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Horton 4 Consulting

Opex Rate of Change and Productivity: Pacific Economic Group's Response to Consultant Reports

Comments by Geoffrey Horton

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Opex Rate of Change and Productivity: Pacific Economic Group's Response to Consultant Reports

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1 Summary

As part of its calculation of the allowed price to be charged by gas distribution businesses (GDBs) in Victoria the Essential Services Commission (ESC) makes an assumption about the future rate of increase in their operating costs. I was asked by SP Ausnet and Alinta (for Multinet) to undertake a review and critique of the July 2007 report produced by Pacific Economics Group (PEG) on this topic and to comment on the overall level of productivity improvement calculated by PEG. PEG has since commented on my review and I have now been asked to respond to those comments. I was not made aware of PEG's comments until two days ago and so this response, while considered, may not deal with all the details in PEG's paper.

In general, PEG has not addressed the serious criticisms I made of its work but has continued to rely on a mechanistic re-estimation of its cost model, rather than on economic theory, and on a continued use of inconsistent price forecasts. Instead of addressing the issues it has challenged my competence, but on the basis of an almost farcical discussion of some of my minor comments.

This note reiterates five major points that are either not addressed in PEG's response or are addressed incorrectly

- Factor prices: PEG has forecast the CPI, wages and other factor prices independently. This produces an inconsistent result and a real wage forecast that was, in the original paper, lower than that of either of the two forecasts on which it purported to be based.
- Different modelling results: There is a substantial difference between the two sets of results in PEG's US and ANZ models. Either the US and ANZ gas distribution industries do not have the same cost function or these substantial differences suggest that the functions are uncertain and that the estimated results need to be treated with considerable caution. Either conclusion implies that they are not a reliable basis on which to calculate productivity growth. Taking an average of the two, as PEG does, has no valid justification.

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- Lack of dynamic specification: The restrictive specification adopted by PEG may account for their model's divergence from economic theory. Either feature, the restriction or the divergence, raises serious concerns about PEG's attempts to draw conclusions from the estimated coefficients.
- Overshooting: PEG's distinction between overshooting and outperformance is essentially that the former is temporary. This does not remove the incentive regulation difficulties its approach entails.
- Aggregation: PEG has not addressed the basic point but makes a false assertion about the approach suggested by Horton 4. It seems to have confused itself with some simple algebraic manipulations.

A final section deals with PEG's personal observations.

The conclusions of my previous report, "Opex Rate of Change and Productivity: Comments on Pacific Economic Group's Response to Meyrick and Associates' reports - An opinion by Geoffrey Horton" 26 October 2007, remain valid.

2 PEG's price mis-specification

I have been unable to find any section of PEG's wordy response that deals with the fundamental criticism that it uses wage forecasts made by others on the basis of an expectation of a particular level of general (CPI) inflation and combines them with the use of a different, higher, and therefore inconsistent, CPI inflation rate. Page 67 of the report says that it is addressed in pages 8-11 but it is not.

PEG has revised its CPI forecast downwards, which reduces the size of the impact of its error but does not remove it – particularly since it favours the nominal wage forecast that is based on the lowest CPI forecast.

It is extraordinary that any serious economist would make this mistake in the first place, let alone fail to discuss it when it had been brought to his attention.

3 Difference in estimated results

I have also been unable to find a response to the criticism that the estimated cost models for the United States and Australia/New Zealand are different.

In several cases the difference in the size of coefficient exceeds twice the standard error of the coefficient estimate, indicating that the two estimated relationships are significantly different. This is even the case with the wages term where at first sight the estimates seem relatively close. Either the US and ANZ gas distribution industries do not have the same cost function, which is possible but surprising and in which case the US results would be not relevant to ESC's decision, or these substantial differences suggest that the functions are uncertain and that the estimated results need to be treated with considerable caution. Either conclusion implies that they are not a reliable basis on which to calculate productivity growth.

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The practical impact of the differences is very large, as can be seen from comparing tables 6 and 8 in PEG's earlier paper. The response in that paper – to take the average of the two sets of results with some modification of their least plausible elements – has no valid justification.

