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Discussion

Unsteady wind loading on a wall

Discussion by Nicholas J. Cook†

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The recent paper in this Journal by Baker (2001) provided independent analysis of full-scale data from the Silsoe boundary wall experiment, reported elsewhere (Robertson *et al.* 1996a,b, 1998). This analysis included "conventional methods", comprising mean, rms, probability, auto-correlation, spectra and coherence measurements, as well as less-conventional methods including conditional sampling, proper orthogonal decomposition and wavelet analysis. In the discussion section of the paper, the quasi-steady hypothesis and its use to predict extreme values was reviewed. This discussion contribution concerns only Baker's treatment of extremes, challenging some of his assumptions and interpretations of the data and revealing anomalies that undermine many of his conclusions.

1. Baker's method for deriving extremes (Section 3)

Baker selected the 99.95th percentile values of the probability distribution from 1-hour long records to represent the extreme values, describing this as a "simple form of analysis" and "corresponding to the maximum 1.8s of the data". He claimed this was necessary because Hoxey *et al.* (1996) had shown "the use of extreme value analysis using full scale data is fraught with difficulties because the use of data that is not absolutely stationary can result in significant errors".

In reality, the Hoxey *et al*. (1966) paper uses the stationarity issue as an argument against all methodologies for estimating extremes. It seeks to eliminate all methods, except for the quasi-steady method, in order to justify the exclusive use of analysis procedures advocated by Hoxey (1986). These procedures deny the existence of non-quasi-steady components, treating them as "errors" to be averaged out, and preventing them from being detected and investigated. Although the quasi-steady model may adequately define the major component of pressure fluctuations on most buildings, there will inevitably be occasions where this is not true. Take, for example, a switching flow as in Muramatsu *et al.* (1997) where the flow switches between two states and the measured mean values represent neither state.

Non-stationarity introduces errors into all time-averaged measurements, including the mean, with greatest errors in the highest moments around the mean. The Hoxey *et al*. paper ignores the fact that this problem was diagnosed in 1979 and a method proposed and demonstrated for rendering full-scale data stationary (Mayne & Cook 1979). This method applies a low-pass filter to the dynamic pressure at the spectral gap (around 10 minutes) to identify the non-stationary trends, then re-scales the data into coefficient form using the filtered dynamic pressure. Fig. 1 shows a one-hour record of wind speed. The record is mildly non-stationary, as shown by the running ten-minute mean, superimposed as the thick white line. Fig. 2 shows the same data rendered stationary as a velocity

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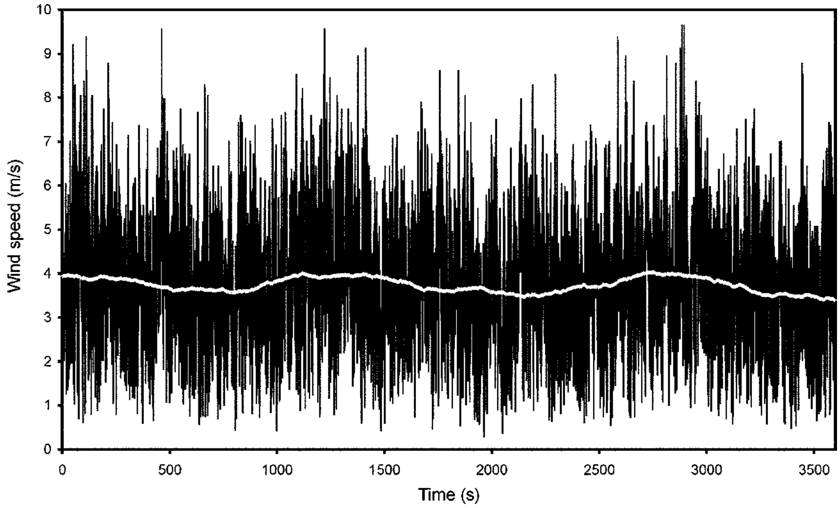

Fig. 1 One-hour record of wind speed, sampled at 5 Hz, with running 10-minute average

