Reply:

We would like to thank Professor Otto and his colleagues for their interest and comments on our paper. We highly appreciate their contribution in validating our novel score predicting complications after liver resection.[1] Although our patient collective was large and internal validation was based on randomly selected cases,[1] external validation is indeed crucial before its wide acceptance. The analysis presented in the letter critically evaluates the applicability of the score in different populations, and thereby may contribute to large-scale adoption for relevant groups of patients.

However we are pleased to learn that they could reproduce the predictive value of this new score, the lower concordance in patients with hepatocellular carcinoma (HCC) is to be expected. Our score targeted only noncirrhotic patients. Patient selection for surgery, perioperative risks and postoperative outcome are different in HCC patients, when compared to patients with other types of tumors, due to concomitant impaired liver function in most cases.[2,3] For this reason, we have excluded cirrhotic patients in the development of the present score.[1] Only 20 of the 615 patients (3%) included in the development of the score presented with HCC, and none had cirrhosis with a maximal fibrosis score of METAVIR 2 (low fibrosis). In the analysis of Professor Otto et al more than 100 patients with HCC were included. Although they might be labelled as noncirrhotic, it is likely that most had viral hepatitis infection and severe fibrosis. Therefore, we agree that the score is not suitable for the population of patients with HCC, unless there is convincing evidence for the absence of significant underlying disease.

Next, the observation in their series that the score applied well to patients with intrahepatic cholangiocarcinoma (ICC) enhanced its validity. Regarding the slightly underestimated complication rates of the score in the intermediate and high risk groups in the series from Mainz, we would like to underline that our score-population only included patients with liver resections performed after 2002, and most after 2006. The series of patients included in the validation study of Professor Otto goes back to 1998. This could be one factor explaining the higher complication rate in the intermediate and high risk score in their series considering the improvements in the outcome in liver surgery over the past decade.[4–6]

We look forward to further validation of the score by other groups in various patient populations with the need for possible adjustments, and thank Professor Otto for the additional information provided.

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Potential Prevention and Treatment of Intestinal Barrier Dysfunction Using Active Components of *Lactobacillus*

To the Editor:

The article entitled "Intestinal barrier dysfunction in a randomized trial of a specific probiotic composition in acute pancreatitis"[1] suggests that although probiotic strains lead to a reduction in overall bacterial translocation, translocation in patients with organ failure is increased. These results are supported by studies from our lab that suggest translocation of probiotic strains increases with extended clinical application, especially in old, weak, immunosuppressed and/or transplant patients.

We think the surface layer protein of *Lactobacillus* is important for mediating the protective effects of this strain. We would like to discuss some of our work on the surface layer protein of *lactobacillus* in the prevention and treatment of intestinal barrier dysfunction.

In the gastrointestinal tract, the symbiotic bacteria *Lactobacilli* play a significant role in maintaining the homeostasis of the gut flora by adhering to and colonizing the intestinal mucosa and competing with pathogenic bacteria. Although probiotic strains can be useful for preventing and treating intestinal barrier dysfunction, their clinical safety needs further study, as translocation of probiotic strains may be a problem during the clinical application of *Lactobacillus*.[2] First of all, probiotics are potentially pathogenic upon translocation through the intestinal barrier and entry into the human blood system, particularly after injury of the intestinal barrier, in immunosuppressed patients or via venous and/or urinary catheters.[3,4] Furthermore, simultaneous use of probiotic strains and antibiotics to prevent antibiotic-associated diarrhea and colitis may lead to the emergence of antibiotic-resistant probiotic strains. Using *Lactobacilli* and antibiotics simultaneously during the treatment of intestinal diseases may limit the beneficial effects of *Lactobacilli*, as these strains can succumb to the antibiotics, too. Developing the active components of *Lactobacillus*, such as the domain surface layer protein, may enable the development of new drugs that can overcome these challenges.[5]