4 Dynamic specification

PEG has failed to understand the criticism of the dynamic specification of its cost function. By dynamic is meant the aspect that deals with changes over time.

As has been shown in the work of Professor David Hendry and others dynamic misspecification can bias econometric results. An example of this is when the specification does not allow the short run impact of a variable to differ from the long run impact. This is the case here. The data set includes observations for companies A and B in years t and $t+1$. The model should be capable of distinguishing between the short run cost impact of a possibly temporary change in output between t and $t+1$ and the permanent difference in levels of output between companies A and B, which will probably have been operating at different scales for decades. It is a short-run cost model in that the dependent variable is variable costs but it is also long run in that it includes long run effects.

PEG responds, *"The first such impact is commonly referred to as "within" variations and the latter as "between" variations. PEG's model estimates for each variable are, in fact, weighted averages of the "within" and "between" GLS estimators, with a "group-wise" heteroskedasticity correction to eliminate potential bias in the estimated standard errors.⁷³ Our estimates therefore capture both of the effects Horton mentions. There is also ample precedent for the basic form of PEG's cost model and estimator in the applied literature."* However, the fact that the estimates are weighted averages is precisely the problem. They are weighted averages of short run (intertemporal or "within") and long run (inter-company or "between") responses. Correcting for heteroskedasticity changes the weights but does not remove the basic problem, which is as described in Horton 4's original comments. There are omitted variables and this may bias coefficients, particularly those on variables that are collinear with the omitted variables.

There are recognised methods of dealing with these problems, such as pooled mean group (PMG) estimation (although even this may have problems¹), but PEG does not appear to have considered them.

My earlier paper suggested that this restriction in the specification might have biased the estimates on collinear variables, possibly including the time trend – the most significant element of the productivity projection – and the capital stock. The coefficient on the latter variable is of the wrong sign – the opposite of what would be predicted by economic theory – and significantly so in both US and ANZ estimations. PEG's paper does respond to this point but does not do so convincingly.

Its main response seems to be that peer-reviewed journals have accepted articles reporting studies with similar perverse results. I have not had an opportunity to study the articles cited by PEG but acceptance of an article does not imply endorsement of its estimated coefficients as a suitable basis for detailed calculations. Negative results can be interesting and, as PEG

¹ Discussed, for example, in Pesaran and Smith "Estimating long-run relationships from dynamic heterogeneous panels", *Journal of Econometrics*, Vol.68, pp.79-113

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says is the case here, can initiate interesting debates over their causes. However, without a reasonable explanation of a perverse coefficient, it is reasonable to infer that there are defects in the model or the data and the other coefficients must be suspect.

PEG's second response is that the debate in the literature has produced an explanation of the positive coefficient. It offers the view that the Averch-Johnson effect may produce investment in regulated industries to an extent that is more than is efficient. It is true that this "gold plating" is a risk but it seems most unlikely that it can explain the perverse signs. PEG says it is a particular risk with Australia's "building block" methodology but the coefficient is positive in both its equations – for the US as well as ANZ. More to the point, the degree of inefficiency would need to be improbably large to explain the coefficient. Gold-plating is normally thought to be a problem in that marginal investment is undertaken to the extent that it does not reduce other costs sufficiently to offset the cost of the investment. Gold-plating that reaches the point where marginal investment actually *increases* other costs would imply inefficiency of startling proportions.

A more plausible explanation is given in the passage that PEG quotes on page 57 from Fraquelli et al. "*According to Filippini (1996), the positive sign of β_k is due to a problem of multicollinearity in cases where there exists a positive correlation between the dependent variable and the capital measure.*" This accords with what is said on page 17 of the Horton 4 report, "*It seems more likely that the variables that vary with the scale of the company – throughput, customer numbers, line length and capital stock – are collinear and that the capital stock coefficient is picking up a scale effect.*" This probability is increased by the restrictive specification and the distortion is likely also to affect the other coefficients that PEG uses to calculate its forecast of productivity growth.