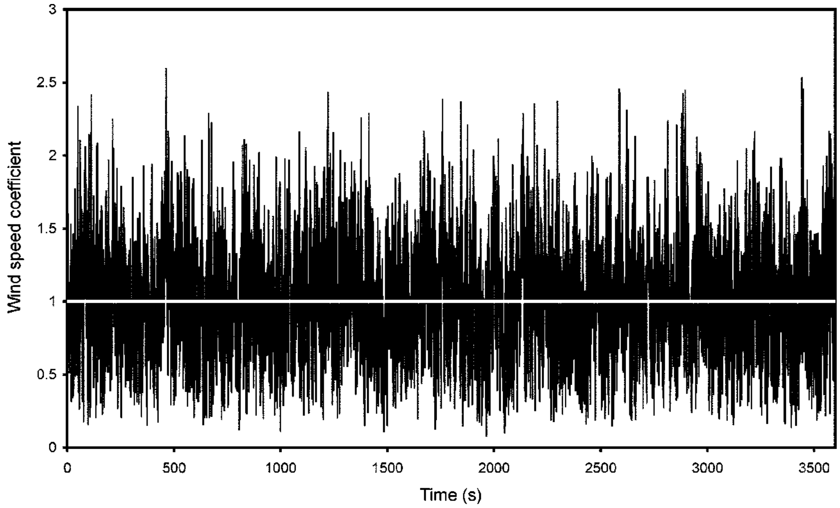

Fig. 2 One-hour record of wind speed coefficient rendered stationary

coefficient by dividing the original record by the running ten-minute mean. This procedure requires that the data are length-scale dependent and that the wind direction remains constant. Where this is not so, for example when there is a fixed frequency such as the natural frequency of a building, it becomes necessary to use “selective ensemble averaging” (Littler & Ellis 1992) to assemble a stationary record from selected self-stationary segments of the full data record. While Mayne & Cook’s method renders the whole data record stationary, the largest stationary ensemble obtained by Littler & Ellis represents about 5% of the full data record.

Baker also reports that “division of the datasets into 10-minute intervals and the calculation of mean and standard deviation for each interval, revealed no discernible drift in the data during the course of the collection period.” This is a roundabout way of saying that the data were effectively

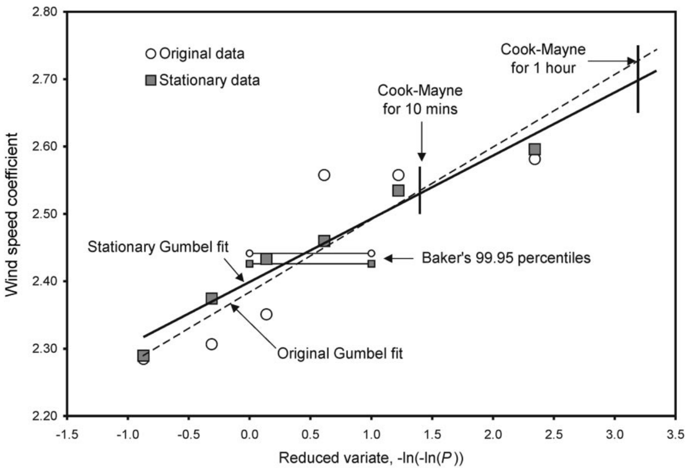

Fig. 3 Extreme-value analysis of one-hour record of wind speed coefficient using six ten-minute sub-records

stationary, so that conventional analysis of the fluctuations, including extreme-value analysis, would have been appropriate.

While the percentiles used by Baker do represent "the level that is exceeded for 1.8s in every hour", this represents the ninth-highest value in the data record because the data were digitised at 5 Hz. So while it does represent "the maximum 1.8s of the dataset", it does not represent the maximum 1.8s-duration event, i.e. it is not an averaging time. The data would need to be low-pass filtered at τ=1.8s for this to be true, in which case the quantile would represent a single event and be even less reliable. Baker's method is much less reliable than a standard Gumbel analysis of the records divided into, say, ten-minute sub-periods and very much less reliable than Peterka's (1983) "100-highest peaks" approach.

