the median is two months, and

I do not recall if there is any additional information for this, but it is at least plausible that during the relevant period some of the promotions were similarly renewed.

So it would not be possible to make a consistent adjustment by just adjusting after the relevant period for the one particular deal that Mr Beard is identifying and that the way to assure consistency is to treat deals that could have been renewed the same way in both the during and the after period.

That is why it was not appropriate to make that adjustment in these data.

Q. I see. So even if you had evidence -- let us take this Aviva (Quote Me Happy) deal that starts towards the end of the after period, and you know it gets renewed, you have evidence that it has got renewed, nonetheless you think you should roll in all of -- let us assume it is three times renewal -- all of the 12 months rather than three even though the vast majority of that time you know was agreed to by Aviva (Quote Me Happy) at points of renewal when there was no wide MFN in place?

A. That is essentially correct and that is because we are most likely doing the same thing with deals that are during the relevant period, and so in order to make a consistent -- to have a data set be consistent in the two periods, the numbers be consistent in the two periods, in order to make the comparison one would have to do it that way.

Q. If you do not have evidence in relation to renewals, I understand some of your problem, but if you do have evidence you are effectively ignoring it in this approach, and you are saying because I do not have evidence in relation to some of them, I adopt an approach, a consistent approach, of pleading ignorance in relation to renewals and timing of renewals. That is what you are actually saying here, is it not?

A. Well, taking the tone out, the words that you have, the characterisation out about ignorance and I forget the words you used, but it is essentially correct that I did not do the adjustment in the after the relevant period because I was not able to do the comparable adjustment in the during the relevant period data.

Q. Just to be clear, the approach that Ms Ralston adopts of only looking at the months during the relevant period and the after period where there is a promotional deal does not suffer from that problem, does it?

A. I am sorry, could you repeat? I lost concentration.

Q. I am sorry. The approach adopted by Ms Ralston of just using the months during the relevant period in the comparison after period when there were promotional

deals and doing the calculation on that basis does not suffer from that problem, does it?
A. Oh, you mean because she has -- well, I am not sure. I would need to think about that.
Q. Okay. Just to close this out, I am not going to take you to it, but there is actually evidence that in relation to the deal -- well, I can take you to it. Can we have {F/513/9}, please.
If you look under 2, that is question 2, because this is a response to questions from the CMA from Legal & General, you will see there:
"The promotional deal with Confused commenced on August 2015 and was intended to be for the period until December 2015. However, the discounted commission rate in fact continued to be paid until November 2016."
What you have there is an indication that actually the initial version of the Legal & General deal was to run to December 2015, but then it was effectively extended or renewed.
Now, we do not know the legal detail so far as I know because the CMA did not follow up on it, but you see the indication there. Of course December 2015 was the first month of the relevant period, so if that evidence indicates at least some doubt, but we would say clear indication, that actually there was a point of

reconsideration or renewal of the Legal & General deal, then in those circumstances it should be counted within the period in any event, should it not?

A. I think I cannot tell from this what is written here when -- assuming you are correct that there was an extension, I cannot tell whether the extension was negotiated in November or December or October which would matter for the suggestion you are making.

Q. Yes, but the point I am making to you is there are two consistent ways of approaching this: the way that Ms Ralston has done it and the way that you have done it. The problem with the way that you have done it is as soon as there is evidential uncertainty effectively you count out of the early period, the relevant period, deals entered into beforehand that run on but then you count in to the later comparison period deals that are entered into but then run on substantially, and that creates a real risk of systematic upward bias. The two examples we have show that.

A. I agree that there are two consistent ways of doing it and one would be to make all the adjustments for renewals and one would be to make none, and my problem is that we do not know which -- it is not possible to make all of the adjustments, and so I prefer the approach that is in my table of making none.

Q. I think that would be a matter for submission because Ms Ralston does not need to make adjustments either.
Could we go now just briefly -- I am going to finish off on the tables -- to your paragraph (b)55 in your report, {A/7/19}, please.
This paragraph 55 just introduces the table that I think comes in over the page {A/7/20} which is a further recalculation of value and duration that you had asked the CMA staff to do using daily values rather than monthly values. Is that correct?
A. It is the average daily value of promotional deals, that is correct.
Q. There your recalculation suggests that there is one time window of comparison where you have a negative change in the average daily value of promotional deals comparing the relevant period and the after period, but what you draw from this is that for the other time windows, having done the recalculation actually you see a positive change, in other words the average daily value of promotional deals moves upwards from the relevant period to the after period. Is that correct?
A. That is what the table shows. I want to just clarify that that first line, the full period, there was the slight change in the numbers. That is I think a correction that I submitted.

Q. Yes, sorry, I will not ask you to guess. I have those. I am not going to take points on this. Instead of 2.58 in the top right-hand corner it should be 2.00, I think.
A. I do not recall, it was slightly lower.
Q. That is generated by the fact that 11.10 should actually be 10.53. I only have those in my notes because these are things you sent through, so I am literally just reading out what I think you say are the relevant corrections.
A. Yes. So the reason I did all of the periods here is that Ms Ralston did them. I prefer the full period comparison, as the most indicative of what --
Q. I will come back to that. I just want to close out the story on the tables and then we will come back to the periods.
A. Okay.
Q. Just for your notes, for the Tribunal's notes, the description of the Aviva (Quote Me Happy) construction of that deal is actually given in Ms Ralston's second statement R2, 4.90 at {A/9/105}. I just forgot to give you that reference as I was asking those questions because the witness knew broadly what I was talking about.
    Might we call it up, I am sorry {A/9/105}. It was my fault, I should have taken it in order. 4.90.

Then what we see below 4.90 and under 4.91 is Ms Ralston correcting for that error in relation to renewals but using all of the other maths that you have used in your table that I have just gone to, and there we see shifting this one -- reconsidering the way in which this one deal is to be treated, whether as a block or as a series of renewals, makes quite a fundamental change to the numbers on the right-hand side of your table.

When it says "Professor Baker's Table 2 analysis correcting", Ms Ralston has taken exactly the same maths and so you have a situation where in the long time period which you favour there is a positive change. In all the three periods of 12 months pre, 12 months post, 9 months pre, 9 months post, 6 months pre, 6 months post, you get negative indications, but in the six-month window you do get a positive indication of three pre and three post.

What Ms Ralston has said is that all of these calculations are very sensitive to the time period that you choose in relation to what you rely on, because if you choose a different time period from the long time period you are talking about, for instance the 24-month window or an 18-month window, then in those circumstances you get an opposite result from the one that you are relying upon.

As a matter of maths, that is correct, is it not?

A. As a matter of math, the correction involving the HIP that is not being named --

Q. Yes, 41, yes.

A. -- only matters in shorter terms. It does not make any practical difference over a long time period to the change in the average daily value, which is a reason to be concerned about using a short-term window.

Q. Let us deal with the timing issue now. If we could go to your report {A/7/20}, please. It is actually footnote 73 I think I need to go to because this is where you make the assertion about -- one of the two footnotes where you make the assertion about timing:

"Table 2 was prepared by the CMA's staff at my request. The shortest windows, including the exception, are the least probative because it took more than six months for the number of promotional deals to ramp up after removal of the [wide MFNs] -- a change in the economic environment that likely led various HIPs and PCWs to rethink their promotional strategies."

There is something quite odd about that statement. I think you would accept that there is a degree of circularity to it, that if you assume the change in promotional deal activity is due to the withdrawal of

the wide MFN then you say, well, it happens later, therefore we should use a longer period. That is what you are saying there, are you not?
A. Perhaps I have been less than clear. In the first months the deals may not be representative because the firms are figuring out what to do, and there are, you know, fewer of them for that same reason, so the average daily value calculation would not be as reliable an indicator as it would be in a longer period.
Q. Just for completeness, I will just take you to footnote 52 which is on page {A/7/16}. I think this is just making the same point, but just so you have it.
So same point, I think. I am just taking you to the places in your report where you deal with it, and you have rearticulated the position. I am not going to ask you to go through it again.
A. Okay.
Q. But let us just deal with your proposition that says, well, it would take them a long time to deal. The first point is we know that a number of people, including those with wide MFNs, if they were interested in doing promotional deals, did do them during the relevant period, did they not?
A. There were promotional deals during the relevant period, that is correct.

Q. I think the position of the CMA is that the PCWs who were interested in doing promotional deals could hand out offers relatively readily.
A. I am sorry, this is my not hearing your accent.
Q. No, I am so sorry.
A. No, this is my fault.
Q. The PCWs who were interested in doing promotional deals could offer them relatively readily.
A. A PCW could have the physical capability of offering a promotional deal readily, relatively readily, yes, I would accept that.
Q. Then we also need to think about the insurers, of course, because if we are thinking about promotional deals it is promotional deals in relation to their prices.
A. But, I am sorry, may I make -- qualify something?
Q. There is going to be time for you to make some comments on these points. Let me just work through.
Let us just think about the insurers. The insurers are major firms in financial services industries who have sophisticated pricing strategies. You accept that, do you not?
A. I accept that the insurers are major firms, and that some of them are sophisticated in their --
Q. Some of them are sophisticated?

A. Yes.
Q. You are suggesting that major insurance firms that are putting up insurance products on PCW websites are not sophisticated entities?
A. I am saying I do not know the range of sophistication among all the insurers and that they range in size and significance in the market but I will accept that many are sophisticated in the sense that they think hard about their -- the market and learn about it and make decisions on rational bases by collecting information and that sort of thing.
Q. It is really not plausible, is it, in relation to these sophisticated entities to say it will take them nine months to work out whether to do a promotional deal, is it?
A. It is certainly -- I would not agree with that. I think if you look at -- there are several reasons to think that both the PCWs and the HIPs took their time in adjusting their conduct after the wide MFNs were removed, and it would be helpful in my explaining that to look at Decision figure 9.1 if I am permitted to show you that.
Q. Let us go to that in a second because I have a couple of questions about the evidence first.
A. That is not the only reason that there would be --

Q. We will go back to 9.1 in a moment, but just for the moment first of all this question of how fast HIPs or PCWs could change tack, that is critical to your analysis here, is it not?
A. The question of how long the adjustment delays might exist is important in various aspects of my analysis here, yes.
Q. But so far as you know, the CMA did not actually gather any material from insurers or PCWs specifically on that issue, do you?
A. I believe I recall something in the Decision where the CMA talked about at least some kinds of adjustment delays. I think there was a provider that talked about how it did not immediately respond and waited to look at what happened in the competitive environment before deciding what to do and then made a decision, but I cannot say I recall the precise details.
Q. You do not refer to that in your report.
A. I do not, no.
Q. No. So far as you are aware, the CMA did not ask about these matters except in relation to their one witness, Ms Glasgow. Could we have her statement, please, {A/13/6}. If you just read 18. (Pause)
What she is saying here is that they are pretty sophisticated even in relation to the deals which would

involve a large amount of TV advertising and an awful lot of coordination of when the TV ad is going to be run and all that sort of thing. You can do these deals very quickly.

So the evidence we have there is that things can be done very quickly, and that is the evidence from the CMA's witness, but as far as we know no one was asked, well, actually, did you have a real problem changing your strategy either as an insurer or as a PCW in reaction to the wide MFN, and you do not know of any evidence to that effect, do you, being sought?

A. Just to clarify what we are talking about, as I understand paragraph 18 -- and I think this is what you are saying -- the paragraph is saying that in the event that a PCW and a HIP decided to promote, they could implement that, you know, within four weeks or something like that.

Q. Yes.

A. Now you are asking about whether the decision-making process might take longer, and the concern is that it might -- that in the wake of the removal of the wide MFNs, that is a change in the marketplace that could lead to a rethinking of how -- of business strategies so that that might take time to work through for the firms for them to see what the effects of the removal are in the marketplace, think about what it means for them, see what their rivals are doing and what the PCW -- if they are a HIP, what the PCWs are doing; if the PCW -- if it is a PCW, what other PCWs are doing.

Some could make those decisions quickly and some may take time, and so that is the concern here.

Q. I understand the point of principle, but it is essentially speculation as to how it would work. I want to take you to {F/650} if I may.

This is an article I think you have seen before:

"Online platform price parity clauses: Evidence from the EU Booking.com case".

A. Yes.

Q. If we could just go down to page {F/650/9}, you recall that this is about a situation where in relation to hotels there was a wide MFN in place with Booking.com and it was removed. You recall that that is the context of this?

A. That sounds like what I remember. I do not remember the article very well, but, yes.

Q. As you can probably tell from both the document title and indeed the format of it, it is an economic paper looking at these issues, and in particular carrying out econometric analysis in relation to the before and after situation in relation to pricing of hotels in France, in particular in Corsica, following the removal of the wide MFN clause.

If you look at page {F/650/9} you will see it looks at the short-run effects of eliminating price parity.

You will see that in relation to this in 6.1, you can read down, but it is in the second paragraph, it says:

"The estimated prices indicate a slight but insignificant divergence before the policy change. Notice that this price gap starts when the law was adopted ... as some hotels may have started reducing their prices right after. This possible 'anticipation effect' will be further discussed below."

So they are worried about whether or not people might be anticipating things and they test for it.

Then they say:

"While we use the graphical representation of the event study specification to establish common trends, we then employ a baseline [difference-in-difference] model to estimate the average ... policy change ...

"Column (1) [shows] ... This is in line with the predictions of the theoretical models ...

"Moreover, the coefficients for the number of days ..."

Do you see all of that?

A. I am not sure what I am looking for here.
Q. I am just taking you through it. I want to emphasise the line under the second paragraph in "[difference-in-difference] evidence":
"Notice that such pro-competitive price effect occurred relatively quickly, only few weeks from the legislative intervention."
Do you see that?
A. Yes, I see that.
Q. So here you have a study that was carried out of Corsican hoteliers reacting to the removal of a wide MFN in the context of Booking.com, and what you have here is evidence that actually people reacted very quickly.
Now, I am not going to cast any aspersions on the commercial acumen of Corsican hoteliers, but the idea that very sophisticated financial services companies in the insurance industry would react more slowly than Corsican hoteliers to these matters is not a proposition that I think we could simply assume, can we, Professor Baker?
A. I think it is possible that some providers and PCWs could react quickly and possible that some could react slowly and that there is some evidence of adjustment delays in the PCW sale of insurance which is a different market from hotels and bookings.

Q. Just to take that one step further, these same insurers that we are talking about here, these same insurers that we are talking about the reactions to, they are the same insurers that sell motor insurance in the main, are they not?
A. I believe that is correct.
Q. So those are the same insurers who about two and a half years previously had been through the process, following on from the private motor insurance study, of dealing with the withdrawal of wide MFNs in that context, had they not?
A. I am assuming you are correct. I do not recall, but I am not sure that is determinative of what they would do in a different market.
Q. It is not determinative, but you are talking about insurance companies dealing with insurance products on PCWs. Some are in the motor insurance market, many of those same people are in the home insurance market. They would have had the experience of how you modelled your systems or whatever and thought about the learning experience after the motor insurance investigation, would you not? In this case that is something the CMA holds against CTM at various points in its Decision.
A. They would know what happened in motor insurance and whether -- but that does not -- I do not know whether

that would have made sense to assume the same thing would have happened in home insurance. After all, they did not remove the -- they chose to treat the two markets differently, they did not remove the -- CTM at any rate did not remove its wide MFNs at the same time here, and the product is different, there are features of the kind of industry that are different.

I am not saying that the experience in motor insurance was irrelevant in understanding, in the firms trying to understand what would happen in home insurance, but I do not think you could assume as you are suggesting that because they had the experience in motor insurance, that made it unnecessary to think hard when the wide MFNs were removed in home insurance.

