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Antithrombotic Therapy To Prevent Stroke in Patients with Atrial Fibrillation

To the Editor: I would like to note two methodologic caveats regarding Hart and colleagues' admirable systematic review (1). First, the authors describe the SPAF I (Stroke Prevention in Atrial Fibrillation Study), AFASAK (Copenhagen Atrial Fibrillation, Aspirin, and Anticoagulation Study), and EAFT (European Atrial Fibrillation Trial) as comparing anticoagulants to "placebo." This is misleading. The placebo in these trials was an aspirin placebo (these trials also tested aspirin), not a warfarin placebo. None of the main potential benefits of using placebo (for example, reduced bias because of blinding) accrued to these trials' test of anticoagulants. In contrast, the SPINAF (Stroke Prevention in Nonrheumatic Atrial Fibrillation) trial was truly blinded. Blinding or no, the large effect of anticoagulation was seen across all the trials.

Second, Hart and colleagues chose not to provide a single summary estimate of effect if results were heterogeneous across trials, a rigorous position. However, with regard to aspirin's effect, the crucial heterogeneity is not seen across trials but within one trial, SPAF I. This trial included two randomized studies of aspirin that had separate entry criteria. The results were remarkably and statistically significantly heterogeneous: a 94% relative risk reduction in the group 1 trial compared with an 8% relative risk reduction in the group 2 trial ($P < 0.001$, test for homogeneity of odds ratio) (2). The results of the group 2 trial were consistent with those of the other aspirin trials. In their primary report, the SPAF I investigators provided a single pooled estimate of aspirin's effect, a 42% relative risk reduction (3). It seems appropriate in a formal meta-analysis to quantitatively address the heterogeneity of the aspirin results in SPAF I. If the results of group 1 in SPAF I are left out because of heterogeneity, or if they are included with an expanded confidence interval reflecting a random-effects model, aspirin's effect becomes clearly not statistically significant. Hart and coworkers highlight many problems in the data supporting aspirin's effect in atrial fibrillation, but they should more directly address heterogeneity in the SPAF I trials of aspirin. After all, SPAF I remains the primary source of enthusiasm for aspirin's use in atrial fibrillation.

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In response: Thanks to Dr. Singer for pointing out a misleading heading in Table 2 of our meta-analysis: "Control group" was inadvertently changed to "placebo group" in the process of revision.

Dr. Singer's lack of enthusiasm for aspirin use in atrial fibrillation is long-standing and often voiced (1, 2). His current concerns appear based on post hoc exploratory subgroup analyses of SPAF I, which are not relevant to our meta-analysis. To date, all six randomized trials (five of them double-blind) have shown trends toward reduction in stroke with aspirin use in patients with atrial fibrillation (see the Figure in our meta-analysis). Furthermore, randomized trials comparing warfarin with aspirin have consistently shown a smaller magnitude of stroke reduction than seen in trials comparing warfarin with control or placebo. In short, it seems overwhelmingly likely that aspirin provides some protection against stroke in atrial fibrillation, but the efficacy is clearly much less than that provided by adjusted-dose warfarin, particularly when considering disabling cardioembolic strokes.

It is also clear that many patients with atrial fibrillation who are treated with aspirin have low rates of stroke (whether in part due to or, per Dr. Singer, despite aspirin use) and would not benefit substantially from alternative treatment with warfarin (3-5). The salient clinical issue for the next millennium is not whether aspirin works in atrial fibrillation (it does, but only a little) but rather how to reliably identify individual patients with atrial fibrillation who have relatively low risks for stroke during aspirin therapy and who might not choose anticoagulation once the modest benefits, risks, and disutility are made clear (5).

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5. Patients with nonvalvular atrial fibrillation at low risk of stroke during

Chronic Infection and Coronary Risk

To the Editor: Ridker and colleagues (1) conclude that "little evidence was found that previous infection, as measured by IgG antibody titers to *C. pneumoniae*. . . , is associated with subsequent risk for cardiovascular disease." This conclusion was based on a 3-year follow-up of 17 women with antibody titers of 1:64 or greater, only 5 of whom had titers of 1:128 or greater (the authors' Table 1). The adjusted rate ratio for cardiovascular events (acute myocardial infarction, stroke, or coronary revascularization) in these 17 women was similar to that in antibody-negative controls (titers \geq 64 are considered positive according to 1999 data from Mayo Medical Laboratories).

