**Measurements of Velocity Wave Forms in the Dog Aorta**

**D. P. Giddens.** Although the authors have given some attention to aortic flow disorder occurring physiologically and under the effects of cardiac stimulants while our interest has been focused on disorder created by subtotal vascular stenoses, there are certain similarities in our findings which reinforce the conclusions drawn in the paper "Measurements of Velocity Wave Forms in the Dog Aorta." With regard to the rapid decay of turbulence predicted by Kiser, et al., we find that flow disorder creased during the period immediately following peak systolic velocity for a very mild stenosis (20 percent reduction in cross-sectional area in the descending thoracic canine aorta) is not maintained until the next heartbeat. This is determined by examining the energy spectrum during this "deceleration" phase and comparing it with that of the "acceleration" phase, i.e., the portion of the velocity waveform corresponding to increasing systolic velocity, with and without the occlusion present (Fig. 6 of footnote 3). However, as the stenosis, and hence the distal flow disturbance, becomes more severe, the disorder created in one heartbeat has not completely decayed by the time the next beat occurs. Footnote 3 describes the methods employed for our experiment and data analysis of the time history of flow disturbances.

A second point of similarity relates to the observation of periodicity in the disturbances observed in some of our experiments. Although those authors did not describe their method of detecting this, one very interesting technique is to form a time ensemble average waveform. In the poststenotic flows we have studied there is a clean periodicity in the apparently random velocity waveforms (Fig. 2 of footnote 3).

Finally, we suggest that, in addition to the total waveform energy spectrum method employed by Kiser, et al., it is very beneficial to examine the time history of events in the data analysis.

**T. J. Pedley.** There are three points which I would like to raise with the authors.

(i) *Entrance flow* (p. 299). In a recent paper [1] I argue that linearization of the equations of motion is an inappropriate way to handle the development of unsteady flow in a tube. I propose an alternative method in which the flow at a particular point and time is either approximately quasi-steady or approximately diffusive according to whether the influence of the entrance has been felt at that point and time during the pulse. I conclude that, in a representative canine aorta, the oscillatory components of flow are effectively fully developed only 4 or 5 cm from the entrance. The mean flow, decoupled from the oscillatory components, develops slowly, as if it were steady. From the point of view of wall shear stress, the oscillatory components are likely to be at least as important as the mean.

(ii) *Change of shape of velocity waveform* (Figs. 2 and 5). The increase in amplitude of the velocity pulse with distance down the aorta is somewhat unexpected in view of the usual data quoted by McDonald [2, p. 356]. See also Mills, et al. [3]. Could the authors outline the physical mechanisms which might cause this difference, and (b) cause the difference between Figs. 2 and 3? These mechanisms are a little difficult to infer from Anliker's theoretical work. Also, which dog is curve D, Fig. 5, taken from?

(iii) *Turbulence.* Whether or not the very disturbed flows reported by Neren and Seed represent turbulence depends on what you mean by turbulence. I take a flow to be turbulent if, when the experiment is repeated many times, and an ensemble average waveform taken and subtracted from the signal for each beat, then there are significant random elements remaining. This is the case with Neren and Seed's waveforms, as demonstrated by Parker [4]. The frequency spectrum of these random elements is continuous, has no obvious spikes, and is concentrated at the higher frequencies, which are the ones associated with random small-scale eddies. These eddies, when they are present, certainly decay by the end of diastole, but are none the less turbulent for that. It is unlikely that the spectrum will be the same as in steady pipe flow, except at the highest frequencies. Note too that disturbances generated in the boundary layers would be felt immediately in the core, since the pressure fluctuations are propagated at the speed of sound (not a diffusive process—see p. 298).

**Additional References**


**S. C. Ling.** The paper presents some additional physiological observations which are generally known. It lacks complete

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Authors' Closure

The authors wish to thank Professors Giddens, Ling, and Pedley for their discussion. The similarities in certain results reported simultaneously by Giddens, et al., were certainly pleasing to see; however, it should be noted that the present studies were confined to dogs which were free of circulatory disorders and, furthermore, none were induced artificially. It is expected that disorders created by major stenoses would tend to increase turbulence. This, coupled with a decrease in the diastolic period, could produce a turbulence which does not decay during one cardiac period. The experiments described by Giddens, et al., seem to confirm this.

