Nederlandse Mededingingsautoriteit

BIJLAGE F BIJ METHODEBESLUIT

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Betreft zaak:	Methodebesluit X-factor en rekenvolumina regionale netbeheerders derde reguleringsperiode
Onderwerp:	Aanvullende studie op het rapport van The Brattle Group naar aanleiding van de zienswijzen op het ontwerp-methodebesluit, Juni 2006

The Brattle Group

WASHINGTON

LONDON

SAN FRANCISCO

BRUSSELS

MEMORANDUM

TO: DTe
FROM: The Brattle Group
SUBJECT: Response to Companies' Comments on Report
DATE: 27 June, 2006

Below we provide reactions to the companies' comments that relate to our report.

Data Reliability

Some of the companies claim that we have made insufficient efforts to obtain reliable data. We consulted extensively with the companies regarding the data request. We travelled to The Hague on several occasions to meet with the companies to discuss the data request. We produced several drafts of the data request in response to the comments of the companies. We also discussed with the companies the types of publicly available data that we would consider. After receiving the data requests we contacted the companies that produced erroneous data, disclosed to them the problems, and gave them opportunities to revise the data. We had to contact several of the companies repeatedly until they produced adequate data. The companies that did not obtain adequate endorsements from their auditors had several months to revise the data and obtain the requisite endorsements.

The current comments by the companies do not consider the extent of our efforts to obtain data, and the responsibility of the companies to provide adequate data. All our

efforts described above have exceeded the levels reflected in our proposal, which the companies selected for this engagement. We have travelled to The Hague more often than contemplated in our proposal. We have produced more drafts of the data requests than contemplated in our proposal, and dedicated more hours to obtaining data than contemplated in our proposal.

Significance Criteria

Continuon disagrees with our 1% threshold for a regional difference factor to be substantial and with our stability criterion. We provided several reasons concerning our choice of the 1% threshold. One reason involved the difficulty of measuring efficient costs accurately within 1% especially given the quality of the available data. This might have been different if the submitted data had been of high quality, based on a proven track record and written endorsement of auditors. Continuon argues that the original benchmarks for efficiency were set within 1%. Continuon's argument makes misleading use of the decimal points found in the original DTe decision. While a company's DEA score may have involved several decimal points, it does not mean that someone ruled out the possibility of error within 1%. For example, consider a company with a DEA score expressed as 98.7. While 98.7 may have been the best estimate for a particular company, the reference to seven-tenths of a point does not mean that the analysis had sufficient accuracy to rule out the possibility that the true score was between 98.6 and below 98.8. In many cases statisticians cite the precise results of their analysis while acknowledging a wide range of potential inaccuracy. It would not be responsible for us to choose a threshold that happened to coincide with the decimal points reported for the original efficiency scores.

Continuon's discussion is not clear, but Continuon appears to argue that we can reject the role of management discretion or luck if the reported costs come directly from a company's internal system or from invoices. Continuon appears to confuse the accuracy of the reported costs with their underlying causes. It is possible to measure costs quite accurately, through invoices or a company's internal accounting system, even though the costs are heavily influenced by inefficient management or bad luck.

Perhaps a certain project should have cost €100 but ended up costing €120 through a combination of inefficiency and bad luck. Confidence that the actual cost was €120 does not provide any insight into the total costs that an efficient company should have incurred in the absence of bad luck.

Our defence of the 1% cut-off also cited the difficulty of collecting precise levels of revenues through tariff adjustments. Continuon says that some uncertainty would exist with any precise cut-off. Continuon states that the Raad uses percentages of only 0.1%. In making this argument Continuon ignores that the level of uncertainty would be extremely high as a proportion of the revenues that we sought to redistribute, if the analysis focussed on potential cost differences below 1%. As Continuon acknowledges, even a 10% threshold could imply tariff adjustments far below the 0.1% that the Raad uses. Continuon estimates that 10% higher costs for Rendo would lead to a change in revenue to the other companies of only 0.003%.

We extrapolate from Continuon's Rendo example to assess the implications of determining a regional difference as small as 0.1%. If we selected 0.1% as the appropriate threshold, we could end up recommending an incremental 0.1% for Rendo, which according to Continuon's calculations would reduce the revenue of other companies by 0.00003%. With such small differences we are concerned that it would not be worthwhile to establish an imperfect administrative mechanism for adjusting tariffs. We have never seen anywhere in the world a regulator who sought to revise a tariff by 0.00003%, because tariff systems are costly to implement, and cannot redistribute such small amounts precisely.