Q. I am not asking about people thinking hard. People can think hard but quickly, can they not, Professor Baker? The question we are talking about --

A. Yes, I should have said think quickly, you are correct. Not think hard.

Q. It is a factor at least on the basis of the CMA's case that surely indicates the insurers would be expected to react more quickly in this instance, would it not?

A. I think the experience in motor insurance that you are referring to, particularly -- well, assuming that the decision-makers within the two firms on motor insurance and on home insurance were the same, which I do not know, or that they shared information internally, that the experience in home insurance could have been useful in informing the -- the experience in motor insurance could have been useful in informing the home insurance folks in the firm about how to think about the consequences of removing wide MFNs, but there are other factors as well that they would have to consider.

Q. I do not think I can take this much further. I should just say you did not refer to or consider those issues to do with PMI and motor insurance, and you did not consider the issues in relation to the Booking.com study in your consideration of what the relevant time period was when you were writing your report, did you?

A. I did not consider the -- what you are describing about motor insurance or the Booking.com study in understanding adjustment delays in home insurance. If that is what you are asking, that is correct.

MR BEARD: I am conscious of time. I think probably now is a convenient moment if that works for the Tribunal.

THE PRESIDENT: It certainly does. We will resume, then, at 12.10.

MR BEARD: I am grateful, thank you very much.

(12.02 pm)

(A short break)

(12.19 pm)

MR BEARD: I am conscious of time, I will try and move things along. I have one or two more questions to deal with on promotional deals and then I will move on to issues to do with econometrics. So we will try and shift through this a little more quickly.

The points I want to discuss are concerning Ms Ralston's various tables, trying to put the promotional deals that we are seeing in perspective.

I think -- but you can tell me whether I am wrong -- that when we are thinking about promotional deals and potential impacts if there is any impact by the wide MFNs, looking at the nature, scale, context of promotional deals is important, I think, Professor Baker, is it not?

A. At the level of generality of that question, I think I would agree.

Q. I can go less general, slightly more granular, if that is helpful. So value, duration, scale, things like that, they matter?

A. Depends on what we are looking at, but for many purposes, those are the relevant questions.

Q. For the purpose of looking at whether or not the impact that we are talking about here, or the putative impact that we are talking about here of the wide MFNs is something that is likely to adversely affect competition in circumstances where I quite understand you do not get into a threshold of materiality or appreciability.

A. So if you are saying that in order to understand how promotional deals change from a period with the wide MFNs to the period after they were removed, it is potentially useful to look at -- what did you say, number and duration, did you say? Value?

Q. Value, duration, scale, those sorts of things.

A. Scale, I am not sure what scale is, but certainly duration, value, number, those were all potentially relevant factors.

Q. Rather than dancing around this, let us go to {A/5/159} if I may. I am just going to run through one or two tables that Ms Ralston put in her first report but you have commented on so I just want to put them to you.

"Table 9.1 shows that PCWs' investment in promotional deals represents only a small fraction of their commissions revenue ..."

You have seen this.

If we go over the page, please, that is table 9.1 dealing with "Investment in promotional deals as a proportion of commissions revenue", and the point that Ms Ralston makes is that that indicates the value invested in promotional deals as a share of commission

revenue is very small, and I think you probably agree that it is very small, the significance of that you may disagree with. Is that right?
A. I agree with Ms Ralston's math. I at least had no reason to question it.
Q. Then if we go on to 9.2 on page {A/5/161}, this is "Materiality of promotional deal activity to PCW and new business sales", and here I will not read out the numbers because although they are 2018's, they are a little old, I am concerned that these are quite detailed numbers.
I think what you will see there is what has been identified on occasion that where promotional deals may be run on occasion, or some of the time, they will involve a co-investment by the PCW and the HIP, the provider. You understand that?
A. I understand that the PCW may reduce the commission and the provider may choose to reduce the price by more than the commission reduction. Is that what you mean?
Q. Yes, that is effectively what I mean, yes.
A. Okay.
Q. That is what is called investment on either side. I think there is nothing controversial about that. But you see the figures in rows A and B for 2018 of the total investment.

Just to put it in simple terms, assuming all of that investment is passed through into reduced prices, what Ms Ralston is doing here is looking at what the promotional deals do in terms of an average impact on the value of PCW sales or the average percentage, I am sorry, in row F. You see that?

A. I see the math in row F, I see how she computed it.

Q. You see in row I:

"Value of promotional deals as a percentage of estimated value of new business sales."

So that is because here she is looking both at PCW and direct new business sales. Not renewal sales, just new business sales. Then in K she is looking at promotional deals as the value of the home insurance market. So you are looking at it as part of all home insurance sales.

I think you would accept that each of those figures indicate just what a small investment was made in 2018 by PCWs and HIPs compared to the overall sales that we are talking about, even if we only focus on PCW sales. You would accept that?

A. I would accept that the figures here are correct, I have no reason to question them. I think there are reasons to think that the promotional deals are a significant aspect of the business of the firms but I accept that

the numbers here are correct.
Q. So promotional deals are an important part of the business, but they are tiny numbers in terms of the overall sales that we are talking about. You would accept that?
A. How do I put it? I do not know whether I can -- can I characterise the percentages without -- the bolded percentage numbers are less -- can I say what they are less than or something like that?
Q. I think you can probably say what they are less than in broad terms, as long as you do not --
A. So they are all less than 1%, is that a fair statement?
Q. Yes. That is a small --
THE PRESIDENT: Professor, we can certainly see the point you are making because we have it up on our screen, so do not worry, the point is made.
A. I think at this point it is a point Mr Beard is making.
MR BEARD: I just want to put another contextual comparator to you. We know from the evidence that has been given recently that MoneySupermarket alone spends over £90 million a year on pay per click online marketing.
A. I am sorry, I missed the word.
Q. I am sorry.
A. No, this is my American ear, I am trying to understand a different accent. Just wait until we have a Scottish

accent, I have real trouble with that.
Q. We know that MoneySupermarket spends over £90 million a year on pay per click online marketing alone. That was evidence given by Ms Glasgow.
A. What was the word that sounded like paperclip to me?
THE PRESIDENT: Pay per click.
MR BEARD: Pay per click.
A. Oh, pay per click, yes, thank you.
Q. It is often abbreviated to PPC.

Thank you very much for translating, sir.

If we compare the investment by PCWs and HIPs in promotional deal activity against other investments they make, it is relevant to consider the scale of that investment as compared with say marketing because it gives a sense of the importance of promotional deal investment in the -- I suppose one could call it competitive dynamics of the market, however defined. You would agree with that?
A. Not entirely. My understanding is that a lot of the marketing investments that these firms make are brand awareness kind of investments, they are aimed at consumers who are -- well, they are aimed at keeping the brands -- or the PCWs or HIPs, let us talk about the HIPs, that the HIPs are trying to keep their brands at top of mind and in the background and give consumers

a good feeling about them, and the promotional deals, my impression is they are aimed more at consumers who are in the process of making an actual purchase decision and might be doing comparisons.

To some extent they have different purposes, and one would not necessarily expect to be able to compare the two values and come to a conclusion about relative importance for that reason.

Q. You are not trying to suggest we use one metric for contextualisation. What I am suggesting is the investment commitment that PCWs make in relation to marketing online or indeed offline gives a significant indication as to the priority they give and the importance of their marketing activity in the competitive dynamics of this market as compared to the promotional deal activity. That is what I am suggesting. It is an indicator. I am not saying we should reach a final conclusion, but it is relevant, is it not, Professor Baker?

A. It is something like talking about the averages rather than the marginal value of an activity. It may be that the last dollar of value from an additional advertising campaign and the last dollar of value from an additional promotional deal is equated by the firms, and if that were the case, you know, that would be what their simple economic model might predict, you would not necessarily expect the total investments to be equated, and you might not be able to infer anything about the difference -- about whether there is a difference in marginal value by looking at the total investment. But I agree that both are types of investments that the firms make and they certainly appear to spend a lot of money on advertising and marketing.

Q. All I am talking about is whether or not it is a relevant consideration for the assessment of context of the potential impact of promotional deals. I cannot actually tell whether you are in fact disagreeing with me. You have placed a number of caveats which I understand, but I think you may be saying that you accept that looking at these levels of marketing investment may be relevant for giving a sense of context for other investments and their importance.

A. I accept that these numbers are telling you about relative investment levels and that for some purposes it might be useful to compare the levels. What I am questioning is whether the comparison is -- whether one can take from that comparison an implication about the significance or insignificance of investments in promotional deals.

Q. Even when the marketing investment is orders of magnitude larger?

A. I think we are interested in the marginal value of each and that they may have different purposes and to the extent that is true then that would reduce the value of the comparison that Mr Beard is suggesting.

Q. I understand it might reduce the value is your position, but you are not saying it is irrelevant, are you, in those circumstances?

A. I am not saying I know how relevant it is.

Q. I think there is a limit to how far I can take this. I do have some further figures in relation to marketing, but we will submit those in due course.

You criticise the tables I have already taken you to. I have put to you the position that Ms Ralston suggests that these provide relevant context. I want to deal with the second of the criticisms you level. Just so you have it, let us go to paragraph (b)53 which is {A/7/18}.

So in 53 you are talking about you cannot judge magnitude of competitive impact. I put to you the relevance issue. The second part is:

"In addition, Ms Ralston's estimates of the magnitude of commission {A/7/19} reductions specifically are unlikely to be reliable. They depend upon the implausible assumption, made without justification, that each HIP-PCW paring has uniform sales throughout the year, and they adjust those figures in an arbitrary way, also without justification, by assuming a 15% increase in sales volume ..."

Now, you said without justification. Could we go to Ms Ralston's first report, {A/5/163}. If you could just read 9.26. (Pause)

Then if we could go to page {A/5/175}, if you could just read the bottom of the page in 9.73. You do not have to read the whole paragraph, but take it from the yellow highlighting:

"... for example, stated that ..."

Then we go over the page.

A. Wait a second.

Q. I am sorry, did you want to go back?

A. Sorry, I was reading the paragraph.

Q. Please, if you go back, I am sorry, I am not trying to constrain you; I am just trying to move things along.

A. I apologise for taking time.

Q. No, it is fine. (Pause)

A. All right.

Q. If we go over {A/5/176}, thank you. (Pause)

That is the sort of evidence that you would say is relevant to the consideration of whether or not the uplift was justified, the uplift calculation was

justified, I assume.
A. The uplift calculation?
Q. The assumption that was used by Ms Ralston that a 15% increase in sales volume during a month, that is the sort of information you would take into account in deciding whether or not that sort of approach was justified, is it not?
A. Yes, that could be relevant.
Q. It is not that it is without justification. As you know --
A. But it does not tie down the particular number.
Q. No, it does not, I am sorry, absolutely, and I am not suggesting it would, Professor Baker.
As we know, Ms Ralston did not just use 15% as an uplift. In her second report she used a 100% uplift which might be seen as quite a significant uplift. You know that. I think it is perhaps worth turning to. {A/9/82}, please. If you just read from, "Given that several HIPs have stated ..."
(Pause) There she is taking on board the concern that you have that 15% is too low, and actually applying a 100% uplift to any promotional deal, even though that is contrary to the evidence that I have just taken you to. You understand that?
A. So here she is giving a justification which she had not

provided in the section of the (inaudible) report. I believe -- and it is a justification for -- that says some HIPs turn out not to have gotten a large increase in sales with promotional discounts, but there is no one else's here of, for example, the distribution of effects of the change in sales as a result of promotional discounts, and the numbers, the 15% and the 100%, are not really tied to anything. So I think I will stand by my number, my suggestion, that they are arbitrary and I have no sense of whether 15% or 100% is a good number or not, but I agree Ms Ralston has looked to whether a larger number, namely 100%, makes a difference to her results.

Q. Just to be clear, Professor Baker, you said that she had given the justification in the second report now. The evidential material I took you to to begin with, that was in her first report, Professor Baker.

A. Yes, I understand that, but I do not recall, at least the way you showed it to me, I do not recall that in discussing the 15% number that the evidence from the HIPs that you pointed me to was there. Perhaps I have forgotten, in which case I was wrong.

Q. You are suggesting that it is not meaningful to try and carry out the exercise that Ms Ralston carried out even if you use relatively aggressive numbers such as 100%

uplift for promotional deals?
A. I would like to have some basis for understanding what the right number is, about how the, you know, distribution of uplift, which is the word we seem to be using, looks in order to be confident that either a 15% or a 100% is a good number.
Q. That was not something you asked the CMA staff to look at when you were preparing your report?
A. That is correct.
Q. So far as you are aware, that is not something that the CMA itself looked at when it was carrying out its Decision?
A. I have no idea whether they looked at it.
Q. But you recognise that if you could -- on your account, if you could make some sort of assessment by reference to the evidence this would be a relevant approach to adopt?
A. Well, there is this other problem about the uniform sales throughout the year that I referred to in my report, but if one puts that aside then I would agree with you that if one could tie down the number, that that would be a better way to make the calculation.
Q. Just two points on that. Again, the pattern of sales across the year is something the CMA could have explored. You would accept that?

A. I do not know whether they explored it or not.
Q. No, could have explored.
A. It depends on what information they collected, but --
Q. Just to be clear --
A. I do not know whether they collected the relevant information or not.
Q. I do not think they did collect the relevant information, as you refer to it, Professor Baker, no.
Two further points. I think you have made it clear you did not look at any documents that I have been referring to or any other documents talking about uplifts when you were preparing your report or your critique here, did you?
A. That is correct.
Q. Ms Ralston has explained under cross-examination and in her report that using 100% she thinks is an overestimate in the circumstances of the material she has seen and that in those circumstances the putative differences you are talking about in patterns of purchase across the year would be at least accounted for, perhaps over-accounted for, by those numbers. You understand that?
A. I heard that Ms Ralston testified roughly to that effect.
Q. Can we go to figure 9.1 {A/5/162}, please. This is

a bar chart of number of HIPs engaged/not engaged in a promotional deal each month. Those HIPs which you refer to as covered, wide MFN HIPs, engaged in a PD are in the -- I am not sure quite which blues I am picking here, I do not know whether it is the cerulean blue. Wide MFN HIPs are a lighter blue and then HIPs not engaged in a PD, not engaged in a promotional deal, in dark blue.

You explain your issue with this measure in paragraph 50 of your report. You said -- sorry, I should perhaps just direct you to paragraph 9.20 above that table from Ms Ralston {A/5/161}. (Pause)

In your report at paragraph 50 {A/7/17} you say:

"That statistic misleads, however, because it does not recognize that promotional deals were disproportionally employed by HIPs that accounted for a substantial volume of sales."

So you asked the CMA staff to recalculate that. We see that in your report paragraph 51, so that is {A/7/17}.

What you say is here that essentially if you take the market shares of the insurers engaged in discounting, you generate these various percentages that you refer to. That is correct, is it not?

A. Yes, if you take the fraction of sales that the promoting HIPs undertook on PCWs, you get these percentages.

Q. No, Professor Baker, that is not what you are doing. You are not taking the fractions of sales on PCWs, are you? Sorry, I may have misheard. Just sales overall, not promotional deal sales?

A. That is correct.

Q. That is right, but the obvious problem with that is of course that using market share is predicated on the idea that you have a situation where the promotional deal is being offered by the insurer across all their business and of course that is not true, is it?

