Correspondence

Predictors of Outcome in Asymptomatic Aortic Stenosis

To the Editor: In their prospective study of patients with asymptomatic aortic stenosis, Rosenhek et al. (Aug. 31 issue) note that the rate of progression of aortic-jet velocity was significantly higher in patients who underwent aortic-valve replacement or died from cardiac causes. Others have shown similar results.

However, the rate of change in the aortic-jet velocity might in itself influence the decision to perform surgery. Physicians may refer patients for valve replacement after observing an increase in aortic-jet velocity because they believe that the increase indicates a worsening prognosis. This approach is supported by the guidelines of the American Heart Association, which include severe asymptomatic aortic stenosis as a class IIb indication for valve replacement.

The British Cardiac Society recommends surgery both in the presence of symptoms and if the gradient is particularly high. In the prospective study by Pellikka et al., 18 of 143 asymptomatic patients (13 percent) underwent valve replacement because it was thought that the severity of stenosis itself warranted intervention.

Alternatively, physicians might be more inclined to interpret mild, nonspecific breathlessness as overt exercise limitation if the aortic-jet velocity is high. We recently reviewed a series of patients with severe aortic stenosis and mild breathlessness (with a score of 2 on the Specific Activity Scale). In those referred for aortic-valve replacement, the average peak velocity was 4.4 m per second. However, despite similar symptoms, other patients were being treated conservatively, and in these the average peak velocity was only 3.9 m per second.

It would therefore be an advantage to have an objective measure of functional capacity. Treadmill exercise testing has been shown to be safe in patients without overt severe symptoms. A decline in exercise time may be a more useful predictor of the onset of symptoms than an increase in peak aortic-jet velocity alone, and we suggest that this possibility should be investigated prospectively.

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pertrophy, and an aortic-valve area of less than 0.6 cm² or an aortic-valve index no higher than 0.6 cm² per square meter of body-surface area). 2 However, none of these echocardiographic findings have demonstrated the predictive power that Rosenhek et al. report for the degree of valvular calcification and the increase in aortic-jet velocity. Their results add to the existing evidence that an absence of symptoms is an insufficient reason to delay aortic-valve replacement in certain cases. The findings also shed light on the matter of which patients should be considered for early surgery.

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The authors reply:

To the Editor: We appreciate the interest of Drs. Das and Chambers in our study of patients with asymptomatic but hemodynamically severe aortic stenosis. Our data suggest that the echocardiographically determined extent of calcification of the valve and the observed rate of hemodynamic progression permit the early identification of patients at risk who should undergo surgery despite the absence of symptoms. As pointed out in our article, the patients who were followed and treated conservatively underwent surgery only when symptoms developed, not because of hemodynamic progression or a certain degree of hemodynamic severity.

The current American College of Cardiology–American Heart Association guidelines 1 do not support the approach of operating on asymptomatic patients just because their stenosis is severe. According to these guidelines, valve replacement is definitely recommended in symptomatic patients only (those with a class I indication) and is probably indicated in asymptomatic patients with impaired left ventricular function or abnormal exercise response (class IIa).

Although there is some controversy surrounding the matter, these guidelines state that evidence is lacking to support surgery in asymptomatic patients solely because hypertrophy is severe or because the aortic-valve area is smaller than 0.6 cm² (a class IIb indication). The rate of progression is not mentioned at all. Thus, in a patient with an increase in aortic-jet velocity whose aortic-valve area is still at least 0.6 cm², the guidelines would not recommend surgery. As discussed in detail in our article, we agree with Das and Chambers that it may be difficult to distinguish between asymptomatic and mildly symptomatic patients. Finally, we also agree that exercise testing plays an important part in the treatment of asymptomatic patients. Nevertheless, there are few data addressing this issue.

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The editorialist replies:

To the Editor: The primary factors determining the optimal timing of valve replacement for severe aortic stenosis are the relative clinical outcomes with and without surgical intervention. The observations by Rosenhek et al. that moderate-to-severe valve calcification and a rapid increase in aortic-jet velocity are associated with a higher rate of onset of symptoms are helpful in ensuring that patients with these conditions receive close clinical and echocardiographic follow-up and are educated about the expected course of disease and the probable need for valve surgery. However, one cannot extrapolate from these data the suggestion that valve replacement should be performed before the onset of symptoms in these patients.

