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DISCUSSION

DR. JOHN H. C. RANSON (New York, New York): Clinical studies by Dr. Richard Kessler at New York University have also shown a close relationship between the ability of the hepatic artery to maintain total hepatic blood flow following diversion of portal blood, and

prognosis after portacaval shunt. Since the adverse effects of diverting portacaval shunts appear to be related in part to a decrease in the total quantity of hepatic blood flow, Mailard, Adamsons, and others have suggested preservation of hepatic blood flow by arterialization of the portal vein stump. There have, however, been difficulties in delivering arterial blood to the portal vein at physiologic pressures.

Either the arterialization behaves like an arteriovenous fistula elsewhere, with progressive increases in flow and pressure and resultant hepatic damage, or else there has been progressive obliteration and late occlusion of the arterialization.

In preliminary experimental studies, we have attempted to avoid these problems by carrying out portal arterialization using a vein graft banded by a 1 cm cuff of Teflon. The cuff was adjusted to deliver blood at the same flows and pressures which were measured prior to portacaval shunt. This type of arterialization has been carried out in 17 dogs. In animals with patent arterialization, body weight was well maintained and general health was good. By contrast, in animals with no portal inflow, there was significant weight loss, encephalopathy developed frequently, and one half of the animals died within 20 weeks.

In 15 kg dogs the normal portal vein flow is in the range of 300 to 400 ml per minute, and the diameter of the lumen of the banded segment of arterialization was only 2 to 3 mm. Portal flows and pressures were measured every 6 weeks in these animals, and, with this cuff technique, the patency rate was 77% at 6 weeks, but fell rapidly thereafter, to reach 15% at 24 weeks.

It is clear that this method does not, as yet, provide the answer to the long-term problems of portal arterialization, and further studies are needed. However, the benefits of physiologic arterialization were quite striking in this model, and I would like, therefore, to ask Dr. Burchell whether, if a more reliable method could be developed, portal arterialization might not be a useful adjunct in the patients whom he has identified, in whom the hepatic artery cannot compensate for portal diversion.

DR. WILLIAM V. McDERMOTT, JR. (Boston, Massachusetts): A little over 15 years ago, in collaboration with Dr. Nardi, data were presented (at the Surgical Forum and at this Society) concerned with the effect of portal-systemic shunting on total liver blood flow, as measured by the disappearance rate of chromic phosphate labeled with ^{32}P . These data pointed out that there was no relationship between the preoperative liver blood flow and the ultimate course of the patient, in terms of mortality, cumulative survival, or any other parameter. The change in liver blood flow, in terms of a significant drop following operation, was directly related to prognosis, and any significant fall would indicate a poor prognosis.

Dr. Schenck and his colleagues at about the same time were able to fractionate liver blood flow with an electromagnetic flowmeter, as you have seen today, with the same type of techniques, and pointed out at that time that, after a total diversion of portal blood in the cirrhotic, it was the increment of change, measured by an increase in hepatic arterial flow, which would best predict, what would occur in terms of encephalopathy and survival.

From this, we are now at the point where this type of study presented by Dr. Burchell, is beginning to clarify some of the very confusing aspects in the hemodynamics. Certainly, none of the other parameters, either preoperative total liver blood flow or any combination of pressure measurements, have consistently been useful as predictors, either as reported by us or by others. Hopefully, Dr. Burchell, Dr. Nealon and their colleagues will be able to provide us with mechanisms of predicting which patient will do well, as so many of them do, and which will get into some of these really disastrous problems of serious encephalopathy, despite the fact the bleeding is controlled.

I wanted to ask two questions of the authors. Did they analyze the preshunt proportion of hepatic arterial to portal venous inflow, and was there any type of correlation to prognosis by analyzing these combinations in various ways?

The other question: Do they have any data in patients with portal hypertension secondary to schistosomiasis? This group has a particularly poor prognosis after shunt surgery, and, in distinction to the cirrhotic, will often have sinusoidal pressures which may be much lower than the portal pressure—a factor which I think would be interesting for this group to apply the same type of studies as they have done in the cirrhotic. I don't know if they have already done so.

DR. ROBERT E. HERMANN (Cleveland, Ohio): Both this paper and the one preceding it, by Dr. Malt and his associates, address the problem of improving our ability to identify those patients who are high surgical risks for portal-systemic shunt operations. From our

past experience, I have come to recognize at least four factors associated with a high surgical mortality and postoperative morbidity: first, the severity of the liver disease; second, emergency shunt operations, because of the deteriorating liver function which occurs during this stressful period; third, the age of the patient; and, finally, to a lesser extent, the histologic pattern of the cirrhosis.

