"Electricity Capacity Assessment: Measuring and modelling the risk of supply shortfalls"
Response

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Note: I have confined my answers to those questions, mostly statistical, which I believe fall within my range of expertise.

Chapter 3

Question 1: *Do you agree that the de-rated capacity margin is a good indicator of future capacity adequacy?*

My view is that this is inadequate even as a crude indicator of future capacity adequacy. The reason is that, as an expectation, it takes no account of uncertainty in capacity availability. Thus to achieve a given level of risk a considerably larger margin is required for uncertain capacity such as wind, than for reasonably certain capacity such as conventional plant. (Figure 1 tends to reinforce the erroneous idea that risk is just a function of de-rated capacity margin.)

While an exact level of risk depends on the entire distribution of the excess of capacity over demand, my belief is that it would be adequate, to a first approximation, to report both the *mean* and *standard deviation* of predicted excess capacity. The ratio of the two is readily interpretable, even to the layman, as an (albeit crude) measure of risk. More exact measures of risk are in any case given by the LOLE and EEU.

Question 2: *Are there any measures of risk other than LOLE and EEU that we should report and what are their comparative advantages?*

LOLE and EEU seem the correct measures to use, at least for the risk as averaged over time (or equivalently at a randomly selected point in time). While they would be completely descriptive if successive time periods could be treated as independent identically distributed, in practice they say nothing about the extent to which outages tend to "clump" in time. However, the latter issue is probably of secondary concern and there is almost certainly insufficient data to address it in any case.

Chapter 4

Question 3: *Are there any additional key input assumptions that we should consider in the modelling?*

Not that are obvious. In particular the approach of Paragraph 4.3 seems broadly correct, i.e. estimate the distributions of net-of-wind demand and available conventional capacity and treat these two variables as being *independent* (which conditional on a given specification of capacity before outages is probably the case). It is somewhat misleading to talk about a "distribution around a mean" as this implies a symmetry which will not in general be the case.

Question 4: *Do you agree that the use of stochastics (probability distributions) to model short-term variation of key input variables is the best available method? Do you agree with the use of scenarios and stress tests for capturing long term uncertainty in key input variables?*

Yes, and yes.

Question 5: *Do you agree with the proposed approach to modelling wind availability?*

The approach via historical wind-speed data seems correct: this is the only realistic possibility at present, although there are in reality considerable limitations even on the present availability of adequate historical wind-speed data.

However, the detailed proposals for estimating demand-net-of-wind seem flawed (even though they may reflect present practice elsewhere). Further the proposals for handling the dependence between demand and wind availability are quite unclear. The starting point for any analysis is (historical) concurrent data on demand and wind availability, and as observed these two variables are not in general independent. An appropriate statistical analysis requires to be developed, and is necessarily driven by a consideration of actual data. The present document begins the prescription of such an analysis (Paragraph 4.15)—the details of which are in themselves strange if what is required is to estimate the distribution of demand from an historical time series; it then goes on to delegate the completion of the analysis to NGET. But this, in my view, is impossible, and I believe NGET should simply be charged with producing an appropriate and scientifically sound analysis of the data to produce the required estimate of the demand-net-of-wind distribution.

Question 12: *Will treating half-hour periods independently have significant effects on our estimates of the de-rated capacity margin and risk of supply shortfalls and how should the model take into account half-hourly cross-correlations?*

My answer to this question is, at a high level, the same as that to Question 5: an appropriate, *and data driven*, analysis of the available demand-wind data is necessary, particularly with regard to the estimates of risk probabilities (LOLE, etc). The likely consequence of the above independence assumption—if it is not further (and mistakenly) assumed that demand and wind are independent—is that while naive point estimates of measures of interest will be relatively unaffected, assessments of uncertainty about the quality of these estimates will be wildly over-optimistic. (A Bayesian analysis could allow for uncertainties in estimates of probabilities, arising from the finiteness and other limitations of the available data, by making *predictive* estimates, and here it would be essential not to make unwarranted assumptions of independence between successive half-hour periods.)

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