antianginal medications compared with testing on such medications. Dr. Burns suggests that patients should be risk-stratified after discontinuation of antianginal medications. This is obviously an important issue to resolve. Unfortunately, Dr. Burn's study, as he points out in his text, has significant methodological weaknesses that do not allow it to address this issue. Because it was a retrospective study, the groups of patients tested on medications versus off medications were mutually exclusive and are simply different patient populations that are not comparable. Patients whose physicians felt that they should be taking antianginal medications at the time of thallium-201 imaging would be expected to have a higher prevalence of coronary disease and more severe disease than patients whose physicians did not feel they needed such treatment. In fact, in Dr. Burn's study, the frequency of positive thallium-201 studies was higher (64%) in patients tested while on medications compared with patients tested off medications (48%, p<0.05). This is certainly counterintuitive if the patient groups had equivalent prevalence and severity of coronary disease and is in contrast with previous observations. Furthermore, the incidence of cardiac events was nearly twice as high among patients tested while on medications (13% versus 7.6%), although because of the relatively small number of patients, this did not reach statistical significance. Therefore, although Dr. Burn's observations are very interesting, I do not feel that they are sufficiently compelling evidence to call for routine risk stratification of patients off medications. I would stand by my original statement that the impact of antianginal medication on the prognostic value of thallium-201 myocardial imaging is not known. For the time being, I think it would remain reasonable to perform risk stratification either on or off medications, depending on the circumstances.

I also thank Dr. Oosterhuis and colleagues for their interest in the article. The authors correctly point out that in two of the studies reported in Table 2, the number of patients with normal thallium images should be 61 and 95 rather than 100 and 344, respectively. However, the third study identified by Oosterhuis was simply incorrectly cited in the table. The paper by Iskandrian et al should be Reference 8 rather than 9 as indicated in Table 2. The number of normal patients is correct for this reference.

In addition, there is a separate error in the table regarding the arithmetic sum of the total number of patients. The actual number of normal patients cited in all the references of Table 2 (corrected for the above errors) is 3,594. The recalculated weighted mean follow-up period is 29 months and the cardiac death/myocardial infarction rate is 0.9% per year, essentially unchanged from Table 2. Regarding the choice of statistical approach for estimating the event rate, the Kaplan-Meier method is used to estimate survival function. I am presenting what are essentially incidence rates for a group of patients who have been identified as having normal thallium-201 studies. It is more appropriate to estimate incidence or event rates, as I have done, for a group of patients identified as having a normal thallium-201 study rather than looking at survival function from the date of the test because the date is arbitrary and not biologically related to the onset of risk.

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**References**


**Conventional Angioplasty Versus Percutaneous Transluminal Laser Angioplasty**

In their article on the use of Nd:YAG laser-assisted angioplasty (PTLA), Dr. Pilger et al (*Circulation* 1991;83:141–147) reported a success rate of only 14% for attempted recanalization by using the guide wire alone. This is dramatically inconsistent with the published reports of the use of guide wires, even in the very early days of angioplasty.

In comparing the results of PTLA, the authors are guilty of selecting published reports that document some of the lowest primary success rates for angioplasty and ignoring studies that report higher primary success rates, in particular publications that take account of recent developments in guide wire and balloon technology (a criticism that can also be made of the author of the editorial comment in the same edition of *Circulation*). When the results from the start of an angioplasty program are compared with the results after a program has been established for several years, there is a significant improvement in primary success rates even though the restenosis rate after peripheral angioplasty may be little changed. In our experience in Bristol, United Kingdom between 1980 and August 1987, the primary success rate for crossing femoropopliteal occlusions was 70%. With improvements in guide wire and balloon technology (and in particular the introduction of hydrophilic guide wires), this primary success rate (1990) increased to 97% in a preliminary report despite the inclusion of a larger proportion of patients with long occlusions (mean length of occlusion in the latest study was 7.6 cm; range, 2–30 cm).

When calculating the 3-year patency rates after PTLA for all patients who underwent PTLA (including the failed attempts), the overall patency rate falls to less than 50%, and this is not so different from the currently expected results on balloon angioplasty to suggest that there is a role for the use of laser in its current form for opening peripheral arterial occlusions.

