LETTERS TO THE EDITOR

Comments on "Sympathetic Modulation of Hypercapnic Cerebral Vasodilation in Dogs"

We have read carefully the recent study of D'Alecy et al. (1979) which compared two methods for measurement of cerebral blood flow, microspheres and the D'Alecy and Feigl (1972) modification of the venous outflow method. The experiments suggest that, under some conditions, microspheres and the venous outflow method provide similar results but, during sympathetic stimulation, microspheres indicate no reduction in cerebral blood flow while the venous outflow method indicates a marked reduction in flow. Thus, one of the methods is not accurate during sympathetic stimulation.

We are concerned about the authors' hypothesis that streaming of microspheres might lead to overestimation of blood flow to extracranial muscle and underestimation of flow to the brain. To examine this possibility, the authors inserted catheters into the lingual and infra-orbital arteries to produce turbulence in the carotid and internal maxillary arteries. In contrast to the authors' hypothesis, production of turbulence did not increase values for cerebral blood flow. Insertion of the catheters decreased flow to the upper lip, tongue, and nose, presumably because these regions are supplied by the arteries that were ligated. To test directly the possibility of streaming of microspheres, the authors compared the concentration of spheres in brachial and extracranial arteries. They found a 2% difference, which suggests that there is little or no streaming. Thus, the hypothesis that axial streaming of microspheres may impair the validity of measurements of cerebral blood flow is not supported by the data.

What, then, could account for the different results obtained during sympathetic stimulation with microspheres and the modified venous outflow method? A recent study by Pearce (1979) suggests an explanation for the discrepancy. Pearce reported that the retroglenoid vein is densely innervated and very responsive to sympathetic stimulation. The retroglenoid vein is the extracranial continuation of the temporal sinus, which carries all of the cerebral venous effluent in the D'Alecy modification of the venous outflow method. It seems likely that stellate stimulation may constrict the retroglenoid vein, increase pressure in intracranial veins, and divert cerebral venous outflow to other veins (such as orbital veins and emissary veins from the dorsal sagittal sinus) in which flow is not measured. The authors have reported that pressure in the retroglenoid vein does not increase during sympathetic stimulation, but this does not exclude a significant increase in pressure in intracranial veins. Thus, we suggest that the discrepancy between measurements of cerebral blood flow obtained with microspheres and venous outflow during sympathetic stimulation may be due to adrenergic constriction of the retroglenoid vein and diversion of venous outflow to other channels. If the authors are able to exclude this possibility, we will be left with the difficult problem of explaining the discrepancy between the methods.

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References

Reply to the Preceding Letter

In their letter, Drs. Heistad and Marcus make two general points concerning our study (D'Alecy et al., 1979). The first is that we failed to demonstrate significant streaming of microspheres in the cephalic circulation of the dog. The second point is presented as a proposal to explain differences between the venous outflow and microsphere methods of measuring cerebral blood flow. On the first point we agree, and appreciate their partial reiteration of our Discussion. We would like to point out that, of the three stated objectives of the study, the first was successfully demonstrated, the second was successfully determined, and the third objective was evaluated in two experiments.

Their suggestion of proposed venoconstriction has two levels of interpretation. The first level is the simple, yet non-traditional, participation of veins in the control of blood flow to an organ. We admit that there is an unusual possibility that venoconstriction could contribute to the decrease in blood flow observed in response to electrical stimulation of the stellate ganglion. To test this possibility, further experiments with the venous outflow preparation would be required. The second level of interpretation of their proposed venoconstriction is
that of a spurious reduction in the measured flow by diversion of cerebral blood out of the brain via some unmonitored venous channel. If the retrogle-noid vein (which is part of the outflow tract in our preparation) constricts during sympathetic stimu-lation, this could increase intracranial venous pres-sure and possibly divert blood out some other chan-nel. The examples they gave for such channels are: the orbital veins and the emissary veins.

This concept is difficult to justify anatomically. The occipital emissary veins and the emissary veins on the surface of the parietal bone are routinely destroyed in the process of surgical implantation of a transducer and the occlusion of the sigmoid ve- nous sinuses. In humans, the orbital vein can be shown to communicate with the cerebral sinuses, but such hydraulic communication has not been demonstrated in the dog. The acrylic verification used in each of our studies fills vessels as small as 125 μm. If larger vessels had been identified by acrylic verification, then we would have had to occlude them or measure flow through them.

From a hemodynamic standpoint their suggestion is also difficult to justify. The size of the “suggested” channel must be such that sympathetic stimulation would cause approximately 21 ml/min during normocapnia, and 61 ml/min during hypercapnia to flow out (Table 2). A conduit capable of carrying flow of this magnitude, at venous pressures, would be observable by gross dissection. We have not been able to identify such a channel and to the best of our knowledge no one else has re-portcd such a channel, in the venous outflow prep-a-ration.

An unusual functional requirement must be con-trived for the “suggested” channel due to the re-sponse of the preparation to hypercapnia. In our study, hypercapnia produced a 450% increase in cerebral blood flow as monitored by the venous outflow technique. If such an alternate channel did exist, then it would have to be closed during this profound vasodilation or one would observe an attenuation of the hypercapnic response. That is, if not closed, some of the flow would “leak” out through the “suggested” channel, thereby giving only a moderate increase in measured venous out-flow. As documented in our study, the hypercapnic vasodilation is not attenuated in this preparation (Table 3).

The above three concepts were developed in the Discussion and we concluded: “The presence of such a channel cannot be denied on the basis of our data and therefore, this proposed channel remains a possible explanation for the observed decrease in cerebral venous outflow.”

Drs. Heistad and Marcus conclude that the ex-planation of the discrepancy between the methods lies in the assumption that one of the methods is not accurate. Ours is the only laboratory that has reported the use of both techniques simultaneously and we have thus far been unable to explain the discrepancy. As argued in the Discussion, numerous studies using methods other than microspheres have found some degree of sympathetic cerebral vasoconstriction. Microspheres, at low blood flow, in our study and numerous others, fail to show sympathetically mediated changes in cerebral blood flow. Our current position is that both methods have limitations, but under selected conditions both can accurately measure different aspects of the cerebral circulation. The question of which method is the most precise indicator of brain blood flow remains to be resolved.

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