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BSkyB

Average Pay TV Revenues per Subscriber across Europe

A review of LECG's response
of 29 February 2008

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EXECUTIVE SUMMARY

1. LECG have undertaken an econometric analysis comparing country-level data on “average revenues per subscriber” (ARPS) of pay TV operators across Europe (LECG’s “First Report”) in the period 1997 - 2005.¹ Interpreting ARPS as a measure of pay TV prices, LECG claimed to have found that – once adjusted for differences in quality – pay TV prices are significantly higher in the UK than elsewhere. Moreover, they claim that this is essentially caused by the market share of the “leading DTH firm”.
2. We showed in an earlier report (dated 29 October 2007 – CRA’s “October Report”) that LECG’s econometric work was fundamentally unreliable, as there are serious flaws in the approach, the data and the interpretation of the results they obtain. Indeed, we explained that once LECG’s specifications were correctly re-estimated using our data, UK ARPS turned out to be 3.1 percent *below* the European average (and not 9 percent above, as per LECG’s conclusion). Further, LECG’s “results” provided no basis for drawing any meaningful conclusions on the role that Sky’s alleged market power plays in relation to higher ARPS that LECG claim to observe in the UK for packages of similar quality.
3. LECG have now provided a response to our critique (the “LECG Response”). Their response predominantly repeats the assertions of their earlier work, and fails to address our criticisms. After reviewing the LECG Response, the substantial conclusions of our October Report remain unchanged:
 - a. The quality of pay TV programming varies enormously both across Europe and over time. LECG attempt to control for this using information from Screen Digest on “programme expenditure” per subscriber. However this data is a poor proxy for programme quality as it covers only about one fifth of expenditures. It is primarily expenditures on premium channel movie rights and excludes expenditures on sports content. The extent to which it includes expenditure on programming for basic channels is unclear. LECG claim in their response that this does not matter. But this is wrong.
 - b. While the programme expenditure-based proxy for quality is poor, it is particularly poor in the time series dimension where it is basically uninformative in explaining changes in ARPS.² This is important as the fixed effects regressions used in LECG’s study rely on meaningful time

¹ *Pay TV prices in Europe (1997-2005): an econometric analysis*, dated 18 June 2007. We responded to LECG’s initial analysis with *Average Pay TV Revenues per Subscriber across Europe: A Review of LECG’s empirical study* (dated 29 October 2007). This report reviews LECG’s document *Pay TV Prices in Europe (1997 – 2005): A Response to CRA’s Comments of 29 February 2008* (that we received on 15 May 2008).

² October Report, paragraph 52.

series variation in programme expenditure to identify (i.e. control for) the effects of quality. As a result these regressions fail to properly control for quality.

- c. Using a slightly more recent version of the same Screen Digest data that LECG use we obtain very different results to them - for example the size of the coefficient on programme expenditures, which is a key variable in this study, completely changes in both magnitude and statistical significance. In our results, it becomes statistically insignificant at any conventional level of significance.
- d. LECG's measure of market structure ("market share of leading DTH firm") is inappropriate, and its use is unjustified when there are more conventional measures readily available.
- e. The positive coefficient on "market share of leading DTH firm" likely reflects the omission of an effective control for quality. As DTH customers on average buy higher quality packages the market share variable is, in effect, acting as a proxy for quality.
- f. Finally, like their First Report LECG's Response contains numerous data and methodological errors, and false claims.

LECG's analysis does not control for difference in pay TV "quality" across Europe

4. The quality of pay TV programming varies enormously across Europe. This variation is magnified by the fact that in many European countries cable is used as the primary distribution method for households that receive free-to-air (FTA)-only services. In the Screen Digest data used by LECG, payments to cable operators by households who use cable to receive FTA-only services in this way are treated as subscriptions to pay TV services – even though they receive *no* pay TV channels.
5. In order to "control" for differences in pay TV quality, LECG use information on "programme expenditure" per subscriber across countries, and a variable to indicate whether cable was the largest platform in a country in every year of the study. However, as observed in our October Report the data used by LECG on programme expenditure is very poor as a proxy for programme quality, as it does not include large amounts of expenditure on pay TV programmes that are in fact vital for a good quality measure – such as expenditure on acquiring sports rights and programming on basic channels. LECG apparently did not realise these omissions when preparing their First Report. Having been made aware of this, they now claim that missing out on around *four fifths* of content expenditure does not matter. We show to the contrary, that it does matter.
6. Looking at the relation across countries between ARPS and per subscriber programme expenditure at one point in time can be misleading. There may be many factors that could generate a correlation that we cannot easily control for. For example, ARPS may be higher in one country because consumers in that country have a stronger preference for watching movies on TV rather than at the cinema. It might be possible to capture some of these effects through measured variables like

average country income, but others (such as “cultural” factors) are hard if not impossible to measure. If these omitted variables are correlated with variables that are included in the regression, like programme expenditure, the estimates will be biased. A possible solution arises when we have data on the same countries *in multiple time periods* (i.e. “panel data”). In this case, if we assume that hard-to-measure factors like “culture” do not change much over time, we can just look at *changes* in ARPS and programme expenditure (as well as other variables). A “fixed effects” estimator does just that, relying on changes in the variables *over time*.

7. But to use a fixed effects approach one needs reliable measures of variables that change over time. Unfortunately, not only is Screen Digest’s programme expenditure data a very poor measure as it misses out many dimensions of quality, but this problem is exacerbated when one looks at changes *over time* (discussed further below).
8. We showed in our October Report that one of the main problems with the Screen Digest data is that it covers only a small part of spending on programming by pay TV operators, excluding spending on (at least) sports programming. LECG’s efforts to use data on the allocation of Champions League revenues to show that Screen Digest’s programme expenditure data is “good enough” fail on numerous counts. Most importantly, LECG fail to recognise that the payments they have used cover both free-to-air and pay TV exploitation, and that the division of broadcast of Champions League football between free to air and pay differs significantly from country to country. Furthermore, there is no reason to believe that payments for Champions League football are a good proxy for spending by pay TV services on sports rights in all the fifteen countries in LECG’s study; sporting tastes vary significantly across Europe.
9. Even if Champions League payments only related to pay TV programming, however, LECG’s argument would still fail. LECG compare Screen Digest data on programming expenditure for 2004 with Champions League payments for 2005/06. However, when we examine the changes over time between the Screen Digest measure of programme expenditure per subscriber and Champions League revenues per subscriber (and this is the relevant measure) we find that there is no relationship between the two variables - the correlation is approximately zero. Accordingly, it remains the case that Screen Digest’s data on programme expenditure are a poor proxy for total actual programming expenditures, which makes them a poor proxy for the quality of programming delivered to consumers.
10. The result of the fact that Screen Digest programme expenditure is a poor proxy for quality becomes clear when we examine the relation between the proxy and ARPS. We find that there is essentially no relationship over time between changes in the Screen Digest programme expenditure variable and changes in ARPS. This is important as the fixed effects regression relies on changes over time in the proxy to identify (i.e. control for) the effects of quality. As a consequence the regressions fail to control for quality and the relation we observe between the “market share of largest DTH firm” and (simple) ARPS is likely explained by the fact that DTH subscribers take higher quality packages.

11. LECG's response is thus simply wrong, and misleading, on this issue. They offer nothing to refute our criticism that their measures of quality-adjusted prices do not properly control for quality. Again, therefore, we conclude that LECG's "finding" that pay TV "prices" are higher in the UK than elsewhere simply reflects their failure to control properly for relative differences in the quality of programming.

Differences between our data and LECG's have a material effect on the results

12. Secondly, although we used only a slightly more recent version of the same Screen Digest data as LECG, we found a large number of substantial unexplained discrepancies between the two datasets. In the worst cases the magnitudes of the differences are a factor of *four*. LECG's response is that the differences are simply due to Screen Digest's revision process, they are few and in any case they are not as large as we suggest³. Furthermore, they claim that using our data does not materially alter any conclusions⁴.
13. This is incorrect. The discrepancies are not few, nor they are small, and they are materially important. Indeed, we find substantially different results from LECG even when reproducing the same specifications that they estimate. In particular, we find that the coefficient on the programme expenditure variable is completely insignificant with our dataset, and this is not – as LECG suggest – a function of how we report our results.⁵ With our data, the effect of quality on price is essentially zero. It is entirely unclear how LECG can claim that the results are "very similar" when the coefficient on a key variable drops from a value of 0.576 to essentially zero, and goes from being statistically significant to insignificant. Above all, if using a slightly more recent (and improved) version of the same dataset produces such different results, Ofcom should be very wary of attaching any weight to the conclusions drawn from such a study.
14. LECG also argue that we should have corrected the Screen Digest data (or the outliers we found), as these may be the cause of our substantively different results. But this would have been inappropriate, particularly as LECG declare they have not dropped or manipulated any data points in their original study (that we were seeking to replicate). Moreover, we clearly show that – contrary to LECG's suggestion – our different results are not driven simply by a few outliers specific in our version of

³ At no point do LECG suggest that our data is incorrect, and indeed they confirm some of the specific errors we identified – such as the calculation of Telenor/Canal Digital market share and the classification of Norway as a country where "cable is always largest".

⁴ LECG Response, page 6, paragraph 2: "we find that, contrary to what is claimed by Sky's economists, the results of their models are *very similar to ours*" (emphasis added). We address below LECG's spurious argument that the main difference between their results and ours arises due to differences in the reporting of significance levels.

⁵ The coefficient on "programme expenditure" for example, is insignificant in our version of the data (0.015 with a standard error of 0.024), compared to the estimate based on LECG's data that is small but statistically significant (0.576 with a standard error of 0.185).

Screen Digest's data. We are unable to replicate LECG's results even when we correct for these extreme data points through various techniques.

15. We have so far been denied access to LECG's data, despite offering to share our data. Swapping datasets would enable issues which arise from differences in data to be resolved. The Complainants' unwillingness to share their data without imposing unnecessary pre-conditions raises obvious questions as to the reasons for such reluctance.

LECG's measure of market power is misconceived and unjustified

16. Third, LECG use a non-standard measure of market power (the "market share of leading DTH firm") in order to capture the effect of competition amongst pay TV suppliers on prices for pay TV services. This approach is simply not credible. LECG had claimed in their First Report that the reason they had chosen to use a non-standard measure was that it was not possible to calculate conventional measures of concentration (such as HHI's). In contrast, we showed in our October Report that it is possible to do so using the available data. Moreover, "market share of largest DTH firm" is unrelated (or even negatively related) to such more standard measures of concentration.
17. We do not find evidence of any relation between "market share of largest DTH firm" and market power. As we have suggested, it is likely that any simple relation between "market share of largest DTH firm" and ARPS reflects instead the higher average quality of DTH packages, together with the omission of an adequate control for quality. LECG are unable to refute this.
18. Given the unconventional nature of the variable used by LECG, the burden is on them to demonstrate why it is a superior measure of competitiveness in relation to pay TV compared to standard measures used in competition analysis. Where they attempt to do this, the logic of their argument is entirely circular – they argue that they prefer "market share of the leading DTH firm" as a measure of market competitiveness *because* it is correlated with ARPS whereas the HHI measures are not!⁶ It is also surprising that LECG have failed to adopt a more conventional approach to the measurement of competitiveness now that they recognise the data allows the calculation of appropriate measures.
19. The implication of LECG's argument – were it to be taken seriously – is that market concentration as conventionally measured has nothing to do with the determination of pay TV prices in Europe, but that these are, instead, significantly determined by the market share of the leading DTH operator in each country. But then LECG should be showing that there is some unique structural feature of DTH broadcasting that allows DTH operators (as compared to cable operators) to exert market power.