The effect on extreme value analysis of rendering the data stationary and the differences between Baker's percentiles and the Cook-Mayne coefficients (Cook & Mayne 1980) are illustrated in Fig. 3. Here the extremes from six ten-minute sub-records on the standard "Gumbel plot" show that removing the mild non-stationarity reduces the dispersion of the fit and greatly reduces the scatter. The effect on the Cook-Mayne design value at $-\ln(-\ln(P))=1.4$ is small for the ten-minute reference periods, but becomes larger when this is transformed to the required one-hour reference period at $-\ln(-\ln(P))=1.4+\ln(6)=3.2$ because of the reduced dispersion. However, it is important to note that Baker's 99.95th percentile value is also reduced a similar amount by rendering the data set stationary. Baker's percentiles also give significantly smaller values, corresponding to lower risks, than that required by the Cook-Mayne method that selects the required risk level – however the effect on pseudo-steady coefficients is minimal provided that exactly the same process is applied to the surface pressure data and the reference dynamic pressure.

In summary, anomalies in Baker's analysis of extremes are:

1. Accepting Hoxey *et al.* (1966) uncritically and not recognising that non-stationarity affects all fluctuations and so affects any extreme, however estimated.
2. Estimating the extremes anyway, using the least reliable method available.
3. Assuming that the quantile representing the maximum 1.8s of the record is equivalent to a 1.8s averaging time.

4. Not recognising that the "drift" test indicates stationarity and justifies conventional extreme value analysis.

2. "The quasi-steady hypothesis" (Section 7.2)

Application of the quasi-steady method to surface pressures requires that the contributions are correctly partitioned between the rear and front faces. This, in turn, requires that the reference static pressure is accurately known – but this reference quantity is the most difficult of all to obtain reliably at full scale. The mean pressure coefficients are $C_p \approx +0.5$ in Fig. 3(a) for the front face and $C_p \approx -0.6$ in Fig. 3(b) for the rear face with wind normal to the wall. These values are not consistent with the consensus of published values which indicate $C_p = +0.8$ for the front wall and $C_p = -0.3$ for the rear wall, e.g. as in ESDU (1971). However, the net drag coefficient is close to that expected $C_D \approx 1.1$, suggesting a systematic bias in the measurement of the reference static pressure. This bias is also evident in the data from other full-scale experiments reported by the Silsoe group and has not, so far, been satisfactorily explained.

3. "Specification of extreme pressure coefficients" (Section 7.3)

Baker's review of the methods for specifying extreme pressure coefficients also suffers from the anomalies reported above. Extreme value analysis is immediately dismissed "based on the work of Hoxey *et al.* (1996)", while the other three methods: quantile level analysis, quasi-steady method and peak factor method, are approved even through they are equally vulnerable to non-stationarity.

Baker recognises that the quantile-level method "is prone to experimental scatter" when the data record is short. Lawson (1980) gives a method for reducing this error based on fitting the tail of the probability distribution to a Gaussian distribution for positive pressures and an exponential distribution for suctions. However, for the quantile to represent the 1.8s-duration extreme still requires the data to be low-pass filtered using the corresponding time constant.

The peak factor method makes explicit use of the time constant and Baker used an averaging time $\tau = 1.8$s "for consistency" in the equation for zero crossing rate. Accordingly, his estimated peak factor is not consistent with the quantile-level approach, which corresponds to the ninth-highest $\tau = 0.2$s value, assuming that the data signals were low-pass filtered at the sampling frequency. However, the paper reporting the measurement technique (Robertson *et al.*, 1966b) makes no mention of any low-pass filtering, so that the signals, although digitised at 0.2s intervals, may actually contain fluctuations of much shorter duration.

The comparison of quantile-level, quasi-steady and peak-factor method in Fig. 24 requires that the contributions be correctly attributed to the rear and front faces and that the averaging time is consistent. The discrepancies in Fig. 24 are entirely consistent with the reported anomalies:

- The peak factor method is consistently of lower magnitude because the $\tau = 1.8$s averaging time used to calculate the gust factor does not match the data.
- The quantile-level method predicts higher values than the quasi-steady method on the front face because the mean pressure coefficient is biased too low.
- The quantile-level method predicts lower values than the quasi-steady method on the front face because the mean pressure coefficient is biased too high.

