

Comments from S. R. Caplan

Dear Sir:

I should like to comment as follows on the points raised by Drs. Wilkie and Woledge, if only to draw the reader's attention to the discussion of these questions already given in the papers:

(1) No new features whatever have been added to the theory—it remains just as originally presented.

(2) One "exact prediction" is that tension should be a linear function of both contraction velocity and "chemical flux." A second, involving the same parameters, is illustrated by Fig. 3, Part II. A third is that the Hill force-velocity relation should be obtained when the muscle is loaded by means of purely dissipative mechanical resistances. A fourth is that the *rate* of free energy expenditure (during the initial stationary state) should not be greatly different in unloaded contraction and in isometric contraction, contrary to the graph presented by Wilkie and Woledge.

(3) The "chemical flux" in the regulator is identical to that in the converter in the stationary state.

(4) The quantitative treatment of Wilkie and Woledge *is* disputed.

(5) Only the *initial steady-state* rates of reaction and contraction in *tetanus*, for which the Hill equation holds exactly, are considered.

(6) The values of ξ_2^1 which might fit the experimental facts, when steady-state reaction rates become available, are those which neither exceed unity nor are very small in comparison to unity.

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