⁶ LECG Response, page 12, paragraph 4.

No credible case for this has been made.⁷ We also note that the LECG theory has the absurd implication that all the UK and other authorities need to do to get lower prices for pay TV services is to ban their supply via DTH. LECG's analysis has no basis or support in conventional economics.

LECG's Response contains many fundamental errors and false claims

20. Finally, LECG's Response, consistent with their First Report, contains numerous technical errors and false claims. As an example of the latter, LECG allege that differences in our results and theirs are attributable mainly to the fact that, in our October Report, we reported only 5% significance levels, and LECG regarded coefficient estimates as being significant if they were found to be significant at the 1%, 5% or 10% levels.⁸ This argument is clearly intended to obfuscate. There are obvious, significant differences in the *magnitude* of coefficient estimates. As noted above, for example, our estimate of the coefficient on the programme expenditure variable is much smaller than LECG's – and variables that LECG find to be significant (notably, the programme expenditure variable) are statistically insignificant in our results at *any* conventional level of significance.
21. In order to avoid detracting from the key arguments, we have discussed all of these errors and false claims in the appendices to this report. However this does not mean they are second-order: taken together they are of serious concern.

Conclusion

22. Having reviewed the LECG Response, we believe that their arguments in that report, where they are material, are in all cases incorrect. As a result we believe that the available evidence continues strongly to support the conclusions of our October Report:
 - ***First, the observed variation in ARPS across countries is likely explained principally by variation in the pay TV content of the packages sold in different countries;***
 - ***Secondly, LECG's analysis provides no evidence that the higher ARPS observed in the UK is due to high margins and a lack of competition. It therefore cannot be relied upon to tell us anything about the competitive state of the UK pay TV sector, and hence about any alleged consumer harm.***

⁷ At slide 11 of their First Report, LECG appear to claim that wherever DTH operators have a large "market share" their "vicious circle" comes into force. Such an ad-hoc approach to analysis borders on the absurd.

⁸ Paragraphs 2-3 on page 17.

1. INTRODUCTION

23. We have been asked by BSkyB (“Sky”) to consider the report by LECG *Pay TV Prices in Europe (1997-2005): A Response to CRA’s Comments* of 29 February 2008 (published by Ofcom on 15 May 2008). The LECG report is a response to our report *Average Pay TV Revenues per Subscriber across Europe: A Review of LECG’s empirical study* (dated 29 October 2007). This in turn referred to a set of slides prepared by LECG, entitled *Pay TV prices in Europe (1997-2005): an econometric analysis*, and dated 18 June 2007. This presentation was appended to the *Submission to Ofcom on the need for a market investigation into the Pay TV industry* dated 3 July 2007 submitted by British Telecommunications plc, Setanta Holding Ltd, Top Up TV Europe Ltd and Virgin Media Limited (“the Complainants”).
24. In their First Report LECG claimed that the high average revenue per subscriber (ARPS) observed in the UK relative to other European countries is due to the market power of the leading DTH operator, Sky. Their conclusion is based on an econometric analysis (using a “fixed effects” regression technique) which seeks to estimate ARPS as a function of a number of variables, including a measure of relative *quality* (“programme expenditure per subscriber”), LECG’s preferred measure of market structure (the “market share of the largest DTH firm”) and individual country effects. LECG interpret a positive coefficient on the variable “market share of the largest DTH firm” as an indicator of a causal impact of market structure on ARPS.
25. In our October Report we observed that the quality of pay TV packages varied enormously across countries and over time. This variation is exacerbated by the fact that in many European countries (the legacy cable countries) cable is used as the primary distribution method for households that receive free-to-air (FTA) only services. In the Screen Digest data used by LECG, payments to cable operators by households who use cable to receive FTA-only services in this way are treated as subscriptions to pay TV services – even though they receive no pay TV content.
26. The use of cable to distribute FTA-only services in legacy cable countries has other effects. On average, countries with high absolute pay TV penetration as measured in the Screen Digest data (legacy cable countries) also have high cable market share and low average package quality. As the main platforms for pay TV during the period up to 2005 were cable and DTH, this implies that countries with high cable market share have low DTH market share and that countries with high DTH market share have high package quality. Hence if one looks at the simple relation between the level of DTH subscription and ARPS, we would expect it to be positive due to the higher quality of DTH packages.
27. The question that LECG want to look at is the effect of DTH subscription on ARPS *holding quality constant*. If their claim that programme expenditure per subscriber (as defined by Screen Digest) is a good proxy for quality was correct, then the regression coefficient on the “market share of the largest DTH firm” would give some indication of the effect of the level of DTH subscription independent of quality. However the overwhelming evidence discussed below indicates that the programme expenditure per subscriber variable is a very poor proxy for quality. If this is the case,

“market share of largest DTH firm” is itself likely to act as a proxy for quality and it is not possible to estimate the independent effect of the level of DTH subscriptions on ARPS.

28. This report is organised in three sections. First, in section 2 we address the key evidence that LECG’s proxy for “quality” is poor, and as a result their model fails to control explicitly for variations in package quality across countries. In section 3 we discuss the impact of discrepancies between our data and LECG’s. In section 4 we look at the evidence that LECG’s chosen measure of market structure is a poor measure of concentration and of the degree of competition in the pay TV sectors of the countries in the study. In Appendix A we explain in more detail the faults in LECG’s data and methodology; in Appendix B we formalise the notion that a very poor proxy for quality will lead to misleading results; and in Appendix C we address LECG’s “solutions” to the endogeneity issue.

2. LECG’S ANALYSIS REMAINS FLAWED BY FAILURE TO CONTROL FOR QUALITY

2.1. THE PROBLEM OF CONTROLLING FOR QUALITY

29. As explained in our October Report, pay TV packages are not homogeneous and vary enormously in terms of content and price across Europe, both (i) within countries and (ii) across countries, and over time. Given this variation, controlling for quality plays a crucial role in any study that seeks to examine what determines charges for such services. LECG’s key quality measure is a measure of “average programming expenditure per subscriber”, with the numerator of this variable derived from data provided by Screen Digest.
30. On examination by CRA, it became clear that the Screen Digest programme expenditure data covered only a subset of programming expenditure by pay TV operators. LECG appear to accept that this is the case.⁹ Nevertheless, LECG continue to assert that a combination of the average programming expenditure per subscriber variable, based on the Screen Digest data, and the fact that they include a “cable always largest” zero-one dummy variable in their regression, provides an effective control for quality in its study.¹⁰ In our view, this is not correct.
31. There remains a significant lack of clarity about the coverage and reliability of the Screen Digest data on programme expenditure. Further discussions with Screen Digest have indicated that the programme expenditure variable (i) includes expenditures on movie rights for premium channels and (ii) excludes all sports rights.

⁹ LECG Response, page 7, paragraph 5 where LECG states that they “are not going to dispute here the description given by Sky’s economists...to Screen Digest’s “total spending by broadcasters on programme rights” variable.”

¹⁰ LECG Response, page 7.

Contrary to LECG's assertions,¹¹ Screen Digest is unclear about the level of coverage for programme expenditures for basic pay TV channels. However they do say that the extent of the coverage in relation to such programming is far from being complete and varies very substantially across countries.

32. These issues confirm that the Screen Digest data on programming expenditure is unreliable as an input for econometric research. There is little likelihood that this data can provide a robust, consistent measure of relative programming expenditures by pay TV operators for the 15 European countries in the study over an eight year period.¹² The likelihood is that (i) the Screen Digest data on programming expenditure captures a relatively small proportion of total spending on programming by pay TV operators in each country,¹³ and (ii) *within each country* in the study, the proportion of the total expenditure by pay TV operators on content covered by the Screen Digest data varies from year to year – for example due to differences in the level of publicly available data in each year, as well as in approaches to the classification of programming expenditure by Screen Digest over time.¹⁴
33. LECG's arguments, discussed in the next section, take as given the reliability of the Screen Digest data on programming expenditure. LECG's efforts at supporting their use of the programming expenditure variable as fit for purpose are fundamentally flawed. The lack of reliability of the Screen Digest data for the purposes to which LECG attempt to put it make the results of their study unreliable.

2.2. LECG'S ARGUMENTS IN RELATION TO CONTROLLING FOR QUALITY

34. LECG acknowledge the incompleteness of their programming expenditure variable (based on Screen Digest data). Yet they put forward four reasons (at pages 8-11) for their belief that, even if the measured programme expenditure comprises only a subset of total programme expenditures by pay TV operators, their combined use in the regression of the programming expenditure variable, and the "cable always largest" dummy variable, provides an effective control for quality in their study.
35. **LECG's first argument** is that the programme expenditure variable is "correlated across countries with ARPS", and that this shows that "the programming per subscriber variable used in the LECG study is likely to capture the differences in

11 LECG Response, page 7, paragraph 5.

12 This is not intended as a criticism of Screen Digest. The observation that the data is not fit for the purpose to which it has been put by LECG does not mean that it does not serve useful purpose at all. However Screen Digest have now stopped providing this series – the latest year for which data is available is 2004.

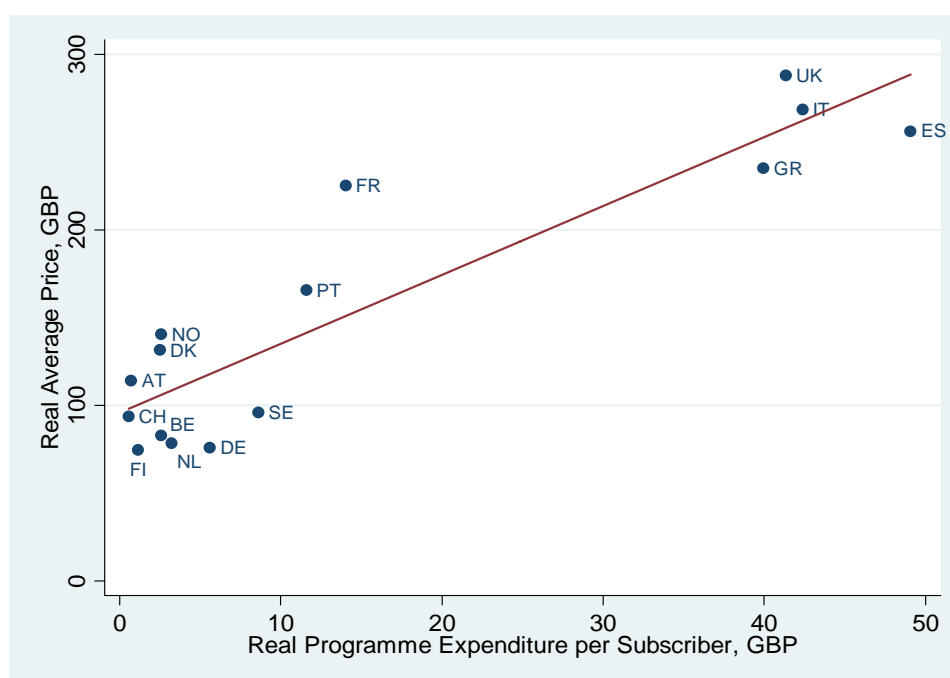
13 In our October Report we noted that Sky calculated that the Screen Digest figure for 2004 represented only around 18% of total programming expenditure by pay TV operators in the UK.

