
24/41/111750

Reply

We thank Dr Mower and Dr Quinones for their interest in our article. In their comment they express their concern about several issues.

As they correctly pointed out, the correlation coefficient is not useful for detecting differences in pressure. For this reason, we also compared the pressure measurements in the radial artery and in the thrombus mass by using a Bland and Altman plot.1 This plot is very useful for showing the level of agreement between both pressure measurements. Therefore, in our opinion, the conclusion that the thrombus does not reduce pressure to the aneurysmal wall is justified.

The measurement of the systemic pressure in the radial artery will lead to a higher systolic pressure and a lower diastolic pressure compared with the pressure measurements in the abdominal aorta.2,3 The radial measurements will have a wider pulse pressure, but the mean pressure will not change significantly compared with the pressure in the abdominal aorta.3 If we would compensate the fact that the radial pulse pressure is wider than the aortic pulse pressure, the level of agreement between the arterial pulse pressure and the pulse pressure in the thrombus would probably be even greater. Although claimed by Dr Mower and Dr Quinones, the reference quoted by them4 does not mention the level of change in mean pressure and pulse pressure between the abdominal aorta and arteries in the extremities. This article shows the change in pulse wave from the ascending aorta to the iliac arteries in nine patients, a number equal to our patient number, but claimed to be too small to draw conclusions. However, we do agree about the indistinct effect of thrombus on the aneurysmal wall.

In case of aneurysmal growth and rupture, the pressure on the aneurysmal wall is only one of the main responsible factors. By the performance of direct pressure measurements just inside the aneurysmal wall, a protective effect of thrombus by lowering the mean and pulse pressure on the aneurysmal wall in a clinically significant way could not be documented in patients.

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REFERENCES


24/41/111749

Regarding “Acute enlargement and subsequent rupture of an abdominal aortic aneurysm in a patient receiving chemotherapy for pancreatic carcinoma”

To the Editors:

We read with keen interest the article by Palm et al (J Vasc Surg 2000;32:179-200) regarding the acute enlargement and rupture of an abdominal aortic aneurysm in a patient receiving chemotherapy for stage IV pancreatic carcinoma. A life expectancy of less than 2 years is considered a relative contraindication to repair of a an abdominal aortic aneurysm.1 The clinicians wisely decided not to repair the abdominal aortic aneurysm at the initial operation since the patient was also found to have incurable stage IV pancreatic carcinoma, which carries a median survival of less than 6 months.2 Even when resection is possible, the survival is less than 1 year.3 Moreover, patients with limited life expectancy usually die of their other medical problems rather than rupture.4 Thus, nonoperative management was medically and ethically sound.

The authors did not tell us why they allowed the aneurysm to grow to 7.1 cm and rupture before performing an emergent aneurysmorrhaphy. Certainly, the patient was an even worse candidate for surgical intervention at the time of his rupture than perhaps at his initial surgical exploration in January 1998. Perhaps the authors could explain to us their discussions with this patient and his family regarding death and dying. The resolution of such end-of-life issues may be more important than any surgical treatment that could be offered.5

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REFERENCES


24/41/113300

Reply

I appreciate the comment regarding our article, “Acute enlargement and subsequent rupture of an abdominal aortic aneurysm in a patient receiving chemotherapy for pancreatic carcinoma” (J Vasc Surg 2000;32:197-200). I also agree that the management of this patient was both medically and ethically...