Continuon also believes that assessing regional differences between 2000 and 2003 is inappropriate because the three-year period is too brief and too distant from the period 2007-9, over which the new tariffs will apply. However, we discussed the availability of data with the companies, and they never indicated the ability to provide data from 2004 and 2005. Nor was it feasible to consider costs prior to 2000. Continuon ignores that the brevity of the period considered and its distance from 2007-9 would support applying even a stricter threshold than 1%. Looking for cost differences as small as

0.1% makes little sense if the available sample of data will not reflect the true regional differences accurately by the time that the new tariff period commences.

Continuon considers the stability criterion inappropriate. Continuon observes that companies may be faced with costs that fluctuate over time due to changes in local government taxes or market developments. We see a contradiction between this opinion and Continuon's complaint concerning the brevity and remoteness of the 2000-2003 period. A stability criterion is especially important when the only available data come from several years prior to the contemplated tariff adjustments. If the costs were not stable between 2000 and 2003, we could not have any reasonable anticipation that the perceived regional difference would still exist between 2007 and 2009.

Continuon believes that the compensation methodology should consider the fluctuating nature of costs. We agree. We have determined that taxes constitute a regional difference, but the actual adjustments to the tariffs should depend on the eventual taxes that the companies pay closer to the 2007-2009 period.

Water Crossings

Delta considers it inappropriate to limit its regional difference to only the incremental costs of the water crossings. Delta believes that the total costs constitute a regional difference. Delta also claims that we are inconsistent in our treatment of the Oosterschelde and the Westerschelde. Delta says that using only the incremental costs assumes that we count the Oosterschelde and the Westerschelde as part of Delta's service area, but that in our connection density analysis we assume that the Oosterschelde and the Westerschelde are not part of Delta's service area.

In our final report we consider the incremental costs of water crossings and, following comments from the companies, use in our connection density analysis the surface area of land plus inland water, rather than just the land area (see p.49 of final report). According to the CBS, however, the Westerschelde and the Oosterschelde are not included in the area of inland water. If we take the whole of the Westerschelde and

Oosterschelde to be part of Delta's service area, then Delta's service area increases by approximately 640 km2, reducing its connection density from 141 connections per km2 to 106.[1] This adjustment is relatively minor, given that the scale of differences in density previously extended from 137 connections per km2 (Essent) to 1,924 (ONS). It does not alter the results of our connection density regression.

Soil Type/Quality

Rendo believes that soil type/quality is a regional difference. Rendo says that in January 2006 it submitted a written reaction regarding this issue, but did not receive a response. Rendo complains that we have included in our final report some other information that only appeared relatively late. Rendo's relatively late submission is a report by KEMA that relates to soil quality. Several months ago we questioned in writing whether the KEMA study was conducted in the ordinary course of business, and Rendo has not responded with any evidence. We are concerned that the Kema study was undertaken in 2005 only after the commencement of the study concerning regional differences. The study examines a problem at a dairy factory that occurred in the ordinary course of business, but we cannot rule out the possibility that Rendo put pressure on the consultants to discuss soil quality in the hope of influencing our analysis of regional differences.

Even if we assume that Rendo did not interfere with the independence of the consultants, no reasonable interpretation of the study could draw any meaningful conclusions concerning the contribution of soil quality to costs. The study examines the degradation of cable supplying one dairy factory in Rendo's service area. The report states that the cable degradation was caused in large part by the relatively heavy electrical load of the factory, and the highly variable nature of the factory's load which caused thermal stress on the cable. The report clearly views the client's load as unusual, and therefore not representative of typical Rendo clients. While the

[1] Based on the surface area given by the CBS for the year 2000.

study mentions poor soil quality as a contributing cause, the study does not isolate or quantify the contribution of poor soil quality to the cable degradation, and in no way suggests that poor soil quality would affect a typical Rendo client.