14 It is equally likely that the proportion of the total expenditure by pay TV operators covered by the Screen Digest variable varies between countries, although due to the fixed effects methodology used in the study, this would have no bearing on the study.

content quality that contribute to explain the striking price differences across countries that are observed in the Screen Digest database.”¹⁵

36. As noted in Appendix D of our October Report, this comparison is *irrelevant*. Given the fixed effects methodology used in the study, what matters is not the *cross-country* relationship discussed by LECG but the *within-country* relationship *over time*. Figure 1 below gives the cross-country relationship and Figure 2 gives the within-country relationship between ARPS and programme expenditure per subscriber based on the Screen Digest data.¹⁶ Figure 2 indicates that there is *no relationship between variations over time in subscriber programme expenditure and ARPS for specific countries*: the correlation coefficient is -0.15. This is in contrast to the positive cross-sectional relationship across countries (in Figure 1). If there were a positive relationship between programme expenditure and ARPS, we would expect the points in Figure 2 to be arranged around an upward-sloping line, which is clearly not the case.

Figure 1: ARPS as a function of per subscriber programme expenditure, country averages

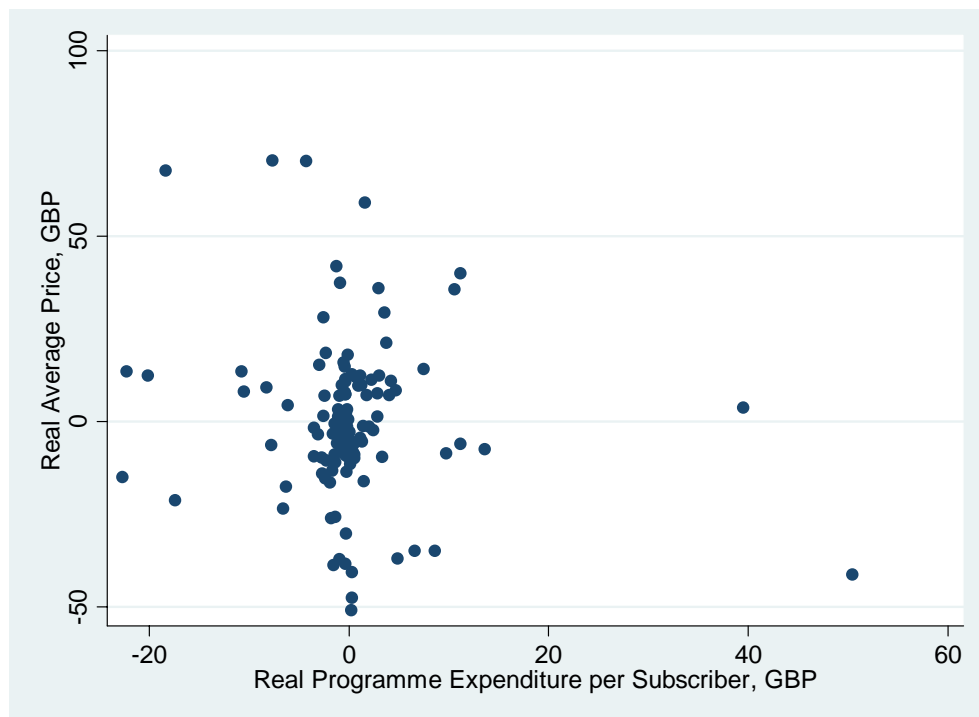


Source: CRA October Report, Figure 4.

¹⁵ Much of the discussion below simply repeats arguments that appear in Appendix D of our October report. LECG's analysis essentially duplicates the discussion in paragraph 141 of our October report. However they have chosen to ignore the rest of the discussion.

¹⁶ The analysis in Appendix D omitted two very large outliers in programme expenditure for Italy in 1997 and 1998.

Figure 2: ARPS as a function of per subscriber programme expenditure, deviations from country averages



Source: CRA calculations based on Screen Digest data

37. This basic insight is confirmed by examining the results of both the LECG and CRA fixed effects regressions. The magnitude of the estimated coefficient on “programme expenditure per subscriber” is implausibly small. As noted above, Sky estimates that Screen Digest’s estimate of programme expenditure in the UK in 2004 was less than 20 percent of actual expenditure on content by pay TV operators. If we assume that expenditure on content is substantially passed through, we should expect the coefficient on Screen Digest’s per-subscriber programme expenditure variable to be about 5 in magnitude. Furthermore, LECG’s cross country analysis in their Figure 1 (and our Figure 1 above) suggests that the effect of an increase in per-subscriber programme expenditure on ARPS is also about 5.
38. In Table 1 we give estimated fixed effects regression results using CRA’s and LECG’s data. LECG’s estimate (0.576) is small, (less than 12 percent of the magnitude that we would expect). Our estimate of this coefficient is 0.015,

essentially zero.¹⁷ This is entirely as one would expect on the evidence of Figure 2. LECG's regression results indicate a coefficient value for the relation between Screen Digest's programme expenditure variable and ARPS that is small and entirely inconsistent with the cross country evidence that they present in their Figure 1.

Table 1: Pay TV ARPS, LECG specification 4, Fixed Effects

	CRA	LECG
GDP per capita	9.526***	5.967***
	[1.366]	[1.193]
Program exp per subs - lagged one year	0.015	0.576***
	[0.024]	[0.185]
Indicator - pay per view presence	0.461	13.394**
	[2.816]	[5.429]
Number of different platforms	-2.202	2.585
	[2.702]	[2.900]
Market share largest DTH supplier - lagged one year	0.989***	1.179***
	[0.144]	[0.375]
Indicator - Cable always largest platform	-163.752***	-72.642***
	[11.219]	[17.319]
Constant	79.165***	n/a
	[15.600]	
Auto correlation	0.620***	n/a
	[0.076]	
Observations	118	118
Number of countries	15	15
R-squared (within regression)	0.734	0.795

Notes: Robust standard errors in brackets. * significant at 10, ** significant at 5, *** significant at 1 percent level. These results are fixed effects estimates with the fixed effects reparameterised as discussed in paragraphs 78-80 of our October Report. Individual country effects are not reported. Source: CRA, LECG Response Table 10, page 35.

39. **LECG's second argument** notes that countries they classify as "cable always largest" have very low absolute and relative values of programme expenditures per subscriber compared to the remaining countries. They concede, as we explained in our October Report, that this may be because cable is being used to provide FTA-

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Further evidence that within-country variation in Screen Digest's programme expenditure variable has little or no impact on ARPS is to be found in the specifications introduced at page 15 and reported at Annex F of LECG's response. The specifications reported at Tables 7 and 9 do not contain the programme expenditure variable and do not appear to control for quality at all. The specification reported at page 31, Table 8, column 2, gives a coefficient on programme expenditure of 0.007 with a standard error of 0.245 which is insignificant at conventional levels. LECG do not comment on this, but it is consistent with the over-time variation in programme expenditure being noise.

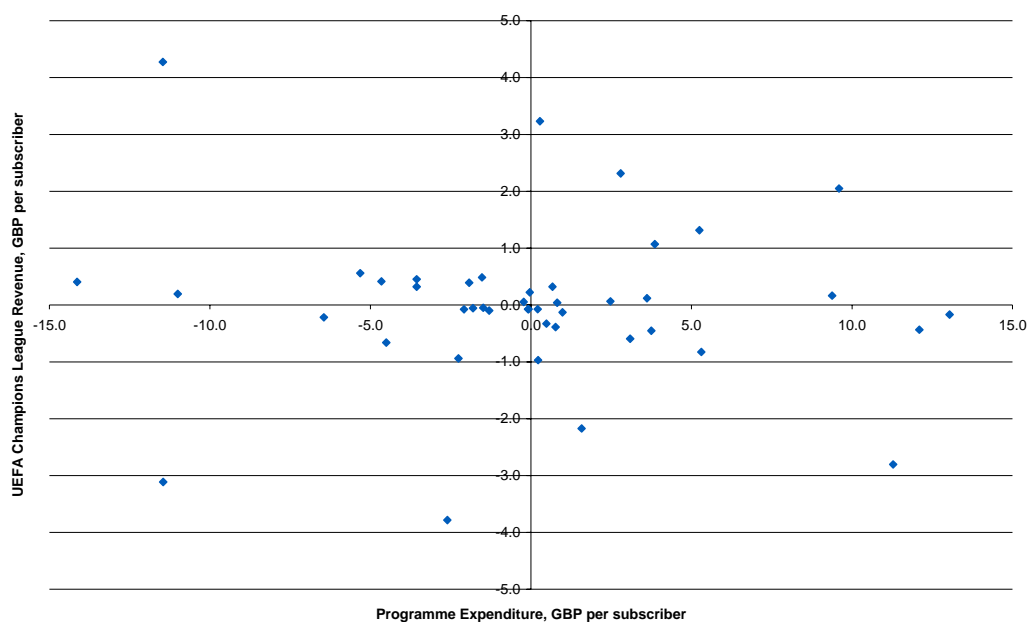
- only services in the legacy cable countries.¹⁸ They claim however that these differences are adequately dealt with by the “cable always largest” zero-one variable they introduced in the regression. But this is wrong: because there is no variation in this variable over time, if within country changes in market structure are being used to identify changes in prices, “cable always largest” is useless as it does not vary within countries over time. The “cable always largest” variable is simply a re-parameterisation of the fixed effects, and as a result its introduction has *no* effect on the estimated coefficients of the covariates in their fixed effects specifications.¹⁹
40. **LECG’s third argument** attempts to show that programming expenditure that is excluded from Screen Digest’s measure (such as sports programming) is nonetheless highly correlated with Screen Digest’s measure. LECG assert (incorrectly) that Screen Digest’s measure excludes only spending on sports programming and then seek to show that spending on one type of sports programming – Champions League football – for 2005/06 is correlated with the Screen Digest measure of programme expenditures for 2004 *across countries*.
 41. LECG fail to recognise, however, that Champions League rights are shared between free-to-air and pay TV services and the split of matches differs significantly between countries. LECG treats Champions League payments as though they were all made by pay TV operators in every country in the study. LECG also assert that spending on Champions League rights is a good proxy for spending by pay TV services on sports programming generally. LECG provide no evidence to support this proposition and it is unlikely to be correct. Consumers in many countries in the study have strong demands for sports programming which is of little or no interest in other countries (for example basketball in Greece, and ice hockey in Sweden). The clear implication is that the cross-country correlation found by LECG provides no evidence at all that Screen Digest’s programming expenditure data are a reasonably constant proportion of total programming expenditure by pay TV operators, even for the single period that they examine.
 42. Even if the Champions League payments examined by LECG were payments by pay TV operators, however, given the fixed effects approach used by LECG, their argument is *irrelevant*. What matters in the fixed effects approach is the relation between *within-country* variation in the Screen Digest measure of programming spend, and actual total programming spend over time. It is not at all obvious that an increase in payments for football rights should be correlated with an increase in payments for premium channel movie rights or other types of programming. LECG provide no evidence about the relationship between *within-country variation over time* in the excluded expenditures, and the expenditure covered by Screen Digest’s measure, which is *the key issue* in the context of their modelling approach.