Fig. 4 shows Baker's Fig. 24 re-plotted as the net pressure (drag) coefficient across the wall in

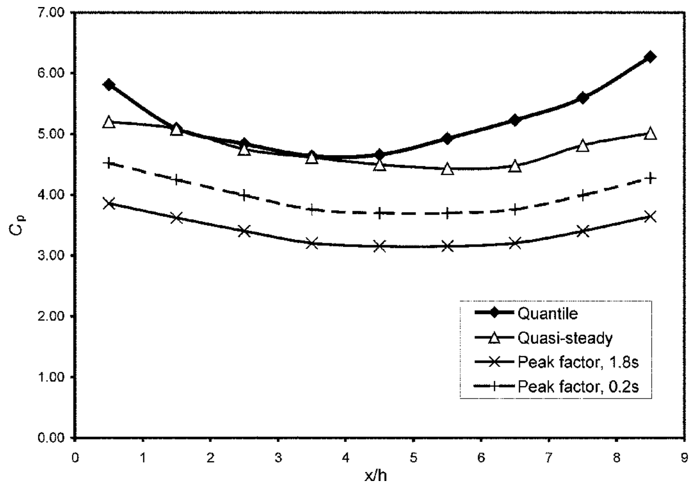

Fig. 4 Comparison of different methods for calculating extreme values of net pressure (drag) coefficient for α=1.2°

order to eliminate the effect of static pressure and with the averaging time for peak factor method reduced from 1.8s to 0.2s (the acquisition rate). As expected, the difference between quantile and quasi-steady methods is much reduced, but now the quantile method values are slightly higher than the mean, indicating some additional non-quasi-steady contributions. Changing the time constant to 0.2s, using the size effect factor of BS6399-2, brings the peak factor estimates about halfway towards the other methods from their previous values for 1.8s. This is clearly insufficient to bring the peak factors to the higher measured values reported by Baker. The Wieringa (1973) model for peak factor used as the basis of the factors BS6399-2 yields an equivalent peak factor of 5.6, when converted from wind speed to pressures, as opposed to Baker's predicted values of around 3. This is almost exactly the value required to match the other methods, suggesting the problem lies in the derivation of Baker's base 1.8s-duration values through the Davenport approach.

4. "Codification of wind loading data" (Section 7.4)

Baker claims that BS6399-2 contains three assumptions:

(a) That "*k* values in the empirical fit to the coherence values is constant at 4.5."
(b) That the pseudo-steady pressure coefficients "have a simple one to one relationship with the mean pressure coefficients."
(c) That "the lack of correlation between the pressure forces on the front and rear of the structure can be allowed for by a simple empirical correction."

4.1. The "k" value

In respect of (a), Baker joins forces with Holmes (1997) and Dyrbe & Hansen (1999) in criticising the derivation of the constant *k* in the "TVL formula", Eq. (25), from the work of Newberry *et al.* (1973). This criticism comes principally from the poor fit of the equivalent moving average filter to the theoretical aerodynamic admittance function. But, whilst Newberry *et al.*'s work did initially