A. Whether this is an appropriate calculation depends on what you want to measure. My point in paragraph 51 is that the HIPs that are promoting are substantial HIPs and so I was not making a point about the fraction of sales that were covered by the promotions.

Q. Because the problem with these headline figures is that they might be suggested to be giving an overly large suggestion of potential role of promotional deals because if you had a situation where one insurer worth -- let us think of it as being a very, very large insurer having 10% of the market and we will use the CMA's market of home insurance PCW sales. If it offered a discount on its GoCompare business which was taken up

by relatively few people, if the examples we have seen are relevant, the evidence we saw from Ms Ralston's report are relevant, even if it was taken up by very few, you would still say that was 10% of the market or rather that the HIP in question, you would give a figure referring to the 10% market share of that business even though it was a tiny fraction potentially of their overall home insurance business, would you not?
A. I was thinking about the answer while you were giving it, but I think your math was probably correct, but my point was that the HIPs involved in promoting are substantial HIPs here, and there is a problem with the implication of your calculation which is you are assuming that the way to measure the competitive importance of a promotion is just the value of the take-up by the consumers without thinking about what a promotion might do competitively and the responses by other firms and the like.
So I am not -- that was sort of how you were characterising the alternative approach here, and I am questioning whether that captures the significance of promotions.
Q. I see, but you recognise that it might be naturally rare to suggest a much higher operation of promotional deals using those statistics that you have set out in your

report?

A. I thought I was clear on what I was calculating here.

Q. Thank you. Let us move on. Let us go to some econometric evidence issues. I am actually going to just start with an article that you wrote back in 1999 if I may. It is called "Empirical Methods in Antitrust Litigation: Review and Critique". It is in {F/730/1}. That is just the front page.

If we go to page {F/730/4}, please, if we pick up paragraph two, you say:

"Courts are finding, to a greater and greater degree, that reliable statistical evidence can be invaluable in deciding questions of impact [so effect], harm and damages in a range of cases, including antitrust."

You refer to the Federal Judicial Centre's manual.

Then if we go on to page {F/730/5}, paragraph two:

"The great promise of statistical methods is that they permit a systematic synthesis of quantitative evidence, weighting the most informative data points the most heavily."

Then just in the final paragraph on that page:

"Empirical methods can help courts identify what happened and why. This can often be accomplished through multiple regression analysis that distinguishes among a number of competing factors that were correlated with a fact pattern -- allowing the court to isolate a key relationship or critical influence using models that describe the statistical relationship between one variable and a number of others."

Here you are saying actually econometrics is very important and very helpful in presumably data rich industries in understanding antitrust problems including effects problems. Is that correct?

A. I am saying here that econometrics can be helpful and the first quote was about comparing how courts have changed -- this was in 1999 and we are thinking about even 20 years before there was a lot less econometrics here, so we are writing to explain to courts and others that this is something that econometric approaches can be useful and you should consider them when it is appropriate and available.

Q. When they are available?

A. When the relevant data is available, the methods that are appropriate, that sort of thing.

Q. I see. I do not think you have an issue in general terms in recognising the industry within which we are dealing at the moment as a data rich industry, do you?

A. I agree there are a lot of numbers in various data sets that are in this industry. It is a different question about how informative those data are, but, yes.

Q. I think you have also recognised that in recent years there have been a number of econometric studies looking specifically at the impact of removing platform-wide MFNs, is that correct?

A. There was the study that we looked at in the article before about bookings, and I have the impression there may be others, but I do not recall specifically how many.

Q. I am just going to focus on one. Could we go to {A/5/108}. This is the part of Ms Ralston's first expert report where she is talking about the value of econometric evidence in general terms. She is saying at 6.14:

"Regression analysis has a number of features that make it suitable for assessing the effect of alleged anticompetitive infringements, in particular its ability to isolate the effect of the infringement from other factors, as described in more detail in section 6C.2."

I do not think you actually disagree with that as a proposition, do you?

A. I agree the regression analysis has features that make it helpful to isolate the effects of -- isolate harm under particular circumstances.

Q. "This is particularly relevant in social sciences such as economics, where it is rarely possible to design an experiment to isolate and test a specific hypothesis directly, as is the case with natural sciences."

It is quite a broad position, but I am guessing again you do not disagree with that?

A. In a general way, I would agree with it.

Q. Then she quotes from the European Commission's Practical Guide on assessing antitrust damages which she was cross-examined on, but I want to go to 6.15:

"The CMA itself relied on econometric analysis to assess the impact of wide MFNs in private motor insurance in its DCT market study. In the context of that study, the CMA acknowledged the weight given to its econometric results to reach its conclusions."

She then quotes:

"'The evidence we have gathered, and particularly our econometric analysis shows that the prohibition of wide MFNs [in private motor insurance] has led to an increase in competition between DCTs.'"

So as she mentioned yesterday, she had raised the issue of the DCT econometric study. Indeed, if we could go on to page {A/5/143}, please, at 8.3, she says:

"I consider the economic evidence on both of these metrics in this section."

So this is on high commissions and fees, prices.

"I consider the economic evidence on both of these metrics in this section."

Sorry, this is 8, so it is commissions.

"My focus is on PCW commissions where the data allows for more precise analysis. In particular, I consider two main analyses:

"An analysis across all PCWs, whereby I test whether commissions were higher or lower (on average) between a given PCW and insurance brand when there was a wide MFN in place, compared to a situation in which there was either a narrow MFN or no MFN in place. This gives me the opportunity to consider the effects of the removal of other PCW's wide MFN clauses (not just CTM), in line with the approach used by the CMA to assess the impact of wide MFNs on commissions in private motor insurance."

Can we just see footnote 345, thank you.

There is a citation there from the market study:

"Final report, Paper E: Competitive landscape and effectiveness of competition, Appendix 2."

The B number is just the bundle reference that has been inserted subsequently.

Could we then go to {A/9/130}. Just picking up 5.69:

"Finally, I note that, in its DCT market study, the CMA relied on the same competitive approach across all

PCWs -- i.e. a comparison of covered HIPs to non-covered HIPs during and after disapplication -- to assess the impact of wide MFNs in private motor insurance. While the circumstances and market realities of private motor insurance are different from the present case, the spillover bias assumed by Professor Baker and the CMA is equally applicable to the CMA's previous analysis in motor insurance. The CMA has accepted this in its Defence of the Decision."

If we could then go to 5.129 at {A/9/147}. Just look at 5.129:

"First, Professor Baker overlooks the fact that this is a market-wide assessment [so she is responding to criticisms you have made of her econometrics], which also considers the effects of the removal of PCWs' wide MFN clauses, not just those of CTM. While the issue around negotiation dates might apply to the removal of CTM's wide MFNs, other PCWs removed wide MFNs much earlier. Hence there was ample time for their commissions to adjust to their counterfactual level (i.e. for these other PCWs, there is a much longer post-disapplication period). In this respect, I note that the CMA's econometric analysis in the DCT market study would be equally affected by this issue since it only included one year [so this is a criticism of the data] ... after CTM and GoCompare disapplied their wide MFNs in motor insurance."

Professor Baker, you were absolutely right yesterday when you said that Ms Ralston had mentioned it. She had not just mentioned it; she had made it very, very clear, had she not, in her reports, both of them, that what she was doing in econometric terms, at least in relation to commissions analysis specifically, was doing essentially what the CMA had been doing in relation to motor insurance previously. You understand that from those references, do you not?

A. I understand that Ms Ralston said she did the same thing. I do not know what the CMA did in the earlier study and I have not looked at it, but I would note that it is a different industry and you just cannot assume that the data and the same methods can be applied, that you can solve the problems, the econometric issues that --

Q. Let us pause. I am going to give you an opportunity to deal with this because I am going to go to the study, you can look at it, but you have said, Professor Baker, in relation to the material that has been put forward by Ms Ralston, including in relation to commissions, that it is not possible to carry out any sort of reliable econometrics, but you also made clear yesterday that you have not looked at, nor considered and not interrogated the DCT econometric analysis about wide MFNs in relation to commissions in motor insurance. That is what you said yesterday, and that is correct, is it not, Professor Baker?

A. It is correct that I did not consider it or look at the earlier studies.

Q. Let us go to it. {B/16/95}.

This is the appendix 2 that was referred to in the footnote. Now, it may not be entirely surprising that you did not focus on it because in the lengthy Decision it is remarkable that there is no reference, so far as we can see, to appendix 2 to the DCT market study, and, as you made clear yesterday, no one at the CMA had mentioned this to you, and you will see at paragraph 1:

"This appendix presents the methodology and results from an econometric analysis of the impact of wide MFN clauses on the commissions charged by digital comparison tools ... to insurance companies. The analysis uses data on commissions and MFN clauses in the motor insurance market, collected from four large DCTs over the period 2010 to 2016."

Just to be clear, this appendix is setting out a methodology and results from econometric analysis in relation to the same sorts of clauses that we are talking about in this case, in relation to commissions which we are talking about in this case and, indeed, the CMA and its witnesses have gone out of their way to emphasise the importance of commissions in this case charged to insurance companies, which again is what we are dealing with in this case.

Yes, it is a different product, motor insurance, but that is what this was doing. The analysis uses data on commissions and MFN clauses in the motor insurance market across six years, which is what Ms Ralston does, and if we go down now to paragraph 10, I think it is two pages on {B/16/97}, you will see:

"We collected data from the four DCTs covered the period 2010 to 2016. For each DCT, we collected data on the revenues ... and volumes ... for each insurance brand and for each year. From this, we calculated the commission (in £) as revenues divided by volume."

I cannot remember whether you remember what Ms Ralston did, but was it any different from this?

A. I do not recall. Let us see. The period was different, I guess.

Q. Well, the actual years were different, but in fact the number of years was not and the information gathered was not. Let us go down to paragraph 21. {B/16/100}. Here you see there was some general robustness testing done, testing for pre-removal effects. What this is testing for is looking at whether or not commissions were already changing prior to the removal of the wide MFN clauses. This is not an issue that has arisen in this case, but what you will see is the CMA controlling for potential issues in relation to the data set that they are using for the analysis of the wide MFNs, and they are looking at pre-removal effects here, and then if you go on to paragraph 25, I think it is just over the page {B/16/102}, you then see in paragraph 25, feel free to read it, a whole series of robustness checks which were presumably carefully thought about by the CMA in deciding how to carry out that analysis.

What is really striking is that you and the CMA are saying that in home insurance involving the same PCWs and the same insurers and the same types of clauses, albeit with much less coverage it should be said even on the CMA's approach, you are saying econometric analysis is impossible here in any meaningful way.

The first thing I would have thought that you, as an independent expert, seeing this would have said is: hang on a second, what did you do there? But you did not do that, did you, Professor Baker? You did not even look at this. So I am now in a position where I am faced with a witness on behalf of the CMA who I can ask no sensible questions about how the CMA approached this material and you are not in a position to explain why it is the CMA did that exercise there and says it is impossible to do it here. You cannot answer those questions, can you, Professor Baker?

A. My charge was to review the report by Ms Ralston and comment on it. Without reading this study, I cannot comment on whether I think the CMA approached the question in a different industry in a sensible way there. It is possible that it has all the same problems that I think Ms Ralston's study has, and it is possible it does not, and I just cannot say without looking at it, but my concerns are with Ms Ralston's study, whether it is reliable or not, and there are various reasons that I think her various studies are not reliable, and those concerns are independent of whatever the CMA did in the study that I have not looked at carefully.

Q. Let us just think about that. Let us think about it from our point of view from a moment, shall we? But I think I would think about it from an independent expert's point of view.

You are reaching conclusions about the operation of econometrics in relation to insurance being sold on PCWs in circumstances where the CMA has done that sort of analysis in very recent past. Would you not at the very least want to ask them what they thought about spillovers or persistence or precision or heterogeneity when they carried out that analysis?

A. I was reaching the conclusion about Ms Ralston's econometric analyses, and I did not -- even now, I guess I would be interested in finding out what the CMA did here and whether they had the same problems, just in an academic way, but I do not see that there is any issue for -- that what the CMA did here bears on my views as to what Ms Ralston did in her report.

Q. In this report, so far as we can see, the CMA expresses no apparent concern about spillovers and does not carry out any robustness tests in relation to them. It apparently identifies no concerns or tests about persistence. It does not refer to heterogeneity concerns, and it has no concerns about precision which you subsequently have sought to highlight.

From our point of view, the critical issue is that we think precisely this sort of approach is appropriate and the very authority that is putting forward a decision saying you cannot do this sort of analysis here in any meaningful way has not referred to any of those concerns in its previous enquiry in recent years.

A. Some of the concerns you list -- precision and various, you know -- particularly depend on the particulars of

the data set, or they could easily.
Q. Yes, they could.
A. So it is possible that if you did the same analysis, even if it was appropriate to do the same analysis in two different industries in the same way, you have different data and the data could be more informative one more than the other, so I do not see the relevance of whether there was a precision question in here. It looks like -- so I just do not know why one would think that what the CMA did here is applicable to what Ms Ralston did in her report.
Q. I see. Let us pause there. Let us deal with spillovers which are your most significant critique. We will be coming to the details of those in a minute.
What I am not understanding is how you can be so confident about what is and is not being considered by the CMA here, because on the face of the document we do not see any consideration of spillovers, and yet conceptually the story you tell about spillovers and their significance would seem to be equally applicable in relation to the removal of a wide MFN in the motor insurance market in relation to PCWs.
That stands to reason, does it not, Professor Baker?
A. If the study in the motor insurance study were a sort of difference-in-difference analysis which was similar to

the study that Ms Ralston did and I were looking at it and reviewing it the way I was asked to look at Ms Ralston's results, it could well be the case that I would want to ask them about spillovers and think about whether that would be a problem there.

Q. Yes.

A. But I have no idea what they did here, and it is possible I would conclude that the earlier study was not reliable just the way I do not believe Ms Ralston's study is reliable here. I just do not know.

Q. Yes, you might have done, but the problem we have is the CMA relied on this, they published it, they presented it at an international conference. We do not have the slides unfortunately, but we have understood that that was the case. But there is no reference here to spillover concerns at all.

If you were a witness for the CMA that knew about this or you had made enquiries about what went on in relation to this study, I would be asking you the questions about why it was in this study spillovers did not matter and why here they are so significant that you cannot carry out meaningful econometric analysis, and I cannot do that. That is a significant problem in relation to spillovers, in relation to persistence, in relation to heterogeneity, which are the matters dealt with in the Decision, but it would also potentially be in relation to the issue of precision which, as I understand your point, might depend on the nature of the data that we are considering here.

But you see that and you see the CMA carrying out this study and you still maintain that meaningful econometric analysis is not possible here. That is my understanding of your evidence, is it not, Professor Baker?

A. My view is that based on my review of Ms Ralston's econometric studies that it is not possible to reliably use econometric analysis for various reasons which I would be happy to explain here, and I do not take a view as to whether it was possible to do it in the study of the motor insurance industry that Mr Beard is referring to.

Q. Does it not trouble you, Professor Baker, that this was not highlighted to you as to what had been done, you do not know what was done here, I cannot ask you questions about what was done here, and yet you still maintain your position?

A. It does not trouble me because I was asked to evaluate Ms Ralston's study, not to evaluate the earlier study.

MR BEARD: I am going to move on. I am so sorry, sir. I am conscious of the time, I am going to move on, but I do want to put down a marker here about the seriousness of the concern that arises in relation to this issue.

THE PRESIDENT: Yes, no, thank you. Two things. First of all, I have one question, if you do not mind, but before I articulate that I just want to -- we are 1.15 now, that is fine. I was minded to rise now, if that is a convenient moment, after my question. Do you have a problem, Professor, if we were to resume after half an hour at 1.45?

