

teristics were used as were not changed in the control. The figures for the means, for their standard errors, and for the probabilities of the null hypotheses (*t*-test) are specified in the paper<sup>2</sup>.

2. Discussing these same experiments Dr. Pringle writes on p. 108: "Schneider's (1953) claim to have demonstrated a static sense mediated by the halteres of *Calliphora* has already been criticized, . . ."

I never claimed to have demonstrated a static sense mediated by the halteres. As explicitly stated in the paper<sup>2</sup>, for the investigation of a static sense I examined flies the halteres of which were removed 3 days before the experiments. Therefore I could not claim that the halteres mediate a static sense. On the contrary, I emphasized that the receptors responsible for the observed reactions are not yet known.

3. On p. 109 Dr. Pringle describes the experiments I made which suggest a physical stabilizing effect of the halteres. He states, correctly: "The conclusion from these results, that there is a direct physical action of the halteres on the movements of the wings, relies on the assumption that haltere reflexes are absent in 'anaesthetic flight' and that the heat treatment had destroyed all sensory function."

If one of these assumptions is true, the physical stabilizing effect of the halteres is proved. To my way of thinking my experiments demonstrate that both assumptions are very probable and that it would be difficult to find grounds for assuming the contrary.

Dr. Pringle has, however, ignored the probability of the assumptions. When he argues: "The reasoning is unconvincing in view of the extreme physical difficulties of a possible mechanism. Pringle (1948) showed that the mass of the halteres is about 0.04 per cent of the mass of the fly, so that a direct gyro-stabilizing effect is out of the question," the reader is given the impression that in my paper<sup>2</sup> I have suggested a direct gyro-stabilizing effect of the halteres.

This is not true. On the contrary, I denied this hypothesis with the same argument as Dr. Pringle has used. Furthermore, when he states in the next sentence: "Only by a form of servomechanical influence on the force-transmission mechanism in the basal wing articulation could a direct physical influence operate, . . .", he is concurring with the suggestion made in my paper<sup>2</sup>. For my experiments have demonstrated a close physical interaction between the motions of the wings and of the halteres of a flying *Calliphora*.

Continuing, Dr. Pringle states: ". . . and the anatomy of the fly does not suggest that such a mechanism is present".

Here is the sole argument that remains. I find it a weak argument to state, in the face of physiological evidence, that the morphological basis is not yet known. Between the regions of the haltere base and of the wing articulation there are, however, movable sclerites as a possible anatomical substratum for the suggested mechanism. Dr. Pringle himself has described "a mechanical interaction between the two systems" (namely, the halteres and the wings)<sup>2</sup>.

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<sup>1</sup> Pringle, J. W. S. (Camb. Univ. Press, 1957).

<sup>2</sup> Schneider, G., *Z. vergl. Physiol.*, **35**, 416 (1953).

<sup>3</sup> Pringle, J. W. S., *Phil. Trans. Roy. Soc.*, B, **238**, 347 (1948).

I MUST apologize to Dr. Günter Schneider if I have inadvertently mis-interpreted some of his experiments in my book. I have the greatest respect for his skill as an experimenter, and have acknowledged that his 1953 paper widens considerably our knowledge of the functions of the halteres of Diptera. In his communication Dr. Schneider objects to three passages in my book. The second of these I admit is in error; he does not claim that the halteres mediate static reflexes. I was misled by the summary at the end of his paper in which, having stated that static sense organs are present and that compensatory wing movements occur to rotations about the vertical axis, he continues "These reactions are mediated by the compound eyes, halteres and antennæ (and perhaps by other sense organs responding to the air-flow)".

The first and third criticisms, to which Dr. Schneider objects, I cannot altogether withdraw, but I should, perhaps, have made them more precise.

1. His evidence for a static sense is derived from comparison of the shape of the powder patterns (pterograms) produced by the down-wash of air from a flying fly when it is placed in various attitudes. Some differences are found in the patterns when the fly is performing what is known as 'anaesthetic flight', and these are reasonably ascribed to direct aerodynamic effects, since reflexes are supposed to be absent in this condition. As evidence for a reflex effect in normal flies, Dr. Schneider considers one particular measurement of the pterogram, its fore-and-aft length, and shows that the results are statistically significant as between flies tilted to the left and to the right. This effect is said to be absent in anaesthetic flight, but we are not given a similar statistical treatment of the same measurements in the controls. Even accepting this, however, it remains true that the wing movements of anaesthetic flight have never been shown to be identical with those in a normal insect, and it cannot, therefore, be excluded that this effect also is a direct aerodynamic one.

3. A purely physical, non-reflex stabilizing effect from the halteres is the most startling of Dr. Schneider's claims. I was, of course, well aware that he did not support the old mass-balance theory, which is clearly impossible in the light of measurements of the mass of the haltere; I do not think that the sentence in my book gives a false impression. I could not find room for a full discussion of his evidence for some form of servo-mechanical control, but hoped to stimulate interested persons to read Dr. Schneider's paper in the original and decide for themselves whether or not his evidence is convincing. In effect, he postulates a mechanical linkage between the bases of the halteres and wings so precise that the mechanical coupling between flight muscles and wings is varied by the Coriolis forces generated in the halteres during rotation of the whole fly: and, further, that this operates to stabilize flight in all three planes of space. I agree, in general, that a lack of knowledge of the morphology should not deter one from accepting physiological evidence, but when that evidence itself is not water-tight I do feel that a claim as remarkable as this should be backed up at least by a suggestion of how the mechanism can operate physically.

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