ence in the high risk patients at the moment is the use of portal-azygous disconnection similar to the procedure which has been used for many years by Tanner rather than any type of portal-systemic shunt.

Summary

1. Experimental and clinical data have been presented relative to factors influencing post-shunt encephalopathy and hepatic failure.

2. Post-shunt encephalopathy increases with each decade of life.

3. Maintenance of hepatic blood flow is vital to a satisfactory result following shunt operation.

4. Measurement of FPP and HOPP can be used as an indicator of prograde flow in the portal vein and as a rough predictor of the likelihood of post-shunt encephalopathy.

5. Technics of hemodynamic evaluation should be amplified and used prior to contemplated operation for portal hypertension in order to identify high-risk patients and to select the appropriate operative procedure.

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Discussion

Dr. Frederick A. Reichle (Philadelphia): I wish to describe and comment on a method which is available now, and which we have been using to determine the preoperative portal vein blood flow in the unanesthetized, unsedated human being. This can be done, in principle, by installation of radiopaque water immiscible droplets into the main stem portal vein, by filming droplet motion by cineradiography and then with certain assumptions one can calculate portal blood flow.

(Slide) The technic of umbilical vein cannulation is illustrated here. This is done before the day of testing, preferably under field block or regional anesthesia, and preferably extraperitoneally. Then on a subsequent day an inner catheter can be passed through the umbilical vein retrograde in the left portal vein and into the main stem portal vein in a hepatofugal direction to the the region of the confluence of the splenic vein and mesenteric vein. Then by slowly introducing small amounts of Lipiodol, a water immiscible material, small droplets can be formed, and their motion in the portal vein can be observed by subsequent analysis of cineradio-

grams. The velocity of droplets of this size approximates mean linear velocity of blood in model experiments under conditions encountered in the human portal vein.

By simultaneous biplane portography one can estimate the diameter of the portal vein in two dimensions and therefore one can calculate the approximate average cross-sectional area. Knowing the cross-sectional area and the mean linear velocity, one can determine portal vein blood flow in the unseated, unanesthetized patient.

This technique may have an advantage over tracer techniques, in that if there are shunts around the cirrhotic liver, one has the catheter between the portosystemic shunts and the liver itself, so that portal flow can be determined without regard to interference from portosystemic collaterals.

Flow determination by this method can be influenced by the size of the droplet, the size of the portal vein and the velocity range within the portal vein at a particular time and in a given patient. However, in model experiments where droplets of 1 to 3 mm. diameter are used, and with diameters of tubing which approximates the diameter of the portal vein in the human being, droplet velocity in human blood has correlated very well with the mean linear velocity. These model experiments have been performed recently by Dr. Sovak and collaborators working in Dr. Lynch's laboratory in our Department of Psysiology.

This method measures portal venous blood flow, and we do not know how this correlates with nutritional blood flow to the liver in cirrhosis but we believe it may be as close as we can get at this time to an expression of portal blood flow as part of the total hepatic blood flow in the preoperative cirrhotic.

DR. W. DEAN WARREN (Atlanta): I believe that Dr. McDermott and his colleagues have probably made more contributions of real importance to this field than any but the original Whipple-Blakemore-Rousselot-Lord group that started the whole thing.

He has appreciated one point that his eluded most people who have referred to our work, and that is the significant hemodynamic features that are related to response of a patient to operation.

(1) The estimated total hepatic blood flow can be correlated with the stage of portal hypertension as related to that volume of portal flow estimated to perfuse the liver. The Class 1, or the high portal flow patients, as shown by angiographic technics, have a higher preoperative estimated hepatic blood flow than those who do not. The critical point is that the drop in flow post-portacaval shunt in the high flow group far exceeds the change in the other groups. We feel this is the significant feature—that sudden, complete deprivation of portal flow of a major degree is harmful to the liver. One has to evaluate this in relationship to the severity of the liver disease.

A Class-A patient who loses a lot of portal flow will be worse off than a Class-A patient who loses little flow; the same thing is true in Class-C.

However, the biggest need in this field for accurate hemodynamic studies. The radioactive colloidal gold technic, as you all know, is fraught with errors in the cirrhotic, and you have to do it by groups in order to get statistically significant data.

I believe that the technic as described by Dr. Reichle is going to add important information to this field.