In the absence of symptoms, the risk of sudden death is very low (less than 1 percent per year), even with severe stenosis, as confirmed by the occurrence of only 1 sudden death among 128 patients during more than 2 years in our prospective study. In contrast, aortic-valve replacement is associated with an operative mortality of 2 to 10 percent. 1 In addition, there is no ideal substitute for the valve: the hemodynamics after valve replacement are suboptimal as compared with those associated with a normal native valve; complications occur at a rate of 2 to 3 percent per year after surgery; and it is estimated that the risk of death due to a prosthetic valve is about 1 percent per year. 2-4

The American College of Cardiology–American Heart Association guidelines for the treatment of patients with valvular heart disease should continue to serve as the benchmark for the optimal care of adults with severe aortic stenosis. 5 The only class I indications for valve replacement — those for which there is evidence or general agreement (or both) that the procedure is useful and effective — are severe symptomatic stenosis and severe stenosis in patients who are undergoing coronary-artery bypass grafting, aortic-root surgery, or other valve surgery. The evidence also favors valve replacement in patients with moderate aortic stenosis who are undergoing other cardiac surgery and for asymptomatic patients with severe stenosis and left ventricular systolic dysfunction or exertional hypotension. In other asymptomatic adults with severe stenosis, we should defer valve surgery until symptoms occur, because we can then be certain that our recommendation is associated with improved clinical outcomes. After all, it is difficult to make an asymptomatic patient feel better.

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4. Task Force on Practice Guidelines (Committee on Management of Pa-
Interferon Beta-1a during a First Demyelinating Event

To the Editor: Jacobs et al. (Sept. 28 issue) suggest that interferon beta-1a therapy for some patients with a first clinically isolated syndrome of multiple sclerosis may delay the onset of clinically definite multiple sclerosis. The diagnosis of multiple sclerosis has long been based on dissemination in both space and time. If this approach still holds, can a valid assessment of a potential disorder be made before the appearance of the symptoms that define it? I find this study troubling for several reasons.

Optic neuritis is easy to identify, but no criteria are given for "incomplete transverse myelitis." Is it paraparesis, numbness of one arm, or gait ataxia? The term "brain-stem syndrome" is also vague: is this diplopia, facial weakness, or dysphagia? The criteria for findings on magnetic resonance imaging (MRI) are not helpful, since many conditions mimic multiple sclerosis on MRI. Unfortunately, a radiologist’s interpretation of the MRI scan is often the exclusive basis for the diagnosis of multiple sclerosis. In a previous study, this resulted in a 35 percent incidence of erroneous diagnoses, leading to interferon therapy for patients with disseminated encephalomyelitis and chronic fatigue syndrome.

The study by Jacobs et al. did not address the clinical import of a second bout of multiple sclerosis. In my more than 40 years of experience, the majority of patients with multiple sclerosis had benign disease, even after many years. In only a handful of patients was the second episode devastating or did it fail to respond to corticosteroid therapy. O’Riordan et al. noted that after 10 years, three fourths of patients with fewer than 10 lesions on magnetic resonance imaging at study entry had benign disease (score on the Expanded Disability Status Scale, ≤ 3). They waited six months after the clinically isolated syndrome in order to rule out disseminated encephalomyelitis, whereas Jacobs et al. chose to wait only one month. The lack of data on functional status and the length of time to clinically definite multiple sclerosis in the study by Jacobs et al. make its clinical applicability doubtful.

The lack of correlation between the number, size, and location of areas of increased signal intensity on T2-weighted MRI and the patients’ clinical status militates against the validity of the popular concept of burden of disease. Its clinical significance may be similar to that of the optic atrophy and delayed visual evoked responses of some patients with multiple sclerosis who have perfectly normal vision.

Follow-up studies of clinically isolated syndromes of multiple sclerosis suggest that about half the patients have progression to clinically definite multiple sclerosis. It would seem more prudent to delay treatment with disease-modifying drugs until the familiar criteria of dissemination in time and space are fulfilled.

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To the Editor: The article by Jacobs et al. suggests that the early treatment of a first demyelinating episode with intramuscular interferon beta-1a, when the episode is accompanied by at least two clinically silent lesions on MRI scanning, may be beneficial. This may have important implications, given the expense of the treatment and the current lack of data on the optimal duration of treatment. Before such a policy is applied as part of routine neurologic practice, all available evidence relating to the natural history of isolated demyelinating episodes needs careful review.