18 LECG Response, page 9, first paragraph.

19 Compare the reported coefficients in slide 39 of LECG’s First Report which does not include the “cable always largest variable” and those reported in Table 10, column 2, LECG Response page 35 that do.

43. Further, the variables used in the regressions are programming expenditure *per subscriber* and Champions League TV revenues *per subscriber*. But LECG's Figure 2 presents different variables – the total revenues and total expenditures – which, unsurprisingly generates a strong positive relation as bigger countries have larger revenues.

Figure 3: Relation between changes in measured programme expenditure and changes in UEFA Champions League revenues, GBP per subscriber



Source: Screen Digest, UEFA.

44. Where LECG looked at UEFA payments for one year 2005/6 across countries, we have looked at UEFA payments from 2001/2 to 2004/5 for ten countries of the fifteen countries included in LECG's dataset which had teams in the Champions League in three or more years in that period and compared them with programme expenditure as measured by Screen Digest. Figure 3 gives the relation between changes in programme expenditure *per subscriber* as measured by Screen Digest and changes in Champions League revenue *per subscriber*.²⁰ The correlation is -0.07. So this confirms the intuition above that there is no relation between changes in per subscriber premium channel movie payments and per subscriber Champions League revenue and hence that changes in Screen Digest programme expenditure are a poor proxy for actual programme expenditures. Hence LECG's argument that it is safe to ignore the fact that Screen Digest's data cover only a small fraction of actual programming expenditure (because the missing elements of total

²⁰ Data points are differences from county averages. Both Champions League revenues and Screen Digest programme expenditure are in *nominal* GBP. The analysis covers payments to teams from ten countries, Belgium, France, Germany, Greece, Italy, Netherlands, Norway, Portugal, Spain and the UK. Switzerland only participated once and is excluded.

programming expenditure are correlated with that which is measured by the Screen Digest data) fails. There is no such correlation in changes over time

45. **LECG's fourth argument** is essentially the same as their second argument already discussed. LECG suggest that the differences between legacy and non-legacy cable countries are adequately dealt with by the "cable always largest" variable. As already noted, introducing the "cable always largest" variable is simply a reparameterisation of the fixed effects and as a result its introduction has *no effect* on the estimated coefficients of the covariates.
46. A further check on the adequacy of the programme expenditure variable as a control for quality is to estimate LECG's model with the variable excluded. We believe that there are very large differences in average quality both across and within countries, and that these are a major cause of the variation in ARPS that we observe. If dropping the programme expenditure variable has no major effects on the estimated specification, then it is clear that that variable is failing to account for quality. If that is the case, then we have strong reasons to believe that "market share of largest DTH firm" is correlated with quality and it is likely to act as a proxy instead.

Table 2: Excluding programme expenditure, Fixed Effects Estimates

	1	2
GDP per capita	9.526***	9.549***
	[1.366]	[1.356]
Program exp per subs - lagged one year	0.015	
	[0.024]	
Indicator - pay per view presence	0.461	0.421
	[2.816]	[2.779]
Number of different platforms	-2.202	-2.389
	[2.702]	[2.642]
Market share largest DTH supplier - lagged one year	0.989***	0.990***
	[0.144]	[0.143]
Indicator - Cable always largest platform	-163.752***	-164.663***
	[11.219]	[11.219]
Constant	79.165***	80.114***
	[15.600]	[15.526]
Auto correlation	0.620***	0.615***
	[0.076]	[0.076]
Observations	118	118
Number of countries	15	15
R-squared (within regression)	0.734	0.734

Notes: Column (1) reproduces our specification in Table 3 column (2) of our October Report. The constant terms reported in Table 3 are for a different parameterisation. Estimation is by OLS with robust standard errors in brackets under coefficients. These results are based on fixed effects estimation with the fixed effects reparameterised as discussed in paragraphs 78-80 of our October Report. Individual country specific effects are not reported. * indicates that a variable is significant at the 10 percent level, ** at 5 percent and *** at 1percent. Source: CRA calculations based on Screen Digest data.

47. Table 2 gives the fixed effects estimates of the standard specification in column (1), and estimates of the standard specification but *dropping the programme expenditure* variable in column (2). If the programme expenditure variable really did proxy effectively for quality we would expect that removing it would cause (i) the goodness of fit of the equation to deteriorate substantially, and (ii) the coefficients on the “cable always largest” variable and the “market share of largest DTH firm” to both increase substantially, as they proxied for the omitted programme expenditure variable. Neither of these occurs. In fact the results for the two specifications are substantially *identical*, confirming that the programme expenditure variable used by LECG has little effect in the standard specification. This could only occur if (i) there was little within-country variation in quality or (ii) per subscriber programme expenditure is a poor proxy of such within-country variation. As discussed above, we believe that there is substantial variation in quality which implies that programme expenditure is a poor proxy.
48. In conclusion, there is overwhelming evidence that the Screen Digest programme expenditure variable is a completely inadequate proxy for quality. If programme expenditure does not control for quality, as the results in fact suggest, we have strong reasons to believe that “market share of largest DTH firm” is correlated with quality and is likely to act as a proxy instead. We believe that the evidence is entirely consistent with that explanation, and therefore LECG’s results tell us *nothing at all* about whether the quality-adjusted ARPS in the UK is truly higher than the European average, nor (if it is higher) about whether this is in any way driven by market power.

3. LECG’S RESULTS ARE NOT ROBUST TO DIFFERENCES IN DATASETS

3.1. LARGE UNEXPLAINED DIFFERENCES BETWEEN LECG’S DATA AND OUR DATA MATTER TO THE RESULTS

49. Our October Report detailed (at Sections 4.6 and in Appendix A) our efforts to reproduce LECG’s data in order to replicate their analysis exactly. At paragraphs 128 through 132 we gave detailed information on the discrepancies we identified between the LECG dataset and the one we have assembled from the same sources. We started with an expectation that discrepancies would be few and small, but where we could do detailed comparisons we found many substantial differences. In specific instances we found some observations differed by a factor of *four*, and there was evidence that there were other order of magnitude discrepancies in variables where we did not have detailed data. In certain cases we were able to point to specific miscalculations.
50. At paragraphs 121 to 126 of our October Report we detailed problems that had arisen in the construction of our data set – many of which required us to repeatedly

engage with Screen Digest. LECG's blanket claim²¹ that none of these problems were an issue with their data does not inspire confidence.

51. LECG's overall response to our concern about the reliability of their data (that so far the Complainants have declined to allow LECG to share with us) is that the differences are simply due to Screen Digest's revision process, and in any case they are not as large as we suggest²². Furthermore, they claim that using our dataset does not materially alter any of their results.
52. This, however, is incorrect. The data discrepancies are materially important, and we find substantially different results from LECG even when reproducing the same specifications that they estimate. In particular, we find that the programme expenditure variable is insignificant in the fixed effects specifications, and this is not, as LECG suggest, a function of how we report our results. Table 4 in our October Report showed that LECG's best estimate of the coefficient was 0.675 with a standard error of 0.191, compared to the estimate using our data of 0.015 with a standard error of 0.094, a reduction in the estimate of this key coefficient by *more than 97 percent*. Hence our data implies that the Screen Digest measure of programme expenditure has no effect on ARPS. This implies that there is no variation in package quality, that variation does not affect APRS or that the Screen Digest variable is a poor control for quality. On the evidence, the last of these seems much the most likely. With such an effect – a key variable changing from a value of 0.675 to 0.015, and going from being statistically significant to being statistically insignificant – it is entirely unclear how LECG can claim that the results are “very similar”.

3.2. “CONTROLLING FOR OUTLIERS” DOES NOT MATERIALLY ALTER CRA’S RESULTS

53. LECG's main argument against the many anomalies we have pointed out in their dataset is that we should have corrected the Screen Digest data for the outliers we found.²³ They claim that these may be the cause of our substantively different results.
54. First, it would be inappropriate to simply drop suspected outliers in the data, as we were not starting a *de novo* study but instead seeking to replicate LECG's study. Essentially we find that estimating the *same* LECG models but using a slightly more recent version of Screen Digest's data results in different conclusions. Since LECG declare they have not dropped or manipulated any data points, it would have been wrong for us to drop them. It may be that LECG have adopted some unreported

21 LECG Response, page 3, paragraph 4.

22 At no point do LECG suggest that our data is incorrect, and indeed they confirm the specific errors pinpointed by CRA such as the calculation of Telenor/Canal Digital market share and the classification of Norway as a country where “cable is always largest”.

23 LECG Response, page 4, paragraph 4.

method of dealing with the outliers in their data. They certainly do not report cleaning the original data that they received from Screen Digest in any way in their First Report or their more recent Response to our October Report, so we presume that they have not (otherwise this would obviously be misleading). If they have they should declare this.

55. As examples of outliers, we pointed to programme expenditures per subscriber in Italy in 1997 and 1998 being particularly large, and also Italy again in 1999 and Spain in 1997. There is no reason to believe that this data is incorrect. LECG provide no explanation as to why these observations for Italy and Spain are different for their data.

Table 3: Alternative estimates of fixed effects results correcting for outliers

	1	2	3	4
GDP per capita	9.526***	6.919***	9.497***	9.596***
	[1.366]	[0.582]	[1.383]	[1.343]
Program exp per subs - lagged one year	0.015	-0.007	0.067	-0.183
	[0.024]	[0.022]	[0.220]	[0.396]
Indicator - pay per view presence	0.461	0.14	0.581	-0.68
	[2.816]	[2.700]	[2.877]	[3.063]
Number of different platforms	-2.202	0.615	-2.399	-2.657
	[2.702]	[1.605]	[2.757]	[2.839]
Market share largest DTH supplier - lagged one year	0.989***	1.214***	1.013***	1.049***
	[0.144]	[0.071]	[0.149]	[0.151]
Indicator - Cable always largest platform	-163.752***	-140.659***	-161.213***	-168.254***
	[11.219]	[5.557]	[14.240]	[18.418]
Constant	79.165***	101.292***	77.117***	84.520***
	[15.600]	[6.990]	[18.038]	[19.607]
Observations	118	118	116	114
Number of countries	15	15	15	15
R-squared (within regression)	0.734	n/a	0.735	0.746

Notes: Column (1) reproduces CRA in Table 1 above. Column (2) reports the robust regression results. Column (3) removes two outliers (Italy in 1997 and 1998) and column (4) removes four outliers (Italy in 1997, 1998 and 1999 and Spain in 1997). Robust standard errors in brackets. These results are fixed effects estimates with the fixed effects reparameterised as discussed in paragraphs 78-80 of our October Report. Individual country specific effects are not reported. * indicates that a variable is significant at the 10% level, ** at 5% and *** at 1%. Source: CRA calculations based on Screen Digest data.