A. You are asking whether a brief lunch break would be acceptable to me?

THE PRESIDENT: Yes, indeed, because this is hard work for you. It is hard work for everyone, but it is particularly hard work for the witness.

A. I appreciate the consideration. I would be happy to resume in half an hour assuming that I can find food that quickly.

THE PRESIDENT: I am going to look at Ms Demetriou and I am quite sure without breaching the undertaking they can ensure that you have enough food and a comfortable place to eat it in, and hopefully some company where you can talk about things other than the case so that you have at least a relaxing half hour. Thank you. We will do that.

But my question, which arises out of the chain of questions that you have just been asked. Can we bring up {A/7/6}. You will be familiar with this. This is your first report, Professor.

What we see in paragraph 17 is you summarise the instructions you were given, and there it is quite clear that you were being asked simply to evaluate the conclusions in Ms Ralston's report and not to conduct your own econometric analysis. That is right, is it not?

A. Yes, sir.

THE PRESIDENT: You are obviously very familiar with Ms Ralston's analysis, you have critiqued it extensively, so you know as it were the exercise she was attempting to do.

A. Yes.

THE PRESIDENT: If you had been asked -- and I quite appreciate you were not -- to put together a better model for assessing the effects of wide most-favoured-nation clauses, if you had been told to go away and just do what Ms Ralston was asked to do but do it your own way, is it your evidence that that simply cannot be done in any meaningful way?

A. I could not think of a way to do it. I cannot rule out the possibility that someone smarter than me could, but I think there are fundamental problems here that would actually be difficult for anyone to overcome, and if you would like I could explain very briefly what I think they are, but --

MR BEARD: We are going to come back to some of this.

THE PRESIDENT: I suspect we will, but if we do not I will make sure that you have the opportunity to deal with that at the end of the day.

Essentially then your evidence is that Ms Ralston's analysis is one that she has effectively been asked to do the impossible?

A. Yes, some of it, the problems that arise, some of them are insoluble as a theoretic -- well, I am saying that badly. The answer is I believe it to be impossible. For some of the reasons having to do with how the models are set up and for some of the reasons having to do with infirmities in the data.

THE PRESIDENT: Just to conclude on this, I want to differentiate between weight and zero value. One of the things that judges will do is they will look at evidence, they will recognise that it has deficiencies, whether it is oral evidence or whether it is documentary evidence or whether it is econometric evidence, they will look at it and say, well, you have an incomplete data set or something like that, and that affects the value of the analysis that emerges, and one takes that into account. That I would say is a question of weight. How much weight do we attach to that evidence versus other bits of evidence.

Are you simply saying that the weight of Ms Ralston's evidence is lower than it might be in a case where the data set was different, or are you actually saying it is in evidential terms of no value at all?

A. I believe it to be of no value at all, the econometric evidence in this case. In other cases I have used econometrics before, and I would do it again.

THE PRESIDENT: That is very helpful, Professor, thank you.

Mr Beard, you may want to take that up later.

MR BEARD: No, I was going to only remark that the reason I took the professor to the extracts from Ms Ralston's statements in the beginning of this topic was precisely with the language of those instructions in mind.

THE PRESIDENT: I understand. Thank you very much, Professor. We will resume at a quarter to, and I hope your sandwich is waiting for you. Until then, thank you.

(1.20 pm)

(The luncheon adjournment)

(1.50 pm)

THE PRESIDENT: Professor, I hope you had a comfortable if

abbreviated lunch break.
A. Yes. I suppose it is too much to hope for a nap.
THE PRESIDENT: You never know, Mr Beard may oblige you.
MR BEARD: People have said that about my questioning before.
I am going to move on to spillovers. I am also conscious that there are questions Professor Ulph will want to ask about the Johansen & Vergé paper at some point. I have somewhere in the script where that might be appropriate. Obviously that is with due deference to Professor Ulph, if he wants to intervene and ask those questions earlier, fine. Otherwise I will pause to enable that.
THE PRESIDENT: Thank you.
PROF ULPH: I will take it when you do that.
MR BEARD: Thanks very much. That is great.
I am going to deal -- in your report, you essentially highlight two concerns that you have with Ms Ralston's econometrics. One is spillovers and the other is precision. That is fair, is it not?
A. There are other concerns as well, but those are certainly important ones.
Q. You do not for instance in your report deal with issues of persistence or heterogeneity -- persistence and heterogeneity. They are not really part of your report,

are they?
A. Well, persistence is -- to the extent it is about adjustment delays, that is a part of my analysis.
Q. That feeds into your spillovers critique, does it not?
A. Not entirely, but it is relevant to that, I think.
Q. You do not try and defend the points on heterogeneity, you do not refer to heterogeneity in the report?
A. I do not talk about heterogeneity, although, you know, that is a potential reason one might have variation in responses of firms that would create the precision problem. It is a bit more impossible.
Q. That is a different kind of heterogeneity to that which I am talking about. I am talking about heterogeneity in pricing conduct rather than necessarily nature of firm, which I think is the heterogeneity you are talking about.
A. The pricing conduct -- I am not sure.
Q. I think you do not is the answer.
Now, you say that the difference-in-difference approach will systematically underestimate the effect of a wide MFN because covered insurers' reaction to the disapplication of the wide MFN spilled over to uncovered insurers, and therefore they are not a suitable control group. That is the essence of your spillover issue, is it not?

A. Yes, although the way you put it you do not necessarily -- the term spillover itself can mislead a little bit in the sense that the issue is essentially whether there was a response to the removal of the wide MFNs by the non-covered providers and the metaphor of spillovers makes it sound like there is a temporal order which is not necessarily always going to be correct. You would still have the problem independent of that. But with that caveat I will accept your description.

Q. I think here you do have a temporal order, do you not, because you have the lifting of the wide MFNs in this spillover theory, you have the lifting of the wide MFNs, you have the previously covered HIPs, then -- on your account of spillovers operating more aggressively, and then a reaction from the non-covered MFNs. That is the essence of the story, we picked up on that yesterday.

A. That is a stylised version of the story meant to confer intuition. In fact it is conceivable that some non-covered providers could respond before some covered providers. That would still be a spillovers problem.

Q. Let us go to {B/16/100}. This is actually the table -- we actually went to this page previously. This is in the DCT appendix 2 analysis, and what you have here is the regression results from the DCT analysis, and what the CMA concluded there was that there probably was an effect from the wide MFNs during the period when they were applied in the motor insurance market, when they carried out their econometric analysis that we were referring to before lunch.

Just to put your concerns about spillover in context, if there were a spillover concern in relation to the motor insurance market what you would be saying is, yes, they have estimated there is an effect, but it might be an underestimation. That is correct?

A. Well, I guess I do not know what the dependent variable is and the independent variable is in this. I guess I see the right-hand variables here, and I hesitate to accept all this without reading the paper. So I am a little at a loss to how to answer.

Q. I can see that, but if you have econometric regression results that comes out with a positive finding when you are carrying out this sort of -- a time series or a before and after analysis and it says actually in relation to the dependent variable we do see an effect, positive effect, your point about spillover is that might be an underestimate of the effect. That is correct, is it not?

A. We would have been to be in a difference-in-difference set-up. It is easy for me to talk about in the context of Ms Ralston's results. If Ms Ralston had found, you know, positive, significant -- I am sorry, if -- yes, positive and significant effect of wide MFNs the way those stars seem to suggest that the CMA did in this example, which parenthetically means if I am interpreting correctly they did not have the same kind of precision problem I guess, but I am sorry that was an aside that I should not have made, but to answer your question --

Q. It is a heads you win, tails you lose, is it not, analysis, that if you read the table you say there is not a precision issue here because they produced the table?

A. I do not know whether there is one, and that was perhaps -- I should not have said that because I do not really understand the study.

Q. But you would expect the CMA to have identified a precision problem, would you not, and they would not publish these results if there was a precision problem, would they?

A. One would hope that if the same kind of problem that I saw in Ms Ralston's model, analysis, applied in this study that one would -- I would have hoped that it would -- they would have realised it and had a concern the way I do about Ms Ralston's results.

Q. So you would (inaudible) been put on enquiry in relation to the DCT study. Now, let us go to {A/7/33}.

This is your table 4.

What you are doing in this is trying to illustrate the spillover problem, and here what you are doing is in table 4 you are illustrating how a difference-in-difference analysis works in the absence of spillovers. That is correct, is it not?

A. I believe that is right, yes.

Q. Sorry, I am just taking it straight from your report, Professor Baker.

Then if we go to page {A/7/35}, what you are doing here is adapting the table to show spillover problems. So what you do here is you say, look, brand A covered by the wide MFN was pricing at period 1 200, period 2 without the wide MFN at 190 so there was a difference, but then you say, look, we might get data that says brand B not covered by the wide MFN, period 1 was pricing at 200, period 2 was pricing at 190, and then the spillover effect therefore you are interpreting is minus 10 for the non-covered brand; is that right?

A. Well, this is an illustrative example that shows that in the event that brand B were affected, I guess it is by 10, which is the right-hand column, compared to table 4, that you would not observe a difference, so that would be attributable to the spillover, one would do the

regression results and think there was no effect of the removal of the wide MFN when in fact there was an effect of 10 in the -- on both brands.
Q. It is a bit different from the sort of DCT example where you are just discussing where your spillover effect might have just reduced the identification of difference. This table is treating the spillover effect as completely eliminating it. That is correct, is it not?
A. That was a hypothetical example. It does not necessarily have to eliminate, but it can, and it was easiest to explain this way, so I think I said -- made that point somewhere in the report.
Q. Yes. This would be what you might call complete or absolute or maximum spillover where you are getting an identical reaction between the two groups; correct?
A. I am just trying to think about whether one could have more than complete, but it certainly completely makes the regression impossible -- failed to identify the harm.
Q. When you got identical spillover effects, at that point you have a difficulty identifying the fact that actually there was a price change. That is what you see there, is it not?
A. That is what shows, yes.

Q. Yes.
PROF ULPH: Mr Beard, could I ask a question at this stage?
MR BEARD: Of course, Professor Ulph, at any point. I am sorry, I did not realise.
PROF ULPH: My question was I thought one implication of the spillover effect was that you might have a larger impact with a covered brand than in the absence of a spillover effect. It comes back to that discussion we had yesterday where Ms Demetriou was talking about an iterative process so you remove the wide MFN, the treatment group responds by, say, cutting price or doing more promotional deals, but that causes the non-treatment group to either cut its price and do more promotional deals, and that goes back and affects the treatment group and they further cut their price and do more promotional deals.
So the overall impact of the removal of the wide MFN on the covered brand is larger than in the absence of the spillover. Have I understood correctly what you think is going on with the spillover?
A. I think it would be helpful if I explain and let me see if this answers your question.
The assumption that is made to identify the effect of the wide MFN for the purpose of the econometric analysis is that the control group, the non-covered

brands here, is not influenced in any way by the removal of the wide MFN, and the problem here is that there is competition among providers and there is competition among PCWs.

We have been working in terms of stylised stories where we hypothesise a covered brand changes and looks at reactions, but the world is potentially more complicated than that.

The reactions that are implicit in the 190 in table 5 could be of all sorts. It could be that a non-covered brand -- that a covered brand moved first and a non-covered brand that competes with it responded, and as a result lowered its price.

It could be that a PCW -- it is conceivable that the non-covered brand anticipated the way we saw in that article that Mr Beard showed me too, and it is conceivable that some covered brands moved quickly, other covered brands moved with delay; that non-covered brands, some moved quickly, some covered brands that had not been affected -- changed price immediately changed later perhaps in response to the non-covereds, and that further non-covereds responded after that in iterative rounds, and you have to think about how competition works through the PCWs.

The PCWs are potentially instituting the -- CTM or the rival PCWs either instituting promotional deals or reducing commissions, and they may do so for both covered and non-covered brands. They could do so simultaneously for some of each. It is very hard as a matter of theory to reach a conclusion as to what the dynamics would actually look like here, and what is important for the econometrics is that it is not just the covered brands that are responding to the wide MFN. Whether it is through the operation of competition among providers or competition among PCWs that works through commissions or promotions, the non-covered providers respond as well.

This table, table 5, really only has two periods, a before and an after, so it is collapsing all the dynamics into one period, but the dynamics could be complicated in ways that are hard to say what they would be a priori.

PROF ULPH: I understand the difficulties of taking a very complex situation and reducing it down to a simple table, but going back to the discussion you had in your reports where you were describing what spillover effects were, I understood what you said or one implication of spillover effects was that you could end up with a bigger impact on the treatment group because you have all this interaction between them and the non-treatment group. So I was just really puzzled by an example in which the impact on the treatment group was exactly the same as in your previous table. So if the impact on the treatment group had been magnified by the presence of the spillover, are you saying that would still be very difficult to detect the effect?

A. The answer is yes. I have two comments on what you want to say. One is just as a matter of the theory, the interactions among the providers and the PCWs could amplify not just what happened to the treatment group but also to the control group, and so how that nets out I just do not have an a priori view.

But your other point I believe is suppose the effect of the wide MFN ended up being more substantial for the treatment group, the cover providers than for the non-covered providers. In that case, instead of 190 we might have 195 or something like that in the table, and the estimate of the effect of the wide MFN removal would still be biased towards zero, but it would not be zero.

This table here illustrates a case where there is no effect -- where the spillovers mean that the regression analysis, the econometric analysis, would show no effect when there is one.

The case that you and I are just discussing is one where the regression analysis would show an effect but it is smaller than the true effect, and the spillovers bias the results towards no effect, but it is hard to say a priori how big that bias would be, but it could be very large, it could be enough as a practical matter to make the estimated effect be very small when in fact it is large, but it could also be smaller.

PROF ULPH: Just going back to that point, you say you could also take an effect which was initially found to be insignificant and make that effect significant. So you are underestimating it. Ms Ralston has found there is an insignificant effect of the removal of the wide MFN, so are you saying if you really had properly been able to control for spillovers you could have found a significant effect, is that what you are saying?

A. The problem with spillovers is that there is not a way to control, except with exogenous information. But were it somehow possible to measure the effect of the spillover, it would turn out that the true effect would be more substantial, more adverse than the estimated effect.

PROF ULPH: Thank you. I think you have answered my question.

MR BEARD: Just to pick that up, you said the problem with spillovers is there is no way to control for them, so are you saying, just hypothetically, that in the DCT study you have to assume there is no scope for spillovers because since you cannot control for them, that would otherwise taint the results that the CMA carried out?

A. First, let me qualify my there is no way to control. It is possible to imagine that if you had outside information about the closeness of competition among all the firms, for example, that we had a metric or something like that, something we do not have anything close to here, it would be possible to use that sort of outside information potentially to assess the value of spillovers. But with that qualification, which I am making for theoretical reasons to be precise in my answer and not overclaim, it is not a practical qualification for the study.

So putting that aside, your question is essentially if spillovers were important in the DCT study, as I understand your question, would that have biased the results towards underestimating the effects, and assuming they were doing a difference-in-difference analysis like Ms Ralston's and assuming it was otherwise similar to what Ms Ralston did, that would be what I would suppose would happen, but I would have to read the study to be comfortable with asserting that that was a problem in that study.

Q. You did not read the study. You have just mentioned the study that I took you to earlier about the Booking.com and you grabbed at the reference that you had read about anticipatory effects. You said that is the sort of thing that can create problems.

