Racial Inequity of Access to Carotid Imaging
To the Editor:

While we welcome research into stroke in patients of African and Caribbean origin, we have a number of concerns regarding the conclusions of Oddone et al on racial differences in the use of carotid imaging.1

Lacunar stroke may give a presentation identical to that of a transient ischemic attack (TIA). There is a high prevalence of lacunar stroke in people of Afro-Caribbean heritage both in the United States and the United Kingdom. Furthermore, there is a negative correlation between lacunar stroke and carotid disease.2 Thus, when the strictest indication for carotid imaging is used (to define a symptomatic stenosis in a patient suitable for surgery), a higher proportion of African-American patients may not have qualified. (The brain imaging was not reviewed in the study.)

Of the African-Americans in the TIA group, 34.9% (30 of 86) had had a previous TIA and 33.7% (29 of 86) a previous stroke, compared with 15.2% (28 of 184) and 16.3% (30 of 184) of white patients. As the study only analyzed photocopies of case records from the index admission and for 6 months afterwards, it is conceivable that some of these patients’ carotids had been imaged previously. If a normal carotid study had been performed recently, repeat imaging might be considered an inappropriate use of resources. If this situation held for even 4 African-American patients, the statistical significance for the statement “fewer black than white patients with TIA . . . received . . . studies of their carotid arteries” is lost.

In terms of the lower proportion of African-Americans who underwent surgery for appropriate and uncertain indications, Oddone’s own work has shown differences in risk acceptance between different ethnic groups.3 It is unclear how many patients were offered an endarterectomy and refused.

The implication that physicians deny appropriate investigation and treatment to a particular ethnic group is a serious one. While there may be many reasons, including genetics, risk factors, vascular pathophysiology and sociocultural factors, for the excess of stroke in people of African and Caribbean heritage, we do not feel that inequity of access to carotid imaging has been proved.

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Response

We appreciate the comments of Drs Evans and Kaltra concerning our manuscript and agree strongly that investigators need to exercise caution when interpreting findings regarding racial/ethnic differences in utilization of health care. They raise 3 important points. First, they suggest that a racial difference in stroke subtype, particularly lacunar stroke, may account for the lower proportion of black patients who received carotid imaging. We did not assess stroke subtype in this study because, as a medical record review, determination of stroke subtype is unreliable. Also, patients with small-vessel disease may also have carotid artery disease. If it were possible by some means short of carotid imaging to accurately discern which symptomatic black patients were likely to not have carotid stenosis (or whose carotid stenosis is unlikely to lead to future stroke), we could appropriately avoid many imaging studies. Since that is not possible, it may be prudent to perform a noninvasive imaging study of the carotid arteries even in black patients with lacunar infarctions.

Second, Drs Evans and Kaltra correctly note that we counted imaging during the index hospitalization and for 6 months after discharge, potentially missing patients who had previous carotid studies that did not reveal high-grade stenosis. The issue here is whether old imaging studies from prior symptomatic events are sufficient to rule out new significant disease in a patient with new symptoms. By focusing on imaging studies after the new event, we were assuming that new symptoms typically require new imaging, since carotid disease is progressive. However, it is true that we would have missed some black patients who had had recent negative imaging studies and for whom new tests would not be necessary. We should note that this is unlikely, since such studies would usually be reported in the index hospitalization.

Last, they infer that our report implied that black patients were being denied appropriate referral for carotid endarterectomy. As explained in the discussion, we do not want to imply, nor do we want readers to infer, that this study demonstrates denial of appropriate care. Indeed, as preferences for this procedure may differ by race, black patients may be less likely to accept the procedure if offered. As we discussed, there were too few black patients who received endarterectomy in our study to make inferences concerning the differential use of this procedure by race. However, we do feel that the differential referral for carotid artery imaging, the vast majority of which is noninvasive ultrasound, cannot be explained by this phenomenon. Informed decisions can be made only when all of the important information is in hand. Knowledge of the degree of stenosis is essential for patients who may be eligible for endarterectomy. The main finding of our study is that there was a differential referral for imaging that we cannot otherwise explain. We are currently engaged in a large prospective cohort study designed to determine the degree to which patient and physician factors predict referral. When this study is completed we hope to better understand the specific cause of differential referral should we find it.