The placebo group in the study by Jacobs et al. may not be entirely representative of the natural history of multiple sclerosis. Jacobs et al. quote the findings of the Optic Neuritis Treatment Trial, but in that study the risk of development of clinically definite multiple sclerosis after three years among patients with isolated optic neuritis and three or more clinically silent lesions on MRI was only 38 percent. This figure is very close to the 35 percent probability of progression to clinically definite multiple sclerosis in the group treated with interferon beta-1a in the present study but below that in the placebo group, suggesting that the placebo group had more aggressive disease than is usually seen in clinical practice. Furthermore, the dropout rate in both groups was about 15 percent, and this may have influenced the interpretation of the results. I believe that this study was prematurely terminated. There is no evidence that the treatment effect was becoming more pronounced with the passage of time.

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The authors reply:

To the Editor: To be included in our study, patients had to have symptoms, objective clinical signs, and clinically silent lesions characteristic of multiple sclerosis of the brain on MRI. This combination of clinical and MRI findings is highly specific for multiple sclerosis. During follow-up, a causal diagnosis other than multiple sclerosis was made in only 1 of our 383 patients (a cerebellar infarct diagnosed after one month). We believe that these patients already had pathological multiple sclerosis and are at high risk for a second clinical event, which will fulfill the criteria for clinically definite multiple sclerosis. Clinically definite multiple sclerosis is diagnosed in more than 50 percent of such patients within the passage of time.
3 to 5 years and in more than 80 percent within 10 years.1 In many patients the course is not benign. In one study, after 10 years, 44 percent of patients had moderate disability (score on the Expanded Disability Status Scale, 2-3), and approximately 25 percent already had secondary chronic progressive multiple sclerosis. The number and volume of lesions on T2-weighted MRI of the brain at the time of and after the initial demyelinating event have been shown to be predictive of long-term disability.1,2

In patients who meet the eligibility criteria used in our study, we believe it is appropriate to make a definitive diagnosis of multiple sclerosis and to consider early treatment with interferon beta-1a, an agent with proven efficacy for preventing or delaying subclinical, “smoldering” multiple sclerosis, which can cause inflammation, scarring, axonopathy, atrophy, wallerian degeneration, and cognitive decline, even early in the course of the disease.3,4

The three-year rate of clinically definite multiple sclerosis in our study cannot be directly compared with that of the Optic Neuritis Treatment Trial,5 because the criteria for and the ascertainment of clinically definite multiple sclerosis differed in the two studies. In designing our trial, we extrapolated from the results of the Optic Neuritis Treatment Trial and other published data to project a three-year rate of clinically definite multiple sclerosis in the placebo group of 50 percent, which is what we found. The number of patients who withdrew early from the study was approximately what we had projected, and there was no indication that the withdrawals affected the observed treatment effect.

The trial was stopped early on the recommendation of an independent data and safety monitoring committee. Valuable information could have been gained from continued accrual of data, but the interim analysis so clearly supported the benefit of treatment with respect to both clinical and MRI measures of outcome that continued use of a placebo group was no longer ethically justified.

We agree that for patients in whom the diagnosis of multiple sclerosis is equivocal, interferon beta-1a therapy should not be started. However, when the diagnosis appears unequivocal, as in our patients, initiation of interferon beta-1a treatment at the time of the first clinical demyelinating event is justified and appears to be cost effective.6

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Thromboangitis Obliterans (Buerger’s Disease)

To the Editor: As Olin points out in his review (Sept. 21 issue),1 exposure to tobacco plays a central part in the initiation and progression of thromboangiitis obliterans (Buerger’s disease). However, the cause of Buerger’s disease remains unknown. Not all patients with the disease smoke or use smokeless tobacco, and in some patients who stop smoking the disease is still progressive. Dr. Olin does not mention hyperhomocysteinemia, which may have an important and nicotine-independent role in the pathogenesis of Buerger’s disease. Hyperhomocysteinemia is known to be an important risk factor in the early onset of atherosclerotic occlusive disease and deep-vein thrombosis.2,3 In our study, homocysteine levels increased (to at least 31 nmol per milliliter) four hours after methionine loading (0.1 g per kilogram of body weight) in 60 percent of the patients with active Buerger’s disease (9 of 15) but in none of the 15 matched healthy smokers and 15 nonsmokers (P=0.01).4 We confirmed these results in a new investigation in which 55 percent of the patients with Buerger’s disease (11 of 20) had increased homocysteine levels after methionine loading, as compared with 24 percent of the smokers (5 of 21) and 19 percent of the nonsmokers (4 of 21) without vascular disease (P=0.01 and P=0.04, respectively).