Could we go to {F/650/11}, please. It is the second full paragraph:

"As anticipation constitutes a threat to the correct estimate of the magnitude of the short-run price effects, we complement the results in Table 3 with an analysis of the triple interaction effects for the groups identified above, setting the date of the event [at this date]. Table 4 confirms that the magnitude of the effect on chain hotels is similar to what previously estimated, but it is now weakly significant. When allowing for anticipatory responses, the effect decreases ..."

So they are considering these things very clearly in relation to the anticipatory effect, and testing for them critically, and what they conclude is that there are no significant anticipatory effects at all in relation to this. They say, you will see, overall anticipation effects do not substantially change the picture emerging from 3.

So I am not trying to get into the details of this report. The point I am making is you saw this report earlier, you saw a reference to anticipatory effects. In fact what this report did was it took your theory and it tested it econometrically. That is in essence what Ms Ralston did here. She looked at applying a series of tests to see whether or not the data was exhibiting spillover effect problems and that is the correct way of dealing with spillover effect issues, is it not, Professor Baker?

A. When you have spillover effects, it biases the results towards understating the effect. Now, in some cases, I do not recall what happened in this article, but in some cases, you will find let us say adverse effects, and you will say -- and for whatever reason you are doing the study you will say, well, all right, I have a number here for it that understates it, but for my purposes that number is good enough, you know, or they have -- it is good enough to know that it is at least this amount, and so depending on the reason for doing the regression and what you want to make of the results, it might be in some settings a strategy to do the analysis, look at the estimated effect and say, well, it has to be at least that, and that is good enough for my purposes.

As to how they control for anticipation, I just do

not recall what they did here, and how they set it up.
Q. (Overspeaking)?
A. It is not necessarily -- I do not know whether it is in the context of spillovers.
Q. No, I agree. You were the one who referred to them.
A. I do not know whether it is an attempt to address a spillover problem or just a problem in understanding the magnitude of the effect. I really do not know that this is or is not applicable to what Ms Ralston did here, but on your third point, which is about whether it is possible to test, it is what I said before. You have to bring in outside information and the kind of thing that would potentially work would be if you had a metric about the closeness of competition between various providers and then you could say, well, I know -- well, let me back up.
There is an article Ms Ralston cites, references, where the problem is about what is the economic effect of a fire on various properties, and they know the distance of various properties from outside the perimeter of the fire and that is essentially a metric on how distant the firms are from competition -- not from competition; from the treatment group observations.
Here we cannot say that some of the -- we do not have some group of non-covered providers that do not

compete with anyone else, we do not have a metric for saying non-covered providers are competing more closely with each other and some covered providers than they are with different ones and map out the degree of closeness of competition, and then I think somebody clever could exploit some information like that to do -- to make some progress here, but I could not do it.

Q. Professor, I am going to try and move you along. The situation we are dealing with here with Ms Ralston is that she carried out an econometric analysis where she identified no statistically significant effect in relation to wide MFNs. You understand that, do you not?

A. Yes, I understand that, yes.

Q. So in order for the spillover effect to indicate that actually there was something going on -- the spillover effect to mean that there was something going on that she was not capturing, you had to have a spillover effect that meant that the no statistically significant outcome was covered by the spillover, do you not?

In other words, let me put it another way, to explain Ms Ralston's results, on your spillover theory we have to believe in the complete spillover in order to be able to say actually Ms Ralston's results are not a reliable indication of no -- are not an indication of a lack of evidence of any statistically significant effect.

A. The answer is no. The reasons are first that it does not have to be complete, it could be a less than complete what you call spillover -- I am sorry, less than complete bias resulting from spillovers that was nevertheless substantial, and that could lead to the kind of result, I think, that Ms Ralston finds, and it is particularly hard to tell here because of the precision problem the wide standard errors, so that without going into that problem that makes it hard to tell whether the results of the spillover test are really much different from the results of the primary test at the point estimate level, she does not -- I am sorry, I will stop. I will let you go.

Q. Please. The underestimation that you are talking about to get to zero has to be a spillover effect equivalent to the putative adverse competitive effect, does it not?

A. I think I understand what you mean. I guess that is correct.

Q. So you have to have a spillover effect that wipes out all of your theorised anti-competitive effect, do you not?

A. Well, all within the limits of what you can measure which here are a serious issue.

Q. Right. The point is for Ms Ralston's zero to be

overcome by spillover and therefore it to be unclear whether actually what she has is no effect or an underestimation of the effect, the spillover effect that you are positing has to be the full equivalent of the adverse effect because otherwise you would get some kind of positive outcome, would you not?
A. Well, remember Ms Ralston's zero is not a zero. The reason she says it is a zero is because she says it is not statistically significant. She has point estimates that in some cases are not in this direction of adverse effect, they are in the other direction. She has what we saw in promotions yesterday where the point estimate is an adverse effect and she is calling it zero because she says it is not statistically significant.
So you have to be thinking about precision in order to make the kind of statement that you are trying to make here.
Q. Fine. Let us break this down into stages. Let us hypothesise that you have no significant effect as a point estimate, you have a zero. In those circumstances, in order for the spillover to render that result uninformative as to the absence of effect, the spillover has to be equivalent to the adverse effect that would otherwise be predicted, is it not?
A. I think I can say what you are trying to ask a little

more precisely. Suppose the true effect is some positive number that is an adverse effect, and the estimated effect that Ms Ralston gets is zero, literally zero, and suppose we think that the entire reason for the estimated effected to be biased is spillovers here. Then it would have to be the case that the control group is just as affected by the treatment as the treatment group. I think that is what you are asking.

Q. Yes. When you say "just as effective" it needs to be almost simultaneous and of the same magnitude. That is the two dimensions we are talking about; correct?

A. I am worrying about almost simultaneous because I am not sure that is right.

Q. But you can test for any delayed effect, can you not, and therefore in order to -- in order for Ms Ralston's robustness checks, which she runs -- five of them -- in order for those not to disclose any sort of spillover but still be coming out with the supposedly wrong result, it has to be simultaneous and of the same magnitude. That is why I am emphasising the absolute cover.

A. I do not think that is right. When you say you can test for delays, the tests Ms Ralston has used make various assumptions about sequencing for example of effects that there is no reason to make. They are conditional, not

just -- they are conditional on assumptions like that as well as accounting for the spillovers. I am not sure you can say -- I do not think you can say what -- I have forgotten exactly how you posed the question, but I do not think you could say it that way.
Q. You are saying non-simultaneous but same magnitude?
A. I think same magnitude is correct, yes.
Q. You have got to be making an assumption -- I will come back to delay issues in a moment -- you have got to be making an assumption the non-covered group are affected, if not simultaneously, in a way that cannot be detected by any delay effect tests of the same magnitude.
A. I have to say -- clarify that we are talking on average, that on average the covered group has a certain response and on average the non-covered group would then have to have a similar response for the spillovers to completely remove the -- I mean completely make an effect, go to zero, literally, when there is actually an adverse effect, and that is assuming you are able with precision to understand what the measured effect actually is.
Q. You recognise that Ms Ralston in her report, if we could pick it up, it will be the second report, bundle {A/9/117}.
She is here considering the relative magnitude of spillover effects in general terms. You have read this

section of the report, I think.
A. Well, she is talking about her partial cartel umbrella model and the conversation that she and I had in our reports about that.
Q. Then if we go on to 5.45 at {A/9/123} she is talking about timing of treatment and spillover effects.
A. I think she is talking about the stylised example that you and I just went through in my report.
Q. The assumption that is being made in that is that the timing of the effects will be simultaneous. That is correct, is it not?
A. Well, there is only one period before and one after, so all these aftereffects are combined into one time period, so in that sense, yes.
Q. So what she then does is she looks at 5.48 onwards at various tests for spillover bias in the light of --
THE CLERK OF THE COURT: We have lost Professor Ulph.
MR BEARD: You are quite right. I will pause. (Pause)
THE PRESIDENT: Professor, can you hear us?
PROF ULPH: Yes, I can.
THE PRESIDENT: Very good. I am told the live stream is not working, but I am not going to hold you up, Mr Beard, for that reason. We will proceed and it can engage as soon as possible.
MR BEARD: If we then go to paragraph 5.50 on page

{A/9/124}, you will see there the outline of the tests that Ms Ralston conducted. The first test was lag coefficients for leads and lags which was looking at certain timing issues. That is correct, is it not, Professor Baker?

A. I believe she is assuming that the control group moves first, then the non-control group moves after that.

Q. Then the second test is at 5.51 over the page {A/9/125}. You are saying she makes an assumption that actually covered HIPs would move first, which is not an irrational assumption in relation to these situations even on your approach.

A. It is arbitrary. It is possible, in that sense not irrational, but we just do not know that all the covered providers will move before all the non-covered providers or anything close to that, or not.

Q. It does not have to be all of all, does it?

A. That is why I said "or anything close to that".

Q. In fact we have good reasons to think that on the CMA's account that actually wide MFN, covered providers as you put it, would be the ones that would move first, do we not?

A. You mean as a matter of theory? I am not sure we do.

Q. If we go to the second test, the second test looks at whether the control group's pricing behaviour changed in

the three months following the disapplication, so this is a further test looking at spillovers using a relatively tight window. Is that correct?
A. Right, this is the test that assumes that all the responses are in a -- by everyone, in a three-month period that is either immediate and then she does it delayed start and then that the responses disappear after that. That is to say these are all examples of bringing in assumptions about how reactions work that -- or this and the previous one -- that are possible but by no means necessarily the case and could easily be wrong.
Q. It could easily be wrong, but you are hypothesising they are wrong. You have no basis for saying that they are actually wrong, do you? You do not have an evidential basis for this?
A. Well, I think I recall we mentioned earlier there was that provider that said I went and looked around and wanted to wait and see what the competition was doing before I responded, or at least that is maybe not wanted, I do not recall the motive, at least waited, and I think I recall that there are in the evidence statements by firms to the effect that, well, if they see them, other firms are promoting or their prices are out of line compared to other firms, they would respond. So there are reactions going on, it does not sound as

though they are necessarily immediate, but I guess it is possible they could be.

In other words, I do not think that the assumption that is being made here is supported in the evidence I saw any more than an alternative would be.

Q. That will be a matter of submission because of course you have properly accepted that you did not look at the underlying evidence.

The third test is at 5.55 {A/9/126}, and these build on the assumption that:

"... spillover effects can arise only if there is a significant effect on the covered HIPs in the first place. In particular, I test whether narrowing the treatment group to only the two, potentially most influenced, HIPs [which I will not refer to] makes the effect of the disapplication of CTM's wide MFNs statistically significant."

What she is doing here is she is taking out of the covered insurers group all of those except the two most likely to react to the disapplication and testing it.

So she is coming up with a more extreme version to see whether or not there is a reaction to disapplication and how people react in order to test these sorts of spillover issues. You accept that?

A. I understand that is what she claimed. I was not persuaded either that these two would have to be -- would necessarily be the most influenced treatment group members or that this would solve the problem, because we have competition among all the HIPs treatment group and non-treatment and competition amongst the PCWs that affect HIP behaviour. I did not believe you could reliably say these two would be more or less influenced by the removal of the WMFNs than other treatment group HIPs.

Q. I see. So you are making a judgment as to whether or not there might be others that are more affected and that that would mean that you should choose them. You are not actually saying that selecting those two to focus on was the wrong thing to do?

A. Remember what she is trying to do. She is trying to say I have a -- I think this was her relative price analysis -- I have an original relative price analysis, and I am trying to show that it is not affected by spillovers, so I am going to do a different relative price analysis where I just change the treatment group and see if I get a different answer, and if these two HIPs did not -- were just sort of, I do not know how to say it -- average HIPs, one would expect her to get the same answer regardless of whether there were spillovers or not.

Q. If they were just average HIPs, I see.
A. In terms of their response to the WMFN removal.
Q. But if they were not average HIPs then you would expect her test to be effective potentially?
A. I think if you had outside information about who competes with whom closely and distantly and could figure out that somehow when you work through all of the potential responses that these two HIPs were likely to have a greater impact than others, I could imagine making use of that in a test, but I do not see that there is anything like that in what I read that would permit Ms Ralston or me to do that.
Q. The CMA could have gone and got that sort of information but did not.
A. I think that is very hard. I do not think that is actually practical here even with the greatest of effort, but I could be wrong, but I do not believe I could think of a metric or a way to get at that that would allow this to be useful.
Q. It is impossible to get the relevant information as well as impossible to do the econometrics?
A. For this particular approach to -- yes.
Q. I see. Let us go on, because she essentially tries to skew the test further to try and identify spillover effects. If we go on to paragraph 5.57, "Additional

robustness test ...", what she is doing here is rather than taking out the wide MFN covered HIPs who might be least likely to react, in this element of the test she takes out five of the uncovered insurers who had started to promote after disapplication, in other words the ones who showed the greatest apparent reaction. That is what she is doing, is it not?

A. Yes, that does not make sense either. The ones that promote -- what Ms Ralston is doing is saying if a HIP had not promoted a -- let us say we are in the control group. If a HIP had not promoted before wide MFN removal and then promoted after, that HIP is unlikely to have -- I am sorry, that HIP is also likely to have -- is also more likely than other HIPs to change its prices or have its commissions changed.

In other words, the difference-in-difference analyses that she is testing in her fourth test are the prices, the relative prices, the absolute prices commissions analyses. There is no reason to think that a HIP that is responding to wide MFN removal in the control group responds in commissions only if it responds in promotions as well.

In fact if you think about how the -- or prices, only if it responds in promotions as well. Firms have different kinds of business strategies. Some may prefer to respond to competition with promotions, promotional discounts, some may prefer to change their base price, and the PCWs could change their behaviour in ways that induce commission changes, and it would not make any -- you know, with other firms just as well as with the firms that Ms Ralston removed.

So it is very hard to say -- I am not confident that Ms Ralston has successfully done what she hopes to do which is remove the -- remove some members of the control group that somehow would be most likely affected. I do not think there is reason to believe that that works.

Q. Sorry, you are not confident that she has removed the right people, or are you saying that that test is irrelevant?

A. I am saying that she needs to have a metric that shows that somehow these non-covered HIPs were affected more across the board so that they are affected more in commissions and prices, in their price behaviour too, because her difference-in-difference analyses at stake here are about commissions and prices and I do not think she has one and I do not think the one that she is proposing -- mainly if a HIP promoted more, then it also changed its price behaviour while other HIPs that did not promote more did not change their price behaviour as much, I do not think there is any reason to think that that works.

Q. In each of the tests you can come up with a range of reasons why you think it might not be the full answer in relation to these things, and actually taking all of them together you say they are not instructive because of the speculations you have about identification, who is taken in, who is taken out, about various of the issues that Ms Ralston has used as the techniques to try and come up with further robustness checks here?

A. No, there is a common thread which is that competition can be complicated and that the non-covered providers compete with the covered providers, directly as HIPs and indirectly through the effect on commissions and -- through the effect on PCWs and what they do, and every time Ms Ralston tries to perform a test she is making some restrictive assumption about the nature of that competition which I do not see the basis for, and so I am not surprised that none of them work out to be successful.

Q. Just to be clear, are you saying the assumptions that Ms Ralston makes in relation to each of those tests is actually unreasonable?