This study has three major problems. First, the relatively low titers may indicate past exposure rather than chronic active infection with *C. pneumoniae*, in which titers of 1:256 to 1:512 (or higher) are common. This is even more problematic among women in whom pelvic inflammatory disease due to *C. trachomatis* (with antibody cross-reactivity against *C. pneumoniae*) is relatively common. Thus, as observed by the authors (their Figure 1), low IgG antibody titers would not be associated with elevated C-reactive protein levels, a nonspecific marker of active inflammatory processes. Second, none of the major coronary risk factors, individually or jointly, is likely to cause coronary artery disease in a short (3-year) period. Chronic active infection with *C. pneumoniae* is no exception. Third, the authors have used major end points as surrogates for cardiovascular disease. These end points are not interchangeable with smoldering or "subclinical" coronary artery disease, atherosclerosis in general, or the atherogenicity of various coronary risk factors.

The failure to observe a significant increase in cardiovascular end points during a 3-year follow-up of 17 women cannot be used to dismiss the atherogenicity of any major coronary risk factors, including chronic active infection with *C. pneumoniae*.

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Reference

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Elevated Ambulatory and Normal Office Blood Pressure

To the Editor: I read with interest Liu and colleagues' article on elevated ambulatory and normal office blood pressure (1).

Their finding of "white coat normotension" can be explained quite simply by their practice of restricting the clinic blood pressure to the one obtained after the patient has been in the supine position for 10 to 15 minutes.

The classic "postural reflex" results in an increase in diastolic pressure in many patients in the erect position. In my 40 years of experience, this elevated diastolic pressure is the earliest manifestation of hypertension and is the pressure most sensitive to an increase in salt intake or to stress-related hypertension. Thus, a 24-hour ambulatory pressure would identify patients with "early" hypertension, which the (supine) clinic measurement would miss.

In my office, blood pressure is measured in the erect position immediately after the patient walks in from the waiting area. A second pressure is taken immediately after the patient lies supine, and a third pressure is measured after the patient has lain supine for about 5 minutes. This spectrum of blood pressure will usually indicate the true blood pressure status of the patient and for most cases allows therapeutic decisions to be made without

the expense of ambulatory pressure monitoring. Normalization of blood pressure in the erect as well as the supine position can thus be achieved.

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Reference

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In response: Dr. Goldman raises an interesting point about the clinical utility of postural changes in blood pressure. As he is no doubt aware, measuring blood pressure in the standing position is not a routine part of the clinical examination, but perhaps it should be. The current U.S. guidelines from the Joint National Committee on the Prevention, Detection, Evaluation, and Treatment of High Blood Pressure only recommend measuring the effects of postural change on blood pressure "under special circumstances" (mainly in elderly patients and diabetic patients, to look for postural hypotension) (1). The exaggerated increase in diastolic blood pressure (postural hypertension) to which Dr. Goldman refers is not uncommon in patients with mild hypertension and has been attributed to an exaggerated vasoconstriction that occurs upon standing. Evidence also suggests that an increase in diastolic pressure of more than 10 mm Hg from the supine position to standing is an independent risk factor for myocardial infarction (2).

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The Sharer

To the Editor: I wish to commend you on the publication of Dr. Lorenz's essay (1), which describes the author's experience with a Marine sergeant during training at a military base. The subject is portrayed as a man of courage, dignity, and honor, yet not without moral ambiguity related to past actions while he was in combat.

As an attending physician in a university teaching program, I have been disturbed by the lack of respect shown toward veterans. Derogatory comments are freely expressed, the nature of which could cause considerable difficulty for the purveyor if directed toward a racial or gender category. Why are such attitudes toward veterans accepted in an atmosphere otherwise intolerant of prejudicial statements?