The periodicity in the disturbances in the studies reported were detected by waveform analysis on the digital computer; to a certain extent these can be inferred upon examination of Table 2. Ensemble analysis techniques, which were not used in our studies, should be superior for this purpose.

Professor Ling rightly points out that the velocity measurements cannot easily be interpreted without a knowledge of the physiological parameters. However, these parameters are not easily determined in living animals. Neither the work by Pedley nor the work by Parker were available when final revisions of our work were completed in the latter part of 1975. The authors do agree with the comments on entrance flow and their arguments are supported by the experimental results presented in our paper. It should be noted that McDonald measured average flows while the present measurements are point values. Curve D on Fig. 5 is for dog 5. It is also close to the results obtained for dog 4. There is no obvious reason why the results in Figs. 2 and 3 for dogs 2 and 4 should be the same.

Finally, the authors do not quarrel with Professor Pedley's definition of turbulence. As discussed by Professor Giddens, the question of interest here is whether the disturbances persist from one heartbeat to the next. Our results show that for healthy dogs they do not.

Vertically Downward Two-Phase Slug Flow

D. Beattie.2 Professor Martin's paper is a useful reminder that, even with fields as thoroughly researched as two-phase slug flow, care must be taken in applying supposedly general equations to unorthodox situations. Experimental values of both the distribution parameter and the drift velocity differ significantly from those generally recommended for slug flow. The unexpected distribution parameter values have been logically explained in terms of observed eccentricities in the slug paths. Observed distribution parameter values furthermore provided a partial explanation of the unorthodox drift velocity values.

As noted by Martin, developed flow distribution parameter values have been associated with the maximum/average velocity ratio. As long as voids are confined to the flatter region of the velocity profile, this association is necessary. Furthermore, under such circumstances, the distribution parameter can be generalised and used for the prediction of other two-phase quantities (momentum flux, choked flow, etc.)22b. The situation becomes less predictable if, as is the case with the downflow experiments described, voids concentrate in the steeper region of the velocity profile.

The lower degree of development observed with downflow slug behaviour may not be confined to slug flow. Difficulty was reported in maintaining slug flow with certain conditions. Similar non-development of downward bubble flow near the bubble/slug flow boundary could be expected. It is significant that recently reported air-water downward bubble flow data23 were correlated by a distribution parameter which, like the present values (but unlike other downflow bubble data), was significantly less than the developed flow value. Furthermore, the bubble data of23 are very close to the slug flow equation obtained by interpolating between parameter values reported by Martin to allow for diameter effects.

Similarly, a comparison of the reference24 steam-water void data for slug and non-slug up and down flow indicates that, as observed by Martin, significant changes occur in the drift velocity behaviour with changed orientation.

Since equations of the form of Martin's equation (1) are used for predictive purposes, it is appropriate to note that the experimental technique employed is such that $V_s$ does not have the widely used interpretation $<j_s> <a>$. The two interpretations would be significantly different only where nonslug bubbles contribute significantly to the voidage, as shown in Martin's Fig. 7. (The zero $V_s$ of Table 1 corresponding to Fig. 7(d) clearly does not imply a zero $<j_s> <a>$). Also, an error has occurred in the discussion of the zero net flow data. $<j_s> = <j_r>$ implies $<j_s> = 0$ does not imply $<j_s> = <j_r> = <j_s> = 0$, as stated. The nonzero value of $V_s$ in Table 2, corresponding to Fig. 10(f), for instance, implies a nonzero local $<j_r>$, and so the zero $<j_r>$ implies $<j_s> = -<j_r>$ in the region of interest, even though both velocities are zero away from the bubble.

Nevertheless, his data do provide a useful basis from which downward slug flow behaviour can be reasonably estimated, even though he has not generalized his results into recommended equations. He has helped to remove present gaps in knowledge of two-phase slug flow.

Additional References


Author's Closure

The author is grateful to Mr. Beattie for his comments, all of which are pertinent and in need of clarification. Although he is