A. I think in general I would have to go assumption by assumption, but they are often possible, but not --

Q. Could I say unreasonable?
A. Yes, unreasonable, because we cannot rule out the -- they are not justified relative to alternative ways in which firms --
Q. That is not the question, Professor Baker. I asked whether or not the assumptions were unreasonable or are they reasonable but you do not agree with them?
A. They are unreasonable given the complicated way the competition could proceed. They are possible, but not reasonable to make for the purpose of conducting this test.
THE PRESIDENT: Would you be able to formulate assumptions that would satisfy you yourself that they were reasonable, or do you think that is just impossible?
A. I could not think of a way here. I could conceptually come up with a way, that is what I keep referring to about if we had a metric, about who is close to whom in competing, but I do not see a way to create that here.
THE PRESIDENT: Absent additional metrics, it is not possible to be done?
A. I believe that to be correct, yes.
THE PRESIDENT: Thank you.
MR BEARD: That is helpful. You can perhaps see, members of the Tribunal, the level of concern I have about not being able to cross-examine properly in relation to the

DCT study given the nature of the extreme conclusion that is being put forward.

Could I turn now to {A/7/38}, please. In paragraph 101 just read the fourth sentence which begins "Even":

"Even if the covered brands react shortly after the [wide MFNs] were removed, moreover, there is no reason to suppose that the entire response to the non-covered brands would be nearly immediate, nor that it would not persist beyond three months if the covered brands' reactions also persisted."

So you are there saying you do not anticipate a complete spillover because you think there would be a lag in reaction. That is right?

A. I am saying that competition is complicated and it could happen with a lag in reactions.

Q. Just going back to where we were this morning when you were justifying the 19-month period, you were very confident, were you not, that actually the shorter periods like 24 months or 12 months or 6 months when we were using those comparator tables were not sensible, so you were quite confident about time estimates then, but you are not confident about time estimates now, because there you were saying you need the longer time because there could be long lags in relation to reaction, were

you not?
A. We were talking this morning about adjustment delays, and the --
Q. It was a different subject, but just answer the question. You were talking about a certainty in relation to lags in competition justifying the 19 month either side, were you not?
A. In the promotions data that we were talking about this morning, if you look at the figure, I think it is 9.1 of the Decision, the way I read it it appears that there are at least six to eight months before the trend line goes back to roughly the previous average and some of the PCWs do not begin promoting for -- until after that time too, and in the context of -- and so therefore with promotions I believe that that is one piece of evidence that tends to suggest that some HIPs and PCWs adjusted not immediately. They adjusted with a delay. So --
Q. We are talking about the same protagonists, are we not?
A. Yes.
Q. In the spillover discussion it is all about the immediacy of the spillover. When we are talking about the time comparisons, actually there are long lags. Am I capturing that correctly?
A. No, we are talking about lags in the promotions and the possibility of lags here, and there are possibility of

lags in commission reactions for reasons we have not yet discussed as well.

Q. There are a range of reasons why there may well be lags and lower than full magnitude spillover even before going to Ms Ralston's tests, are there not, given that we have a limited number of wide MFNs, limited number of HIPs covered by the wide MFNs, they are free to compete, there are numerous other insurers that do not even sell through CTM, there are numerous insurers that sell only direct, and these could have an impact on how these matters work in terms of timing, do they not, and magnitude?

A. I was thinking about my answer to the first part of your question and missed the second, but let me try, I think this will answer you.

We are talking about the possibility of lagged -- two different things, of some firms taking time to make their initial responses and then additional potential lags in response to those initial responses, and some HIPs could react quickly and others react slowly, whether they are covered or not. So we are talking about different kinds of firms even within each group, and the PCWs can react at various times and that would affect when the HIPs respond to what the PCWs did.

So, when you put all that together, it is difficult to say what the timing would be between when the covered brands and non-covered brands would react as an a priori matter and that is a problem for several of Ms Ralston's spillover tests.

Q. We will deal with those perhaps in submissions, but can I just clarify one thing. I started off on the DCT market study. I explained that it was difference-in-difference analysis, and it did identify an effect. So in those circumstances, we have to assume that any spillovers that existed in that market were not such as to wipe out the putative effect because they had a positive outcome.

A. Assuming that this is a study of the sort where the spillovers matter, etc, etc, that I do not know --

Q. I completely understand.

A. -- that sounds correct.

Q. What Ms Ralston explains in her report is essentially that your account of complete spillover to render her zero or rather her non-statistically significant impact analysis flawed is simply not the plausible or reasonable analysis of all the information.

A. Can you please repeat the question.

Q. What Ms Ralston explains in her report is essentially that your account of complete spillover to render her zero or rather non-statistically significant impact

analysis flawed is not the reasonable or plausible analysis of all the information. I think you have just disagreed with that?
A. I believe that is what Ms Ralston argues, something like that. I do not -- well, I will stop.
Q. Thank you. I am going to move on to an adjacent topic, unravelling to some extent. I am going to put a statement to you to see whether you agree with it.
In a market in which products and/or suppliers are considered highly substitutable, even a few suppliers without a wide MFN clause may be sufficient to maintain some competitive pressure on commissions and consequently prices through the threat of customers switching away. Can we put it on the Realtime because it is a bit cruel to require to you try and remember that as I read it.
It is [draft] page 121, I think {Day10/122:6}. Thank you, I am going to stop talking so you can read it.
(Pause)
A. So with emphasis on the words "may" and "some" I think that the sentence would be correct.
Q. That statement is accepting that you can have unravelling even if you only have a few suppliers without a wide MFN clause?

A. It says "some competitive pressure", and perhaps it would be useful to say that when I hear the word "unravelling", I interpret that as meaning the prices become the competitive prices, that there is no adverse effect, so some competitive pressure does not mean that prices and commissions would completely be unable to exceed the competitive level.

Should I try that again?

Q. Yes, I think that might be worth another go because I think that one I am going to struggle to understand.

A. All right. So as I understand the unravelling argument issue, I understand Ms Ralston as saying, well, if we think about -- that if for some reason someone were to attempt to raise price above competitive levels, that would be unsuccessful because of various kinds of competition that she points to and that the ultimate outcome would be a return to competitive prices. That is what I understand Ms Ralston to be supposing. I am drawing the distinction between competitive pressure and unravelling here. That is that Ms Ralston's analysis is couched in terms of, well, the perfect cartel price would not be sustained. It would return to some -- prices would fall below the cartel level. There is a simulation result that she uses, that she talks about, where the price does not fall all the way back to the competitive level.

So it is possible she means less than complete unravelling, and if that is what she means, all she is really saying is that the competitive -- the result of the application of wide most-favoured-nation clauses could be a higher than competitive price that is not as high as the cartel price, and if that is her point, then I agree with it, but I thought she was making a stronger point than that when she used the word unravelling.

Q. Let us test the stronger point that we are talking about full unravelling, that we are talking about a situation where, because of the existence of a few suppliers without a wide MFN clause, in fact any pricing above the competitive price would be unwound by a process of iterative competition. So let us deal with that situation which I think is the more extreme position you are suggesting.

If we could just pick it up in Ms Ralston's first report, {A/5/76}. At 5.6:

"The unravelling of any harmful effects due to incomplete market coverage is also consistent with the economic literature. As set out in section 3, negative effects are found in the economic literature only when all suppliers ... are covered by wide MFNs."

I do not think you dispute that. I think you accept

that the economic literature does not deal with partial coverage. That is correct, is it not?
A. The economic models in the literature are --
Q. They are full coverage?
A. Have complete coverage, yes.
Q. Thank you:

"The mechanism is as follows: if coverage is incomplete, suppliers without wide MFNs can list lower prices on platforms offering lower commissions, maintaining the incentives for platforms to do so, and reinforcing the incentive for suppliers to reward such platforms with lower retail prices."

That is the mechanism that she is then talking about, and you can see she goes on to explain that further in 5.7 and 5.8; do you see that?
A. I see that that is what she says in 5.6, yes.
Q. If we could just go to your report {A/7/11}, please. I just pick it up at paragraph 37. Do you have it there?
A. Yes, I would like to have a look at my hard copy here.
Q. Sure.
A. That is fine, yes, I am fine.
Q. 37:

"... CTM's ... MFNs covered a substantial fraction of home insurance sales made through PCWs. The 32 covered HIPs accounted for roughly half of the HIPs using PCWs [and] 40% of the policies sold on PCWs ..."

Then we will be coming back to these statistics in due course, but then:

"Coverage was not complete ... That observation would be expected to affect the magnitude of any adverse competitive effects, but it would not be expected to preclude those effects entirely because the reasons to expect competition to soften ... would continue to apply."

So you are here saying, yes, you could get significant unravelling, but it will never be complete unravelling. Is that a fair summary of what you are saying there?

A. Yes, except that I resist the -- resist is the wrong word. The -- with the understanding that the term unravelling suggests certain dynamics which may or may not be the case, that is to say what we are really talking about is an equilibrium that ends with a higher than competitive price rather than necessarily supposing that the firms tried out a cartel price and then ended up having to reduce prices. These are -- the unravelling metaphor is just a way of describing why one might not get to the cartel price, but with that caveat, I agree we would end up with a less than competitive

price but not -- I would not expect it to be the perfectly competitive price.
Q. It could be above the competitive price level on your modelling, but it could be not significantly above the competitive price level. You would accept that?
A. Well, what do you mean by "significantly"?
Q. I ask you.
A. Okay. It could be above, and the magnitude --
Q. The magnitude could be very small, could it not?
A. The magnitude -- it depends on the circumstances of the industry, and what we are assuming about coverage and the like, and I do not have a way of saying what the magnitude would be, but it could be small and it could be large, yes.
Q. It could be small, but it could be very small as well, could it not, when you are talking about less than 50% coverage even on the CMA's approach?
A. I would have no reason to say it would be very small. I do not have any reason to know what the magnitude would be except that I believe that with more -- with less coverage it probably means lesser magnitude.
Q. Less coverage lesser magnitude. In response to your comments in paragraph 38 and indeed in 39 which talks about the cartel literature, Ms Ralston has expanded her analysis with a more detailed explanation of the

mechanism of unravelling. If we could go to {A/9/62}, please. If you could just read 3.26 through to 3.29, if you just call the page when you want it turning.
(Pause)
A. How far would you like me to read?
Q. I just wanted to go down to 3.29. She is just drawing on the literature on effectiveness of incomplete cartels. I think you understand that that is what she is doing. I am just setting the scene as to what she has done. (Pause) You would have seen this before.
(Pause)
A. Yes, you can move on to the next page.
Q. If we could go on to 3.32 {A/9/65}, I am not going to take you through on to all of this, but 3.32 through to 3.48 is where Ms Ralston in this second report responds specifically to your various concerns.
A. I am sorry, how far did you want me to read?
Q. I am not going to ask you to read this. You will have seen it because this is the response to your concerns. There is only one point I want to put to you essentially in relation to this. Yesterday, Ms Ralston was taken to the Inderst, Maier-Rigaud paper which you will be familiar with, {F/678}, if we could. I have lost in my notes the relevant reference. I may have to turn to see if I can get that.

Whiles we are looking for the reference, you will recall that this is the paper which was put by Ms Demetriou regarding the question whether or not the theory of unravelling that is dealt with here in this umbrella effects paper depends on heterogeneity, do you recall that?

A. I recall that Ms Demetriou raised this paper with Ms Ralston.

Q. Yes. I can probably deal with it without going to the reference on that point, then.

In this market I think we have been told that price is a key driver of competition and that you have already emphasised, I think, in a number of your responses that customers predominantly choose their home insurance on price on the PCWs, so in those circumstances it would be correct to see this as a broadly homogenous product, would it not?

A. Well, there is differentiation in this market. You know, the PCWs are different and there are different brands that the HIPs offer. On the other hand, it is also true that these are price-sensitive customers, and they might not -- some might not care so much about the difference between brands and care more about price. So there is some differentiation and not extreme differentiation, I guess.

Q. As far as you are aware, the CMA did not make any finding as to whether or not you should treat home insurance as sold in these circumstances a more or less homogenous product, did it?
A. I do not recall.
Q. If we could go to {F/372/5} --
MS LUCAS: Mr Beard, I do have the references to the article if you want them.
MR BEARD: Yes, it is page {F/678/7}, thank you. That is really kind of you, but I think unless Professor -- do you want to go back to the Inderst, Maier-Rigaud paper, the quote that was put?
A. No, that is fine.
MR BEARD: Thank you. But thank you very much, Ms Lucas.
I will indicate from the key that here we are dealing with Liverpool Victoria (LV=). If you look at question 6 -- this is a request for information from the CMA -- you will see the answer given by this HIP, and I just note in particular the third paragraph of the answer under 6(a). (Pause)
Here I think you see a HIP talking about this as an homogenous product and indicating its perception in the circumstances which would of course mean that the modelling described by the Inderst, Maier-Rigaud paper as the mechanism of unravelling would be all the more

applicable. Is that broadly fair?
A. No. Well, that is to say you are not grappling with the reason that the modelling in the umbrella effects paper is not applicable or at least a more important reason. That is you are working on the differentiation question, but the modelling in the umbrella effects paper, if you apply it to the industry we are interested in here, it says though it ignores the effect of the wide MFNs on commissions, there are not PCWs -- it is in effect there are not -- from the point of view of that model it says there are higher input costs.
Q. I understand, but I am just dealing with a very narrow point.
A. May I finish, please?
Q. Yes.
A. What I wanted to say was you would not have -- under the CMA's view of how competition works, or how the wide MFNs harm competition, a number of the mechanisms are -- lead to higher commissions, and then those are passed through in whole or part to higher prices, and the mechanism that is emphasised in the umbrella effects market is in effect competition among the HIPs, but if the whole commission level is higher, the competition among the HIPs cannot -- will not reverse that. The whole price level will be higher.

So that whole dynamic is missing from the way in which the umbrella effects article is being applied.

In addition, the strong implication that Ms Ralston takes out of the umbrella effects article that you cannot have anti-competitive effects when a firm -- one firm acts unilaterally or some firms -- here CTM I guess acts unilaterally and lets all of their rivals go along is also inconsistent with what industrial organisation economists have understood since the mid-1980s at least and I talk about that more in my report.

Q. Yes, but I was just asking you a very narrow question because it was put to Ms Ralston that actually you are dealing with non-homogenous products, the Inderst paper that was put to Ms Ralston.

I am putting to you that in fact there is evidence that they are homogenous products and, therefore, that objection to the application of the Inderst paper is simply not sound. You are coming up with different objections, are you not, Professor?

A. Well, the -- for the most part I am coming up with different objections, yes.

Q. Thank you. Before I move on to precision I want to pick up a couple of points on incentives to promote.

In her first report just at {A/5/164}, at 9.28, do you recall that there had previously been some theoretical modelling submitted during the investigative stage, that is mentioned in 9.28. If we go to 9.29:

"The intuition is straightforward -- the commercial success from any promotional deal relies on the profit earned from sales gained (i.e. customers 'stolen' from other HIPs/PCWs) exceeding the profits lost from lower margins on existing sales (i.e. customers 'cannibalised')."

I do not think you disagree with the intuition for considering the potential incentives to promote on the part of HIPs in particular, do you?

A. Do you mean in Ms Ralston's model, whether this is an intuition that she relies upon?

Q. Do you disagree with this intuition?

A. I need to read it slowly now, if --

Q. Please. (Pause)

A. I think this paragraph is identifying the elements of a calculus a firm would consider, yes.

Q. She says at 9.28 that this is why promotional deals are often not financially attractive to PCWs and HIPs, because of the cannibalisation effect. You understand that?

A. I understand that is her position, yes.

Q. In your report, can we look at it {A/7/22}, paragraph 58. You say:

"Notwithstanding the evidence that ... [wide MFNs] suppressed promotional discounts [which we have already been dealing with and I do not accept], Ms Ralston suggests that PCWs and HIPs would have had little or no incentive to negotiate promotional discounts even absent [wide MFNs]."