The prestige and importance of the military remain in eclipse as those who came of age during the sixties assume positions of leadership and power. Current medicine trainees, offspring of the latter, might well reflect these same attitudes of alienation and disregard for defense-related issues. Most professionals do not partake in military service. The "citizen soldier" (2) is probably a relic of the past.

I do find it difficult to understand Dr. Lorenz's identification of the darker aspects of his training experience with the horrors of combat as purveyed by the Gunnery Sergeant. It is harder to reconcile the fact that so many have paid the price of a shattered or lost life so that the rest of us may remain secure, well-compensated, and free to indulge in the self-absorbed angst that seems to characterize our profession, from the training period on. I think of this every day passing the poor souls smoking outside of the New England Veterans Homeless Shelter. It is

imperative that we never lose sight of these sacrifices. Dr. Lorenz's contribution is a good starting point.

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Diagnosis of Intercritical Gout

To the Editor: The high prevalence of intra-articular urate crystals in synovial fluid obtained from previously gouty joints during the intercritical period, as reported by Pascual and colleagues (1), has potentially important implications beyond those discussed by the authors. The data suggest that in a patient with acute arthritis involving a joint previously affected by gout, the finding of urate crystals on arthrocentesis is largely uninformative with respect to the cause of the present attack; the crystals presumably would have been present regardless of the presence or absence of acute inflammation, whatever its cause. Thus, when septic arthritis is part of the differential diagnosis of an acute arthritis episode in a patient with a history of gout that involved the affected joint, the finding of urate crystals in the synovial fluid provides little reassurance of a nonseptic cause. This implies that decisions about antimicrobial therapy perhaps should be based on the clinical presentation, Gram stain, and culture rather than on the examination of synovial fluid crystals.

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Reference

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In response: Dr. Johnson's point is accurate and pertinent. Our data indicate that after monosodium urate crystals form, they stay indefinitely in the joint (and for diagnostic purposes can be found in synovial fluid samples) as long as hyperuricemia persists. Furthermore, during intercritical periods, in these gouty joints there is an intense interaction between crystals and cells, as shown by the regular finding of cells with phagocytosed monosodium urate crystals in synovial fluid samples drawn from these joints (1). The higher than normal leukocyte counts in these same synovial fluid samples (2), which decrease after low-dose colchicine prophylaxis (3), indicate that during the intercritical periods these joints maintain some degree of subclinical inflammation related to monosodium urate crystals. It is on this background that gout attacks occur.

Certainly, other joint diseases (such as infection, as suggested by Dr. Johnson) may also occur. When the clinical features suggest the possibility of joint infection despite the presence of monosodium urate crystals in the synovial fluid, appropriate synovial fluid cultures must be done (4). However, because the most common type of acute arthritis in gouty joints is gouty arthritis, culturing all synovial fluid samples that contain monosodium urate crystals appears inappropriate. Of additional interest, the mild crystal-related subclinical inflammation maintained by gouty joints could modify the presentation of other diseases that could develop concurrently in the same joints; in this respect, it is of interest that rheumatoid arthritis and gout rarely coexist. A negative association between these two conditions has been reported (5).

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Transmission of Tuberculosis in a Jail

To the Editor: The paper by Jones and colleagues (1) describing extensive transmission of tuberculosis in a Memphis jail is quite informative. However, I think the authors of the paper and the author of the accompanying editorial (2) overlooked a factor that probably was operative in this unusual spread of tuberculosis—deficiency of 1,25-dihydroxyvitamin D₃ caused by deprivation of sunlight in this predominantly dark-skinned group (90% of them were black and 90% were male). This vitamin is an important immunoregulatory hormone, deficiency of which has been shown to be associated with reactivation of dormant tuberculous infection (3).

Upon inquiry, I learned that inmates of the Shelby County jail are permitted to go to an indoor basketball court for 1 hour a day. Moreover, they have little direct exposure to sunlight.

Tuberculosis is common among immigrants from the Indian subcontinent to the United Kingdom (3) because in the winter, dark-skinned people may be deficient in 1,25-dihydroxyvitamin D₃ (4) unless they take the vitamin each day.

