Discussion

Dr. Jere W. Lord, Jr. (New York, New York): About two and a half years ago I had the privilege of discussing the first presentation of Dr. Drapanas. At that time he had 25 patients, and my associates and I also had 25 patients. At the present time he has recorded 80, and we have about 40.

We all agree on all but one point: first, he has proven that the patency of these shunts is of a very high order. This was a worry that concerned us in the beginning. When we first began to use them, we were encouraged by Dr. Foster's stimulating paper some years ago which showed that the prostheses remain patent in experimental animals and also in one human between the portal vein and the vena cava.

Secondly, the mortality rate has been acceptable; lower, perhaps, than the mortality rate of other types of shunts.

Thirdly, I think the timing of the procedure, which he did not have a chance to bring out in the presentation, is important. It is better to control the acute hemorrhage by a Blakemore-Sengstaken tube, or by some other method, and operate on these patients when they are brought into the best possible in all patients, and some have to be operated as an emergency. With this we agree.

Our only point of disagreement is the explanation of the relatively low flow rate through these shunts. He believes that the relatively small fall in pressure, 168 mm of saline in his patients, 165 on the average in ours, of the portal pressure is that the pressure in the vena cava is high enough to lessen the gradient of pressure and lower the flow. I believe another reason may be suitable, and that is that the prosthesis is about 18–20 mm in diameter, the vena cava is of equal or a little larger but that the limiting factor is the superior mesenteric vein, which in our measurements has been between 9 and 11 mm in diameter. This is of about the same order as the splenic vein; and whereas this procedure can be accomplished much more frequently than splenectomy and splenorenal shunt, our own experience with the latter procedure has been an extremely salutory one. Our followup of ten years and longer in a small series of patients operated from 1953 to 1963 showed that 67% live ten years and longer. Recently in the office I saw a patient we did 16 years ago, and one 13 years ago, and both are in excellent health. Therefore, I believe that the size of the mesenteric vein may be the limiting factor in the modest fall of the portal pressure, in contrast to the large portacaval shunt with an end-to-side or a side-to-side anastomosis.

In conclusion, I'd like to congratulate Dr. Drapanas. I believe he has shown on sound physiological and clinical bases that the interposition mesocaval shunt is an excellent one.

Dr. Gardner W. Smith (Baltimore): I would first like to congratulate Dr. Drapanas on what is truly a superb series and superb results. Particularly impressive are his five-year survival statistics, which I don't think have been equalled in any other shunt series.

It is interesting to me that he has also presented some elegant hemodynamic data, and I think that this serves to make one point at least, and that is that even when you have good results with some form of portal-systemic shunt or other, you must explain why hemodynamically. I don't know whether that's a real requirement, or just a continued fetish.

I would like to present briefly the experience we have had at The Johns Hopkins Hospital over the past 2½ years with this same shunt; and this represents primarily the work of Drs. John Cameron, Willis Maddrey and George Zuidema. I do this to make two points, one in agreement and one, perhaps, in discussion.

First, among a very modest series, now only 19 patients, we have had very good risk patients; 16 of them or 84% were Class A or Class B. On the other side of the coin, 12 of the 19 were done either as emergency or semi-emergency shunts; so that only 37% of them were elective. The striking part about these 19 patients is that there has been only one death, or an operative mortality of 5%.

I think this is a very important point. It seems clear that in our hands, as in Dr. Drapanas', this shunt can be done with less risk than the standard portacaval shunts.

On the other hand, among these same patients there have been seven with encephalopathy, three in the acute postoperative phase and four long-term, and two of these patients have died with liver failure. That's a 37% incidence of encephalopathy. And that, indeed, is no improvement over what has been accomplished with the portacaval shunts.

I think we would conclude from this modest experience that this operation can be done more easily than a standard portacaval shunt, and with considerably less risk, and that that is its primary value. We have been unable to demonstrate a decreased incidence of encephalopathy. We have had no opportunity to compare this type of procedure with the innovative one that Dr. Warren has described, the distal splenorenal shunt, and the reason is that we simply don't see patients who are candidates. As I indicated to you, two-thirds of these patients were emergencies, and one of the elective ones had significant ascites. Thus, in 2½ years, even had we wished to compare this with the distal splenorenal shunt, we would have had only six patients who were candidates for that procedure.

