DOES THE VENTRICLE EXERT A SUCTION ACTION IN DIASTOLE?

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Although this question has been the subject of much discussion and theoretical argument in the past, valid experimental work on the question is very meagre. Katz (1930) has critically reviewed previous work on the subject and drawn attention to the necessity for a more reliable type of evidence than hitherto adduced. He has further attempted to obtain direct experimental evidence on the matter from which (supported by the data of the volume curves of the ventricles) he has drawn the conclusion that "the ventricle not only can but does exert a sucking action in the intact animal."

If this be true, an important aspect of the mechanics of the cardiac cycle has been clarified. If however the evidence be fallacious, there is every possibility that the stated conclusion may gain currency in physiological thought and lead to error and confusion in matters related to cardiodynamics.

It is the object of the present communication to show that the evidence adduced by Katz involves a fallacy, and that the character of the records obtained were necessarily produced by the hydrodynamics of his set-up, and were not determined by the mechanics of cardiac activity.

In order to relate the present experiments to those of Katz, it is necessary to consider in some detail the precise physical arrangements obtaining therein.

The general argument may be summarised as follows. A system is so arranged that when the ventricle (of the surviving turtle's heart) contracts, fluid is forced upward via the aorta through connecting tubes, into a reservoir, and the concomitant increase of pressure is recorded by an optical manometer inserted into the ventricular cavity through the A. V. ring. Further, when the ventricle relaxes, a drop of the intraventricular pressure curve below the zero line[2] is taken as evidence that a suction action is exerted by the ventricle in diastole.

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[2] Zero determined statically after the ventricle has ceased to beat.

As Katz appropriately remarks, "a narrow constriction[3] in the tubes through which the ventricle fills, will exaggerate any pressure variations which activity of the ventricle might produce, and so make it more appreciable."

While this is true, it is here submitted that the apparent negative pressure registered in the records, is not necessarily produced by any suction action on the part of the ventricle, but is an inevitable hydrodynamic consequence of placing the constriction there.

The detailed criticism of the apparatus may be stated by considering in stages the sequence of events during the complete cardiac cycle.

Stage 1. Just prior to ventricular systole, the pressure obtaining at the level of the manometer membrane is represented by the level portion of the recorded tracing (*P* in fig. 2 of Katz's paper). This level represents the *static pressure* operating on the membrane, due to the height of the fluid level in the reservoir, over the height of the membrane. Now it is deducible from the records themselves, that this level must necessarily be much higher than indicated in the diagram, in order to produce the curves shown, and that the diagram has been reduced vertically *in this respect*, for convenience of reproduction.

Stage 2. With the onset of systole, considerable resistance is offered to the flow of saline through the constriction so that the pressure rises abruptly. (See fig. 2 of Katz's paper.)

Stage 3. When systole ceases, the flow through the constriction is reversed, but, *inasmuch as the constriction is present, the full hydrostatic pressure of the fluid extending above to the level in the reservoir, is unable to operate on the manometer membrane, and cannot do so until static conditions are once more resumed.*

In the meantime, therefore, the pressure in the fluid system below the constriction is much reduced,[4] and the record must inevitably show a fall below the base line, as is actually seen in figure 2 of Katz's paper. This contention receives further support from the statement in Katz's paper that, "the reduction of pressure below the zero level of the system was found to be greatest when this zero level was highest, and when the ventricular activity was most vigorous."

This would also be expected from the hydrodynamics of the system. For in the first place, the higher the zero level, the greater the height of

[3] I.e., in the connecting tubes from ventricle to reservoir.

[4] This effect of the constriction in shielding the manometer system from the full potential pressure, under dynamic conditions, is analogous to the effect of the arterioles in producing a sharp gradient of pressure between the terminal arterial system and the capillaries. This is well illustrated in a diagram published by Landis to express such gradients of blood pressure, which he has established by direct experiment.

the fluid level in the reservoir above the manometer membrane, and therefore the greater the shielding of the membrane from the full hydrostatic pressure, when flow through the constriction is proceeding backwards into the ventricle in diastole.

In the second place a more vigorous contraction would force more fluid into the reservoir, and more time be required for it to return by gravity through the constriction, so allowing more scope for the negative pressure to be developed.