Finally, Jones and colleagues' Figure 2 shows that the peak incidence in development of tuberculosis was in the third quarter (that is, the summer). Douglas and coworkers (5) have shown that the peak incidence of tuberculosis in immigrants from the Indian subcontinent to the United Kingdom occurs in July, in contrast to the winter peak for other respiratory diseases. The development of tuberculosis after the winter nadir of vitamin D₃ is probably delayed because it takes several months for a dormant tuberculous infection to reactivate and produce sufficient symptoms for the illness to be diagnosed.

This experience should teach us that when dark-skinned people are to undergo prolonged indoor confinement, we should be sure they have adequate vitamin D to enable their immune systems to function properly year-round.

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Prophylactic Fluconazole in Liver Transplant Recipients

To the Editor: In their rigorous study, Winston and colleagues (1) reported that fluconazole use led to a significantly lower

incidence of fungal infections after liver transplantation. Although fluconazole was used for nearly 3 months after transplantation, the authors recommended that all liver transplant recipients receive fluconazole for 6 weeks (1). The significant difference in the rate of fungal infections may not have been related to the efficacy of fluconazole but to the definition of fungal infection. A 43% incidence of proven fungal infections in the placebo group is not "relatively high" as stated by the authors but rather the highest reported in liver transplant recipients. This inordinately high incidence might be partly explained by the lack of stringent criteria for the definition of fungal infections, particularly superficial fungal infections. Isolation of fungus from skin, ulcer, plaques, or exudates not explained by other pathogens was considered evidence of proven superficial fungal infection. Mere presence of fungus not explained by other pathogens was defined as an invasive fungal infection.

We have noted a trend toward a decline in the incidence of invasive candidiasis over the past decade in our liver transplant recipients, even though systemic antifungal prophylaxis has never been used. The incidence of invasive fungal infections in our patients not receiving fluconazole is virtually identical to that in Winston and colleagues' fluconazole group (2). Our patients had severity of illness similar to or greater than that in Winston and colleagues' patients; more of our patients required pretransplantation life support in the intensive care unit (32% compared with 11%) and required continuous hospitalization (66% compared with 18%); the frequency of other high-risk factors, such as retransplantation and renal failure, was at least comparable to the frequency in Winston and colleagues' study (3).

Winston and associates expressed concern about the emergence of fluconazole resistance with routine fluconazole prophylaxis. This scenario has been documented in critically ill surgical patients, including liver transplant recipients (4, 5). Winston and colleagues' study should be replicated at other institutions before widespread use of prophylactic fluconazole becomes routine.

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Role of Communication Skills Training

To the Editor: Although Brown and colleagues' findings (1) raise questions about the efficacy of clinician communication skills training in improving patient satisfaction, the authors' definitive statement that such training "is not effective in improving general patient satisfaction" appears overstated given the inability to adjust for essential environmental factors in the analysis. The substantial increase in health maintenance organization enrollment observed during the study period introduces two important independent variables that could adversely affect patient satisfaction: 1) higher practice volume and 2) diminished depth of physician-patient relationships.

Increased practice volume is associated with lower patient satisfaction, independent of time spent with patients (2). Reductions in perceived time spent with providers can further adversely

affect patient satisfaction (3). Conversely, the depth of the physician-patient relationship has been a principal component of patient satisfaction in discriminant analyses of general outpatient practice (3). Not surprisingly, lack of continuity in the physician-patient relationship has been identified by patients as an important quality deficiency in outpatient settings (4).

To assess the independent effect of the communication skills intervention, inclusion of covariates for these potentially confounding environmental factors, including physician panel size, length of visits, and ratio of new to follow-up visits, would provide a more convincing analysis. In their absence, one is left wondering whether providers truly had an opportunity to incorporate new communication skills into their practice and whether these skills could have had a positive impact in a more stable clinic environment.

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Update in Preventive Medicine

To the Editor: In their otherwise excellent Update in Preventive Medicine, Beck and Kumar (1) perpetuate the mistaken inference of the Air Force/Texas Coronary Atherosclerosis Prevention Study (AFCAPS/TexCAPS) (2) by stating that "statin therapy... can prevent or at least delay cardiovascular events and deaths, even in some subsets of apparently healthy persons whose lipid levels are relatively normal."