5 Overshooting and outperformance

PEG's response on overshooting on page 67 completely fails to address the argument on overshooting put forward by Horton 4 (p17).

The argument put forward by Horton 4 is that it is impossible to distinguish between what PEG calls "overshooting" and outperformance (in the sense of a period when costs are temporarily below trend) without referring to some deficiency in quality or performance. The presumption is that any reduction in costs is outperformance even if it leads to a lower rate of cost reduction in the next period. PEG have not produced any evidence to suggest a deficiency in quality or performance but, by presuming that future costs should be assessed by applying trend productivity growth to a position in which "*there is a significant probability that cost overshooting has occurred in Victoria's gas distribution industry*", they have proposed a system of regulation where reducing costs below trend in period A results in a lower cost allowance in period B even when the full extent of the cost reduction is considered unsustainable. This clearly has incentive effects. There is scope for argument as to how such temporary benefits should be shared with customers² but this is not an approach taken by PEG.

It is not the case that there is a failure to understand the distinction between the concepts of overshooting and outperformance put forward by PEG, unless the failure referred to is PEG's own.

² A similar situation arises when efficiency gains enable investment projects to be deferred,

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6 An aggregate approach

PEG has produced a distorted description of the aggregate approach suggested by Horton 4. It seems to have reached this description through a fatuous algebraic manipulation of the concept of productivity suggested.

PEG observes that the rate of change of the suggested productivity definition is equal to the difference between partial factor productivity growth in the sector and TFP growth in the economy as a whole less the difference between factor input price growth in operating costs and that in the economy as a whole³. It then says *“This contrasts with Horton’s statement that “the growth of operating...expenditure productivity in an industry will differ from zero insofar as its TFP growth ...differs from that in the economy as a whole (which is embodied in the CPI), as the...relative price of its inputs changes and as a result of capital substitution.””*

It does not contrast at all. The first difference derived by PEG’s manipulation is equal to the difference between TFP growth in the sector and that in the economy plus the difference between total and partial productivity resulting from capital substitution and the second difference derived by PEG’s manipulation is the change in the relative price of operating cost inputs. The two statements are consistent. PEG’s comment is completely incorrect. It does not seem to understand its own simple algebra.

7 “Dubious and incorrect statements”

There is an attempt to disguise the blinkered and incomplete nature of PEG’s response with an error-strewn attack on my competence with which the section of PEG’s response that deals with the Horton 4 comments begins. The paragraph below contains two quotes from pages 68 and 70-71 of PEG’s response. The first precedes and the second follows eight examples of alleged errors in the Horton 4 paper.

“Horton’s report contains many statements which are at best highly dubious and sometimes flatly incorrect.” “PEG does not believe that these are trivial errors. They show that Horton repeatedly fails to understand many basic facts in our report. It also confuses productivity measures that are central to the issues being analyzed. Horton’s discussion of statistical issues is also deeply flawed. PEG’s analysis involves a certain degree of technical complexity, which we believe was warranted given the necessity to obtain rigorous opex PFP projections when past observed trends almost certainly overstate what is achievable over the term of the GAA. PEG welcomes comment on our report, but an informed and constructive critique must be grounded in an understanding of our work and an ability to apply the underlying economic and statistical concepts properly”.

The eight alleged errors are nothing of the kind. There is one clerical error, which can be clarified by referring to the surrounding material, and seven instances of misinterpretation by PEG. PEG’s commentary on the points borders on the farcical and seems to reveal an inability to think in broad terms about the process in which it has engaged. The paragraphs below quote PEG’s comments (often completely) in italics. They are followed by a further explanation of the remark in question and a refutation of PEG’s assertions.

