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Reviewer's Comments

The authors bring forward data to confirm the findings of others that estimated hepatic blood flow is reduced by exercise. Their contribution consists chiefly in their use of the single injection of indocyanine green as a quantitative approach to the hepatic circulation and in their evaluation of the method under the unsteady state conditions of their study. The lack of information regarding the total blood volume makes it difficult to appraise the value of ICG clearances, particularly in view of the probability of a significant change in splanchnic blood volume. In view of the character of the exercise and the hyperventilation it is not at all unlikely that the systemic blood volume increased in association with a marked fall in splanchnic blood volume. This alone might play a role in altering the calculated clearance volume. Was there no "tail-off" in the disappearance curve? Did the authors subtract a long-term disappearance exponential? The authors do not state the case for the "priming hypothesis" quite accurately since they say their data do not bear it out. (p.20) However, measurements of hepatic blood flow could scarcely be depended upon to detect transposition of blood from the splanchnic bed to the central venous reservoir. Such a translation could take place in seconds and be quite beyond detection by any method currently available for measuring hepatic outflow. The authors do not state why they think EHBF decreases though they imply that it is an active process. This may well be the case but in the absence of arterial pressure levels, correction for hydrostatic shifts, and information regarding thoracico-abdominal pressure gradients and gradient patterns with respiration, it would be very difficult indeed to conclude that the resultant is "homeostasis." Although these considerations might suggest some alterations in the discussion, the data are very interesting and in view of the range of exercise, quite convincing in supporting the authors' basic contention.

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II

Reviewer's Comments

This study extends existing knowledge and introduces new information as to hepatic and splanchnic flow during upright exercise of maximal or near maximal severity. The experimental design was good and evidently executed with care. The techniques as described appear entirely adequate.

The discussion is excellent. It is highly relevant, critical, and lucid. The conclusions are well founded. It was particularly impressed with the soundness of the approach to the estimation of the relative severity of the exercise, a factor frequently not taken into account or inadequately expressed. I was surprised by the very fast heart rates during what were listed as easily submaximal exercise. The heart rates in several subjects were at the levels usually associated with maximal or oxygen uptakes and "aerobic" work levels. I believe this discrepancy should be discussed particularly in view of all the Swedish work using heart rates as the index to relative severity of exercise.

Page 9, par. 1. It would be most interesting to know how this correlation would come out if  $\dot{V}O_2$  were expressed as % of resting. Since the authors have gone so far as to give 2 different expressions here (an admirable thing), this additional one would be valuable (% change in  $\dot{V}O_2$ ) since it has been used by others. Their concept of the approach-to-maximal is provocative and should be defended in as many ways as possible.

Page 20, top. This autotransfusion discussion is below the standards of the rest of the paper. The present results have no bearing on the acute shift in question. This should be deleted.