The AFCAPS/TexCAPS presented relevant and valid results that should affect the management of many patients. However, the title of both the article and the accompanying commentary (3) were misleading and potentially harmful; this recent Update compounds this problem. Those reading only the Update or the titles of these papers would believe that nearly everyone should be treated to reduce cholesterol levels.

In fact, the study was not about treating "low-risk patients" or those "with average cholesterol levels" but rather offered proof that treating a specific high-risk group (patients with low levels of high-density lipoprotein [HDL] cholesterol) is effective in preventing first major coronary events. This is clear in the report and the abstract of the original report but not in the title or the commentary. The Update also accurately describes the study but emphasizes the inaccurate conclusion in the title.

A low HDL cholesterol level is an accepted risk factor for coronary artery disease (4). The ratio of total cholesterol to HDL cholesterol is probably the best predictor of future coronary events (5). The report highlighted in the Update should encourage the National Cholesterol Education Program to consider switching to the ratio of total cholesterol to HDL cholesterol as the major risk predictor of coronary artery disease. What should not be done is accepting the implication that patients with "average cholesterol levels" or "low-risk patients" are candidates for therapy.

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Gated Myocardial Perfusion Scan Leading to Diagnosis of Unsuspected Massive Pulmonary Embolism

To the Editor: Myocardial perfusion single-photon emission computed tomography (SPECT) is routinely done to evaluate patients for suspected coronary artery disease (1, 2). Gated SPECT uses electrocardiographic signals to acquire serial ventricular myocardium perfusion image sets that correspond to different phases of the cardiac cycle. Those image sets can be played back in a cine mode to allow assessment of segmental and global left ventricular wall motion (3, 4). Information readily available on cinematic display but rarely examined is the right ventricular size and wall motion.

We encountered two patients with unexplained right ventricular dilatation and hypokinesis detected by gated SPECT in whom massive pulmonary embolism was diagnosed. We describe one of these patients here.

A 50-year-old, previously healthy, moderately obese woman with a 10-pack-year history of smoking was admitted to the hospital for chest pain and dyspnea on exertion. Her electrocardiogram showed T-wave inversion in leads 3, F, and V1 through V5. She was initially suspected to have angina pectoris and was referred for myocardial perfusion scanning, which showed normal left ventricular perfusion and wall motion but markedly dilated and hypokinetic right ventricle (Figure). A ventilation-perfusion lung scan obtained the same day showed extensive mismatched ventilation-perfusion defects. A spiral computed tomography angiogram also demonstrated multiple central and peripheral emboli. Echocardiography showed a markedly enlarged right atrium and right ventricle with moderate to severe pulmonary hypertension and right ventricular hypokinesis. The patient was treated with intravenous streptokinase.

Right ventricular dysfunction associated with pulmonary embolism demonstrated by echocardiography has been well de-

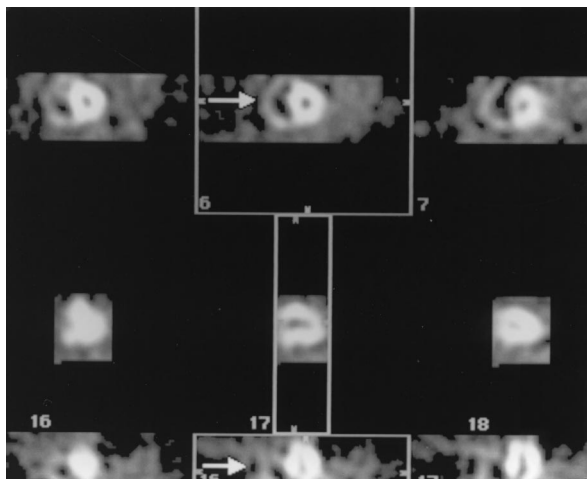


Figure. Short-axis, vertical long-axis, and horizontal long-axis images showing normal left ventricular perfusion but dilated right ventricle (arrows).

scribed in the medical literature (5). Routine reporting of right ventricular size and motion on gated myocardial perfusion scans may lead, in the proper context, to enhanced detection of unsuspected pulmonary embolism as a cause of chest symptoms.