³ PEG’s manipulated equation is $OPG = (\% \Delta PFP_{Opex} - \% \Delta TFPE) + (\% \Delta WE - \Delta W_{Opex})$

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1. *“On page 12, Horton says “all forecasting, indeed the inductive method in general, assumes that the future will be like the past.” In fact, both Meyrick’s and PEG’s forecasts of opex PFP growth for the GDBs start from the opposite premise: the GDBs’ future opex PFP growth rates will not be like their past PFP growth rates and can be expected to be lower. While we differ on methodology and how much PFP will decelerate during the term of the GAA, there is no disagreement on the basic idea that the past is not necessarily prologue to the future, and appropriate forecasting must take this into account. The cryptic reference to the “inductive method in general” also appears nonsensical. Webster’s online dictionary defines (Bacon’s) inductive method as one which “ascends from the parts to the whole....and which may be strengthened or weakened by subsequent experience and experiment.” Induction is thus generally viewed as a “bottom up” approach in which general conclusions are derived from the accretion of diverse facts and experience, which can certainly lead one to form views of the future that differ from what has occurred in the past. Induction contrasts with the “top down” approach of deduction that begins with assumptions and advances through logic to conclusions.”*

This introductory remark appeared so self-evidently true that further explanation would not be required. Forecasting in general, and in particular forecasting through the use of econometric models (of which I have considerable experience), assumes that the relationships that held true in the past will still hold in the future or will change in a way that is predictable and governed by some other relationship. This is entirely consistent with, say, a change in PFP growth rate, if one moves beyond the use of what was described as a “simple method”. This was stated in the following paragraph but ignored by PEG. The passing reference to “the inductive method” was indeed in contrast to the deductive method and referred to one of the standard problems in the philosophy of science. It was discussed by Hume and more recently debated under the heading “grue” – green up to a future date but blue thereafter – by Goodman and Quine.

2. *“On page 13, Horton says PEG adopted a “disaggregated approach...(which) looks at the separate elements in the production function that affect PFP.” In fact, PEG analyzes a (short run) cost function, not the “elements in the production function.” In addition, in describing these elements, Horton describes technical progress by saying “total factor productivity, output per unit of any input, may rise over time as a result of technical progress.” While this is true in a long run cost function, a short run cost function for opex can only be used to estimate the components of opex partial factor productivity (PFP), not total factor productivity (TFP). This is one of many instances in which Horton confuses TFP with PFP and does not correctly identify which productivity concept is reflected in the equations it references.”*

None of the points made by PEG suggests an error. Rather they reveal the limited nature of PEG’s analysis. A short run cost function is the result of a transposition of a production function to set short run costs as the dependent variable. It sometimes refers to circumstances when the capital stock is fixed, but that is not the case here. The elements of a “short run cost function” depend on those of the production function.

Horton 4 does not confuse TFP and PFP. Indeed, it goes to considerable length to discuss differences in productivity measures and cites four on page 18. PEG seems unaware that there can be debate about the nature of the functions that it has estimated, the interpretations of the differences between them and the extent to which, regardless of PEG’s claims, they embody short or long run properties.

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3. *“Also on page 13, in describing the “operating environment” variables in PEG’s model, Horton says “this list illustrates some of the problems of moving from a theoretical discussion to a practical application. None of these factors is really separate from the other categories.” Whether any variable “is really separate from” and has an independent impact on opex is not a question that can be answered a priori. Researchers should look to the data to address this issue, as PEG has done using rigorous econometric methods.”*

PEG’s quotation omits the following sentence that discusses the relationship between these variables and others included in the estimation. The criticism illustrates PEG’s simple faith in the ability of econometric estimation rather than economic theory to supply answers to questions. Anyone with any general experience of econometric work will be aware of the problems of correctly specifying the relationship to be estimated. Mis-specifications can result in errors and biases in the estimated coefficients. PEG is either naive or disingenuous in ignoring this.

4. *“On page 15, Horton says “the coefficient on the log of wages squared in the US equation is surprising. It implies that the elasticity of costs with respect to wages decreases by three quarters of a percent for every percentage increase in wages.” This is not true; the estimated relationship between wages and opex is more complex than Horton realizes, in part because it is also impacted by interaction effects between wages and other variables.”*

The interactions between wages and other variables were considered in making this remark, notwithstanding the poor quality of the reporting of results in PEG’s original paper. It is noticeable that PEG does not offer an alternative estimate or attempt to explain the bizarre coefficient.