56. We also show in Table 3 that – contrary to LECG's suggestion – our different results are not driven simply by a few outliers specific to our version of Screen Digest's data. We are unable to replicate LECG's results when we correct for these extreme data points:

- a. Column (1) of Table 3 is the standard fixed effects specification as in Table 1 above.

- b. Column (2) uses the robust regression technique,²⁴ which is one way of controlling for outliers. The programme expenditure variable is actually negative in sign, but remains completely insignificant.
 - c. Column (3) deletes two observations where per subscriber expenditure was particularly large, Italy in 1997 and 1998. The coefficient on programme expenditure rises to 0.067 but with a standard error of 0.220.
 - d. The final column removes four outliers, the two in column (3) plus Italy in 1999 and Spain in 1997. The coefficient on the quality proxy is now -0.183, perversely signed but insignificant.
57. Our conclusion is that the Screen Digest data are a poor proxy for actual pay TV programme expenditure and that this is particularly true for within-country changes over time. There is some coefficient instability when outliers are removed and the key problem we pointed to – the small magnitude and insignificance of programme expenditure variable – remains a problem regardless of the controls for outliers that we adopt.
58. The key point is that if using a more recent (and improved) version of the same dataset produces such different results, Ofcom should be very cautious in attaching *any* weight to LECG’s conclusions. LECG’s analysis simply cannot be used to provide evidence that quality adjusted prices in the UK are unusually high, nor that this is due to market power.

4. “SHARE OF DTH” IS NOT A GOOD MEASURE OF MARKET COMPETITIVENESS

4.1. LECG’S CHOSEN MEASURE OF MARKET COMPETITIVENESS IS NOT MEANINGFUL

59. The main aim of the original LECG study was to capture the effect of competition amongst pay TV suppliers on prices for pay TV services. For this purpose, LECG introduced two variables describing market structure (and therefore, competitiveness) into their model.
60. One of the two variables was the “market share of the largest DTH firm”.²⁵ In our October Report we observed that this is not a conventional measure of competitiveness, and that conventional measures are to be preferred.²⁶ LECG claimed that they had used “market share of largest DTH firm” not for any positive

²⁴ The `rreg` procedure in STATA.

²⁵ The second variable was the “number of platforms” variable. This is discussed at paragraphs 68 and 69 of our October Report. It indicates that the UK pay TV market is amongst the most competitive sectors in the study.

²⁶ October Report, paragraph 61.

reason, but because more standard measures could not be calculated.²⁷ Our October Report showed this to be untrue. It is possible to calculate (and therefore use) several different more conventional measures of competitiveness in each country.

61. Conventional structural measures of the competitiveness of a market are n-firm concentration ratios and HHI's. Both the platform-level and operator-level HHI's we have calculated show a similar picture, with the UK amongst the *less* concentrated pay TV sectors in Europe. This is in line with LECG's other measure of competition, the number of platforms. In contrast the UK has one of the highest shares for the largest DTH firm.
62. We found that LECG's chosen measure, "market share of largest DTH firm", is either uncorrelated or negatively correlated with the HHI's.²⁸ Overall there is thus no evidence that "market share of largest DTH firm" is positively related to market concentration – indeed there is a great deal of evidence for the reverse. Hence "market share of largest DTH firm" is not a good measure that could be included in a model as a control for the degree of competition.

4.2. LECG'S CRITICISM OF OUR CONCENTRATION MEASURES IS UNFOUNDED

63. LECG's response to our observation is to attempt (i) to cast doubt on the competitiveness measures calculated by CRA, and (ii) to reject our position that "market share of largest DTH firm" is correlated with quality. We explain below that both of these responses are unfounded.
64. LECG criticise our calculated HHI's as "non standard". They state that "the standard HHI measure would have been calculated using the market shares of different operators without grouping them by platform or into a single cable group."²⁹ But this is not correct. We provided HHI's for *both* platforms and for operators.³⁰ Obviously

²⁷ LECG's First Report, slide 23.

²⁸ It is uncorrelated with the later revenue HHI's but it is negatively correlated with both the subscriber and the earlier revenue HHI's (our October Report, paragraphs 107 and 108).

²⁹ Paragraph 2, page 12 of LECG's Response.

³⁰ As noted at paragraph 106 we give two versions of the operator HHIs. The two versions relate to the treatment of a small number of operators offering both cable and non-cable pay TV services in a given country: measure 1 allocates the cable-only services of those multi-platform operators into the single "cable group" and treats their non-cable operations as separate firms; hence measure 1 underestimates the degree of concentration. Measure 2 allocates both the cable and non-cable services of those firms into the single "cable group"; hence measure 2 overstates the degree of concentration. For example, Telenet's 2005 share of cable pay TV services in Belgium is 33% while its share of non-cable pay TV services is 8%. Measure 1 assumes that Telenet's non-cable pay TV services (8%) compete with the cable group (85%), which includes Telenet's cable pay TV services. In contrast, measure 2 assumes that Telenet does not compete with the cable operators and therefore all pay TV services of Telenet are included in the single cable group (92%). By construction, this second measure produces higher HHIs.

the platform-level HHI's correctly group operators by platform. However in so far as the operator-level HHI's have been grouped by platform this simply and correctly reflects the market situation.

65. While some countries have multiple cable operators, the situation in all countries is that they do not compete directly with one another. This is similar to the position in the UK prior to the merger of NTL and Telewest, when a consumer with access to cable had access to either NTL or Telewest but not both. In such a situation it is appropriate to group cable operator shares. In contrast, where a country has multiple DTH suppliers, consumers are usually able to access all DTH suppliers as well as a single cable supplier.³¹ LECG are also incorrect when they claim that the operator HHI's are non standard; the operator HHI's are entirely conventional.
66. LECG (as their more important point³²) query our "implicit assumption [that CRA's] HHI measures provide a good indicator to the competitiveness of the pay TV industry in the 15 European countries considered". HHI's are widely considered standard measure of market competitiveness. Whatever their imperfections, they provide a better measure of market competitiveness than the completely non-standard variable used by LECG.
67. Given the unconventional nature of the variable used by LECG, the burden is on them to demonstrate why it is a superior measure of competitiveness in relation to pay TV compared to standard measures used in competition analysis. Where they attempt to do this, the logic of their argument is entirely circular. For instance, at page 12, paragraph 4, LECG indicate that their more important point is that ARPS and "market share of the largest DTH firm" are positively correlated whereas ARPS and the HHI measures are uncorrelated. They state: "*the countries that they regard as less competitive according to their measures of concentration are precisely those that exhibit lower pay TV prices*", and argue that this demonstrates that market share of the largest DTH firm is a better measure of market structure. But only if you knew with certainty that market structure in this case was the cause of higher prices, would the stronger correlation provide some validation of the measure of competition. One cannot assume the hypothesis that one wants to prove to argue for the quality of a measure.
68. We are surprised that LECG have failed to adopt a more conventional approach to the measurement of competitiveness now that they recognise that the data allows the calculation of appropriate measures.

³¹ In LECG's example, the first scenario has 3 DTH operators with 20 percent and 4 cable operators with 10 percent each that do not compete with one another and are grouped together as a single entity with 40 percent. In this situation the CRA operator HHI as calculated is 2,800. In the second scenario with one DTH operator with 50 percent and two cable operators with 25 percent each, the cable operators do not compete and are grouped into a single entity with 50 percent and the CRA operator HHI is 5,000. The second scenario is clearly more concentrated than the first, and the calculated HHIs accurately reflect this.

³² Paragraph 3, page 12 of LECG's Response.

4.3. LECG DO NOT ADDRESS THE RELATION BETWEEN SHARE OF DTH AND PROGRAMME QUALITY

69. LECG claim to refute our comment that the simple relation between DTH penetration (“market share of the largest DTH firm”) and ARPS that they observe in their data is likely due to the fact that *on average, consumers buy higher quality packages from DTH suppliers than from cable suppliers* – because cable packages without pay content are a substitute for over-air transmission in a number of countries.³³
70. In a confused discussion, LECG imply that we have claimed that countries where the market share of the largest DTH firm is high also have high absolute pay TV penetration. This is not correct: as is confirmed by LECG’s own quotes on page 12, we claim that countries with high DTH penetration also have high average package *quality*.
71. In fact we would expect the relation between the “market share of the largest DTH firm” and overall penetration of pay TV services (as measured by the Screen Digest data) to be negative (as it is). Countries with very high “pay TV penetration” in the Screen Digest dataset are countries where the distribution of FTA services over-air is poor, and many consumers receive such services over cable using packages with no pay content. Such countries are characterised by a high market share for cable, and a low average package quality. As the cable and DTH platforms were the main platforms for the supply of pay TV in 2005, high cable market share translates into a low DTH market share and a low market share for the largest DTH firm. (Overall DTH share and the DTH share of the largest DTH supplier are unsurprisingly closely correlated, 0.97). Putting this together, countries with a high overall penetration have low quality and a low market share for the largest DTH supplier which is what we find.
72. Paragraph (b) in LECG’s response simply repeats the claim that the differences between legacy and non-legacy cable countries are adequately dealt with by the “cable always largest” variable. As already noted at paragraph 39, introducing the “cable always largest” variable is simply a reparameterisation of the fixed effects and as a result its introduction has no effect on the estimated coefficients of the covariates.

4.4. IMPLICATIONS

73. The aim of LECG’s original study was to determine the effect of competition amongst pay TV suppliers on the price of pay TV services. They claimed that the “market share of largest DTH firm” was a satisfactory measure of market power.³⁴ However, if one calculates standard measures of market power (such as HHI’s), one finds that in fact “market share of largest DTH firm” is unrelated or even negatively related to

33 LECG Response, page 12, final paragraph and on page 13.

34 LECG First Report, slide 14, third bullet.

concentration. Hence there is no evidence that there is any relation between “market share of largest DTH firm” and market power as conventionally measured. As we have suggested, it is entirely likely that any simple relation between “market share of largest DTH firm” and ARPS is due to the higher average quality of DTH packages supplied by DTH operators.

74. If we were to take LECG’s argument seriously, the implication is that market concentration has nothing to do with the determination of pay TV prices. All that is left of LECG’s argument is a suggestion that there is some unique structural feature of DTH broadcasting that allows DTH operators (as compared to cable operators) to exert market power. However no credible case for this has been made.³⁵ The absurd implication of LECG’s argument is that they believe that simply prohibiting DTH delivery of pay TV services would result in lower prices for those services. The overall conclusion is that LECG’s argument is unrelated to and unsupported by conventional economic arguments.

5. CONCLUSIONS

75. In summary, LECG have failed entirely to address any of the problems we raised with their original study and, as a result, the main conclusions of our earlier critique are unaffected. Their econometric study does not support LECG’s contention that quality-adjusted prices for pay TV services are higher in the UK than other European countries or that market power is the cause of the higher ARPS observed in the UK.
 - a. The quality of pay TV programming varies enormously both across Europe and over time. LECG attempt to control for this using information from Screen Digest on “programme expenditure” per subscriber. However this data is a poor proxy for programme quality as it covers only about one fifth of expenditures. It is primarily expenditures on premium channel movie rights and excludes expenditures on sports content. The extent to which it includes expenditure on programming for basic channels is unclear. LECG claim in their response that this does not matter. But this is wrong.
 - b. While the programme expenditure-based proxy for quality is poor, it is particularly poor in the time series dimension where it is basically uninformative in explaining changes in ARPS. This is important as the fixed effects regressions used in LECG’s study rely on meaningful time series variation in programme expenditure to identify (i.e. control for) the effects of quality. As a result these regressions fail to properly control for quality.