Rendo also says that we did not conduct a sufficient bottom-up analysis. Our proposal never offered a bottom-up analysis in the absence of statistical results. Rendo's claim ignores our original proposal, which clearly stated our intent to use a bottom-up analysis as a check on statistical results. We independently attempted to see if the companies' costs were correlated to their percentage of connections in poor soil areas, but the companies did not provide sufficient data to derive meaningful results. We have no meaningful statistical results that we could check with a bottom-up analysis.

However, we did consult our own engineering expert, IBC. IBC's core business is to advise companies on the economics of building, purchasing or expanding gas and electricity distribution companies. IBC analyses new projects from the ground up. Based on IBC's past experience with different soil types, IBC concluded that the incremental maintenance costs associated with poor soil are likely to be small.

Connection Density

General Comments & Use of Data

Delta and Essent disagree with our conclusion that connection density is not a regional difference that should be incorporated into the tariffs. They believe that we should repeat the connection density analysis, this time using reliable data and different assumptions. Delta and Essent had plenty of opportunities over the course of several months to suggest alternative data sources when we were preparing the data requests.

Delta says that it has received no response from us to the report it commissioned PwC to write on its behalf. This is not accurate. We discussed the report and made specific comments on the report. We point out, as we did earlier in a reaction memo that was sent to the companies together with the final report on 8 March 2006, that

some of the analyses in the report rely on the regional difference costs presented in a previous version of our own report. Since then, Delta had provided evidence that its maintenance costs related to the unique problems of water crossings and not to general problems that one would expect with underground cables. Delta's evidence substantially raised our estimate of its regional difference, making Delta appear more efficient. Delta's greater efficiency could be expected to reduce the significance of any regressions concerning connection density, since Delta had one of the lowest connection densities in the sample.

Furthermore, we pointed to claims from Continuon that Essent had provided incorrect information that risked an incorrect finding of significance in any connection density analysis. The PwC analysis did not consider the effects of the potential Essent error. We also pointed out that PwC relied on data that had not secured the endorsement of independent auditors.

PwC claims that we should not restrict our analysis to linear regressions. PwC ignores that we also consider several non-linear equations, which we mentioned specifically in our report. These regressions have not provided any indication that connection density is a regional difference.

Rendo says that it is inappropriate to use standardised costs in the analysis because they reflect the results of negotiations, and may therefore fail to reflect the actual costs accurately. Rendo ignores that the companies agreed on using standardised costs for the regional differences study. Furthermore these costs are also at the basis of the method of regulation used by DTe.

Essent says that we seem to put more faith in the data of Continuon than the data of the four companies Rendo, Delta, Essent and Westland. This is misleading. We do not place faith in any of the data. The discrepancy between Continuon's costs and those of the other four companies indicates that some of the data must be either wrong, attributable to luck or differences in efficiency, or may be otherwise misleading. Essent considers it more likely that the difference between Continuon and the other four low density companies is explained by luck or data problems relating to Continuon, than to luck, inefficiency or data problems relating to the other four

companies. However, this is pure speculation. A rigorous statistical analysis does not pick and choose favourite data in rejection of others, except in the presence of specific and well-documented reasons. Essent has produced absolutely no reason to reject Continuon's data. We followed the proper approach in this case of admitting that conflicts among the data undermine the ability to draw any meaningful conclusions from the regression analysis.

Essent has drawn inappropriate conclusions because we discussed Continuon separately from the other four companies. Discussing the four companies as a group was merely a matter of convenience, providing one useful focal point for illustrating the data problems in the sample. Choice of the group and its contrast to Continuon was not material to the results. We see serious data discrepancies within the group of four, even ignoring Continuon for the moment. The costs per composite output of Rendo, Delta, Essent and Westland differ substantially despite the companies' similar connection densities. Rendo's costs per composite output are 12% higher than Westland's. Rendo has almost exactly the same connections per km2 as Delta and Essent but 5% higher costs per composite output. Something such as luck, data problems or inefficiency must be contributing to the substantial differences between these companies. The discrepancies impede the ability to determine reliably whether and to what extent connection density itself has any significant effect.