DR. MARSHALL J. ORLOFF (San Diego): Dr. McDermott's excellent paper deals with three burning issues, regarding the use of portacaval shunt for the treatment of a disease that, without surgical treatment, has a five-year mortality rate of almost 100 per cent. The first of these issues, viewed in the background of the lethality of cirrhosis and bleeding varices, is: What preoperative criteria can be used to select cirrhotic patients with bleeding esophageal varices for portacaval shunt or other legitimate forms of therapy? It is accurate to state that today this problem has still not been resolved. It has been with us ever since the portacaval shunt was devised, yet there are no widely acceptable, clearly identifiable criteria for selection of patients for a portacaval shunt. Certainly, the results of liver function tests and clinical criteria based on examination of the patient do not permit prediction of survival or encephalopathy except at the very extremes of the biochemical and clinical abnormalities in the cirrhosis spectrum. Several years ago Dean Warren and his colleagues proposed before this Association that it might be possible, on the basis of a thorough preoperative hemodynamic evaluation, that included hepatic vein catheterization, pan-angiography and determinations of liver blood flow, to develop criteria for selecting patients for portacaval shunts. This very attractive proposal remains to be tested, and it is essential that it be tested by prospective studies in large numbers of patients. Bill McDermott's work deals with only one facet of the Warren approach to hemodynamic evaluation, namely, the measurement of liver blood flow preoperatively by the disappearance of a radio-colloid from the blood, and it shows no correlation with survival or encephalopathy. This finding, however, is not surprising because, of all of the hemodynamic assessments, it is the one which is fraught with the greatest error in patients who have liver disease.

The second important issue that this paper concerns is: To what extent does diversion of portal blood away from the liver influence liver function and, therefore, survival? This is not an easy issue to resolve, because it involves more than just simply the amount of portal blood diverted. It also involves the amount of hepatic arterial compensation for the diversion of portal blood, and hepatic arterial compensation to some degree invariably follows a portacaval shunt. The problem is, it is very difficult to predict the degree of hepatic arterial compensation.

Again, several years ago, Warren and his colleagues proposed, on the basis of preoperative hemodynamic studies in a small group of patients, that it might be possible to define the influence of blood flow on survival and liver function, and they suggested that patients with a large prograde portal flow, ones who would suffer the greatest diversion of flow by a shunt, would not tolerate a portacaval shunt very well. Conflicting with this logical proposal, Price, Vorrhees and their colleagues, by intra-operative flow measurements, found no correlation between pre-shunt liver blood flow and ultimate survival. Again, this very important issue must be resolved as prospective studies in a large number of patients.

The third issue concerns the influence of diversion of portal flow on the development of encephalopathy. As Bill McDermott points out, this is not a simple issue, since encephalopathy is influenced by both the degree of liver function impairment and the rate at which nitrogen is shunted into the systemic circulation. Unfortunately, to date there are no prospective studies of the incidence of encephalopathy in patients who have undergone shunts.

With this preamble, I would like to show data identical to Dr. McDermott's on a slide.

We have performed hemodynamic evaluations in some 200 unselected cirrhotic patients who have undergone emergency portacaval shunt, and stimulated by Dr. McDermott's abstract, we divided our patients into the same groups that he described. As you can see, in the patients who had *no* diversion of portal flow following the shunt because they had spontaneous reversal of flow, the survival rate was the lowest of all, and the incidence of encephalopathy was quite high. This finding is quite the opposite of what might be predicted by Dr. McDermott's hypothesis.

Similarly, of the patients with prograde portal flow, those who had the smallest diversion of portal blood as a result of the shunt, had the highest incidence of encephalopathy. Lastly, we found no difference in survival between the group with a moderate prograde flow (51–100 mm. difference between FPP and HOPP) and the group with a high prograde portal flow (>100 mm. pressure difference) following portal diversion.

I do not think that Warren's hypothesis and McDermott's support are incorrect, Since their proposal is certainly logical. What I do conclude is that it remains to be tested by prospective studies in a large number of patients. Such studies are badly needed.

DR. WILLIAM V. MCDERMOTT (Closing): I wish to before closing, take a moment to comment on Dr. Drapanas' paper, since I wanted to show our own data before introducing a comment on his.

It is important to note that his patients purposely were all B and C categories by the Child category.

Those of our patients in Group 3 all fell into the B and C category, and in terms of per cent of encephalopathy and long-term survival, they did better over the long haul despite the fact that they were the poorer risks. So Dr. Drapanas' really superb record may be due in part to the inadvertent selection of patients who, albeit have a high operative mortality, once they survive operation, are likely to have a lower incidence of encephalopathy and hepatic deterioration, because they do not have any significant diversion of liver blood flow.

At any rate, that remains to be seen, but it is a possible explanation for these results, rather than the type of shunt itself.

Certainly Dr. Warren's comments, as always, are excellent.

Regarding Dr. Orloff's question—what pre-op criteria would you use?—interestingly enough, the Child category of risk is fine in terms of operative mortality. In our group the B and C group had a 13% mortality; the A group a 2% mortality. But again let me emphasize, once you get through the operative phase, the Group B and C may do much better than many in Group A, because there is no impact on hepatic function in terms of alteration in liver blood flow.

Dr. Reichle presented today is an excellent addition to our repertoire for evaluating hemodynamics, and it is becoming increasingly clear that this is what we need in terms of preoperative criteria, rather than our heretofore standard assessment of liver function by clinical evaluation and by standard tests.