Which Circulates Faster Through the Cerebral Microcirculatory System, Red Cells or Plasma?

To the Editor:

In a recent paper describing interesting experiments to measure the plasma and red cell transit in a cerebral ischemic area of the cat brain (Stroke 12: 218–223, March-April, 1981), Little et al. reported that the transit time of plasma as represented by $^{131}$I albumin was shorter than that of red cells labeled with $^{99}$Tc even in the control State. This is just the opposite of data reported by us in the cat brain using our photoelectric method. It also contrasts with the results of other groups including Larsen & Lassen in the brain,2 Pappenheimer & Kinter in the kidney,3 Rapaport et al. in the lung,4 Moore & Baker in the skeletal muscle, and Freis et al. through the forearm. All these authors found that the velocity of red cells exceeded that of plasma. These findings were supported by the larger plasma volume than red cell volume in tissue.1-9 Such observations have led to the concept of a lower value of tissue hematocrit than large vessel hematocrit (Hct,v). The relationships in this situation can be explained mathematically as follows.

The equation for the tissue hematocrit (Hct,v) derived by Larsen & Lassen2 from the basic definition of the hematocrit value (Hct) as $Hct = 100 \times \frac{V_r}{V_r + V_p}$, where $V_r$ is the red cell volume and $V_p$ the plasma volume, is

$$Hct_v = 100 \times \left[1 + \frac{MCT_r}{100 - Hct_v} \frac{MCT_r}{MCT_p} \right]$$

where MCT is the mean circulation time which is by definition volume divided by flow, and is therefore equivalent to the mean transit time (t). If we consider the “relative” tissue hematocrit which means Hct relative to Hct,v expressed in percentage, or $rHct_v = 100 \times \frac{Hct_v}{Hct_v}$, and if we neglect the term $Hct_r = (1 - \frac{MCT_p}{MCT_r})$ which is much smaller than $100 \times \frac{MCT_r}{MCT_p}$ when $MCT_p/MCT_r$ is close to unity, the Larsen & Lassen equation reduces to

$$rHct_v = 100 \times \frac{t_r}{t_p}$$

In other words, the ratio of the mean transit time for red cells and that for the plasma is approximately the tissue hematocrit relative to that of the large vessels.

The data of Little et al. would imply therefore that the cerebral tissue hematocrit is higher than the large vessel hematocrit, which is incredible. A closer examination of their analytical method shows that the transit time was determined by them from measurements of the total peak time. We believe that the tissue concentration curve which they recorded was a cumulative distribution function, whose peak time represented only the arterial phase, and by no means the microcirculatory phase as they claimed. Even with any contribution from the arterial phase, their data are dubious due to the existence of the Fahraeus-Lindqvist effect. We recommend that they recheck their data pertinent to the relative changes in the transit of these two elements. I think that the relationship of the albumin to the relative changes in the transit of these two elements. I agree that the relationship of the albumin to erythrocyte transit as demonstrated in the investigation was at variance with the previously-described Fahraeus-Lindqvist effect. This may have been related partly to greater resistance in the syringe with $^{99}$Tc-erythrocytes. Measurement of the interval from the arrival by morphological techniques. The results indicated: (1) progressive delay of albumin and erythrocyte transit with longer periods of middle cerebral artery occlusion in cats developing cortical infarction; (2) absence of complete microcirculatory obstruction despite absence of carbon filling in infarcted cortex; and, (3) no findings to suggest plasmapheresis.

The technique used to measure albumin and erythrocyte transit involved the measurement of radionuclide activity over the sylvian region (i.e., core area of ischemia) ipsilateral to the middle cerebral artery occlusion following ipilateral intracarotid injection of $^{131}$I albumin and $^{99}$Tc labeled erythrocytes. Measurement of the interval from the arrival to disappearance of radionuclide activity in cortex together with analysis of the radionuclide activity curve should provide an index of transit for these two elements. I agree that the relationship of the albumin to erythrocyte transit as demonstrated in the investigation was at variance with the previously-described Fahraeus-Lindqvist effect. This may have been related partly to greater resistance in the syringe with $^{99}$Tc-erythrocyte injection and consequently slightly longer injection times. The objective of the study, however, was not specifically directed towards comparing absolute transit times of albumin and erythrocytes per se but to the relative changes in the transit of these two elements. I think that the objectives as stated in the paper have been fulfilled.

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References
8. Sklar FH, B urke JR, EF, Langfitt TW: Cerebral blood volume: values obtained with 51Cr-labeled red blood cells and RISA. J Appl Physiol 24: 79–82, 1968
LETTERS TO THE EDITOR

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within an ICU setting. Recent reports have emphasized the high prevalence of serious cardiac dysrhythmia requiring therapy following acute SAH.\textsuperscript{2-4} Additionally, acute SAH patients are frequently encephalopathic, requiring almost constant nursing attention to keep quietly in bed or, if moderately sedated, frequent monitoring of vital signs. Is monitoring the sedated SAH patient in a sometimes busy ICU preferable to less vigilant monitoring of patient status and cardiac rhythm in the classical quiet, dark room? Is cardiac monitoring in acute SAH mandatory?

Secondly, we were unable to find convincing evidence supporting the use of anticonvulsants in acute aneurysmal SAH.\textsuperscript{5} It seems particularly important to avoid the administration of unnecessary medications in these patients.

What are the data favoring the use of prophylactic anticonvulsants in this setting?

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References

Editor’s Note: The above correspondence was sent to the author cited, and his comments follow.

To the Editor:
I thank Drs. Hart, Miller and Coull for their letter. While they question some aspects of the medical care of patients with aneurysmal subarachnoid hemorrhage (SAH) which were not the emphasis of my recent paper, their comments are important. I am happy to venture my opinions.

I concur with their concern about the risk of potentially life-threatening cardiac arrhythmias in patients with recent SAH. Cardiac monitoring during the first few days after SAH is important, and I include cardiac monitoring in the acute care of these patients. Whether the noisy, hectic atmosphere of most intensive care units is conducive to rest for these patients is another matter. Ideally, the patient should have the combination of a monitored room with intensive nursing care and a quiet environment. Local options would include a Stroke Care Unit, an isolation room in an intensive care unit, or even a coronary care unit.

Drs. Hart, Miller and Coull question the need of anticonvulsants in the preoperative care of patients with SAH. Just as with many other aspects of the preoperative care of these patients, the efficacy of the anticonvulsants has not been proved. The possible effects of a single or a series of convulsions on the brain and the ruptured aneurysm must be weighed against the possible side effects of the anticonvulsants. I believe the 4–26% incidence of convulsions in patients with SAH is of sufficient frequency that the use of anticonvulsants is warranted. Often phenobarbital can be used as both a sedative and an anticonvulsant. I encourage Dr. Hart and his colleagues to pursue this question further.

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References

Atypical Fibromuscular Hyperplasia
Reference Given

To the Editor:
In Dr. Mettinger’s very fine recent article, Fibromuscular dysplasia and the Brain, II. Current Concepts of the Disease, Stroke, Vol. 13, January-February, 1982, he enumerates the reported locations of the disease in table I. I would like to point out that in addition to these locations fibromuscular dysplasia has been described in the common carotid artery. I refer to a recent report by myself and others, Atypical Fibromuscular Hyperplasia: report of two cases, Journal of Neurosurgery, Vol. 54, 685–689, May, 1981.

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Practical considerations in antifibrinolytic therapy.
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http://stroke.ahajournals.org/content/13/5/722.2.citation

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