PwC observes that the data on number of connections including street lighting seem strange. PwC believes we should exclude the street lighting connections from our analysis. We agree that the data on the number of public lighting connections seem strange. PwC points out that if connection density is recalculated without including street lighting connections, Continuon has a connection density twice as large at that of Delta and 50% more than that of Essent. Therefore, Delta argues that we are incorrect to state that Continuon has similar connection density to Delta and Essent but lower costs. Delta ignores our previous demonstration in reaction to comments on the draft report that the exclusion of public lighting connections did not improve the reliability of the results. We still found that almost all of the statistical relationships were insignificant, except for one particular combination of data that still did not permit us to measure any effect reliably due to the wide range of uncertainty in the

estimates. As the graphs below (Figure 1 and Figure 2) show, the exclusion of public lighting connections makes little difference to the results.

Figure 1: Connection density (incl. public lighting connections)

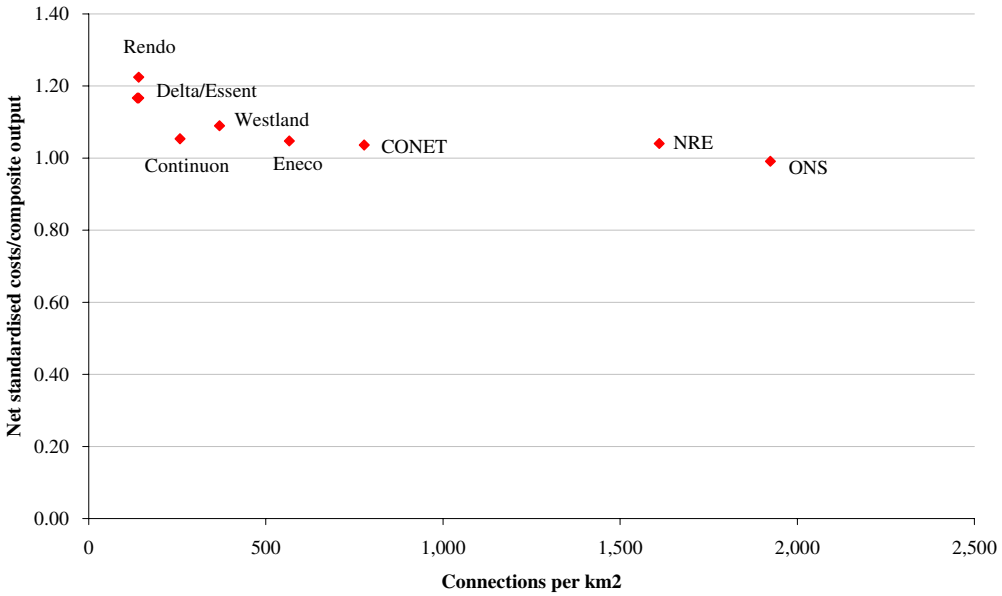
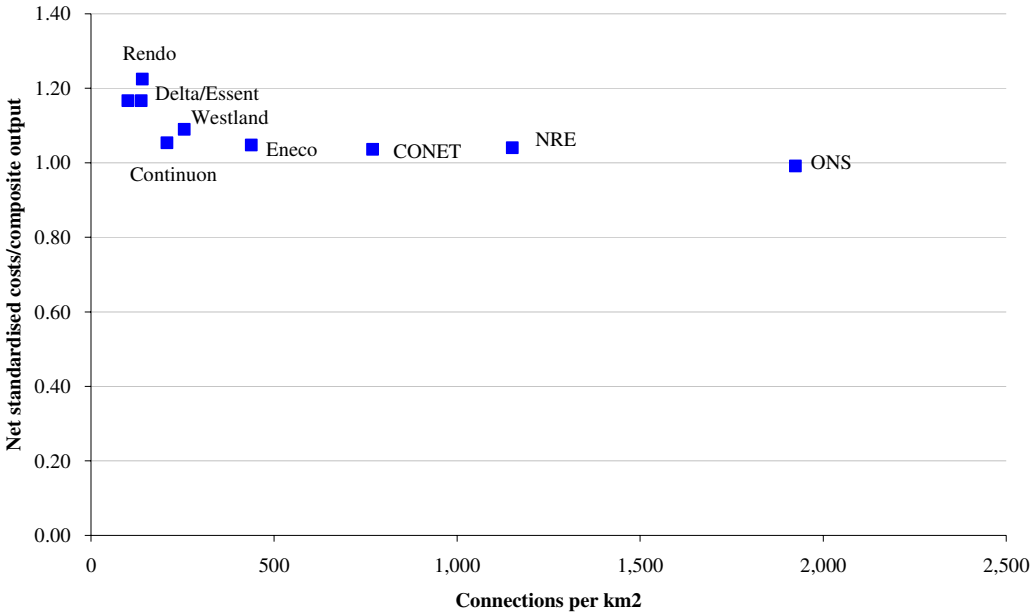