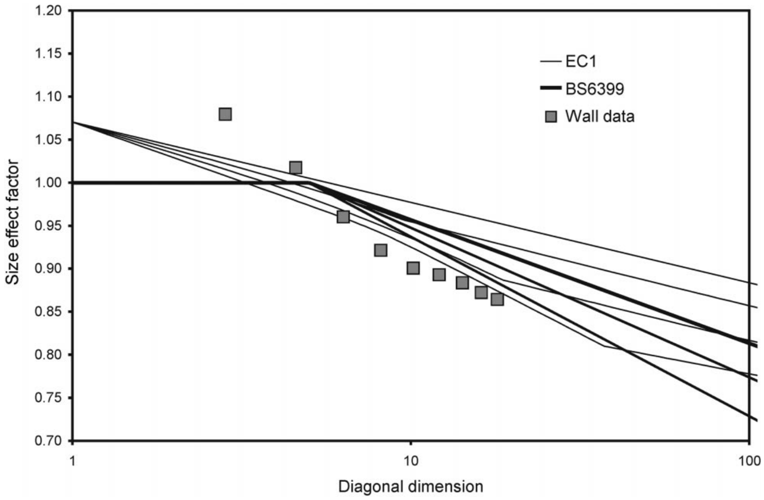

Fig. 5 Comparison of size effect factor from EC1 and BS6399 with wall data

underpin the values used in the 1970 UK code, CP3 ChV pt2, the later revisions of CP3 and BS6399-2 rely instead on empirical calibration of direct measurements. Rearranging the "TVL formula" (Baker's Eq 25) to:

$$k = \frac{L}{V \times t} \tag{1}$$

shows it to be simply the fundamental scaling relationship between velocity, length and time - without which no scaling, or wind tunnel testing would be possible. For every flow situation there will always be a corresponding value of k that, since Lawson pioneered this "equivalent duration" approach at Bristol University (Lawson 1976), might fairly be called the "Lawson number".

So, while the value k=4.5 was originally introduced on the basis of circa 1970 "Royex House" results, its retention is due to the close correspondence to experimental data obtained since then at full and model scale. Fig. 5 compares the (largely empirical) size effect factor from BS6399 and the equivalent (largely theoretical) factor from EC1 (from Dyrbe & Hansen (1999)) with the direct measurements for the full-scale boundary wall reported by Robertson *et al*. (1998), i.e., the same experimental data that was analysed by Baker. It is clear that the BS6399-2, EC1 and the actual measurements show very similar trends and values, except that both EC1 and the data predict factors greater than unity for dimensions less than 5 m. The BS6399-2 drafting panel took the pragmatic view not to use factors greater than unity because the design of small elements in the UK, having been based previously on the CP3 "3-second gust", already took the effect into account through performance testing of the products.

The concept of always taking the most conservative assumption, advocated by Baker, is not acceptable in codes of practice, since accumulation from each factor leads to excessive overall conservatism. The proper course, as used by BS6399-2 and most other codes, is to use the most likely (modal) values and to control the overall conservatism by calibration and the overall reliability through the partial load factors (γ factors). In any case the base "Lawson number" k=4.5 used in BS6399-2 is seen to be conservative for dimensions greater than 5 m and adopting Baker's recommendation of $k=2$ would add a further 6% unnecessary conservatism.

4.2. The pseudo-steady pressure coefficients

Pseudo-steady coefficients are derived by normalising the measured extreme pressures by the measured extreme wind speed. By reversing this process, the design extreme wind speed produces the design extreme pressure. Pseudo-steady coefficients give an exact representation of the extreme pressures, without recourse to any assumption or calibration. One-to-one correspondence to the mean pressure coefficients would occur only if the quasi-steady assumption held perfectly.

Baker claims that Cook's (1990) argument – that ratios of the pseudo-steady coefficient to the mean coefficient less than unity indicate that not all velocity fluctuations contribute to the surface pressure fluctuations (expected for the windward face), whilst values greater than unity indicate turbulence induced by separated flows (expected on the leeward face) – is "paradoxical" and "exactly the opposite of what the current results suggest".

Firstly, this ratio may be defined in terms of the extreme and mean pressures and the gust factor, from the definitions of each coefficient:

$$\frac{\tilde{C}_p}{\bar{C}_p} = \frac{\dfrac{\hat{p}}{1/2\rho\hat{V}^2}}{\dfrac{\bar{p}}{1/2\rho\bar{V}^2}} = \frac{\dfrac{\hat{p}}{\bar{p}}}{\dfrac{\hat{V}^2}{\bar{V}^2}} = \frac{\dfrac{\hat{p}}{\bar{p}}}{G^2} \tag{2}$$

Rearranging Eq. (2) leads to:

$$\hat{p} = \frac{\tilde{C}_p}{\bar{C}_p} G^2 \bar{p} \tag{3}$$

where the ratio is seen to be a multiplier on the quasi-steady gust factor relationship (without the $dC_p/d\alpha$ terms). A ratio less than one indicates less than quasi-steady response (not all fluctuations contributing) and a ratio more than unit indicates more than quasi-steady response (additional contributions from building-generated turbulence, but also $dC_p/d\alpha$ effects from the incident turbulence). Clearly, there is no paradox as claimed by Baker, leading to the expectation that his analysis or interpretation of the data is somehow flawed.