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Determination of Cognitive Hemispheric Lateralization by “Functional” Transcranial Doppler Cross-Validated by Functional MRI
To the Editor:

In their article on functional transcranial Doppler sonography (fTCD), Schmidt et al1 conclude that fTCD can lateralize higher cognitive functions reliably. They suggest that it may be used in patients in the preoperative evaluation before brain surgery. But because “data to validate fTCD . . . are still lacking,” they caution that fTCD should be cross-checked with well established techniques (presumably the Wada test), particularly with regard to language dominance. Stroke published such a study (“Noninvasive Determination of Hemispheric Language Dominance Using Functional Transcranial Doppler Sonography: A Comparison With the Wada Test”)1 1 year ago.

Schmidt et al base their suggestions on group averages. Again, it was this very journal that published a study on the feasibility and reproducibility of single-subject assessment by fTCD.2 A full account of the present state of single-case fTCD analysis has already been published by Deppe et al.3
Schmidt et al state that “concordant differences between the female and the male subgroup could be visualized.” To us, this sentence is somewhat misleading. Changes in lateralization “concordant” in fMRI and fTCD have a 50% chance of occurring in each sex. The authors provide no statistical evidence for sex differences in their paper. Such differences cannot be established by finding significant ($P=0.022$) lateralization only in females and not in males, particularly as long as no correction for multiple comparisons (eg, Bonferroni) was performed. Even then, they found significant sex differences of cerebral blood flow velocity within neither the left nor the right middle cerebral arteries (figure 7).1

We agree with Schmidt et al that fTCD has a clinical potential. This is why the same rigid methodological requirements should be applied to fTCD as to other functional imaging techniques. The methodological tools for single-case studies are available.

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Response

In accord with our colleagues Knecht and coworkers, we are glad that Stroke offers a platform for publishing and discussing the latest results of fTCD research. Despite these promising research findings, fTCD still is not routinely used in the majority of hospitals. To promote this clinical application, we thought our confirmatory findings to be worth publishing. To our knowledge, our study was the first in which the potentials of fTCD and fMRI in identifying cognitive lateralization were compared directly. We could show for the group of subjects that with both methods one can detect a significant blood flow shift to the right hemisphere induced by a complex cognitive visuospatial task.

Our statement concerning the comparison of female and male subjects (“fTCD shows a higher VMCA increase of the rMCA in the male subgroup compared with the female subgroup, corresponding to the larger area of activation found in the fMRI results in the male subgroup”) was purely descriptive. It was meant to be an additional surprising yet possibly accidental finding that we thought to be worth mentioning. A 2-factorial repeated measures ANOVA with hemisphere (left, right) as repeated measures factor and gender as the second factor revealed no significant interaction between both factors ($P=0.62$), indicating that there was no significant gender difference in the degree of discrepancy between VMCA increases in both hemispheres. So our initial statement must either be considered accidental, or a much larger study would be required to detect a small interaction effect with sufficient statistical power. We have been aware of the work by Knecht and his colleagues1–3 quoted in their letter to Stroke, but because we did not utilize their method of evaluation we did not quote these papers.

With both fMRI as well as fTCD we could show the adequacy of both methods in detecting cognitive lateralization in a visuospatial task. In case of an absent temporal skull window, when fTCD fails to be useful for studying lateralization, fMRI might be a future alternative to the Wada test without any known side effects. Therefore, Knecht and coworkers must be credited for having pointed out that every new noninvasive technique must be studied closely for individual patient reproducibility of lateralization effects in cognitive paradigms.

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