Figure 2: Connection density (excl. public lighting connections)



Assessing the Significance of the Regressions

On behalf of Delta, PwC previously submitted a report claiming that "Using the complete Brattle dataset we find a strong statistical relationship between costs and connection density. Higher connection density results in lower costs per connection." PwC claimed that the findings would remain significance even if some of the network length data contained errors of up to 20%.

As we discussed above, we cannot endorse the results of the PwC study because they were based on old data concerning Delta, they did not consider Essent's data problem, they were not limited to data that had secured the endorsement of auditors, and the study arrived too late in the process to permit a full response. Our own analysis showed no statistical significance, except for one combination of data among the many explored.

We noted that even a finding of significance "does not *prove* that the variables are causally related (and, if so, in which direction the causation operates)".[2] PwC and Delta express concern that we never explicitly cited causation as a criterion for finding a regional difference. Essent seems to question why we bother doing the regressions if we do not take statistical significance to indicate that connection density causes lower costs. We considered statistical significance carefully. In our report we discussed the possibility that significance could arrive just by chance. For example, there is a close statistical relationship between the salaries of Presbyterian ministers in Massachusetts and the price of rum in Havana.[3] If Tiger Woods plays at a golf tournament on Sunday, the value of the US stock market tends to increase on Monday.[4]

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[2] *Regional Differences for Gas and Electricity Companies*, March 2006, p.53 (emphasis added).

[3] Darrell Huff, *How to Lie with Statistics* (United Kingdom: Penguin Books Ltd, 1991), p.90.

[4] Sirak, R. "Bullish on Tiger," *Golf World* (May 18, 2001).

We further assessed the issue of significance by citing our concern that since "95% confidence" implies a one in twenty probability that randomness created the appearance of significance, we would expect that one of the thirteen relationships tested might show statistical significance just by chance. PwC counts the relationships differently. PwC calculates that there are three significant relationships from six tested. However, we disagree with PwC.

First, PwC considers the electricity sector separately to the gas sector. We consider the two sectors together because, in our opinion, if connection density is a regional difference for electricity, then we would expect it to be a regional difference for gas. The same underlying principle explains that higher density could reduce the costs for both electricity and gas distribution. The same companies are involved in both electricity and gas, so they have the same variation in densities. Since there is a gas distribution network in each city with an electricity distribution network, we would expect the potentially higher costs of urbanization to affect both the gas and electricity distribution networks equally. The gas and electricity distribution networks in the same city should have to pay the same high wages and face the same difficulty of undertaking repair works in the streets during business hours when streets are congested. We would also expect to see similar roles of management discretion and inefficiency among the groups, since many companies operate both gas and electricity networks simultaneously.

Any reasonable statistician would dig further and question the results closely if the analyses suggested that connection density produced significant savings in electricity but not gas. If no gas regression is statistically significant and only one of the electricity regressions shows significance, it is reasonable to conclude that the one example of significance could be the result of mere chance.

PwC also considers the population density and connection density regressions together. PwC claims that this is appropriate because connection density is a proxy for population density. We understand PwC's reasoning, but note that the inclusion of population density would not eliminate our concerns with the effects of chance. Without including the population density regressions, we concluded that one

particular data combination out of thirteen had shown significance. If we count population density, then two equations out of seventeen showed significance. It is still unreasonable to assume a causal relationship when simple chance could produce the misleading appearance of significance for two out of seventeen regressions.

PwC and Essent do not count the 'net error' regressions. PwC believes that, by virtue of their higher costs, the larger network companies have larger net errors in absolute terms, and that therefore one would not expect to see a correlation between connection density and absolute net error. However, our "net errors" were based on first regressing costs against composite output, omitting any constant. The use of Ordinary Least Squares to perform the regression in that analysis eliminates any *a priori* expectation of a correlation between company size and the absolute size of the net error. A statistician should not expect to see any correlating factor among the errors of an ordinary-least-squares regression, unless the regression has omitted a key variable. When we tested the errors in our report, in effect we tested whether connection density could be such a variable causing a correlation among the errors. However, the results indicated otherwise.