Secondly, the results shown in Baker's Fig. 25 are distorted by the unrepresentative balance between front face and rear face mean pressure coefficients, discussed earlier. Recalculating the ratios in Fig. 25 assuming the ESDU (1971) values C_p=+0.8 on the front face and C_p=-0.3 on the rear face, indicates that:

- values in Fig. 25(a) for the front face are too high by a factor of 1.44 and
- values in Fig. 25(b) for the rear face are too low by a factor of 0.58.

These errors are sufficient to reverse Baker's observations – the ratio is now less than unity on the front face and greater than unity on the rear face, as expected.

4.3. The empirical correction for lack of correlation between front and rear faces

This correction is not susceptible to errors in the reference static pressure. The peak difference in the net pressures across the wall were compared to the net difference in the peak pressures on either side, and in both cases any error in static pressure reference cancels out. Baker's Fig. 26 and the conclusions drawn from it are therefore valid.

5. Bakers conclusions

5.1. Conclusion (c)

The observation that fluctuations on the rear face were influenced by the oncoming turbulence is to be expected if half the value of the "observed" mean pressure coefficient, C_p=-0.6, is due to an error in the static pressure reference. This leads to about 3/8ths of the front-face fluctuations appearing to occur on the rear face. When this effect is corrected the quasi-steady model under-predicts rear face unsteadiness, which reverses Baker's conclusion.

5.2. Conclusion (d)

Baker's derivation of the *k* factor, or "Lawson number", used a methodology long abandoned. The best derivation is by direct comparison of the measured values, as shown for this same experimental data by Robertson *et al*. (1998). On this basis, the *k* value gives slightly conservative estimates of load.

In concluding that the pseudo-steady coefficients are not "entirely adequate", Baker fails to recognise that the pseudo-steady coefficients give an exact representation of the extreme pressure, requiring no assumptions or calibration. The discrepancies reported by Baker are entirely due to anomalies in the collection and analysis of the data.

6. Conclusion of this discussion

This discussion reveals that many of the conclusions drawn by Baker (2001) are undermined by anomalies in the data, its analysis and interpretation. When these anomalies are corrected, the exact opposite of Baker's conclusions are obtained.

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1. Nature of the response

In his discussion of the paper “Unsteady wind loading on a wall” (Baker 2001), Cook discusses at some length the argument of Hoxey *et al*. (1996) referred to in the paper, and also the accuracy of the experimental data. As these data were obtained by Hoxey and his colleagues at Silsoe Research Institute, it seems appropriate for this response to Cook’s discussion to be jointly written by Hoxey and by the writer of the original paper, Prof. Baker of the University of Birmingham.

Before considering the various points raised by Cook, some general points need to be made. The first is that the points raised by Cook with reference to extreme value analysis refer to only a minor

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component of the paper. The method of analysis chosen (the quantile method) was chosen both for its simplicity and for its appropriateness to the situation being considered. In particular its use in the conditional sampling analysis in the paper led to it being used for extreme value analysis. The comments made in section 2 of this response, which follow on from the arguments made in Hoxey *et al*. (1996), further suggest there are a number of very good reasons why conventional methods of extreme value analysis are not appropriate. Secondly it will be seen from section 3 below that there seems to be no reason to think that the experimental data used in the paper are not reliable and accurate. Indeed to question the reliability of these data simply because they do not conform to values that might be expected from other sources, is not consistent with the scientific method. If these two points are accepted, then most of the arguments presented by Cook have no validity. In section 4 we consider Cook's discussion of the use of the quasi-steady assumption and aspects of codification and point out what we believe are misunderstandings of the content of the original paper. These general points being made we believe that Cook's discussion is useful in that it raises a number of issues that are of interest to the wind engineering community as a whole and where there is a need for further work to resolve outstanding questions. These are summarised in section 5 of this response.