Dr. Frederick A. Reichle (Philadelphia): The hemodynamic observations which Dr. Drapanas has reported represent an important contribution to our understanding of the hemodynamic sequelae following mesocaval decompression through a large-bore prosthetic. These data are of special importance because of the favorable clinical response of his patients to H-graft portal decompression.

Hemodynamic alterations caused by iatrogenic portosystemic shunt, superimposed on highly valuable preshunt hemodynamic patterns, are the underlying cause of the metabolic changes of clinical hepatic failure, which is the most common cause of death and morbidity after portosystemic shunt. Thus postshunt metabolic changes may be the result of qualitative or quantitative alterations in the hepatic blood supply.

The relevance of the preoperative and postoperative hemodynamic patterns to the clinical outcome after portosystemic shunt has been emphasized by Dr. Warren, and has also been reported by Dr. McDermott and others. Because of the superior results of operative treatment of patients with portal hypertension which have been reported by Dr. Drapanas, using the mesocaval H-graft, and by Dr. Warren, using the distal splenorenal shunt, we recently began a study of the hemodynamic and metabolic sequelae in randomized patients with these two operations. The method of direct portal flow determination used in this study has been reported previously, and involves the use of intraportal radiopaque water immiscible droplets to determine the flow velocity in the portal vein done together with biplane portography of the portal vein.

Using this technique, which can be done in the unanesthetized and unsedated patient, having gained access to the portal vein by a previously placed umbilical catheter, we have studied a small number of patients before and after Drapanas' shunt. These preliminary results of the differences between pre and postoperative portal blood flow at this time are very comparable to the data that Dr. Drapanas has accumulated by direct operative measurement of flow through the mesocaval H-graft. Whereas these data are highly preliminary, perhaps they are significant only inasmuch as there is a close correlation with Dr. Drapanas' data. As time goes by, such preliminary data is subject to change particularly in view of the highly variable preoperative portal hemodynamic patterns.

In addition to randomizing patients requiring elective shunt into Drapanas H-graft and Warren distal splenorenal shunt, we are currently using the H-graft as the treatment of choice in patients with variceal hemorrhage which we are unable to stop short of operation—that is, for emergency portosystemic shunt—and have been impressed by how well patients frequently come through very severe clinical straits associated with emergency portasystemic shunting.

Inasmuch as there is a 72% survival in five years, I would like to ask whether Dr. Drapanas has had an opportunity to analyze the morphology of the Dacron graft since this is a unique experience in which a prosthetic material is in the functioning venous system for a prolonged period of time. I would also like Dr. Drapanas to speculate on the cause of the increased vena caval pressure which may serve to retard the blood flow through the H-graft shunt.

Dr. Robert B. Smith, III (Decatur, Georgia): We have had an interest in the interposition dacron shunt at Emory, and have used both the mesocaval type described by Dr. Drapanas, and also an innovation, the mesorenal H-graft, as total shunting procedures against which we have randomized Dr. Warren's distal splenorenal shunt in a prospective, controlled series lasting now over 40 months. We have also used the interposition shunt on many occasions when it was not possible to perform the distal shunt because of the presence of persistent ascites of

the nonavailability of the splenic vein for the distal shunting procedure.

Our operative death rate and late death rate with the interposition shunt have been remarkably similar to those reported by Dr. Drapanas, although our followup is not as his.

The one major difference between the Emory experience and that of Dr. Drapanas is that our incidence of post-operative encephalopathy has been much higher with the interposition shunt. We have observed this distressing complication in 36% of our interposition shunt patients. This figure corresponds very closely with our incidence of encephalopathy in other forms of total shunts, and also that reported from other authors with total shunts.