³⁵ At slide 11 of the LECG Report, LECG appear to claim that wherever DTH operators have a large “market share” their “vicious circle” comes into force. However there was no argument as to why this should be specific to DTH and would not also apply to other platforms such as cable.

- c. Using a slightly more recent version of the same Screen Digest data that LECG use we obtain very different results to them - for example the size of the coefficient on programme expenditures, which is a key variable in this study, completely changes in both magnitude and statistical significance. In our results, it becomes statistically insignificant at any conventional level of significance.
 - d. LECG's measure of market structure ("market share of leading DTH firm") is inappropriate, and its use is unjustified when there are more conventional measures readily available.
 - e. The positive coefficient on "market share of leading DTH firm" likely reflects the omission of an effective control for quality. As DTH customers on average buy higher quality packages the market share variable is, in effect, acting as a proxy for quality.
 - f. Finally, like their First Report LECG's Response contains numerous data and methodological errors, and false claims.
76. We stand by our earlier conclusions that LECG's empirics give **no support to the claim that the structure of the UK pay TV sector leads to higher quality-adjusted prices in the UK than other European countries.**

APPENDIX A:

DETAILED REVIEW OF DATA AND METHODOLOGICAL ERRORS

77. This appendix provides a detailed point-by-point rebuttal of the issues raised by LECG. We follow the organisation of the LECG Response, and show that on each point LECG is wrong, misleading, or both.

A.1 DATA AND MEASUREMENT ISSUES

78. Despite a number of requests to Ofcom – together with offers to share our own data – we have not received LECG’s data and the code they have used to generate their results. Nevertheless, from the information in LECG’s slide pack we were able to make some comparisons between our version of the Screen Digest data and theirs.

A.1.1 Data discrepancies

79. As mentioned in Section 3, we have found large and unexplained discrepancies between LECG’s data and our data. Our data was a very slightly later version of the dataset used by LECG.

80. At page 4, paragraph 2, LECG claim that we have overstated the differences between our data set and their dataset. LECG appear to have rather different expectations compared to ours. When we started this exercise we expected that – while we did not have LECG’s dataset available – our data would be substantially similar to LECG’s with most data points within one or two percent of the published LECG data. However we found much larger discrepancies. These include:

- (i) **Significant differences in ARPS figures:** Comparing information given by LECG on average revenue per subscriber, we found differences in the average over time of more than 10 percent for four countries;
- (ii) **Very large differences in “market share of largest DTH firm”:** Comparing data on the “market share of largest DTH firm” where we had detailed LECG data available from their First Report, we identified differences as large a factor of 4 (Italy 1997) and a considerable number of cases with very large discrepancies (more than 50 percent) for Germany, Norway and Sweden;
- (iii) **Incorrect classification of Norway as a “cable always largest” country.** LECG admits their classification of Norway was incorrect, and is misleading on the effect of this error on their results. At page 5, paragraph 2 LECG confirm that for the variable “cable always largest” (i) Norway was classified in their data as not having cable always largest, and (ii) this classification was in fact incorrect. They also state that

“including Norway as in the set of cable always largest countries has no significant effect on our qualitative results”. Annex D of the LECG Response reveals that including Norway in the set causes the magnitude of the estimate of the “effect of cable always largest” to increase by 51 percent from -65.0 to --98.3. This is not negligible. In our data, as noted in our October Report, this was one of the main factors that reduced the estimated UK-specific effect from 9 percent above the European average to 3.1 percent below. Comparing LECG’s Figure 8 at page 26 with slide 49 in their First Report indicates a very similar effect with the LECG data. The fact that the UK country effect goes from being positive to being negative seems to us to be material.

81. For other variables we only had summary data from the original LECG study. The summary data indicate large differences for the key variable “programme of expenditure per subscriber”, which shows a large variation driven by a few observations. LECG claim that “standard practice” is to drop the outliers and re-estimate. This is not correct; other approaches include robust regression techniques or winsorising the data. This might have been appropriate if we had been carrying out a *de novo* study. But we were not; we were attempting to replicate LECG’s work, and other evidence indicated that LECG included those observations. So it was not appropriate to simply drop them.
82. Further, as discussed above at paragraph 53, we found that when we re-estimated the regression using (i) robust regression and (ii) dropping extreme data points, there are substantial changes in the estimated coefficients but the qualitative picture remains the same: and we certainly do not obtain LECG’s results.
83. At page 4, paragraph 2 of their response LECG note that the summary statistics for variables included in the study other than “programme expenditure per subscriber” are similar, and that this implies that the underlying data is also similar. This is simply not so: taking for example the “share of largest DTH firm” variable, where we are able to compare the detailed data, we found that despite the summary statistics for the LECG and CRA data being similar there are, as detailed above, order of magnitude differences for specific data points.
84. Differences of this size in the underlying data can cause large changes in the results. LECG suggest that this is not the case. We do not think that this is true and we discuss these differences at paragraph 100.

A.1.2 Measurement of pay TV prices

85. At page 6, paragraphs 3 and 4, LECG criticise our comment that to describe ARPS as a “price” is misleading. The term “price” suggests that pay TV is a single homogeneous product with a single price. As we noted in our October Report, ARPS is a price paid by no one, for a package that does not exist. Consumers buy a particular package. These packages vary enormously in both price and content within and across countries and over time. It is our view that variation in average package quality is likely the major variable underlying the observed variation in ARPS.

86. LECG also defend ARPS on the basis that an allegedly equivalent measure – ARPU – is often used as a measure of price in telecommunications policy. First, it is clear that in many situations regulators are in fact interested in average revenues earned by operators (across a portfolio of products), rather than the **prices** of the constituent products. In such circumstances Average Revenue per User is, obviously, an appropriate measure to use. Second, in many cases in telecommunications policy – particularly at the wholesale level – products are relatively homogeneous compared to the high degree of heterogeneity observed in pay TV services.
87. Finally, LECG argue that using published retail prices would be a worse measure of pay TV prices (because it does not take into account discounts). This is irrelevant to the point that ARPS is not a good measure of the “price” of pay TV services. You cannot say that something is a good measure simply because something else may be a worse one.

A.1.3 Controlling for quality differentials

88. Most of the key arguments made by LECG in this section are refuted above in Section 2.
89. We note in addition that at page 9, first paragraph, footnote 50 LECG’s dismissal of our criticism of using “Cable always largest”³⁶ is confused and incorrect. Our first comment was that there is no time series variation in this variable. (It is a variable that, for any particular country is either 1 or 0 in every period). If within-country changes in market structure are being used to identify changes in prices, “cable always largest” is useless as it does not vary within countries over time. Hence, if cable penetration changes over time (which it does) and is correlated with changes in market structure and price (which it is), then failure to control for it will generate biases. Our second point which is not contested was that it would be better to use a continuous rather than discrete version of this variable.

A.1.4 Controlling for market structure

90. Most of the key arguments made in this section of the LECG Response are refuted in Section 4, above.
91. In addition, we note that at page 11, final paragraph, LECG misrepresent our argument. In our October Report we stated that “market share of the largest DTH firm is not a conventional measure of competitiveness as it ignores the effect of cable firms and other competitors on the overall degree of competition.”³⁷ This is entirely correct. Standard measures of competition would be n-firm concentration ratios and HHI’s.

36 October Report, paragraph 57.

37 October Report, paragraph 62.

92. At page 12, paragraph 2 LECG effectively admit that the “market share of the largest DTH firm” misrepresents the extent of competition in Austria (and elsewhere), but claim this is overcome by including the “cable always largest” variable in their regression. This is in general untrue, and in this specific case it is completely false. As discussed at paragraph 39, LECG uses a fixed effects regression. As a result their estimated coefficient depends only on *within-country* variation in the “market share of largest DTH firm”. The “cable always largest” variable is simply a re-parameterisation of the fixed effects. As a result its introduction has *no effect* on the estimated coefficients of the covariates (such as programming expenditure and “market share of largest DTH firm”), though it has a very large effect on the estimated (residual) country effects (as we would expect).

A.2 METHODOLOGICAL ERRORS

A.2.1 Endogeneity

93. A major concern we raised in our October Report was that LECG’s results suffered from serious endogeneity problems for some key variables. LECG attempted to respond to our criticisms but their proposed “solutions” do not work. We discuss the problem extensively in Appendix C below.

A.2.2 Attenuation Bias

94. At page 16, first paragraph, LECG do not contest our observation that fixed effects estimation exacerbates the bias caused by measurement error. They simply deny the overwhelming evidence discussed in Section 2 above that this is the case for within-country variation in the programming expenditure variable, and that as a result of attenuation bias the estimated coefficient on that variable is far too small (effectively zero in some specifications). The issue is not, as LECG imply, measurement error in the variable “market share of largest DTH supplier” but in the “programme expenditure per subscriber” variable.
95. As discussed formally in Appendix B, if the programme expenditure variable is measured with increasing error, the coefficient on the UK dummy (or country effect) will pick up the signal and be biased upwards. Consider the extreme case where the programme expenditure variable is pure noise: then we would be back to the model without any control for quality, where we know the coefficient for the UK country effect is much higher.

A.2.3 Dynamics

96. At page 16, paragraph 2, LECG claim that there is no evidence that their dynamic specification is incorrect. This is not so. As noted in our October Report, all of their reported specifications show autocorrelation in the errors. Their preferred IV

estimates show overwhelming autocorrelation.³⁸ Both these findings are clear evidence of incorrect dynamic specification. Finally, LECG's specification of a dynamic panel data model³⁹ is not consistent with the dynamic specification of their standard model and implies that the dynamic specification of the standard model is not correct.

A.2.4 Inference and serial correlation

97. LECG fail to address the point that all their estimated standard errors and t-statistics are incorrect, as they fail to account for the serial correlation of the errors in the panel. This will mean that it is likely that their estimated standard errors are too low and their t-statistics are too high.

A.2.5 Time Effects

98. Nowhere do LECG address the point that there are likely to be cross-European shocks and price trends, so that time dummies or at least a polynomial time trend should be included.

A.2.6 Fixed Effects Estimation

99. At page 16, paragraph 3 LECG do not contest our view that their complex IV estimator is unnecessary and confusing, nor that the correct estimator is the standard fixed effects estimator with re-parameterised country effects to incorporate the grouping by "cable always largest".

A.3 ISSUES OF THE ROBUSTNESS OF THE RESULTS ACROSS THE TWO DATASETS

Substantial differences in results between LECG and CRA

100. At page 18, paragraph 2, LECG claim that the discrepancies in the datasets do not affect the results, so that their findings are "robust". At paragraph 88 and Table 4 of our October Report we compared LECG's reported estimates with estimates based on our data, calculated using LECG's complex and incorrect method of estimation. The key coefficients are those on the "programme expenditure per subscriber" variable, the "market share for largest DTH supplier" variable and the "cable always largest" indicator variable.