Portal pressures and portal flow studies have been a disappointment to all of us. By and large, these measurements have not correlated well with prognosis. Dr. Burchell and his associates have now again directed our attention to another measurement, that of hepatic arterial flow improvement after end-to-side and side-to-side portacaval shunts.

I would like to ask Dr. Burchell if he believes that this increase in hepatic arterial flow provides additional blood to the hepatic cells as the mechanism of improved clinical results. This would be an attractive hypothesis in those patients who have had end-to-side portacaval shunts, or side-to-side shunts where prograde flow to the liver in the hepatic limb of the portal vein persists. However, I don't understand how an increase in hepatic arterial flow could improve liver function or survival, when the increase in flow simply represents a "runoff" phenomenon, a shunting of this hepatic arterial blood through presinusoidal arteriovenous shunts in the liver, bypassing the hepatic cells, and running retrograde away from the liver, out the hepatic limb of the portal vein.

In addition, I would like to ask Dr. Burchell if he has correlated the measurement of increased hepatic arterial flow with either the ages of the patients or with the severity of their liver disease, since these two factors correlate strongly with improved survival.

DR. GARDNER WATKINS SMITH (Baltimore, Maryland): I have had the privilege of reading Dr. Burchell's manuscript, and he and his colleagues are to be especially congratulated for documenting a hemodynamic factor which has for some time been suspected of having something to do with the outcome of a shunt.

Indeed, the ability, or lack of it, of the hepatic artery to compensate for decreased portal blood flow was one of the central tenets of the original hemodynamic staging proposals. Attempts were made some years ago to increase post-shunt hepatic artery flow, both by Dr. McDermott's group and by our own, by doing periarterial neurectomy of the hepatic artery. We were able to demonstrate experimentally that this is possible, using a technique originally described by Professor Mallet-Guy. This method was applied clinically, in our experience at least, in only 7 patients. We could again demonstrate increased hepatic artery flow in those patients, and increased total liver blood flow postoperatively, but the effect on encephalopathy was equivocal.

Dr. Burchell suggested the prognostic importance of a compensatory increase in hepatic artery flow 8 years ago, as he points out in his manuscript. Dr. Ranson just mentioned the work that's been done more recently by Drs. Zimman, Kessler and Tice, in an indirect way suggesting that hepatic artery flow is important. And I think that the authors have now clearly shown, in a significant group of patients, that this is so.

The authors also properly point out in their manuscript that the observation is a simple one, but that its explanation is complex and theoretically difficult. I do have some questions and problems relating to their explanation.

First of all, they imply that the hepatic artery response is related, at least in part, to lowered sinusoidal resistance. If this is so, one would anticipate that a side-to-side shunt would be more effective in this regard than an end-to-side one. We have demonstrated, as have others, that wedged hepatic vein pressure is lowered to a greater extent by a side-to-side shunt. I am therefore disturbed that the authors found the least increase in hepatic artery flow after side-to-side shunts in which hepatopetal flow was maintained in the hepatic limb of the portal vein. No one would presume that the maintenance of hepatoportal flow after a side-to-side shunt would require a low outflow resistance, and therefore a low wedged hepatic vein pressure. I would therefore like to ask if they have any data to indicate any correlation between wedged hepatic vein pressure, measured before and after operation, and the increment in hepatic artery flow after the shunt. If so, the wedged hepatic vein pressure measurement might have some preoperative predictive value, since in our

own studies this was the only preoperative parameter which correlated with the operative measurement of portal blood flow, and with hemodynamic staging. However, at the time that we reported these data, we discounted the significance of wedged hepatic vein pressure as being a static rather than a dynamic measurement.

Dr. Hermann has already pointed out the unanswered question in relation to this increment in hepatic artery flow, and that is: Does it indeed, after a side-to-side shunt, provide nutrition to the hepatic parenchyma? I don't know the answer. I'd be interested to hear what Dr. Burchell thinks about that.

I would conclude by pointing out that this is a very valuable observation which helps to sustain the validity of hemodynamic concepts. At the moment it has two drawbacks, both recognized by the authors: first, the theoretical basis for the observation is complex and unclear; and secondly, thus far there is no practical way to use this information to select patients before the operation. It is only useful at present to assess prognosis in retrospect.