There are several pathophysiological mechanisms that may link hyperhomocysteinemia to Buerger’s disease. In vitro studies suggest that homocysteine limits the bioavailability of nitric oxide, impairs endothelium-dependent vasorelaxation, increases oxidative stress, stimulates smooth-muscle-cell proliferation, alters the elastic properties of vessel walls, and generates a prothrombotic state through the activation of factor V.5

We favor screening for hyperhomocysteinemia in patients with thromboangiitis obliterans, especially those with a progressive course. Since hyperhomocysteinemia is easily reversed by pyridoxine and folate acid supplementation, screening for and treatment of hyperhomocysteinemia may be an important therapeutic strategy for patients with Buerger’s disease.

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Dr. Olin replies:

To the Editor: Drs. Diehm and Stammler state that not all patients with thromboangiitis obliterans (Buerger’s disease) smoke or use tobacco. I have yet to see a well-documented case of thromboangiitis obliterans in a person who did not use tobacco in some form. It is very easy to document tobacco use by measuring nicotine or cotinine levels in the urine. Lie described one case of pathologically confirmed Buerger’s disease affecting the upper limbs of a 62-year-old man who had allegedly discontinued smoking 15 years earlier. However, this report did not contain measurements of urinary nicotine and cotinine levels or of carboxyhemoglobin levels; therefore, it is possible that the man was still smoking.

My colleagues and I have reported on the measurement of plasma homocysteine in patients with Buerger’s disease, and we agree with Diehm and Stammler that it may have some role in the pathophysiology of thromboangiitis obliterans. Plasma homocysteine levels were measured in nine patients (22.5 percent of the population with thromboangiitis obliterans at our institution from 1988 through 1996). The values were elevated (to more than 12.9 µmol per liter) in five patients and were normal in four patients. Four of the five patients with elevated homocysteine levels underwent amputation; all were active smokers. None of the four patients with normal homocysteine levels underwent amputation. Of the five patients with elevated homocysteine levels, two had superficial venous thrombosis and one had deep venous thrombosis. Elevated plasma homocysteine levels may be important in determining which patients will eventually require amputation, but a prospective study is needed to confirm this hypothesis.

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The Diversity of T Cells

To the Editor: The excellent review by von Andriand Mackay (Oct. 5 issue) outlines the challenges faced by the lymphoid system when it responds to a diverse array of pathogens. In their introduction the authors restate a common misconception about the magnitude of the T-cell response to individual pathogens, noting that “the number of cells whose T-cell receptors recognize any individual antigen is very limited (several thousand at most).” Historical estimates of the frequencies of precursor T cells specific for recall antigens (e.g., influenza), derived primarily from limiting-dilution assays and analogous techniques, have been consistent with such an estimate.

These estimates are in sharp contrast to the estimated frequency of antigen-specific T cells in the setting of certain human and murine viral infections. With the use of cytokine flow-cytometric studies to evaluate functional CD4+ T-cell responses, the frequencies of cytomegalovirus-specific cells in healthy subjects and in patients infected with the human immunodeficiency virus type 1 (HIV-1) have been found to range from 1 in 100 to 1 in 10 CD4+ T cells, even in the absence of detectable viremia. Likewise, analysis of the T-cell response to Epstein–Barr virus in humans and of the T-cell response to lymphocytic choriomeningitis virus in mice, in studies using major-histocompatibility-complex (MHC) class I tetramers to enumerate antigen-specific CD8+ T cells, showed that the frequencies of antigen-specific T cells were as high as 40 percent of peripheral-blood CD8+ T cells in the setting of acute infection. High frequencies of antigen-specific CD8+ T cells (>1 in 100) have also been reported during chronic infection with these viruses. On the basis of these estimates, the total number of T cells that respond to human herpesvirus may well be more than several million, rather than several thousand, as von Andriand Mackay suggest.