This might be the reason why we end up looking at quite small numbers.

"Her argument is unpersuasive [you say]. The three closely-related theoretical economic models on which it relies purport to show that, in cases calibrated to reflect the home insurance marketplace, that (tacit) coordination dominates competition for both HIPs and PCWs."

You are suggesting that there is tacit collusion here; is that right?

A. No, not -- so what I mean is that she sets up a -- I seem to recall that in the third Oxera report, that is the underlying analysis here, at least some of the analysis is set up in a sort of prisoner's dilemma framework, and what she is saying is that the dominant strategy will be not promote, not promote, as opposed to both firms promoting, in like say one of the models probably, but probably the one with the -- whatever the first one was, the HIPs, and that is how one might describe in a two by two matrix set-up a coordinated outcome that would be released tacitly, not through an agreement.

Q. I see. So you are accusing her of putting forward tacit collusion suggestions. Let us have a look at her second report {A/9/89}.

I was picking it up at 4.39, thank you:

"First, the theoretical analysis undertaken by Oxera modelled one-time interactions between PCWs and HIPs."

So that is the modelling that you are talking about.

"By definition, tacit coordination cannot arise in models of one-time interactions ..."

You accept that, do you not?

A. No.

Q. You think that tacit coordination does not require repeated interactions?

A. So the way -- there is a -- there are a lot of economic models where coordination arises out of repeated interactions, and these are these oligopoly supergain models and you do not want to hear about them. That is today, probably in the theoretical literature, the common way of modelling coordinated effects, but if you want to just think about it in the context of a two by two prisoner's dilemma which was I think what I got out of that Oxera report, you can understand that the way of modelling is if the value of defection in that model -- firms engage in practices that reduce the value of defection in that model then it is no longer a dominant strategy for both to compete, and there can be an equilibrium for them to end up in the less compete box, and that is fairly described as a model of tacit coordination.

I think that story is in -- it is probably in the Salop article that I reference in my report, and there is also -- and I will not go into it further, but you can have equilibria that are not -- that some people might call coordinated, and one could describe them in different language, with what it is in the older economics language non-zero conjectural variations. So firms interact in ways in which they expect their rivals to respond in various ways, and the result is you can end up at a less than competitive outcome in just a one shot, one time interaction that some people would describe, not necessarily unfairly, as tacit coordination.

Q. No, I think "not necessarily" fairly captures it very nicely, Professor Baker.

Now, at this point, before moving on, I wonder if this is the opportunity or the moment where Professor Ulph might want to raise issues in relation to

Johansen & Vergé, Professor?
THE PRESIDENT: Yes, it is {F/565}.
PROF ULPH: Yes, I am happy to do that.
A. Is there a way to do a shade here with the way the sun is coming in?
MR BEARD: I am getting the same plea from behind me.
THE PRESIDENT: That is an entirely fair point. (Pause)

Questions by THE TRIBUNAL

PROF ULPH: Professor Baker, were you in court yesterday when I put questions to Ms Ralston about this paper?
A. Yes, I do.
PROF ULPH: What I could maybe do today is put the same question to you, but maybe somewhat more rapidly than I put to Ms Ralston, so we do not take too long.
So we can move first of all to page {F/565/8}.
A. I think I could help shortcut the first part by telling you that I thought your interpretation of all the models was correct.
PROF ULPH: Oh, okay, that helps a lot.
A. Save that for mathematics, yes.
PROF ULPH: Then if I can come to the follow-on questions I wanted to ask you, if we take all those results together in the paper, the three theorems that I took Ms Ralston through, do you agree that one implication in order to understand the effect of wide MFNs, it matters

greatly whether the counterfactual is one of no MFNs of any kind or of a situation in which there is an extensive network of narrow MFNs between virtually all PCWs and virtually all HIPs?

A. The answer is no, I do not agree, and it is because I do not find this model applicable to understanding the industry that we are dealing with, and I would be happy to explain why.

PROF ULPH: Yes, please do. I want to understand your position. Please do.

A. First, from a broad perspective there is not much reason to think that the -- looking at a high level there are reasons to think that the model is not describing what is going on. For example, the CMA said in the Decision that there is little evidence of -- or none, perhaps, it was either little or none, of firms, HIPs, attempting to threaten to undercut prices by delisting as a way of negotiating lower commissions.

If one believes this model one would expect to see the -- and to view it as -- and if -- let me back up. The way I understand the model applying is that the authors accept the Boik and Corts and Johnson idea that absent the force of -- absent the threat of delisting and commission negotiations the wide MFNs would have a -- lead to high prices and high commissions and that the threat of the delisting is used to -- sort of in a countervailing way by the HIPs and the result is you could attenuate the adverse effect or, under some conditions in their model, reverse it, and so if it were having the latter effect, really either effect, one might expect to see CTM's commissions lower than the average commissions on the other PCWs, and there is a chart in the Decision showing that during the relevant period CTM's commissions were on average higher, and also if this were an important mechanism by which the HIPs were able to achieve lower commissions than otherwise in the presence of wide MFNs one would expect to see the HIPs themselves favouring having wide MFNs and CTM and the PCWs not wanting them when my understanding of the way the Decision describes it, it is CTM that wants the wide MFNs.

I think there is a reason in the model why -- in terms of the math of the model, you know, why you get these -- why it does not seem to apply to this industry, and that is because of the way in which direct sales are modelled and the cost of direct sales is modelled by Johansen & Vergé, and the part of the delisting threat is that when the HIP delists from the PCW that it wants to negotiate the lower commission with, it will lower price on the other PCW and on direct sales, and the direct sales are very profitable in the model because the way the costs are modelled is that costs are identical for the HIP whether it sells through directly or sells through the PCW, with the exception of the HIP has to pay a commission to sell through the PCW.

That makes costs low on direct sales, but when you think about a delisting threat the idea is in the model with the way in which the two channels are thought of as -- I think it was symmetric differentiation in the assumption in the demand function or something like that, there is a reasonable amount of -- a fair amount of consumer substitution to direct sales, you know quite a lot to make this -- which is not just to direct sales to the other PCW, but to direct sales. That is really attractive to the HIP because the costs are so low and that is what makes the threat to delist credible, but in our market in order for the cutting -- charging a low price for direct sales to attract a lot of business, my interpretation of what I got out of the Decision is that the HIP needs to market the direct channel, not just cut price and leave consumers to find a direct channel. There is a lot of consumers single homing, etc.

The Decision says that when you look at the brands who have a direct channel that are seriously marketing, which is I believe what one would need to do in order to expand sales substantially by cutting price on the direct channel relative to the PCW, when you market the customer acquisition costs for the direct -- for the brand selling through the direct channel, are higher than the customer acquisition costs for the brand -- the same brand selling through the PCW, which is quite different from the picture of costs that the model assumes here.

I believe there were some -- I remember a paragraph where some of the HIPs talked about having low customer acquisition costs on direct sales, but I do not think they were talking about doing brand marketing. I think those were ones that were not heavily involved in marketing.

So the picture I have is that the mechanism here is unlikely to apply in a substantial way to give credibility to the delisting threat because the shifting to the direct channel is not so attractive, and when you get to the narrow MFN, as compared with the wide MFN, the math is not identical but the equilibrium is the same, my interpretation is that the narrow MFN means that -- is that in that situation you still have the delisting threat of shifting to the direct sales, and in the model that is very attractive, and so that is why the equilibrium seems to end up in the same place.

So I think if I am right that the delisting, that in our setting the costs for brands that want to shift sales to direct sales are actually high because they also need to market extensively, then I think that mechanism would not work either, and so I would not take from this model the lesson that it is important to have the counterfactual not involved in narrow MFN.

PROF ULPH: Okay, let me see if I have understood you fully. One part of your argument seemed to be the one thing you think is missing in the model is any real treatment of advertising the promotional costs by both PCWs and by direct providers or HIPs, you think that part of the story has not been modelled in the Johansen & Vergé paper, and that could generate cost features which could sufficiently complicate their analysis that you may no longer get the conclusion that the equilibrium in which all PCWs have a wide network of narrow MFNs is equivalent to the one in which you have wide MFNs or PCWs. You are saying that equivalence between the two situations may not hold if you factor in advertising costs. Have I understood you correctly?

A. Almost. What I said was -- you have to take it in parts -- that if the feature of the modelling that you and I are talking about of how the direct sales costs are modelled had been different, then you would not get the delisting threat working, perhaps not at all, in the original model, even putting aside narrow MFNs, or would not work anywhere near as powerfully as it does in the calibration if it did work, and that that is my guess as to why we do not see it being used, etc, as I started out.

I also suspect that the result about the narrow listing, narrow MFNs is the same as the result with the -- the narrow MFN and the wide MFN results are the same is attributable to the same modelling assumption about costs and do not know what would happen were that modelling assumption varied, but it does not matter because I do not think the model would apply if the modelling assumption were different about the costs of the direct sales.

PROF ULPH: I am not sure whether that quite bears on my question, which is -- how does that affect the question of what the right counterfactual is? In a world where there is absolutely no MFNs of any kind and you then introduce wide MFNs, why does that advertising argument that you use affect the fact that the conclusions could be different if your counterfactual is no MFNs of any kind as distinct from the case where the counterfactual is a substantial network of narrow MFNs. That was my original question. My original question was: is the conclusion to draw here that it matters what your counterfactual is, whether the counterfactual is one of no MFNs of any kind or the counterfactual is one in which there is no wide MFN, but still an extensive network of narrow MFNs; are you saying that conclusion, (inaudible) still it might not matter what your counterfactual is?

A. I think I got that question. Let me try and answer and see if this helps.

I think I am saying that I would not take -- the lesson this model leads you to propose for the purpose of conversation is that there would be a difference in the outcome -- in the counterfactual with narrow MFNs and without, and my view is that for the reasons I gave before I do not believe this model is a good fit for the industry, you know, with the assumption about the direct sales costs, etc, and so I do not believe I would take a lesson about the difference -- potential difference in the counterfactual between having narrow MFNs and no MFNs from the model.

If I were thinking afresh about the counterfactual and I was thinking about narrow MFNs and Mr Beard would say what I am about to say is speculation which would be fair, my first thought would be to think about narrow MFNs in the context of showrooming and free-riding and that they would be potentially solving a free-riding problem for some of the firms, but I do not know if that is the case, but it would be a very different explanation for narrow MFNs, and in that case one would expect to see the narrow MFN in the counterfactual world if it were important for reducing the free-riding.

PROF ULPH: That is still not quite the question I am trying to ask you. I am not asking you whether it is reasonable to have narrow MFNs or not. I completely understand that this is not a case about whether or not narrow MFNs, anti-competitive effects. I completely understand the CMA's position, which I think is in line with what you were saying, is that they regard narrow MFNs as not having anti-competitive effects because they do solve this problem, or they are a mechanism of solving the problem that you are addressing.

My question is more given that all parties agree that narrow MFNs exist both in the current situation and would also exist in the counterfactual, because the counterfactual is all we remove is the wide MFN, my question is does the fact that there are narrow MFNs in the counterfactual, could that have a bearing on how we think wide MFNs might operate in the market and generate anti-competitive harm?

So is it important to take account of the presence of narrow MFNs both in the fact that they might conceivably generate the same outcomes as you are getting with the wide MFNs, but also, even if that is not the case, the mechanism by which wide MFNs generate anti-competitive harm could depend on the presence of narrow MFNs, and that is what you have to understand; would you agree with that?

A. That has about as many parts as my answer did, so I apologise.

PROF ULPH: I apologise.

A. So I may have missed something. The --

PROF ULPH: Would it help if I put the question to you again?

A. Yes.

PROF ULPH: Given that the counterfactual is one in which there is an extensive network of narrow MFNs, to what extent are the claims that are made about how the effects arise on wide MFNs and the evidence provided to support those claims, to what extent are they robust to that presence of a network of -- a wide network of narrow MFNs? Do you agree that is an important question to ask, about how robust any conclusions are, to the presence of narrow MFNs?

A. Let me try and answer and see if you think this is responsive.

If I thought that the Johansen & Vergé article applied I would think that it would be important to understand -- you know, to decide which -- whether the narrow MFN -- the counterfactual includes a lot of narrow MFNs or not. If I thought that the narrow MFNs were a way to reduce free-riding then I would think it would not matter, we would have just seen what would happen would be we would get the mix of narrow MFNs that would sufficiently reduce free-riding to preserve platform viability, etc.

There was a third piece of your -- so I would not care at that point. The third piece was does the anti-competitive mechanism turn on whether there is a narrow MFN or not in the counterfactual, and I think in that -- with the showrooming free-riding story I do not think it does. I was trying to think about that as you were talking and I have not come up with a reason to think it would. So I will tentatively say I do not think that it should matter.

PROF ULPH: Okay, but do we have any papers on the actual literature that tell us that it does not matter?

A. We have largely the universe of paper, the relevant papers, in front of us, you know, between the reports. There is a little bit more but we see what there is.

PROF ULPH: On my reading those papers, most of those papers essentially assume the counterfactual is no MFNs of any kind. I have not come across a paper where the counterfactual is a substantial network of narrow MFNs, nor have I seen a paper that proves that having wide MFNs in that situation is identical to having wide MFNs in a case where there are no narrow MFNs in place. That is the point I am -- or are you aware of any point?

A. I think the Wang and Wright paper about the showrooming I would think, but I think we should both go back and check, would -- says something like you can get the -- there is a difference between narrow MFNs and wide MFNs, and when you have narrow MFNs that prevents the free-riding without risking competitive harm. When you have wide MFNs, it gives you both, preventing free-riding and creating the possibility of the competitive harm as in Boik and Corts and Johnson, and they have just traded off there.

I do not recall whether -- so I think they imply a world with narrow MFNs is better in their model for consumers than a world with wide MFNs and that the world of narrow MFNs is better than no MFNs. In that sense I think they are comparing those settings, but I am not sure if that is directly responsive to your question, but I think it might be.

PROF ULPH: Okay, I think I understand your position now, so thank you very much.

THE PRESIDENT: Thank you. Professor, if it helps, because this is not a memory test, were you to have any more concrete thoughts having a chance to review the papers after you have left the witness box, you wanted to put a very short note in as to why you consider this paper not to apply in the present circumstances because I appreciate that there is a huge amount of literature, if you were to do that then I would give my permission for to you do that. It probably would entail Ms Ralston having a right of reply, but I would not want you to feel that your answer here without any ability to look through the papers is the final answer. So if you wish to do that feel free.

A. Thank you for the generous --

THE PRESIDENT: I am sure you will be guided by your legal team.

MR BEARD: In relation to the other parties apart from Johansen & Vergé.

THE PRESIDENT: Exactly. The point was the professor thought the paper does not apply in this case.

MR BEARD: I completely understand.

THE PRESIDENT: It would be helpful to know. So thank you. I have had a note that the shorthand writer entirely needs a break, and I sympathise. We will rise. We can go on a little beyond 4.30, but I am quite reluctant.

MR BEARD: I think everyone is going to be quite reluctant on a Friday afternoon. I think you may in the end need to give me some degree of leeway about the extent to which I have put certain points to the witness which I think in these circumstances is going to be less significant given that we have the joint statement and we have an interaction. If one can treat observations by Ms Ralston in the statement as the formal challenge to Professor Baker's position, then I think it may be I can cut things short.