DR. LOUIS R. M. DEL GUERCIO (Livingston, New Jersey): As did so many people working in portal hypertension, the authors zeroed in on the liver and portal vein, and forgot about the systemic circulation, the pulmonary circulation, and the other physiologic aberrations which occur in all patients with cirrhosis of the liver, to varying degrees.

We, by some coincidence, studied 47 patients a number of years ago, and showed that death can be correlated very nicely with variations in arteriovenous oxygen difference. This points up the fact that we shouldn't be concerned so much with total hepatic blood flow, or hepatic arterial blood flow, but with oxygen transport and utilization. One of the things we showed was that those patients who had inefficient oxygen transport, in terms of a narrow arteriovenous oxygen difference, were those who were most likely to die following portacaval shunt, or any surgery for portal hypertension; so that it's very hard to interpret the data in terms of hepatic arterial flow alone, unless we know whether these particular patients represent the hyperdynamic group.

In addition, we have also shown that shifts in the P-50, or oxy-hemoglobin dissociation curve, very frequently are responsible for the problems that these patients have, in efficient oxygen transport. These patients die because of inability to transport and deliver oxygen, not only to the liver but to many of the other organs—the kidneys, as is well known with the hepatorenal syndrome, the lungs, and the heart.

I think that we should interpret all of these predictors in terms of the over-all physiology of these particular patients.

DR. ALBERT R. BURCHELL (Closing discussion): In approximate order, first, Dr. Ranson's comments about Dr. Kessler's work: As you probably know, this was a pumped extracorporeal umbilical-saphenous shunt. There is a problem with the study, in that there is no actual documentation by direct measurement of the presumed increment in hepatic arterial flow after the shunt. This is just a measurement

that is done before the shunt. Also, as a diagnostic procedure it is somewhat cumbersome; but we would certainly agree with their conclusions that the capability of the hepatic arterial bed to increase its flow substantially after a shunt is the primary determinant of the subsequent clinical course.

Now, arterialization, I must say, is a puzzlement to me. Our own experimental work in dogs has indicated it to be rather a disastrous procedure, even with reduction in pressure levels. Hemodynamically, our own unpublished data indicate that the hepatic artery flow increments achieved by the end-to-side shunt were then lost following arterialization of the portal vein stump.

Now, of course, this experimental procedure is being done in the normal liver of a dog, versus the good clinical results in the cirrhotic liver of man. Certainly the studies of Matzander, Maillard, and Adamsons would suggest that the procedure is beneficial. They have even done it in patients who have been ascites, which just makes no sense to me.

Dr. McDermott asked if we had done an analysis of hepatic artery-portal vein ratios. We have done that in every possible permutation, and could come up with no positive correlation. We have no patients with schistosomiasis that we have done flow studies on.

Dr. Hermann has wondered about our thinking concerning the side-to-side shunt infusing hepatic cells with highly oxygenated blood. Not presented today, but in the manuscript, there is a breakdown of the patients according to whether there is prograde or retrograde flow in the hepatic limb of the portal vein. The patients who did the best were those with the retrograde flow in the portal vein, and those who did the worst, even compared to end-to-side shunts, were those who had prograde flow. I do not know if this means anything. It may well be that what we are observing here is simply those patients who continue to have prograde flow, and have an hepatic arterial bed incapable of bringing a greater amount of blood to the liver, versus those that are capable of doing this, and therefore have a reverse flow.

Dr. Hermann also asked about correlation with age. There was no such correlation. The average age was 54 for all groups, and the group of patients that did particularly poorly ranged from 36 to 67 years of age, with an average of 54.

As far as clinical and laboratory correlation, the clinical and laboratory assessment of risk identified only 5 of the 17 patients who had a small increment (29%) in hepatic arterial flow. So I would suggest there was not good correlation in that regard.

Dr. Smith's question about the degree of drop in sinusoidal pressure resulting in the side-to-side type of shunt being more effective, has been partially answered, I trust, in the previous discussion. The side-to-side patients with the retrograde flow had the best prognosis of all subgroups studied. We have no data at all on wedged hepatic vein pressures.

In closing, I certainly would agree with Dr. Del Guercio, that we have zeroed in on only a very small section of the over-all picture, and that there needs to be some integration of our type of information with his type of studies. It should be emphasized, however, that our single hemodynamic variable, the increment in hepatic arterial flow, did carry with it a highly significant correlation with the subsequent clinical course.