The authors ask, how is it that a relatively small number of antigen-specific T cells can locate and eliminate remotely scattered targets over a broad territory? Their question is no less important in the light of these revised estimates. Another challenge facing those studying T-cell immunity is to understand why memory T cells specific for other viruses (e.g., HIV-1) and cancers appear to be much more rare than those that respond to cytomegalovirus and Epstein–Barr virus. An answer to this question may provide insights into both the pathogenesis of these diseases and the use of vaccines to prevent them.

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The authors reply:

To the Editor: Komanduri and McCune comment on a statement we made in discussing the diversity of “T cells that have never encountered antigen, referred to as naïve T cells.” We arrived at the number of such cells that bear any particular T-cell receptor on the basis of the estimate that about half the 5×1011 lymphocytes in adults are T cells. About 70 percent are naïve, but this fraction is much smaller in older persons. The most common T-cell receptor, consisting of α/β chains, is found on approximately 90 percent of all T cells. Thus, we estimate that the naïve α/β T-cell pool in adults contains approximately 1.5×1010 cells. There are at least 25×109 different α/β T-cell receptors on naïve T cells in human peripheral blood. On average, this amounts to 6000 naïve T cells at most for each α/β chain.
T-cell receptor. Although the size of individual clones may vary, the number of cells that actively recirculate at any point in time may be smaller, since 98 percent of all lymphocytes are sequestered in tissues. Studies using MHC tetramers and T-cell receptor sequencing in nonimmune mice have shown that the upper limit for the number of T-cell precursors in the spleen is 200, a finding that is consistent with our estimate in humans.

Komanduri and McCune correctly point out that the frequency of antigen-reactive T cells increases dramatically during and after viral infections. Indeed, clonal expansion of antigen-primed T cells is a fundamental tenet of immunity and immunologic memory. This concept is entirely consistent with one of the central points in our review—that T-cell migration changes after antigen priming. T-cell expansion is accompanied by differentiation into effector and memory cells and the adoption of new pathways for migration and positioning, allowing for immunologic protection at sites such as epithelial surfaces, where pathogens are first encountered. Epithelial tissues cover an enormous surface area, which cannot be efficiently surveyed by small numbers of antigen-specific lymphocytes in the pool of naive T cells; this feat can be accomplished only with a greatly increased frequency of antigen-specific T cells, such as the frequencies noted by Komanduri and McCune. As they point out, the degree of clonal expansion can vary, depending on the nature of the pathogen. This variability is indeed intriguing but does not bear directly on our model of T-cell migration and functional responses.

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Wireless-Capsule Diagnostic Endoscopy for Recurrent Small-Bowel Bleeding

To the Editor: We used wireless-capsule endoscopy³ to assess patients with obscure or uncontrolled gastrointestinal bleeding. The capsule endoscope contains a miniature video camera, a light source, batteries, and a radio transmitter (Fig. 1). Video images are transmitted by means of radio telemetry to aerials taped to the body that allow images to be captured. The strength of the signal is used to calculate the position of the capsule in the body. Moving images from a period as long as six hours are stored on a portable recorder. With the approval of the ethics committee, four patients swallowed the device. We present here the first images of pathologic conditions in the human small bowel we obtained using this new endoscopic system (Given Imaging, Yoqneam, Israel).

Figure 1. Capsule Endoscope (11 mm × 26 mm).
The camera lens and four white-light-emitting diodes can be seen beneath the transparent optical dome.

Patient 1 was a 60-year-old woman with hereditary hemorrhagic telangiectasia who required the transfusion of 12 to 15 units of blood per year. Treatment with estrogens and push enteroscopy with bipolar electrocoagulation of bleeding lesions reduced the requirement to approximately 4 units per year. Capsule endoscopy revealed angiodysplasias in the stomach, duodenum, and proximal jejunum. Two large, actively bleeding lesions were seen in the jejunum and ileum. Surgery was considered, but the bleeding stopped spontaneously.

Patient 2 was a 39-year-old man with hereditary hemorrhagic telangiectasia who required transfusion with 10 units of blood every two months and was unresponsive to hormonal therapy and to endoscopic treatment. Capsule endoscopy revealed eight angiodysplasias in the duodenum and proximal jejunum. No lesions were seen in the distal small bowel. Good views of the large bowel to the sigmoid colon revealed three angiodysplasias that had not been seen during recent colonoscopy. Intraoperative enteroscopy was deferred, and further treatment of the colonic and upper gastrointestinal angiodysplasias was recommended.