THE PRESIDENT: I think I made the point to Ms Demetriou and I make it with you, but you are right to ask, and nail our colours to the mast. The breadth of the disputes and the detail mean that it is really not possible to put your whole case, and we hope that you will put the high points, but we know that you cannot put everything and that will be fully taken into account when we review the submissions and write our judgment.

MR BEARD: Well understood.

THE PRESIDENT: Very good. We will rise until 4.05.

MR BEARD: On that basis, I can get through in 25 minutes, I hope.

THE PRESIDENT: Very good.

MR BEARD: If I may, without in any way appearing

impertinent, some of the answers may have been perhaps longer, Professor, than you may originally have intended when you started on them. If during the 25 minutes we might try and compress the scale of answer, that would be extremely helpful, I think, to all of us.
A. I will try.
MR BEARD: Thank you.
A. But I will also try to make my views clear.
THE PRESIDENT: That is absolutely right, Professor. Do not worry about the time. If I think you are in need of hurrying up, I will hurry you up.
A. Okay.
THE PRESIDENT: So 4.05, thank you very much.
(3.51 pm)
(A short break)
(4.06 pm)
Cross-examination by MR BEARD (continued)
MR BEARD: If we could call up {A/9/140}, please. You were in court when Ms Ralston was cross-examined in relation to this confidence interval bell curve. It relates to her econometric analysis of promotional deals and you will know from her report, it is on the preceding page, what that explains, and the red dot, I am not going to say the red dot is you but the red dot is the upper bound that you selected as evidence of economically

significant effect whilst Ms Ralston looks at the central point, estimated coefficient.

I am going to cut to the chase, I hope. We can immediately see from this bell curve there is quite a lot of information there about the results in the confidence interval. It is true we cannot say we can rule out 1.49 with 95% certainty, the same is true of minus 98, but I think we can say that 1.49 is much less likely than zero on this bell curve. You would agree with that statement?

A. What we can say is that the -- loosely speaking, the probability that you would observe 1.49 or greater in the sample if the true number were zero is lower than the probability that you would observe 0.26, say, than if the true number were zero.

Q. Yes, or the probability that you would observe zero?

A. That is correct on this diagram.

Q. Notwithstanding your JD, you are here as an economic expert, and so naturally you are not being asked to place these results into a legal framework, but obviously we have to and the Tribunal has to, and the CMA accepts -- this is common ground -- that it must prove on the balance of probabilities that the wide MFNs had an appreciable adverse effect.

So in principle would you say that the information displayed by the bell curve could be informative to someone applying that framework, the legal framework for assessing whether something was true on the balance of probabilities?

A. I am trying to think how to answer quickly for you, Mr Beard. I am having trouble. (Pause)

I think the information about the point estimate, the confidence interval, the range of coefficient estimates that are within the 95% confidence interval all could be relevant to assessing the likelihood of harm to competition in the particular application. This was promotions as I recall.

Q. Thank you. If we can go to Ms Ralston's report at {A/9/132}, moving on to look at goodness of fit data. Paragraph 5.78:

"The goodness of a fit of a statistical model is relevant to assess the suitability of the model in question to describe the data observed empirically."

I think you agree with that and you agree that using R squared techniques is the accepted and orthodox way of doing that; correct?

A. With the emphasis on "relevant", I would agree with you. I do not think it answers the precision questions, for example, that we have been discussing.

Q. If we go on to page {A/9/134}, two pages on, we see the

R squared table -- adjusted R squared tables that Ms Ralston produced. They are adjusted from your R squared calculations, and what she does in very broad terms is she makes adjustments in relation to your R squared terms because she says you are not accounting for the explanatory power of brand-specific or brand PCW-specific characteristics.

I think you accept that brand-specific and brand PCW-specific characteristics are relevant, do you not?

A. Are relevant for what?

Q. For the proper adjustment of the R squared calculation in order to assess the explanatory value of the relevant statistical analysis.

A. If you wanted to have an R squared calculation that includes both effects you can do that, and that is what Ms Ralston has done.

Q. There is one other issue I wanted to raise in relation to precision. You say at paragraph 109 on page 41 of your report, so that is {A/7/41}, you say:

"In a general way, one would expect the effects of removing CTM's [wide MFNs] to be measured imprecisely when there is a great deal of variation in the outcome studied (such as price or commissions) not explained by observable variables, and when the outcome itself is measured with error in the data."

Just to be clear, the error in the data that you are talking about, that is going back to the consumer intelligence data set here, is it not?

A. I think I was making an econometric proposition, but one source of error in the pricing data is the consumer intelligence data, if that is what you are saying.

Q. Yes, I am not going to revisit that.

Now, failure of the common trends assumption, you set out some criticisms of Ms Ralston in your report.

Just to be clear, I think you accept that none of those criticisms relate to the promotional deals analysis; correct?

A. Correct.

Q. You are hypothesising the possibility of a common shock but different responses by different groups in the common trends assumption discussion; correct?

A. I guess testing for rather than hypothesising in the pre-WMFN removal period, and different average effects, but, yes, with those qualifications, I agree.

Q. In cross-examination, Ms Demetriou was positing to Ms Ralston a common shock in the period before her 12 months period that she took data from which she said enabled her to control for these issues. You are not suggesting that there was a common shock 14 months beforehand or shortly before Ms Ralston's data set was

collected, are you?
A. I think if you look empirically at some of the significant lead coefficients, if they were before however long a period you were describing, then they are presumably reflecting the result of a differential effect to a common shock.
Q. We will leave that to Ms Ralston's evidence. Did you explore whether or not there were empirically common shocks that could be identified in this market or tested?
A. I did not do any analysis other than look to see whether -- other than the leads test that we talked about.
Q. I see. So you did not for instance look at significant IPT rises -- sorry, insurance premium tax rises?
A. No.
Q. No?
A. I did not put those into a regression or think about them.
Q. I am just asking what you did and what the CMA did in relation to this.
A. Yes.
Q. I have some questions on the Nobel prize winners papers, but I am going to have to leave those. They were fun.
Absolute price analyses. I am just going to refer

to the relevant points of debate between you. Ms Demetriou did not cross-examine in relation to this. You deal with these matters on page 58 of your report, 148 to 153 {A/7/58-61}. Ms Ralston responds at 5.161 to 5.195 {A/9/156-165}. The same in relation to the one year of data for all the PCWs' commissions analyses, Professor Baker paragraph 130 through to 135 of his report {A/7/52-54}, Ms Ralston's responses paragraph 5.122 through to paragraph 5.130 {A/9/146-148}.

With that I am going to ask you one or two quick questions about one or two pieces of your writing.

As we canvassed at the start you have written a book relatively recently, The Antitrust Paradigm, and you have said in that book that antitrust doctrines and enforcement actions once thought adequate to protect competition are proving insufficient in your view, and so you want that problem fixed.

Obviously your book is focused on the US, I should predicate it, albeit that it does refer to some European and UK matters.

You argue that a substantial and widening market power justifies more interventionist rules and judicial presumptions in the US. That is one of the key themes of your book. That is right, is it not?

A. That is correct.
Q. Actually, although there is not that much in your book on platform MFNs, you do touch on it I think in relation to Amazon as I recall, but you did write a paper in the Yale Law Journal talking about antitrust enforcement against platform MFNs, and I am not going to work through it all in any detail. So the Tribunal has it, it is at {F/729}.
I certainly do not want to be reductive because it is a very interesting paper, as is your book, but you conclude that platform MFNs generally harm competition except in narrow circumstances in which free-riding concerns are especially strong.
So when you talk about free-riding concerns, is that a broad term covering things like showrooming issues that you referred to earlier in the discussion with Professor Ulph?
A. Yes, that is correct.
Q. Right, so platform MFNs generally harm competition except in narrow circumstances in which free-riding concerns are especially strong. So when you have strong free-riding concerns, there is a collateral positive competition benefit of having a platform MFN; is that broadly what you are saying there?
A. Yes.

Q. You argue for more vigorous antitrust enforcement against them at least in the US, but just capturing that idea that you have set out in the Yale Law Journal, what your broad position is in relation to MFNs is the nature of them is that they are a sort of form of coordination that can be regarded as harmful to the proper functioning of normal competition unless you can show there are positive countervailing benefits. Is that fair?
A. Well -- so I will accept that it is my article, though it is coauthored.
Q. Yes, that was unfair of me to your coauthor.
A. It is. She would be the first to tell you.
Q. No, no, absolutely. I shall not demur on that and I apologise to her at a distance.
A. So the general perspective of the article is that one should be sceptical in general about platform MFNs and particularly -- I guess it was wide more than narrow I think we talk about --
Q. Yes.
A. -- and that we call for essentially greater scrutiny of it, but I do not think we said that they are all necessarily bad, but we are sceptical.
Q. You do not say they are necessarily bad, but you are essentially saying they are bad unless you can show

positive countervailing benefits, are you not, in broad terms?
A. I think we are saying that one should put more effort into looking at them than the US system has done because the literature -- the economics literature suggests that they are bad in a general way.
Q. Bad by nature sort of thing?
A. No, not by -- no, I think you are using a legal term in the UK law or the EU law when you say that. It is just that the way I read the literature, that there is a -- the economic literature -- there is a tendency for these to harm competition, and that is a concern that has not been recognised in the US as much as it has in Europe, and that is what we say in the article.
MR BEARD: We will leave that for the moment. Thank you very much, Professor. I do not have any further questions for the Professor. Thank you very much.
THE PRESIDENT: Thank you. Professor, do you have any other questions?
PROF ULPH: No, I do not, no.
THE PRESIDENT: Neither do I, Professor. Ms Demetriou, any re-examination?
MS DEMETRIOU: I am going to be very quick.
THE PRESIDENT: Right.

Re-examination by MS DEMETRIOU

MS DEMETRIOU: Could we please get up the [draft] transcript at 103, line 7.

I think the page numbers may be different to the page numbers as I was going through it which is difficult. Anyway, I am going to ask the question without going back to the transcript because I think the page numbers have changed.

Mr Beard put to you that in your view, he put to you the question whether Ms Ralston's analysis is only undermined in your view if the spillover effect, if the effect on non-covered brands is equal in magnitude to the effect on covered brands, and you started to answer that question and you introduced the idea of precision in your response. It would be quite helpful if we could find it. The reason I am coming back to it is because I think you did not actually then ever come back to the explanation of why precision might be relevant to that point and I wanted to give you the opportunity to comment on it if you want to comment on it further.

Do you understand the point I am putting to you?

A. I think so. I think this was one of the times I was less than articulate -- one of the many times, so I apologise for wasting everyone's time.

Q. No.

A. As I recall, Mr Beard wanted to establish that --

essentially that one could have -- the spillover effect could lead to an underestimate of the adverse effect without making -- without making -- leading to a zero effect, estimating zero effect, and as a -- and I accepted that, but what I was trying also to say is that -- and it had to do I think perhaps with the way he put the question -- that if you are comparing Ms Ralston's two estimates, that you need to not just -- and that is her primary estimate and the one in her spillovers test where she says the one in her spillovers test ought to show, well, let us say less adverse effect, and she may find, well, there is no difference, and maybe there is no difference but maybe there is, and it is hard to see because of the imprecision of both of those estimates, and I think that was really the only point I was making about the precision in that context.

Q. If coefficients are measured with imprecision, so if coefficients are measured with imprecision, then what will be the effect, if any effect, of a downward bias, even if that downward bias is incomplete?

A. Well, it makes it -- the -- so in the primary analysis, the downward bias is independent of the precision. The precision makes it hard to know whether we are seeing a zero or an economically significant number. We do not really know what number we see, but if we could measure

it precisely, it would be too low. If we are comparing the two -- I have lost the thread of your question.
Q. Let me ask it again. If you have downward bias say from spillovers, so let us take downward bias from spillovers, and that downward bias is not complete, so it is an incomplete downward bias, then what effect if any does that have on the analysis?
A. Oh, it means that the entire confidence interval is shifted in the direction of zero so that the -- we might find, if you remember on the bell curve, the extreme point that I say, well, look, this includes -- this confidence interval includes economically significant effects as big as the one that was marked with the red circle there, but if there is a bias in estimating the effect in the first place, the whole confidence interval gets shifted most likely, and it could go in the direction of making that upper bound low although actually what happened also depends on what happens to standard errors, so that is a little more complicated than what I just suggested, but one would not assume that -- you cannot assume that when I say look these figures are within the -- these economically significant values are in the 95% confidence interval so therefore the regression is uninformative, you should not assume that those are the only economically significant

estimates that are ruled out by the regression, it could even be larger ones and we just would not know.

MS DEMETRIOU: Thank you very much. I do not have any other questions in re-examination.

THE PRESIDENT: Thank you very much, Ms Demetriou.

Professor, thank you very much. We are very much obliged to you for your assistance, and we wish you a safe journey home. So thank you very much, you are released.

A. Thank you, sir.

MR BEARD: I think that draws to a close both proceedings for today and evidence. I think all on this side of the court at least are looking forward to the weekend and light relaxation and then we will be providing our closings on Monday in readiness for the oral closings beginning on Tuesday.

THE PRESIDENT: I had one point on that. We are aiming for three days of closings, oral closings.

MR BEARD: Yes.

THE PRESIDENT: We have half a day on Friday as it were spare. What I was wondering was whether we should start perhaps earlier than usual on Wednesday and do Wednesday, Thursday, Friday, perhaps sitting longer days, but give you a little bit longer to submit your written submissions. Instead of Monday you could do it

Tuesday lunchtime.

MR BEARD: If that gives the Tribunal sufficient time too digest, I imagine there will be metaphorical whoops from different corners of the court. So can I just turn and check?

THE PRESIDENT: Please do.

MR BEARD: I am getting very clear messages from all of those behind me.

THE PRESIDENT: Ms Demetriou, are you in a similar position?

MS DEMETRIOU: We are in a very similar position, sir, but I am conscious that that places an immense burden on the Tribunal in terms of reading.

THE PRESIDENT: We will plan for a late evening on Tuesday.

MS DEMETRIOU: We are extremely grateful.

MR BEARD: We are most grateful, thank you.

THE PRESIDENT: Frankly, we think you need every assistance that we can within reason give, and we know that we will benefit because it will make your submissions all the better, but I really do not like putting huge and impossible burdens on the parties and you have a very large amount of work to do, so anything we can do we do very willingly.

MS DEMETRIOU: Thank you very much.

MR BEARD: Enormously appreciated, thank you.

THE PRESIDENT: What we will do is we will think about how early we sit on Wednesday. I am not going to say anything now. We will presumptively say 10.30, but the parties can think whether they would like to have more time, because you can expect we will be quite interventionist in our questions, and we find that very helpful, but the parties who have to keep an eye on their clocks find it correspondingly less helpful just for that reason.

So we will say 10.30 on Wednesday, but if you want to say anything before 9.30, then we will hear you loud and clear, but we will leave it there for you to think about.

MR BEARD: That is very kind, and perhaps I will liaise with Ms Demetriou, because we can discuss the -- because the timing was one and a half days, one and a half days, plus a relatively brief, comparatively brief reply. I think we are probably going to want to stick to that sort of timing, but precisely where we cox and box the starts and ends I think we should discuss.

THE PRESIDENT: I agree. We just want to radiate a degree of flexibility to the parties.

MR BEARD: That is very helpful.

THE PRESIDENT: So presumptively 10.30 on Wednesday, but we will adjust that as necessary. Thank you all very much. I will not wish you a good weekend, because you are obviously not going to have one, so thank you very much.

(4.33 pm)

(The hearing adjourned until 10.30 am on Wednesday, 17 November 2021)

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