2. The concept of stationarity and the use of extreme value analysis

The concept of stationarity is fundamental to wind engineering. However whilst it is relatively easy to define stationary conditions in wind tunnel testing, the same cannot be said for full scale measurements, where large low frequency swings in wind direction occur (i.e., high values of lateral turbulence). The other fundamental concept of relevance here is that of the spectral gap i.e., that there is little energy within the wind fluctuations at frequencies of around 1 to 10 cycles/hour, and that the large scale and small scale wind fluctuations can be considered separately and the results superimposed. Recent investigations have questioned the validity of this concept (Jensen 1999) and the spectral gap seems to be very much less prominent in recently obtained data than it is in the early data of Van der Hoven. This casts doubt on Cook's statement that a time series can be rendered stationary by dividing by a 10 minute running mean. The technique of normalising with this running mean also implies that the peaks scale with mean velocity, which ignores completely the thermal effects within the atmosphere, which can affect mean wind speeds and turbulence levels /gust wind speeds in completely different ways. Finally, the results presented in the original paper suggests that the extreme values of loading are caused by discrete coherent structures within the oncoming wind, and examination of the time series suggest that these tend to cluster in such a way that the one-hour data sets used in the analysis are difficult to consider as fully stationary, even if the means and standard deviations for sub-hourly periods show little variation. This again suggests that the concept of stationarity as applied to full-scale data is not wholly appropriate.

These points being made, there appears to be a misinterpretation by Cook of the paper by Hoxey *et al*. (1996). That paper is concerned with simultaneous independent measurements of surface pressure on a building and of the wind dynamic pressure some distance away from the building in the undisturbed air flow. In such cases there arises an error associated with the spatial separation of these two measurements which, when the pressure coefficient is formed, produces a normally distributed error in the coefficient which is related to the spacing. In a truly stationary signal this would not arise for measurements made over the stationary period. However, as discussed above, the natural wind is intrinsically non-stationary and although there is evidence of a spectral gap there remains energy at the low frequencies, beyond the spectral gap. In practice, removal of these non-stationary, or low frequency, effects is impractical as they occur in the *u*, *v* and *w* velocity components or in wind pressure and direction, including the vertical flow direction. The paper by Hoxey *et al*. (1996) shows that if the pressure coefficient has an error that is normally distributed then an extreme value analysis is related to this error and not necessarily to any underlying real effect. The paper quantifies the error according to the spacing of the anemometer from the tapping point, and concludes that the method is unsuitable for full-scale data sets.

It is accepted that if the record is truly stationary then this random error will be considerably reduced and extreme value analysis would be meaningful. However, it is unlikely that any method can reduce a natural wind record into a truly non-stationary record, and even if this was the case, what confidence can be placed on a record that has been adjusted for non-stationarity in terms of the extreme values?

One aspect of the arguments of Cook concerning extreme value analysis does have some force - the statement in the original paper by Baker that the 99.95th percentile is equivalent to the maximum 1.8s gust value. This is indeed not strictly true, and the author is guilty of loose usage of terms. An analysis of the experimental data for pressure coefficient reveals that the values obtained by the quantile method and from the maximum value of a running 1.8s time series are very close to each other on the rear face (with an average difference of 0.007). On the front face there is a significant difference however with the quantile method result being on average 0.43 higher than the 1.8s running average maximum. Also, it was found that the quantile and maximum running average values of the wind velocity were again very close (0.14 m/s difference). This implies that, if the results of figures 23 and 24 in the paper are expressed in terms of the running average, the experimental values on the front face should be reduced somewhat, whilst the rear face values remain more or less the same. Because the quasi-steady values depend directly upon the velocity quantile values, these will not be significantly altered in either figure. This improves the agreement between the front face values and the 2nd order quasi-steady method in figure 23, and with the quasi-steady method in figure 24, whilst leaving the rear face comparison more or less the same. This change in the front face values represents the very short-term extremes on the front face revealed in the conditionally sampled results of figures 14 and 15.

