the Reynolds number is increased, and turbulent flow is more prevalent, the efficiency of most turbomachines also rises. Furthermore I do not believe that the authors are measuring turbulence in the conventional sense. They have clearly made measurements in a machine in which there are very high levels of unsteadiness, but this is not the same as high levels of turbulence for which conclusions relating to the flow physics can be made.

Turbulence is normally understood to mean the random motion of a fluid and is contrasted with the steady flow. This description begins to fail when the amplitude of the fluctuating motion is comparable to the mean motion, or when the basic flow is unsteady. Such a flow is that in an impeller running in a volute; whichever frame of reference one adopts, the flow is unsteady. One can retain some of the usual meaning of the term turbulence by removing the unsteadiness characteristic of the impeller motion, and this is normally done by a process of ensemble averaging. Ensemble averaging has been carried out in the present paper, whereby a laser anemometer sample is taken for each rotation of the impeller to give measurements at particular positions inside the impeller. These samples can then be averaged to give a mean at each position inside the impeller where measurements were taken; an estimate for the variance in velocity can be obtained by averaging the square of the difference between each sample and the mean. This variance is the square of what I believe that the authors have called turbulence.

Unfortunately the variance obtained is not really turbulence in anything like the normal meaning of the word. What has really been obtained is simply a measure of the variation in the passage flow from one realization to another. A clear indication that the unsteadiness is not turbulence by any conventional definition is the very high level: Fig. 11, for example, shows levels of so-called turbulent intensity in excess of 30 percent extending over about half the passage. Experimenters will know that it is quite difficult to generate turbulence levels in ordinary flows above about 10 percent.

One can give a concrete example of what is meant by considering the flow in a simple diffuser with a relatively large included angle. Suppose that the inlet velocity to the diffuser is 10 m/s and that the diffuser is 1 m long. There will undoubtedly be fluctuations in the flow, but one is unlikely to term those unsteady motions with frequencies below 10 Hz as turbulence; turbulence will be mainly that associated with the boundary layers and shear layers and this will be typically one or two orders of magnitude higher in frequency. One can distinguish between overall unsteadiness and what might be called real turbulence only if there is some frequency information: with the method of measurement used for these tests this is unfortunately impossible. In flows of a more complicated type one would expect this unsteadiness to be of largest magnitude where the flow is most likely to separate, near the suction surface of a blade.

It can be argued that a word only means what the user chooses it to mean, and that the authors may therefore call their unsteadiness turbulence if they wish. Unfortunately others should not follow this by attributing to this quantity the conventional meaning or significance of turbulence. The Reynolds stresses based on this unsteadiness will not, for example, have the same significance that they have with true turbulence in a boundary layer or wake. Nevertheless the authors do associate their unsteadiness with turbulence in its usual meaning in the latter paragraphs of the section "Flow Rate Variations—Centered Impeller."

Authors' Closure

The comments by Professor Cumpsty are welcomed. He indicates some viable points that need both further explanation and further investigations. The authors will respond to each of the points separately.

First, the authors made a general statement that as the turbulence is increased the losses increase and reduce efficiency. The reviewer responds that as the Reynolds number increases so does the turbulence but usually the efficiency increases. The authors certainly agree with this. The only point that the authors were trying to make was that for a given operating condition (i.e., Reynolds number, specific speed, general flow patterns, etc.), if the turbulence increases, so do the losses. The energy equation bears this out in that if energy is used to heat the fluid by turbulent action, the energy that can be used to generate head is decreased.

All of the other comments relate to the relevance of the reported fluctuation levels. The authors will discuss different ramifications. For example, earlier data taken in the discharge region (i.e., window 11) indicated fluctuations of 4–5 percent at all flow rates. These fluctuations would have certainly had some blade pass frequency variations (4X synchronous components) and probably some 1X buried in the measurements. Unfortunately, these velocity data were not correlated with any frequencies in these early measurements. Regardless, the point here is that the overall flow rate was not varying to the extent of some of the fluctuations reported in the recent paper. The throughflow was not unsteady to the extent of the reported turbulence levels.

Furthermore, measurements were also made in the inlet duct at the centerline two diameters from the inlet. Here fluctuations were measured to be only 2–3 percent. Again, the mass flow rate for the system is obviously not grossly fluctuating and is very steady and, thus, would not contribute to the measured fluctuations in the impeller.

Commenting on the high levels of turbulence, the authors should point out that these were in general in regions of low velocity (very near or in separation regions). Also, the largest values were often for small radii, where a poor inlet condition produced a skewed axial profile and a separation region on the front shroud (in the radial direction) as was reported earlier (Miner et al., 1989). At larger radii the values were more in line with the reviewers' comments (< 10 percent) and what the authors would expect. The authors also should point out that both 4X and 1X correlations of the data with running speed were performed and these yielded the same values of fluctuations for the nonorbiting case.

The authors agree with the reviewers' comments that all frequencies cannot be extracted from the data system used for this paper. He does imply, however, that the authors made no attempts to correlate at any frequencies. As noted above, the system was used to correlate data at both 1X and 4X synchronous frequencies. The reported fluctuations are at frequencies other than these. The variations reported here are the same as those that would be sensed by an instrument mounted on the rotating impeller, if that were possible. The authors do agree that "local unsteadiness" at other frequencies cannot be separated from the fluctuations, namely, unsteadiness that does not show up in the mass flow rate for the overall system. For example, if a rotating local unsteadiness was present in the flow at some frequency at a factor other than 1 or 4 of the running frequency, this would be "buried" in the so-called turbulence.