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Infection with Hepatitis C Virus Genotype 4 Is Associated with a Poor Response to Interferon- α

To the Editor: The hepatitis C virus (HCV) genotype is a crucial predictive factor of response to interferon- α and is associated with the route of infection (1, 2). Although HCV genotype 4 is generally assumed to be resistant to interferon- α therapy, detailed studies on this issue are still lacking (3, 4).

We studied 74 (5%) patients infected with HCV genotype 4 out of the 1520 HCV-infected patients in our files who had undergone genotyping. Genotype and quantitative viremia were assessed by the Inno-Lipa (Innogenetics, Zwijnaarde, Belgium) and Monitor (Hoffman-LaRoche, Meylan, France) assays, respectively.

The risk factors were blood transfusion in 13 patients (17%), intravenous drug use in 27 (36%), and birth in endemic countries in 23 (31%) (Central Africa, 11; Egypt, 10; North Africa, 2); risk factors were unknown in 12 patients (16%). Eight patients had HIV infection (11%). Infection acquired through intravenous drug use was more frequent in French patients (26 of 51) than in patients from endemic countries (1 of 23) ($P < 0.001$). Five patients from endemic countries and 1 of the French patients had cirrhosis ($P < 0.02$).

We compared primary and sustained response to interferon- α therapy in 20 treated patients with the response in patients infected with genotypes 1b and 3a; the latter were matched for age, sex, fibrosis score, viral load, and doses and duration of interferon- α therapy. Primary and sustained response rates in patients with genotype 4 were significantly lower than those in patients with genotype 3a (10% and 5% compared with 85% and 35%; $P < 0.05$) and were identical to those in patients infected with genotype 1b (25% and 10%; $P > 0.05$). Three of 20 treated patients had cirrhosis, and only 5 of the treated patients had a viral load higher than 200 000 HCV RNA copies/mL.

We found two different epidemiologic types of HCV genotype 4 infection: infection acquired from endemic countries and infection acquired through intravenous drug use. The identical poor response to interferon- α in both groups suggests an intrinsic ability of HCV genotype 4 to resist interferon- α therapy. These patients should be treated by reinforced combination therapy.

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A Common Problem with Therapy for HIV Infection

To the Editor: During the past 3 years, techniques of quantifying HIV viral load have allowed us to assess our success in the control of viral replication resulting from drug therapy. The immunologic status of HIV-infected patients has dramatically improved in all countries where protease inhibitors have been accessible (1).

However, treatment of HIV infection imposes an enormous economic burden on national health services and health insurance companies responsible for providing care to these patients. In Spain, the National Health Service provides appropriate drugs to these patients on a monthly basis. Unfortunately, frequent modifications of drug regimens are necessitated as a result of adverse side effects, response of CD4⁺ counts, or increasing viral load. These changes in therapy produce large amounts of leftover drugs. The usual method of packaging capsules or tablets of didanosine, zalcitabine, lamivudine, and protease inhibitors in bottles actually hinders their reuse by other patients, because these patients would then assume the risk of using drugs that may have been handled, contaminated, or switched by the previous user.

In our opinion, dispensing capsules or tablets in blister packaging would be a solution to this problem and would promote safe reuse of the remaining drug supplies. In fact, many hospital pharmacies are repackaging the drugs on their own, a situation that could be avoided if drug manufacturers provided initial unit packaging devices. If this were done, an additional service could be rendered by printing the day of the week on the back of the blister; this could contribute to improved adherence to the treatment regimen.

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Most Gastric Low-Grade B-Cell Lymphomas of Mucosa-Associated Lymphoid Tissue Persist after *Helicobacter pylori* Eradication

To the Editor: Eradication of *Helicobacter pylori* may induce remission of low-grade gastric mucosa-associated lymphoid tissue (MALT) lymphoma (1-3). Is this remission a real regression? We followed 16 patients described in a previously published series (2) who had localized primary low-grade gastric MALT lymphoma and received anti-*H. pylori* therapy only. Response, evaluated every 6 months, was graded according to Wotherspoon's histologic scoring system (0 to II, complete response; III, partial response; IV to V, no response) (1).