PwC also takes to be significant the regressions in Table 32 that have four degrees of freedom (excluding the net error regression). We found these regressions did not produce statistically significant results. PwC claims that these regressions are wrongly considered to have four degrees of freedom. We disagree. They are the regressions that involve connections per km. The four degrees of freedom are calculated as the six data points we are able to use (Delta, NRE, ONS, Rendo, Westland and CONET) minus the two variables being tested in the regression (connections/km and the constant).

Essent criticises our use of a 95% confidence interval. Essent claims that the confidence interval was chosen randomly and had no scientific basis. This is not true: we chose 95% based on our experience, international precedent and textbooks. In the history of The Brattle Group, Ltd we have performed many statistical analyses, and have consistently used 95% despite considering small samples at times. If the choice of confidence interval were random, we would use different intervals in different

engagements. Essent argues that with such small samples, confidence intervals of 80% are commonly used. We do not know whether this is correct. However, we do know that 95% is commonly used. The choice of confidence interval remains a matter of judgment, and we have applied our independent judgment.

Measuring Density

Delta raised a question concerning its "outland" water in connection when arguing against a focus on only the incremental costs of its water crossings. We explained earlier in this memo that including Delta's outland water in the measurement of connection density would not make a difference. We also anticipated a broader question concerning the appropriate measurement of connection density for all companies. We tested the impact of simultaneously including the "outland" water for Delta and for the two other companies that reported water crossings of greater than 1km: Continuon and Eneco. Continuon and Eneco did not provide information that complied with the data requests concerning water crossings. However, we anticipated an argument that we should include outland water for Continuon and Eneco when measuring connection density, to be consistent with our treatment of Delta. We concluded that this step would only make it more difficult to believe that low connection density raised costs. Continuon has relatively low density, and Eneco is in the middle of the range, but both companies have similar costs per composite output to each other and to the companies with the highest densities. If we reduce the perceived densities of Continuon and Eneco, then it becomes more difficult to believe that connection density can be a regional difference. We would see two companies with even lower densities than before, but with costs per composite output that remain close to those of the extreme high-density companies.

Literature Review

Essent complains that our review of the connection density literature is selective – considering only those studies that find a positive link between urbanisation and costs. Essent also says that we should not consider studies relating to the United States, Australia and Switzerland because these countries are not similar to the Netherlands. The companies state that connection density only raises costs in areas that have far

greater densities than the Netherlands, and that we therefore should reject the possibility that denser cities might face higher costs that offset the potential efficiencies of their denser networks.

However, this argument is speculative. We have seen no study concluding that urban environments fail to impose costs except after exceeding certain densities. Just because two or three studies only find an effect at high densities does not imply the absence of an effect at lower densities. The only way to be sure is to test the relationship empirically in the Netherlands.

The companies who disregard the possibility of higher costs in urban areas are in effect asking us to abandon evidence and proceed on the basis of faith. We are asked to ignore statistics in the Netherlands, to proceed with an engineering approach, and not to worry when we see that some low-density companies in the Netherlands that have costs per standardised output comparable to those of networks with far greater densities. We are implicitly asked to presume that if a low-density company has similar costs to a high-density one, then the high-density company must be relatively inefficient and has failed to exploit the inherent advantages of its denser network that some engineering equations would predict. This is not a reasonable way to proceed.

The high-density companies have raised multiple specific complaints about the higher costs of working in urban areas, and have provided presentations to the group. It would be irresponsible for us to assume without evidence that the costs of the multiple issues raised by these companies were actually zero. If a low-density network has comparable costs to a high-density one, the most reasonable conclusions are: a) that density makes no difference, b) that it makes a difference but has been cancelled out by some countervailing factor that we were not able to measure separately, or c) the result has occurred just by chance, which would prevent us from deriving any meaningful conclusions about efficiency. The least reasonable conclusion would be to presume that the high-density company was completely wrong about its various claims concerning the higher costs of urban areas, and that it is actually inefficient.