Although these considerations seemed clear enough to invalidate the conclusions of Katz, it appeared worth while to obtain direct experimental evidence in support of the present criticism. An effective proof of the present contention would be realised if a system could be set up, similar

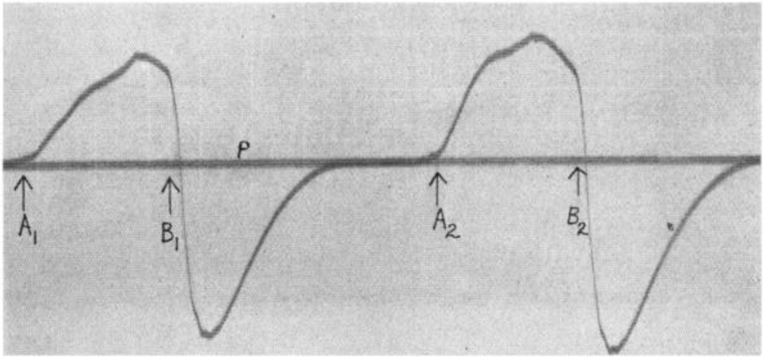

Fig. 1. P = base line of pressure.

At A_1 and A_2 plus external air pressure applied to "artificial ventricle."

At B_1 and B_2 external air pressure released, causing a rapid decrease of "intraventricular" pressure well below the base line in imitation of "ventricular suction" of Katz's records.

in all dynamic essentials to that of Katz, but in which the heart could be replaced by a variable capacity, which could not possibly exert any suction, and then to cause the system to function in such a way as to produce similar records to those published by him as demonstrating suction.

It was therefore arranged to set up such an essentially similar system. The reservoir used in place of the ventricle was a small rubber balloon, which had been immersed (when just moderately distended with water) several times in a solution of collodion, to provide an inextensible covering without interfering with flexibility.

This balloon was enclosed in a similar fashion to the ventricle in Katz's experiments. Inlet and outlet tubes and taps were provided, so that it was possible by turning one tap, to apply quickly positive air pressure to the balloon, to lend it, as it were, a substitute for contractile force, in imitation of systole, and then by turning a second tap, to secure a rapid removal of external pressure by the quick escape of the compressing air. A typical record so obtained is shown in figure 1.

Since this record shows the same essential features as those obtained by Katz with the turtle's ventricle, the conclusion is drawn that his experimental evidence involves a fallacy and cannot therefore be accepted to decide the problem attacked.

Further evidence is, however, adduced by Katz, which it is now necessary to consider. This evidence consists of inferences drawn from curves, published by Wiggers, of pressure and volume changes of the heart during the cardiac cycle. (Vide fig. 6 of Katz's paper.)

The argument developed by Katz from these curves is stated by him as follows:

> During rapid inflow (VI to VII) the pressure drops and the volume increases; . . . . Were filling entirely due to a passive distention, during this phase, one would expect to find an increase in both the volume and pressure, as is the case during distention of any elastic body and is actually the case during diastasis and early auricular activity. The fact that the pressure drops during filling indicates that the ventricle is relaxing at a faster rate than it can fill. Such a state of affairs is inconceivable if the filling were entirely passive during this phase.

The criticism offered is that such a state of affairs is not incompatible with passive filling, but that the opposite trend of the volume and pressure curves is merely an expression of the fact that *between VI and VII the decreasing tension of the ventricle wall in late relaxation has fallen below the prevailing tension in the walls of the atrium* so that blood passes from this chamber into the ventricle with a *simultaneous drop of pressure in both.*

The writer believes that Katz has devised the best experimental attack on the problem to date, and that his method suitably modified to avoid the hydrodynamic artifact is really capable of deciding the question. With respect to the query raised in the title of this paper, it can only be remarked for the present that valid evidence remains to be adduced that the ventricle exerts a suction action.

In conclusion I wish to extend my thanks to Doctor Wiggers in whose laboratory these experiments were carried out, and without prejudice to the present views of Doctor Katz, to thank him for his courteous and impersonal discussion of the whole matter.

SUMMARY

1. It is argued from theoretical considerations, that the experiments of Katz, from which the conclusion was drawn that "the ventricle not only can but does exert a sucking action to draw blood into its chamber," involves an experimental fallacy.

2. These theoretical arguments are further supported by experimental evidence, *showing that a purely passive system* can be utilised to reproduce the curves from which the active "suction action" was deduced.

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