Infection with *H. pylori* was eradicated in all patients. With a median follow-up of 61 months (range, 44 to 87 months), 3 patients (all with autoimmune disease) showed no histologic response after 45, 47, and 52 months. Thirteen patients achieved complete remission at least once during follow-up (median time

for detectable regression, 19 months [range, 2 to 54 months]). Only 3 of 13 patients (in contrast with Neubauer and colleagues' patients [4]) had complete continuous remission; of the remaining 10 patients, 3 relapsed after 3, 6, and 30 months, and 7 had fluctuating histologic scores (from 0 to III) with no evidence of *H. pylori* reinfection. Five patients had multifocal disease, and anatomic gastric site changed in 3 patients. Disease did not progress to advanced stages or transform into high-grade MALT lymphoma.

Our data support the favorable clinical behavior of low-grade gastric MALT lymphoma but emphasize that gastric MALT lymphoma is a multicentric disease with many foci showing different histologic features. Several biopsies are necessary to demonstrate real regression. The late relapse and fluctuating histologic scores in our patients and the persistence of monoclonal B cells in many patients with histologic complete remission (4, 5) may represent different aspects of a multicentric disease. Moreover, the only patients who never responded had autoimmune diseases; this could support the importance of antibody-mediated mechanisms, as reported for the evolution of MALT lymphomas. As observed by Isaacson (5), antibiotic therapy probably suppresses, but may not eradicate, the neoplastic clone. We do not know whether the regression in histologic score after *H. pylori* eradication reflects real remission of low-grade gastric MALT lymphoma.

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Immediate Cerebral Angiography and Mechanical Fragmentation of Cerebral Embolus after Percutaneous Myocardial Revascularization

To the Editor: Cardiac catheterization may be associated with complications, but ischemic stroke is rare (1, 2). With the recent availability of endovascular procedures and intra-arterial thrombolysis, treatment should be considered when a patient with ischemic stroke is encountered in an angiography suite. However, because of the risk for life-threatening bleeding, thrombolysis is probably contraindicated when the procedure involves laser techniques. We describe a patient with an acute middle cerebral artery occlusion who underwent immediate cerebral angiography after percutaneous myocardial revascularization and recovered fully after mechanical disruption of the embolus.

A 75-year-old man was admitted to the coronary care unit for percutaneous myocardial revascularization. His coronary angiogram showed normal left ventricular function with diffuse coronary artery disease and 100% stenosis of the mid-left anterior descending artery. A dipyridamole thallium scan showed reversible apical, atrial, septal, and inferior defects, and it was decided that the patient was a suitable candidate for percutaneous myocardial revascularization. Almost immediately after completion of the laser procedure, while the femoral arterial sheaths were in