(Slide) This illustrates the Emory experience to date with interposition shunts over the last 40 months: 45 total patients, 22 of whom fell in the randomized study; there were no operative deaths among the randomized patients and five operative deaths among the nonrandomized ones. Many of these were poor risk patients who had emergency procedures. Our late deaths are five among randomized patients, and three more among the total. Again I would point out that the encephalopathy rate with the total shunts by graft interposition is 36% in the whole group, and 32% among the 22 randomized patients.

(Slide) By comparison, this is our experience over the same period of time at Emory with the selective distal splenorenal shunt: 38 patients total, 22 of whom were in the randomized series; no deaths have occurred to date in the randomized series, and only one among nonrandomized distal shunts; late deaths were one in the randomized group, and two more in the total series. Only one patient from the entire group of selective distal shunts has developed postoperative encephalopathy. I would hasten to add that the diagnosis of encephalopathy has not been based entirely on assessment by the operating surgeons, since the majority of these patients have been carefully evaluated by a hepatologist and by a research clinician whose primary interest is in the field of protein chemistry.

In conclusion, I would like to ask Dr. Drapanas if he could describe the criteria that he employs to make the diagnosis of encephalopathy, as that might explain some of our differences.

DR. THEODORE DRAPANAS (Closing discussion): I'd like to tackle, first of all, Dr. Gardner Smith's question about when we do these shunts. We still feel that cirrhosis is a bad disease to have, and that surgery is bad for the liver. Regardless of what type of shunt is performed, it should be performed under nonemergent conditions when the patient is in optimal clinical condition.

We have recently found that a number of surgeons have reserved the mesocaval shunt for the poorer risk patients (those with acute exanguination) massive ascites, convulsing with coma; and I am reminded of a statement often made, that when you operate on dying patients, you get deaths.

Our encephalopathy rate was 35%, in this type of patient operated upon in poor condition as an emergency.

Therefore, we spend a great deal of time in getting these patients into better condition preoperatively. We were successful in this approach with 85% of our patients for whom the operation was done electively. We do not feel, as we have commented upon in the manuscript, that an emergency shunt should be performed except in a few selected good risk patients and we don't see many of the latter in our institution.

Finally, I think it's most important to be aware of the metabolic alterations in these critically ill patients and there are many. Of great importance is the prevention of alkalosis and hypokalemia which tends to predispose to encephalopathy, with or without a shunt. We prefer to place these patients on spironolactaone, an aldosterone antagonist, and also restore potassium ion deficits, which almost all of these patients have.

With good medical care we think we can control encephalopathy. The criteria that we use for the diagnosis of encephalopathy include careful history, physical examination, neurological evaluation, and, finally, arterial blood ammonia levels, which don't always correlate, as we all know well, with ecephalopathy.

Now, I would like to answer Dr. Lord's and Dr. Reichle's questions by showing some additional data from our manuscript. This depicts an electrical analog of the portal system based on our flow hemodynamic studies. The important point to be made relates to R_t, (total portal system resistance) calculated from hemodynamic studies obtained for three different groups of patients, with side-to-side shunts, without shunts, and patients with interposition mesocaval shunts. In the patient with cirrhosis and portal hypertension without a shunt R_t is 0.5 units; with interposition mesocaval shunt, about half of that, 0.23 units. But look what happens to R_t with a side-to-side shunt. It is reduced even further. We think that this is what increases the diversion of flow from the liver.

We do not agree with you, Dr. Lord that it's the function of the size of the mesenteric vein. By simply occluding the vena cava distal to the shunt, thereby removing competitive flow from the legs, while studying flow through the shunt, we find that it doubles.

Perhaps, if I might answer Dr. Frank Spencer's question which he asked me three years ago when we presented our initial studies, now, with this data, I can tell you that one of the reasons we feel the flow is low is that we are shunting into an area of higher vena caval pressure, or higher capacitance in the caval system.

Finally, Dr. Sabiston, if you will permit me to borrow a page from your splendid address this morning, I would like to give a tribute to one man who has, perhaps, stimulated us more than anything else to go into these studies, and that is, of course, our erstwhile Secretary, Dr. Warren, who is being eminently fairminded and totally objective about the matter of shunts, and challenged us last year at The Homestead to look at our studies.