3. The adequacy of the experimental data

Cook's discussion rightfully questions the validity of the measurement of reference static pressure. The measurements of pressure on the wall were compared with a static pressure sensor positioned upstream of the wall and slightly to one side. This sensor was a static pressure probe which had been "calibrated" against a tapping point set flush into level ground. There can be little doubt that the measurements reported have been made using a reference static pressure that is close to the distant surface pressure that would be sensed by a tapping point flush into level ground. However, it is clear from recent measurements that a steady static pressure does not arise in the natural wind and hence in full-scale experiments because vorticity in the surface boundary layer generates fluctuations in the static pressure, causing it to be depressed compared to a region of zero vorticity in the flow. This depression in the sensed static pressure does not assist in making the coefficients more positive on the windward wall and would in fact reduce them even further if corrections were made. In any case, the load on the wall is more dependent on the local static pressure than it is on some distant static pressure above the boundary layer. So, although there is some uncertainty about the reference static pressure this does not support Cook's statement "suggesting a systematic bias in the measurement of reference static pressure".

There is also considerable evidence from wind-tunnel measurements made on a wall of similar geometry, and on other structures such as a cube, that shows that the full-scale measurements are consistent with wind-tunnel results when a similarly located reference static pressure is used. From published results, the concept of a representative (area-averaged, or mean) coefficient for a windward wall of +0.8, for the perpendicular flow direction, implies a wall geometry where height is greater than length ($h/L>1.0$), as in practice the only way of obtaining a coefficient close to +1.0 is to reduce the length to near to zero (Holmes 2001, quoting Baines 1963).

4. The quasi-steady hypothesis and the nature of codification

It would appear from the discussion that Cook views the quasi-steady assumption differently from the way it is interpreted in the paper. In the paper the quasi-steady assumption is understood as having a number of different levels of complexity, and the higher levels of complexity include the pressure coefficient derivative terms. Such an approach seems to be that generally adopted in the more recent literature. Cook, however, seems to regard the quasi-steady assumption to only refer to the simplest, lowest order formulation used in the paper, and some of his arguments seem to rest on this misapprehension.

The quasi-steady assumption, together with the assumption of a value of k of 4.5, is fundamental to the equivalent static gust method that underpins the UK code. Whilst the fundamental nature of these assumptions were not discussed in the paper, recent work by Holmes (1995) and Dyrbe and Hansen (1999) shows conclusively that such an approach is to some degree flawed, in that the equivalent static gust method is non-conservative for load effects where the influence function for wind actions changes sign across the structure. In such circumstances methods such as the Load-Response-Correlation method are required. Even where the influence line does not change sign they show that the value of $k=4.5$ is non-conservative, and seems to be a factor of about 3 too high. Nonetheless the simplicity of the method makes it very attractive in codification terms. The paper by Baker concerned itself only with calculations of the value of k for the wall data. Fig. 13 in the paper shows that k varies significantly across the wall, and whilst an average value of 4.5 is probably not inappropriate (and thus the results agree reasonably well with the load reduction factor of BS6399 as shown by Cook), there are points where the value falls to around 2.0. Taken together these points suggest that if the equivalent static gust method is to be used for codification purposes, and its simplicity makes it ideally suited for such use, then rather lower values of k may be appropriate. How these values are incorporated and matched against other sources of uncertainty in codes is a matter for those concerned with codification and were by no means the primary concern of the paper.

5. Outstanding issues

On the basis of the discussion by Cook and this response, there are a number of areas of further research that might prove fruitful. These are as follows.

(a) How does the increasing evidence that the "spectral gap" might be less prominent than long assumed affect the assumption of independence of macro- and micro-meteorological wind conditions?

(b) What is the nature of the very short term gusts that cause the peak loads on the windward face of the wall - and can different scales of coherent structure be detected in the lower boundary layer?

(c) How should fluctuations of experimental static pressure be allowed for in full-scale measurements, and in the specification of design loading? Do these fluctuations also occur in wind tunnels?

(d) What influence do the geometric parameters of cross wind breadth, along wind depth and height of a non-streamlined structure have on the pressure coefficients on the upwind and downwind faces?

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