5. *“On page 15, Horton also asserts that “there are always errors in coefficient estimates” in econometric models, which is not true. It is true that there is always uncertainty in coefficient estimates; this should be obvious from the term “estimate.” However, this point applies to every econometric analysis that has ever been conducted. Simply recognizing that statistical analysis 70 deals with probabilities rather than certainties is no reason to discount the results of any econometric study. Rather, the credibility and quality of econometric studies should be evaluated using rigorous statistical tools, assessment of the underlying data, and an understanding of the fundamental economic relationships that were examined.”*

PEG admits that there is uncertainty in coefficient estimates “in every econometric analysis that has ever been conducted.” The estimate of the coefficient will have an error and there will be an extremely small probability that the error is zero. Yet PEG disputes the statement to that effect made by Horton 4 Consulting. The criticism is nonsense.

6. *“Footnote 6 on page 15 says “the quoted t statistic is merely a measure of the probability that the coefficient differs from zero.” In fact, the coefficient is what is estimated by the econometric method and is an estimate of the underlying population parameter. The t statistic is a test statistic on the hypothesis that the underlying parameter value is zero.”*

This criticism is, if anything, even more incomprehensible than the others. PEG says “the t statistic is a test statistic on the hypothesis that the underlying parameter value is zero”. It does not add, but could not dispute, that the value of the statistic is an indicator of the

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probability of that hypothesis being false. As such it is “a measure of the probability that the coefficient differs from zero”, as stated in the Horton 4 footnote.

7. *“On page 16, Horton says “PEG takes the scale variables to be customer numbers and line length.” This is not true.”*

This is indeed a clerical error in the Horton 4 report. It is inconsistent with what is said elsewhere in the report. Earlier on the same page it is correctly stated that “When calculating the *scale effect* PEG took the coefficients on customers and units” and the sentence following that quoted by PEG states, “In my view km of pipe is a further scale variable”, which implies that PEG has not treated it as such. The clerical error makes the passage harder to read, and should have been picked up by Horton 4, but it is not difficult to allow for it and it does not invalidate the argument.

8. *“Also on page 16, Horton says “excluding the scale variable, network length, the only significant factor is that of the change in the proportion of iron and steel pipes. This is estimated to increase productivity growth by up to 0.3% pa.” It is not clear what Tables Horton is referring to, but neither of these statements is true in any of PEG’s econometric models.”*

In PEG’s table 6 the product of the coefficient on the percentage of non-iron/steel pipe (-0.1786) and its largest growth rate (1.6%) is -0.286%. For some reason, PEG has set out the contribution of “business conditions” with the opposite sign to the other contributions to productivity growth so this *adds* almost 0.3% to PFP growth.

8 Experience and qualifications

Geoffrey Horton is an independent consultant specialising in advice on economic regulation to regulatory bodies and regulated companies. He holds an MA degree in Philosophy, Politics, and Economics from Oxford University and an MSc in the Economics of Public Policy from London University.

His credentials were given at greater length in the earlier Horton 4 paper. He has worked as an economist or regulator for over thirty years. The first half of his career was spent mainly as a macroeconomist at the United Kingdom Treasury, in consultancy or as a university lecturer, during which time he conducted a significant amount of econometric research and was responsible for co-ordinating the UK Treasury’s UK, European and world economic forecasts. Since 1988, he has been involved in economic regulation, particularly in the energy industry. He was the Senior Economic Adviser in the UK Department of Energy (1988-90) responsible for all economic advice on electricity, coal, and the environment including the redesign and reorganisation of the electricity industry, Director of Regulation and Business Affairs at the British Office of Electricity Regulation, Director General of Electricity Supply for Northern Ireland, and Director of Consumer Affairs at the UK Office of Fair Trading. Since 1998, as a consultant in regulatory economics, he has worked on many topics covering ten industries in more than twenty jurisdictions.

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