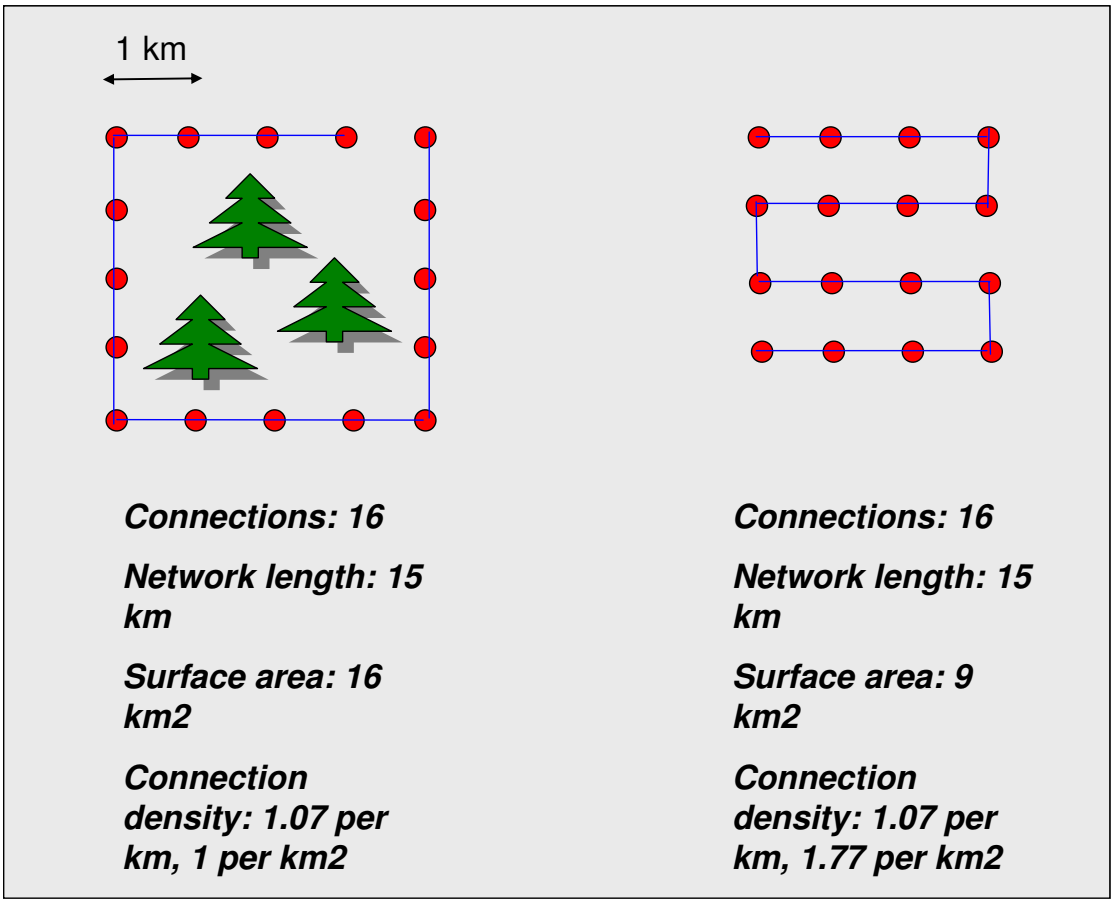
We have never disputed that a large body of literature suggests that higher connection density and/or population density may in some cases help reduce the costs and/or

improve the efficiency of a network company. We pointed this out in our final report but emphasised that only the data could tell us whether such a relationship applied to the network companies in the Netherlands. Essent itself (in the January 2006 report prepared by KEMA on Essent's behalf) presented a literature review that discussed the results of a variety of countries, including the United States, Switzerland and Italy. Clearly Essent did not consider the results of these studies irrelevant.

Measure A versus Measure B

Essent believes we should rely on "Measure A" (number of connections per km2) rather than connection density measured as connections per km ("Measure B"). Essent points out that Measure A is objective because network companies have no influence on it. In addition, there are more data points for Measure A. In contrast, Essent argues, network operators can influence the number of connections per km ("Measure B") and there are a smaller number of data points.

Essent also complains that we fail to justify adequately our preference for Measure B. In our report we discussed the advantages and disadvantages of Measures A and B. We explained that, from a theoretical perspective, we believe Measure B is the most appropriate measure. Simply covering a lot of square kilometres does not necessarily relate to costs. We can imagine situations where companies with the identical amount of network infrastructure cover far different areas measured by kilometres squared. We illustrate with an example.