Our previous studies demonstrated that *Lactobacillus plantarum* (LP) was able to prevent colonic damage by enteropathogenic and enteroinvasive *Escherichia coli* as well as prevent inflammation in vitro, in vivo and in patients with acute pancreatitis.[6–9] In our unpublished study, we used molecular techniques to identify a domain of the LP integral membrane protein IMP515-575 (MIMP) that interacts with the intestinal epithelium. Furthermore, we verified the protective function of MIMP on the intestinal barrier in vitro, in vivo, and via interactions with dendritic cells, indicating that MIMP is the main functional component of LP that contributes to its protective effects, and thus may be a potential therapeutic agent for intestinal infective and inflammatory diseases.

Additional studies are currently underway in our lab to investigate the signal transduction pathways that are responsible for

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mediating the protective effects of MIMP. As well, we are preparing to conduct controlled trials of patients with intestinal barrier dysfunctions to compare MIMP versus LP for the treatment of patients, with conditions such as inflammatory bowel disease and acute pancreatitis. We are preparing to conduct this study in patients from several different hospitals and have completed the trial registration. We predict that when combined with antibiotics, the healing effects of MIMP may be higher compared to those of LP. We also predict that sole application of MIMP will yield similar therapeutic effects and have fewer complications, such as the bacteria translocation, in the treatment of intestinal barrier dysfunction.

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Reply:

We appreciate Professor Huanlong Qin and coworkers' response to a paper from our group published in *Annals of Surgery* in 2009.[1] We should like to point out that although the mixture of probiotics in our study did not prevent bacterial translocation, the probiotic bacteria themselves, including 3 Lactobacillus species (*Lactobacillus acidophilus*, *Lactobacillus casei*, *Lactobacillus salivarius*), did not translocate.[2] In their response, Qin et al indicate they have characterized an integral membrane protein from *Lactobacillus plantarum* (IMP515-575) that mediates the protective effects on bacterial translocation.

Furthermore, they announced that they will conduct a clinical study with this protein in patients with IBD and acute pancreatitis. Qin et al[3] earlier published a letter on this subject. We look forward to the outcome of these studies, and especially to see whether the IMP515-575 protein is able to reduce bacterial translocation in critically ill patients.

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Letter to "Rate and Predictability of Graft Rupture and Open Abdominal Aortic Surgery"

To the Editor:

I read with much interest the article titled "Rate and predictability of graft rupture after endovascular and open abdominal aortic aneurysm repair" by T. Wyss et al., which appeared in the November 2010 issue of *Annals of Surgery*.[1] In this study, the authors analyze the causes of graft rupture after endovascular aneurysm repair, reporting 27 cases of graft rupture after endovascular aneurysm repair among 848 patients, with a 30-day mortality of more than 60%. There was no case of rupture among more than 500 patients who underwent open repair.

These patients were collected by the UK investigators and reported a few months earlier in the *New England Journal of Medicine*.[2,3] Reading the article by Wyss et al. and the previous 2 studies (EVAR 1 and EVAR 2), one could assume that "Endovascular repair of abdominal aortic aneurysm should be reconsidered."

The vascular community should thank the EVAR UK participants for their efforts in trying to clarify many of the doubts around endovascular surgery for abdominal aortic aneurysm.

In the EVAR 1 study, 1252 were randomized to the 2 forms of treatment and there were 25 cases of rupture among patients who received endovascular aneurysm repair. Despite this fact, the cumulative survival at 8 years was similar between the 2 groups of patients.

Clearly, the endovascular technique is valid and will remain for the simple reason that the newly developed technique (with all the problems related to its recent birth) was compared with a 70-year-old technique like open repair first reported by Professor Dubost in the early fifties in France, and still, the 2 techniques gave similar results in terms of late survival. The endovascular technique was more costly mainly because of a significant number of endovascular reinterventions.

The reason endovascular surgery is very attractive is because it comports a small trauma to the patient, but were the patients themselves asked if they were satisfied or not

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