Patient 3 was a 16-year-old boy with melena and a previous intestinal hemorrhage at the age of 2. His hemoglobin level was 9 g per deciliter with normal results on push enteroscopy, colonoscopy, and Meckel scanning. Capsule endoscopy revealed no abnormality. The proposed intraoperative enteroscopy was averted.

Patient 4 was a 78-year-old man with multiple gastrointestinal angiodysplasias who required transfusion with 72 units of blood per year. Treatment with bipolar electrocoagulation of angiodysplasias reduced his transfusion requirement to 36 units per year. The patient had undergone gastroenterostomy and vagotomy for duodenal ulcer in 1955. Capsule endoscopy revealed multiple angiodysplasias affect-
ing only the proximal small bowel. The results of previous surgery did not hinder the movement of the capsule. Continued treatment of the proximal angiodysplasias was recommended.

In general, capsule endoscopy provided good views from mouth to colon and successfully imaged small-bowel pathologic features (Fig. 2). All four patients described the capsule as easy to swallow, painless, and preferable to conventional endoscopy. The information gained was helpful in directing further treatment in these patients. Although this technology cannot be used for biopsy or therapy, it may prove valuable in the assessment of bleeding in patients with negative results on gastroscopy and colonoscopy.

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Editor’s note: Dr. Swain has a consulting agreement with Given Imaging, which is making the capsule. Dr. Glukhovsky is an engineer responsible for research and development of the capsule. Dr. Appleyard is a research fellow in receipt of a grant from the Special Trustees of the Royal London Hospital.


Abiotrophia Species Bacteremia and a Mycotic Aneurysm in an Intravenous Drug Abuser

To the Editor: Recently there have been reports of unexplained sepsis and deaths among intravenous drug abusers in Scotland, Ireland, and England. Approximately one quarter of these cases have been associated with clostridium species, of which half were identified as Clostridium novyi. We report a case of bacteremia in an intravenous drug abuser who developed a mycotic aneurysm due to another unusual, fastidious organism.

A 42-year-old man presented with a three-day history of fever and soft-tissue swelling in his right upper arm. The patient was an intravenous drug abuser. Four days earlier he had crushed a lorazepam tablet, mixed it with tap water, and injected it into his right antecubital vein. Over the next 72 hours, fever developed, with swelling and erythema around the site of injection, which he incised and drained himself. He continued to have fever and swelling of his arm and came to the hospital.

His temperature was 38.6°C. A 3-by-4-cm area of erythema and swelling with drainage was present in the right antecubital fossa. The physical examination was otherwise unremarkable. The white-cell count was 18,000 per cubic millimeter (11 percent band forms, 70 percent neutrophils, 15 percent monocytes, and 4 percent lymphocytes). A computed tomographic scan of the patient’s arm revealed soft-tissue swelling, with no evidence of an underlying aneurysm or abscess. He was admitted, and treatment with ampicillin–sulbactam was initiated.

Blood cultures at the time of admission grew abiotrophia species (formerly known as nutritionally variant streptococ-
cus) from the anaerobic bottles of two separate cultures at 72 hours. Transesophageal echocardiography revealed no vegetations on the heart valves. The antibiotic regimen was changed to ampicillin and gentamicin.

On the 15th hospital day, a pulsatile mass was noted in the right antecubital fossa. A Doppler ultrasound study confirmed the presence of a pseudoaneurysm in the right brachial artery (Fig. 1), which was ligated. Intraoperative cultures and repeated blood cultures were negative.

In a recent review of abscesses and cellulitis among intravenous drug abusers, Binswanger et al. reported a high rate of abscesses in this population (68 percent), and 27 percent of those with abscesses reported that they had incised and drained their own abscesses. Most soft-tissue abscesses and associated bacteremia in intravenous drug abusers are caused by staphylococci (Staphylococcus aureus) or streptococci (Streptococcus viridans or group A streptococci). In a large review of cases of bacteremia in intravenous drug abusers, mycotic aneurysms, pseudoaneurysms that develop when an extravascular hematoma communicates with the intravascular space, occurred in 9 percent of the cases. Bacteremia involving abiotrophia species in intravenous drug abusers appears to be unusual, but given the fastidious nature of this organism, the incidence may be underreported.

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