- (i) Comparing our results (CRA (1)) for "programme expenditure" with LECG's, LECG get an estimate of 0.675, and we get an estimate of 0.015 – essentially zero. *Our estimate is about 2 percent of the LECG estimate. We regard a reduction of a key coefficient by more than 97 percent to be*

38 See Table 1 above and October Report, paragraphs 15, 85 and 87, Table 4.

39 LECG Response, page 15, final paragraph.

material. We believe that our comment that “programming expenditure goes from having a small effect to no effect” is justified. The fact that within-country variation in programming expenditure has no appreciable effect on ARPS is a key result and wholly undermines LECG’s analysis.

- (ii) Comparing estimated coefficients for the “market share of largest DTH firm”, our estimate is 79 percent of LECG’s. Hence as we noted the effect of the market share of the largest DTH firm is materially reduced but not eliminated.
- (iii) Comparing the estimated coefficient for “cable always largest”: LECG’s estimate is large (37 percent of average ARPS in 2005) and our estimate is enormous, 2.5 times the LECG estimate and 92 percent of average 2005 ARPS. Again we would regard an increase in magnitude of this size as material.

Misleading statements as to results and use of “significance levels”

101. LECG’s discussion in Section 4 of their Response systematically misrepresents our position in order to purport that our results are similar to their own. They imply that we are misleading readers about the implications of our analysis – for example as a result of sleight of hand with regard to the reporting of significance levels.⁴⁰ This is completely untrue. The reality is the reverse: the discussion in our October Report was straightforward and correct while LECG’s Response is incorrect and appears intended to obfuscate.

102. LECG’s argument at page 17, paragraph 2 is misleading and incorrect. They imply that we are misleading readers with our discussion of our results and that in reality our results are similar to theirs. They do this by comparing their results with a specification where we use the LECG market share data (“CRA(2)”). Our results are still different, but the main point we made is that they are radically different from LECG’s when we estimate their specification using the market share (and other variables) from our data (“CRA(1)”). *The quoted statement from paragraph 88 of our October report refers to a comparison between columns LECG and CRA(1) in Table 4.* The coefficient correctly reported in Table 4 for “programme expenditure per subscriber” for the LECG data is 0.675 with a standard error of 0.191. This is statistically significant (greater than zero) at the one percent confidence level and hence as correctly reported in Table 4 at the five percent level. In contrast the estimated coefficient using CRA data is 0.015 with a standard error of 0.094. This is not significant at either the five percent level or the ten percent level. So the

⁴⁰ For example, at paragraph 3 on page 17 of their Response, LECG allege that our approach to the reporting of significance levels in our October Report – which was entirely straightforward – generates a “false impression regarding the robustness of the empirical results in the LECG study”, while footnote 80 on page 17 alleges that the same issue creates a “misleading impression of lack of robustness”. The simple fact is that, as explained fully in both our October Report and above, LECG’s results are not robust.

discussion at paragraph 88 of our October Report is correct and LECG's conclusions at page 17, paragraph 2 are simply untrue.

103. LECG's discussion is based on a comparison of the columns "LECG" with "CRA(2)". The estimates given in the CRA(2) column use LECG data on the variable "market share of largest DTH". As noted above contrary to the assertions of LECG there are substantial differences between the LECG and CRA data for "market share of largest DTH". LECG have provided no evidence that their data is correct and that the CRA data is incorrect. Indeed, we believe that the reverse is true. Where we have been able to identify specific problems such as with Norway, LECG have conceded that our data is correct.
104. LECG's discussion at page 17, paragraphs 3 and 4 is again confusing obfuscation. At paragraph 3 LECG state that in the "CRA report the estimated coefficient for the variable market share of largest DTH supplier is reported to be statistically significant in the LECG and CRA(2) specifications but not under the CRA(1) specification." This is not accurate. In fact we report correctly that the coefficient in the LECG and CRA(2) specifications are significant at the 5 percent level and that the coefficient in the CRA(1) is not. LECG go on to comment that "the estimated coefficient for this variable in the CRA(1) specification is statistically significant at the 10 % level". While this is true (the coefficient is significant at the 10 percent level but not at the 5 percent level) it is not material.
105. At page 17, paragraph 3 LECG suggest that we use different criteria for different specifications. This is not correct. We use (as stated) the five percent criterion throughout⁴¹.
106. At page 17, final paragraph LECG suggest a simplistic and mechanistic approach to the interpretation of statistical results. In particular they concentrate on statistical significance, to the exclusion of almost any other consideration. We do not believe that this is appropriate. The mechanistic application of statistical tests is usually discouraged. In our results we indicated significance at the 5 percent level and also gave standard errors so it was possible for a reader to compute the critical value and obtain the p-value if so minded. However in our view the magnitude and sign of an estimated coefficient is as (or more) important than the statistical significance. LECG's point is a complete red herring. Even if we had followed LECG and reported three levels of significance for each coefficient estimate (1%, 5% and 10%), rather than 5 percent alone, the findings of our report would have been *entirely unaffected*.

Outliers do not cause the differences in results

107. Finally, as discussed in detail, both in Section 3 and in A.1.1 above, it would not have been correct to drop those observations where the programme expenditure

⁴¹ There is a minor typo in the reported significance of the LECG coefficient for the number of platforms which is reported as significant at the 5 percent level when it is only significant at the 10 percent level. So it should not have been given an asterisk. But this is in no way material.

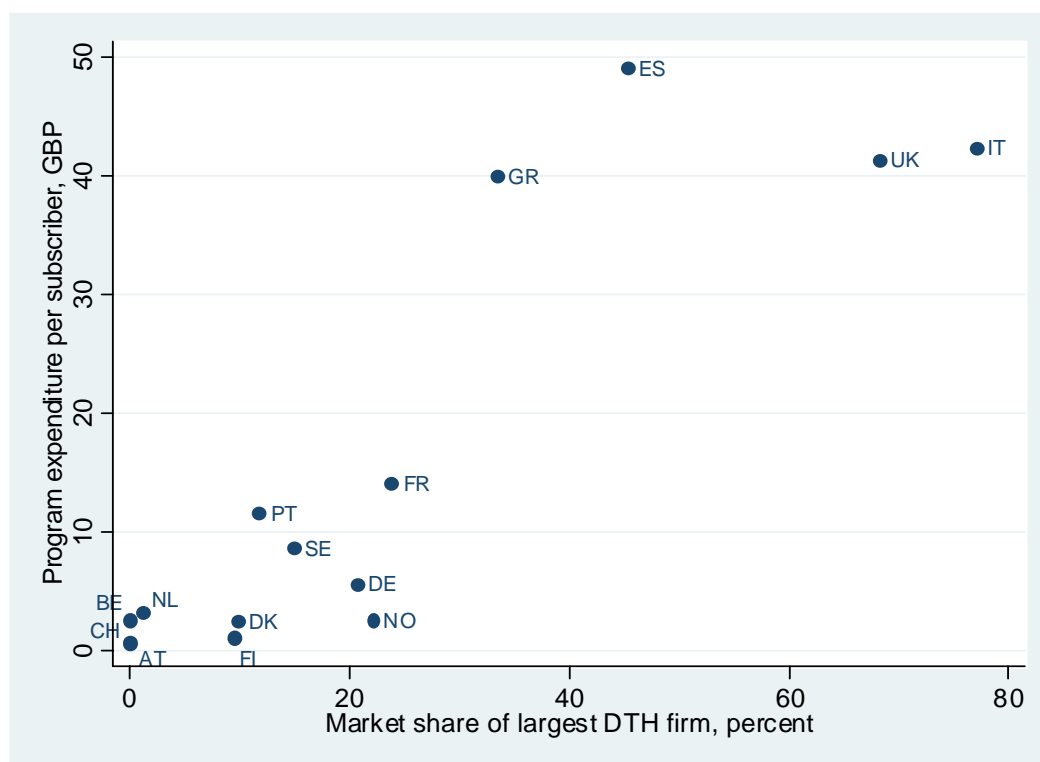
variable took on extreme values, as LECG reported that they *included* those observations in their data set. Moreover, as reported above, when these observations are dropped, while the coefficient estimates change, we do *not* get LECG's results.

A.4 INTERPRETATION OF RESULTS

108. At page 18, paragraph 2 LECG use selective quotation to misrepresent our arguments in the October Report. Paragraph 16 of our report concludes that “the Screen Digest data are consistent with the observed variation in ARPS being explained by variations in the pay TV component of the average packages sold in individual countries.”

109. As discussed above, LECG do not provide any relevant evidence that within-country variation of the programme expenditure variable explains ARPS. They only provide evidence of cross country variation, which is not relevant for fixed effects estimation. In fact both LECG's and CRA's data indicate very clearly that there is no such within-country relationship.

Figure 4: Program expenditure per subscriber and market share of largest DTH firm, country averages



Source: CRA calculations based on data from Screen Digest.

110. LECG go on to examine the relationship between the “market share of the largest DTH supplier” and the Screen Digest “programme expenditure” variable. They claim that evidence does not support the proposition that DTH subscribers buy higher quality packages. However their evidence, Figure 4 of their Response has been

censored and is misleading.⁴² LECG include observations for only six of the fifteen countries in their study. If you include the legacy cable countries that LECG has omitted as in (our) Figure 4 above, the result is quite different.⁴³ Countries where DTH share is low also have low programme expenditure per subscriber and countries with high DTH share have high programme expenditure. This is entirely consistent with the idea that on average DTH subscribers take packages with higher pay TV content. Hence if you look at *all* the facts rather than a carefully selected subset, they are consistent with our interpretation and not LECG's.

111. Even if the facts had not been censored, it is likely that they are not relevant as again LECG is focussing on the cross country relationship when this is not relevant for fixed effects estimation, which depends on within-country variation. For the reasons discussed above we believe that within-country variation in per subscriber programme expenditure is a poor measure of changes in quality, so it is unclear that that the relation between changes in the market share of largest DTH firm and changes in per subscriber programme expenditure tells us anything about the relative quality of DTH subscriptions.

⁴² LECG Response, page 18, fourth paragraph and page19.

⁴³ The data points are country averages for 1997-2004. As for the analysis in section 2.2 (extreme) observations for Italy in 1997 and 1998 are omitted. Their inclusion if anything strengthens the conclusion. The axes are reversed in our Figure 4 (compared to LECG) as this is more intuitive and reflects the assumed direction of causation.

APPENDIX B:

WHY USING “PROGRAMME EXPENDITURE” AS A MEASURE OF QUALITY BIASES RESULTS FOR THE UK

112. A major issue in this econometric analysis is whether the variable “programming expenditure per subscriber” does a good enough job at capturing the variation in quality of pay TV services supplied to consumers. We observed that the data on programming expenditure is missing out large amounts of expenditure, to which LECG respond that this does not matter, as the data is *correlated* with the true variable. In addition to the comments made above in Section 2, we note that even if a proxy is correlated with the true variable, it does not necessarily remove all the bias associated with an omitted variable – but only some of it.

113. In this case we have good reason to believe that the poor measure of quality used by LECG ***systematically biases the estimate of UK quality-adjusted prices upwards***. This is because we know from data that even their poor measure of quality is much higher in the UK than in other countries. So it stands to reason that the unobserved measure of quality is also higher in the UK, unless programming expenditure per subscriber is a perfect measure of quality, which not even LECG would claim. Hence any failure to control for the unobserved quality will make the UK coefficient “too large” and the size of this bias will be increasing in the degree of measurement error in the proxy for quality.