Essent believes that it is unnecessary to choose between the two measures of connection density, since both measures indicate a negative relationship between connection density and costs. First we note that the only significant electricity regression result involved Measure A. There were no significant results for Measure B. We also explained that a choice was necessary since companies seemingly had very different connection densities under the two measures (e.g. Westland). In our report we commented that Westland's connections data seemed strange. Essent does not consider Westland's connection data to be strange. Essent explains that, since Westland serves a large number of market gardeners that are located close to each other, Westland has an unusual network configuration and therefore it is not appropriate to compare connection density measured as connections per km2 and as connections per km. In our report we raised the possibility that Westland's seemingly strange data could be explained by a unique network configuration (p.55):

> The striking difference in Westland's relative density under alternative measures would indicate one of two problems. Perhaps the data are accurate, and a significant difference arises between the two alternative measures of density because of Westland's unique network configuration. If the data are accurate, then Westland just by chance happens to have the same network length per connection as Rendo and Delta, but covers only half the square kilometres per connection. If this is true, then we would prefer an analysis based on density measured by reference to network length, since network length relates most directly to costs. However, none of our regressions involving network length were significant, perhaps in part because several companies failed to provide data that secured sufficient endorsement from auditors.

The comments concerning Westland's configuration only support our preference to measure connection density by reference to the number of connections per kilometre of network length. Even if both measures of connection density were significant, we would still need to choose between the two methods. To evaluate the magnitude of any regional difference, we would need to choose one of the measures.

Final Results and Conclusions

PwC observes that some of the critical t-statistics shown in Table 32 do not match those in Table 2. PwC is correct. The critical t-statistics should be slightly lower. However, using the slightly amended critical t-statistics does not alter our conclusions. None of the connection density variables become significant.

Rendo argues that the results of various studies by KEMA indicate that the higher costs of laying cables in urban areas do not cancel out the higher costs for rural networks due the lower connection density in rural areas. Essent discusses the KEMA study and makes similar arguments. Rendo says that KEMA found that the costs of laying cables in urban areas are 20% higher than in rural areas, but that the average number of connections per km of cable in urban areas is 300% greater than in rural areas. Neither we nor DTe have copies of the KEMA studies. We have seen no indication that the studies were commissioned in the ordinary course of business. In any case, we can only assess regional differences on the basis of the data that the companies have submitted. We have done this and found connection density not to be a regional difference.

Population Density

PwC believes that, since one of our population density regressions yielded a significant result, population density is a regional difference that should be taken into account in the tariffs. We disagree. Our engagement requires us to recommend tariff adjustments for only those factors whose cost implications we could measure reliably. As we explained in our report, the results of the population density regressions imply a range in the magnitude of the cost implication sufficiently large to prevent us from measuring the effect accurately.

In our report we expressed concern about the range in results implied by the electricity regression. We showed that the results of the regression imply that the efficient value of net SC/CO for ONS could be as low as 0.89 or as high as 1.05. PwC argues that whichever value is the true value, the relationship is still statistically significant and, consequently, population density should be taken to be a regional difference. PwC also claims that if the value is 0.89, the curve is steeper and so more compensation would be required for this regional difference. PwC is missing the point. We emphasised in our report that even though the result was statistically significant, the range in results yielded by the regression prevented us from accurately estimating the magnitude of the cost impact. Of course the compensation depends on the slope of the line. Since we are unable to determine the true slope of the line accurately, we consider it inappropriate to allocate consumption on the basis of these results. As we explained above, we are also concerned that the significance occurred just by chance because we explored so many different combinations of data in this engagement.

PwC says that we did not list all the parameters for the population density regressions. We assume PwC is referring to the details of the regressions we mentioned but did not give the detailed results for. For electricity we performed the following non-linear regression: LN(*Net standardised costs/connection)* $= \alpha + \beta$ *LN(population density).* We obtained the following results: t-statistic: -0.99, degrees of freedom: 7, R^2: 12%. We also performed a linear regression *Net standardised costs/composite output* $= \alpha +$ *β(population density)* for the gas companies including ONS. The results were as follows: t-statistic: -1.01, degrees of freedom: 9, R^2: 9%