There have been a number of criticisms of the quality of clinical trials of the use of PTLA and calls for assessment by prospective, randomized controlled trials. Two such studies have now been reported (although they are usually ignored when discussing PTLA) and neither shows any benefit from the use of PTLA over conventional balloon angioplasty. I cannot agree with the authors or with the author of the editorial comment that the current study provides a basis for suggesting that the laser system investigated is particularly encouraging but wholeheartedly agree that properly conducted prospective randomized controlled trials are required both for evaluation of laser (when improved systems become available) and alternative recannalization devices.

The primary success rate of conventional balloon and guide wire angioplasty for peripheral recannalization is now so high that the developments and assessment of new recannalization devices, especially those as expensive as laser systems, can only be justified by the objective demonstration of a significant improvement in long-term patency rates.

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**References**


**Reply**

As spelled out in the title of our paper, it was the aim of our study to present long-term results of percutaneous transluminal laser angioplasty (PTLA). Our results show a 36-month patency rate of 63%. This was a novel observation, resulting in publication in the journal and an accompanying editorial. We did not attempt to compare PTLA with any other treatment modality, but we do agree with Dr. Hartnell that this should be the next step in evaluating the true benefit of such treatment.

Dr. Hartnell points out that in his hands, the use of the guide wire alone may result in a primary success rate of up to 97%. Such results, although not the subject of our study, are of great interest, and we look forward to seeing full publication in a peer-reviewed journal. Again, Dr. Hartnell is not giving long-term results; also, the two studies that he wants us to quote pertain to primary success rate and not to long-term patency.

As discussed in our article, we neither used the guide wire very aggressively nor applied a very stiff or hydrophilic guide wire. Aggressive use of the guide wire in our study could have jeopardized the subsequent use of PTLA.

We are happy to report that a randomized, controlled clinical trial between PTLA and percutaneous transluminal angioplasty as suggested by Dr. Hartnell is now nearing completion in our laboratory and will be reported in due time.

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**Reference**


**Fibrinolytic Activity and Response to t-PA in Blacks and Whites**

In their interesting article, Dr. Sane et al compared responses to thrombolytic therapy in black and white patients with acute myocardial infarction. They noticed that black patients had an apparent enhanced sensitivity to rt-PA manifested by increased thrombolytic efficacy, a more pronounced systemic fibrinogen breakdown, and increased transfusions. The authors speculated that differences might exist in the hepatic clearance of rt-PA between blacks and whites.

I suggest that the data of Dr. Sane et al can be explained by racial differences in resting blood fibrinolytic activity. Gillman et al and Merskey et al were the first to report higher fibrinolytic activity in African than in European men. These results were subsequently confirmed by a number of authors. Such differences were due to racial rather than cultural or environmental factors. In black and white Americans living in the same county of southern Georgia, blacks exhibited a significant increase in fibrinolysis at rest in comparison with whites. Blacks had higher whole plasma fibrinolytic activity, higher content of plasminogen in euglobulins, and lower antiplasmin levels. Under such circumstances, the same stimulus (e.g., t-PA) might be expected to raise fibrinolytic activity to higher levels in blacks than in whites. Indeed, Howell found greater fibrinolytic activity in whole plasma of Nigerians than Europeans and also showed that Nigerians had a greater response to stimulation by intravenous nicotinic acid.

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**References**


**Third World Profile of Racial Differences in Thrombolytic Effects of Streptokinase**

We read with interest the article by Sane et al suggesting racial differences in the thrombolytic effects of rt-PA. We feel that such a difference may exist as well with streptokinase, which seems to have greater effects in Indian patients compared with whites.

The standard dose of streptokinase used in the United States and the United Kingdom is 1,500,000 IU, with reperfusion rates of 31–80%. In India, doses of 500,000–750,000 IU are often used, with patency rates of over 78% reported. We have recently completed a pilot study in which we gave 750,000 IU or 1,500,000 IU intravenously to alternate patients admitted within 8 hours of acute myocardial infarction. The study involved 54 patients but data analysis was done in 47 patients because in seven patients, there was no subsequent rise in creatine phosphokinase MB (three from the 750,000-IU group and four from the 1,500,000-IU group). Enzymatic criteria of Garbedian et al were used to assess reperfusion. We found no significant difference in the reperfusion rates between the two groups (15 of 23 in the 750,000-IU group, 19 of 24 in the 1,500,000-IU group; p>0.05). Mortality rates were also similar (two of 23 in the 750,000-IU group, two of 24 in the 1,500,000-IU group). There were three cases with hemorrhagic problems, one in the lower-dose group and two in the higher-dose group.
Conventional angioplasty versus percutaneous transluminal laser angioplasty.
G G Hartnell

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