114. To show this formally, consider the parameter on the UK dummy.

$$p = \alpha z^* + \beta UK + e$$

115. Where p is ARPS in country c , UK is a dummy = 1 if the country is the UK and zero otherwise, and z^* is true quality. Instead of z^* we measure $z = z^* + u$, where u is the unobserved component of quality (i.e. the measurement error).

116. The bias on an estimate of β using the mis-measured variable z will depend on the correlation structure between the UK and the error term ($e + \alpha u$). Given that $E(z^*UK) > 0$ we will expect that $E[(e + \alpha u)UK] > 0$ and so the OLS estimate of β , $\hat{\beta}$, is biased upwards, i.e. we falsely infer that quality adjusted prices are high in the UK.

117. So what are LECG saying? First, they might be claiming that true quality is not higher in the UK so $E(z^*UK) = 0$. But this is contradicted by their Figure 1 (and our Figure 1 above) showing that their measure of quality is higher in the UK in all countries bar two. Second, they could be saying that the UK dummy is correlated with the observed part of programme quality but uncorrelated with the unobserved part of programme quality, i.e. $E[(e + \alpha u)UK] = 0$. But what could possibly justify this? Any force (e.g. income growth, regulation, etc.) generating a correlation with observed quality will also generate a correlation with unobserved quality. So this is also a non-starter. Thirdly, they may be claiming that there is no measurement error

in the quality variable so $u = 0$. This seems to be the content of LECG's footnote 49. But this is misleading: it is true that there is no bias if "good proxy" means "perfect measure". But a proxy is by definition an imperfect measure and LECG are not claiming it is perfect.

118. The only charitable explanation we can think of for LECG's case is that they are arguing that u is "small" so the bias $(\hat{\beta} - \beta)$ is "small". The arguments we have given in Section 2 suggest instead that this bias is non-negligible.

APPENDIX C:

LECG'S "ENDOGENEITY SOLUTIONS"

C.1 THE ENDOGENEITY PROBLEM

119. In our October Report we noted that LECG's results suffered from serious endogeneity problems in key variables. LECG's position on endogeneity is far from clear despite extensive discussion in their most recent response⁴⁴. We will start with some general discussion of the issues, before considering the three proposed models that LECG offer to solve the problem.

120. First, we note that LECG misstate our original comments on endogeneity. At paragraphs 65 to 67 of our October Report we discussed the endogeneity of the "market share of the largest DTH supplier" variable, and at paragraphs 71 to 74 that of the "programming expenditure" variable. LECG *themselves* suggested that both variables are endogenous. In their First Report, LECG suggested that by lagging the variable one period this problem is avoided⁴⁵. At paragraphs 65, 66, 73 and 74 of our October Report we noted that this is not true if the errors are serially correlated. An estimate of LECG's preferred specification 4, reported in Table 4 of our October Report indicated severe autocorrelation with first order serial correlation in the residuals of 0.98.

121. As noted in our October Report, if the errors are serially correlated, then the lagged variables are also endogenous, and as a result (i) fixed effect estimation on a short panel is not consistent, and (ii) the instruments in the LECG instrumental variable estimator are endogenous, which renders the IV estimators inconsistent as well. Hence LECG's claim that by simply lagging the right-hand side variable by one period it can solve all endogeneity problems is not correct.⁴⁶ LECG do not dispute these conclusions.

122. Both CRA and LECG are agreed that the "reverse causation mechanism" is likely to cause the coefficient on the programme expenditure to be biased up. This suggests that the true values are even smaller than the actual estimated coefficients. But the estimated coefficients are tiny, 0.015 so this simply reinforces the conclusion that the programme expenditure variable fails to control for quality.

44 LECG Response, pages 13-15.

45 LECG First Report, slides 20 and 23.

46 October Report, paragraph 66.

C.1.1 Reasons for endogeneity

123. In our October Report we gave several reasons why endogeneity is likely to be important, and likely to be severe. Some reasons to suspect endogeneity from basic economic principles would include:

- a. Prices and market shares are both determined in equilibrium by the same forces: quality and cost and demand differences.
- b. LECG's market share is a function of price and so is ARPS, hence this will lead to a spurious positive correlation between ARPS and market structure.
- c. The left-hand side in the regression (revenue per subscriber) and right-hand side variable (programme expenditure per subscriber) both have the number of subscribers in the denominator leading to "division bias" (and generating a positive bias).
- d. Quality affects price, and within-country variation in quality is effectively independent of the proxy. But the omitted quality variable is correlated with the left-hand side and right-hand side of the equation.

C.1.2 Direction of endogeneity bias

124. LECG argue that there is no reason to expect endogeneity biases to generate the relations they claim to identify in the data. It is true that in general the direction of the bias is hard to sign unambiguously. In our case, however, problems b. and c. above clearly suggest that there is an *upwards* bias on the quality proxy variable, exaggerating its power to control for true quality.

125. We have also shown at paragraph 26 that share of largest DTH firm is likely to be positively correlated with unobserved quality, generating an upwards endogeneity bias on the market structure variable.

C.2 LECG'S PROPOSED "SOLUTIONS" ALL FAIL

126. LECG present three approaches that claim to deal with endogeneity through econometric means. ***None of these are at all convincing: the instruments are not credible, and the estimation procedures poorly described. None of the models deals with the endogeneity of programme expenditure.*** The first approach instruments market share, the second replaces market share with the number of years since entry of first DTH pay TV provider, and the third is some dynamic panel estimation. We discuss these in turn in the next three sub-sections, explaining why they all fail.

C.2.1 Instrumental Variable estimates of market share

127. This is contained in LECG's Appendix F on page 29 (Table 7). There is no text explaining clearly what they do, just a single paragraph at the top of page 15, a table and notes to the table. There is no formal statement of their model, but we guess their estimated model is:

$$p_{ct} = \gamma g_{ct} + \lambda s_{ct} + \eta_c + \varepsilon_{ct} \quad (A1)$$

where p_{ct} is ARPS in country c at year t , modelled as a function of GDP per capita (g) and market share of largest DTH firm (s). Country fixed effects are η_c , and ε_{ct} is an error term.

128. A first oddity of equation (A1) is that programme expenditure – the key quality variable in the model – has been mysteriously dropped from the specification. Obviously omission of a key variable implies a misspecification and therefore bias. It is perplexing why LECG should think that this can be anything like a test of endogeneity on market share when they exclude one of their key variables from the equation.
129. LECG use two instruments for s , the market structure variable (i) GDP per head in other countries (split by whether the country is cable always largest or not), (ii) the number of years since entry of the first DTH pay TV provider. No justification is given for the use of these instruments from an economic or econometric viewpoint.
130. There are numerous and obvious problems with this instrumental variable approach. A valid instrument requires two necessary conditions. First, it must be uncorrelated with the error term and second it must be correlated with the endogenous variable. Both of these conditions raise problems unaddressed by LECG.
131. First, GDP of other countries is particularly problematic as an instrument: there may be common unmeasured shocks or trends across countries (and these shocks could be stronger for countries where we have “cable always largest” compared to other countries). If this is the case, the GDP of other countries enters directly into (A1) and the instrument is invalid. This problem is exacerbated because, as usual, there is misspecification because of the absence of controls for any common time effects – e.g. no time dummies or time trends.
132. Second, number of years since entry is related to the attractiveness of the country as a location for DTH. This is correlated with expected income growth, regulation, etc. which will also be correlated with ARPS. Note that additionally there is perfect multicollinearity between this variable and the country and time dummies or time trend (that should be included in all these specifications).
133. The first stage of the regressions (the regression of market share on the instruments and other exogenous variables) are not presented by LECG, so we have no idea whether these instruments have any power. Hence, there may be the standard problem of weak instruments where a low correlation means that there will be a finite sample bias in the instrumental variable results toward finding the same results as OLS – which is what seems to be happening here.
134. The one diagnostic of instrument validity is the Sargan test, but this is well-known to have weak power to reject the null (of instrument validity). It relies on having at least one valid instrument and testing the over-identification restrictions, but there are not good reasons for thinking we have one good instrument here.

135. The standard errors only correct for heteroscedasticity and not for serial correlation. This means that the t-statistics are incorrect, almost certainly too high. As we noted many times, this affects all of the analysis in the paper, whether endogeneity is ignored or not.

C.2.2 “Reduced form” estimates

136. This approach is presented on page 31 of LECG’s Response (Table 8). Again, there is no clear text explaining what they do, just a single paragraph on page 15, the tables and notes to the table. In this case, LECG appear to have dropped market share and replaced it by one of the alleged instruments from the previous table: “Numbers of years since entry of the first DTH pay TV provider”.

137. They find that the entry variable has a positive and significant coefficient and argue that this backs up their main claims. But why? This is a different variable that could be correlated with other factors that affect price rather than simply market share. Furthermore, as noted above, “Number of years since entry” is related to the attractiveness of the country as a location for DTH (expected income growth, regulation, etc.) which will also be correlated with ARPS.

138. All the problems mentioned above are also relevant here. If the IV is invalid then the variable remains endogenous.

139. Again, LECG do not show us the “first stage”, i.e. that there is a positive relationship between the entry variable and market share.

140. Unlike Table 7, programme expenditure now is included in the regression. However the estimated coefficient is 0.007 and statistically insignificant, effectively zero. So this regression again fails to control for quality like the other specifications. This is despite the worries that it may be endogenous.

C.2.3 Dynamic panel data estimates

141. This is not described in Annex F of LECG’s rebuttal, but sketched in the last paragraph of page 15, and footnote 73. In the absence of tables and a proper description it is unclear exactly what LECG have actually done. They seem to have regressed ARPS against lagged ARPS and the entry instrument described above. They interpret the a positive coefficient on the years since entry of first DTH pay TV provider as confirmation of the hypothesis that market structure causes higher quality adjusted prices.

142. The interpretation of the coefficient on entry is subject to the same difficulties discussed in the previous sections. It is unlikely to be exogenous, collinear with the country and time effects, not shown to be related to current market share, etc.

143. Furthermore, the inclusion of lagged ARPS does not control for time varying quality. There are many interpretations of what the lagged coefficient could mean in the context of a dynamic panel data model, the standard one being to allow a difference between short and long-run effects. The most obvious problem, however, is that if

quality changes between last year and this year, *last year's prices will not control for this problem.*

144. In terms of the estimation technique, in the absence of serious description, and presentation of any results such as coefficients or standard errors, diagnostics, etc. it is hard to really comment. From the references in footnote 73 the method that LECG are claiming to follow is a version of the Anderson-Hsaio approach with some correction for small N (15 countries). This begs the obvious question of why they have not simply shown the Anderson-Hsaio results. Among the well-known problems of this approach is that the lagged t-2 levels of price are weak instruments for the change in price when prices are persistent. Since this is suggested by the strong serial correlation of price this is likely to be a problem in this context too.
145. It would be also interesting to know – but we are not told – whether the lagged price was significant in these regressions. If so, then every other regression in LECG's paper is also misspecified.