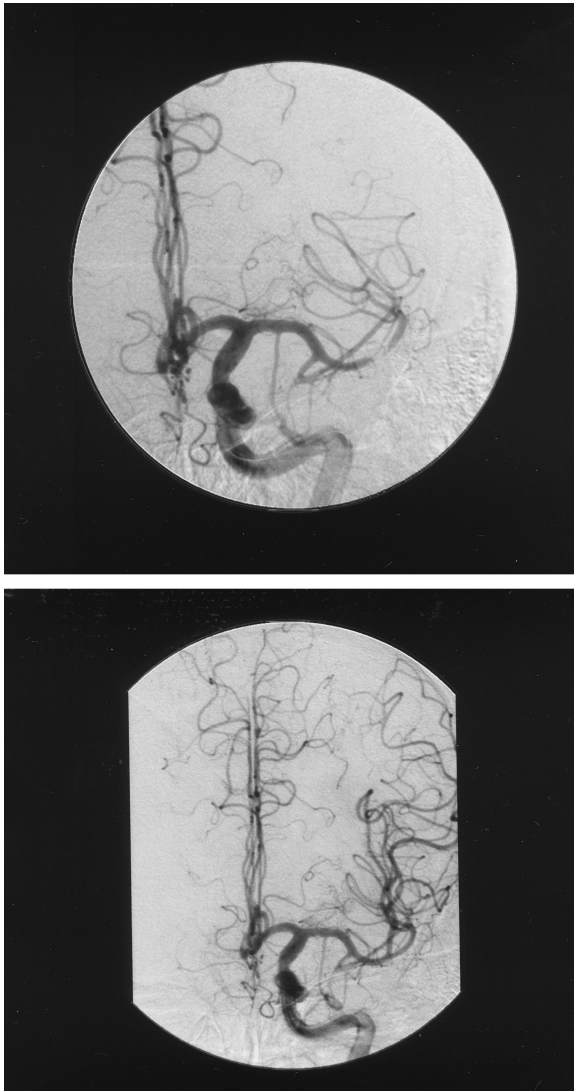


Figure. Towne views on cerebral angiography. **Top.** Initial Towne view of selective left internal carotid artery demonstrating obstructive embolus in left middle cerebral artery bifurcation, which partly occludes the anterior division and completely occludes the posterior division. **Bottom.** Towne view obtained after mechanical fragmentation showing restoration of flow within both divisions of the middle cerebral artery.

place, the patient could not form a sentence, had paraphasic errors, and had anomia in addition to a right hemiplegia. Findings on noncontrast computed tomography of the head were normal. Within 1 hour, cerebral angiography was performed; this test demonstrated occlusive thrombus at the left middle cerebral artery bifurcation (**Figure**). A microcatheter and microwire were passed through the thrombus and distally beyond the thrombus into both divisions of the left middle cerebral artery to mechanically disrupt the thrombus. After the catheter was repositioned three times just proximal to the left middle cerebral artery bifurcation, flow in both the anterior and posterior divisions of the left middle cerebral artery increased markedly. The patient recovered immediately after restoration of flow, and neurologic examination conducted in the angiography suite and on the following day was normal.

Our case demonstrates that immediate cerebral angiography with mechanical disruption of the embolus alone can lead to full neurologic recovery. The immediate availability of an interventional neuroradiologic team made this successful outcome in our

patient possible. Spontaneous fragmentation could have occurred with observation only, but the latter approach was less attractive because of a major neurologic deficit that persisted for 1 hour.

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Rapid Bioprosthetic Valve Degeneration in a Patient Undergoing Hemodialysis

To the Editor: A 76-year-old man who was undergoing hemodialysis had aortic valve replacement for calcific aortic stenosis. Stenosis was performed by using a 25-mm Carpentier-Edwards porcine valve. Heart failure recurred 20 months later. Cardiac catheterization confirmed significant prosthetic stenosis with a calculated aortic valve area of 0.6 cm². There was no evidence of hyperparathyroidism. The patient underwent a second aortic valve replacement with a St. Jude mechanical valve. At surgery, the bioprosthetic valve was found to be heavily calcified, with fusion of the leaflets.

Native cardiac calcification is a well-known consequence of chronic renal failure. Only two reports in the English-language literature (1, 2) have described early bioprosthetic valve failure in patients undergoing dialysis. The extremely rapid calcific degeneration of the bioprosthesis in our case is striking.

The type of valve that should be used in patients with chronic renal failure has practical implications. Current American College of Cardiology/American Heart Association guidelines for valvular heart disease rank chronic renal failure and hemodialysis as a class II indication for mechanical valve selection and as a class III indication for bioprosthetic valve selection (3). These recommendations are based on the purported higher rate of structural deterioration in tissue valves implanted in this subset of patients. However, the only study evaluating long-term outcomes in patients with chronic renal failure who have bioprosthetic or mechanical replacements found no difference in the rate of death or valve failure between the two groups at a mean of 60 months; in addition, the incidence of bleeding and stroke was lower in the bioprosthetic replacement group. This finding led these authors to recommend the increased use of bioprosthetic valves in this patient population (4).

The rapidity of the calcific degeneration of the prosthetic valve in our case is noteworthy. Dialysis patients who do have bioprosthetic valves implanted should probably undergo closer clinical follow-up and more frequent echocardiographic evaluation.

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