

## Isolated systolic blood pressure measurement

We appreciate the Viewpoint by Bryan Williams and colleagues (June 28, p 2219).<sup>1</sup> However, we are not in agreement with their suggestions and wish to voice our concerns.

First, if only systolic blood pressure is monitored in people aged 50–59 years, it could falsely reassure up to 10% of the hypertensive population.<sup>1</sup> We feel this is not insignificant.

Second, Williams and colleagues imply that measurement of systolic blood pressure alone will “simplify” the management of hypertension. We challenge them to produce such evidence. If a minority of physicians or family practitioners is “confused” about the management of hypertension, as suggested in the Viewpoint, we suggest that this relates to lack of appropriate education rather than the fact that we have two values for blood pressure.

Third, there is sufficient evidence from the Framingham studies to suggest that, in elderly people, measurement of pulse pressure is more accurate than either systolic or diastolic blood pressure in assessing cardiovascular risk.<sup>2</sup> How can we disregard such powerful evidence?

Finally, not measuring the pulse pressure might not alert the physician to possible valvular pathology, notably aortic stenosis or incompetence.

Hypertension is steadily increasing and we need to have the most sensitive test to identify hypertensive patients, not the most “simple” one. We feel that measurement of both systolic and diastolic values should continue and calculation of the pulse pressure encouraged before diagnosing and treating hypertension.

We declare that we have no conflict of interest.

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Like Bryan Williams and colleagues,<sup>1</sup> we believe that systolic blood pressure is the key contributor to cardiovascular disease in older people with hypertension, and that a simple, clear message is of utmost importance for general practitioners. However, we believe that some concerns should be addressed to avoid weakening the strength of this message.

Our major concern regards excessive lowering of diastolic blood pressure in older patients. Williams and colleagues state that “trials have not shown that a resultant fall in diastolic blood pressure would impart harm or offset the benefit of systolic blood pressure reduction”, referring to four studies, with SHEP<sup>2</sup> and Syst-Eur<sup>3</sup> among them. A further analysis of the SHEP study, however, showed that a decrease of 5 mm Hg in on-treatment diastolic blood pressure increased the risk of stroke, coronary heart disease, and cardiovascular disease; moreover, the relative risk became significant for a diastolic blood pressure of less than 70 mm Hg and approached a two-fold increase for a diastolic blood pressure of less than 50 mm Hg.<sup>4</sup>

Similarly, in the Syst-Eur trial, low on-treatment diastolic blood pressure was associated with increased risk of cardiovascular events in patients with coronary heart disease at baseline;<sup>3</sup> moreover, a J-shaped relation between diastolic blood pressure and stroke in treated patients was found in the Rotterdam study.<sup>5</sup>

We do not wish to question the importance of treating isolated systolic hypertension; we just believe that general practitioners should be aware of the facts about diastolic blood pressure too, especially when treating patients with isolated systolic hypertension and previous coronary heart disease.

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In their Viewpoint,<sup>1</sup> Bryan Williams and colleagues elegantly list the evidence showing that systolic pressure is indeed the more important of the two blood pressure readings, especially among individuals aged 50 years or older.

To give due credit, C M Fisher argued exactly the same thing in *The Lancet* more than 20 years ago in his Point of View [sic] article “The ascendancy of diastolic blood pressure over systolic”.<sup>2</sup> Fisher even confessed to having discontinued taking repeated diastolic blood pressure measurements a decade earlier, defying frequent questions about “the underneath one”.

That Williams and colleagues’ Viewpoint is timely in the year 2008 shows that the progress of medicine is not rapid in all areas, and that important ideas must be unearthed from time to time.

I declare that I have no conflict of interest.

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## Thrombus aspiration during percutaneous coronary intervention

Pieter Vlaar and colleagues' TAPAS study (June 7, p 1915)<sup>1</sup> is the first to suggest that thromboaspiration in patients with ST-elevation myocardial infarction (STEMI) has an effect on mortality. However, we would like to raise a cautionary note about a major limitation in the study's methods, which might have affected the results.

This study compared a strategy of aspiration plus direct stenting versus balloon predilatation followed by stenting. The use of balloon predilatation only in the control group, including patients with thrombosis in myocardial infarction (TIMI) grade 2 and 3 flow before percutaneous coronary intervention, is a flaw because previous studies have shown that direct stenting alone is superior to balloon predilatation followed by stenting in STEMI patients, irrespective of aspiration. Loubeyre and colleagues<sup>2</sup> showed that direct stenting decreases the rate of no-reflow and improves microvascular reperfusion compared with balloon predilatation followed by stenting. As confirmed by the TAPAS study,<sup>3</sup> these surrogates of flow are strong predictors of short-term and long-term clinical outcome. Therefore, whether the mortality differences between the two groups can be attributed solely to thromboaspiration remains unanswered since the difference in outcome could be explained by the differences in the rate of direct stenting.

Although we believe and have published on the fact that aspiration or thrombus removal is beneficial in the setting of STEMI,<sup>4</sup> we would like to emphasise that the results of the TAPAS study cannot be attributed to aspiration alone; a well designed,

randomised study with equal use of direct stenting in both groups is warranted to determine the role of aspiration in the reduction of mortality in patients with STEMI.

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- 1 Vlaar PJ, Svilaas T, van der Horst IC, et al. Cardiac death and reinfarction after 1 year in the Thrombus Aspiration during Percutaneous coronary intervention in Acute myocardial infarction Study (TAPAS): a 1-year follow-up study. *Lancet* 2008; **371**: 1915–20.
- 2 Loubeyre C, Morice MC, Lefèvre T, Piéchaud JF, Louvard Y, Dumas P. A randomized comparison of direct stenting with conventional stent implantation in selected patients with acute myocardial infarction. *J Am Coll Cardiol* 2002; **39**: 15–21.
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Pieter Vlaar and colleagues<sup>1</sup> show that thrombus aspiration before percutaneous coronary intervention (PCI) improves the 1-year clinical outcomes in patients with ST-elevation myocardial infarction.

Distal blockage is known to induce microvascular obstruction—ie, the “no reflow” phenomenon and to result in suboptimum reperfusion. In addition to thrombus aspiration, upstream administration of a glycoprotein IIb/IIIa inhibitor, nicorandil treatment, ischaemic postconditioning after coronary stenting, and deployment of an embolic protection device are known to reduce microvascular obstruction.<sup>2</sup> Periprocedural glycoprotein IIb/IIIa inhibitors are particularly known to improve microvascular flow and reduce the infarct area after coronary occlusion and reperfusion.<sup>3,4</sup>

So treatment with glycoprotein IIb/IIIa inhibitors might be an important confounder in Vlaar and colleagues'

analysis. In the Methods section (p 1916), Vlaar and colleagues mention that patients received a weight-adjusted glycoprotein IIb/IIIa inhibitor (abciximab) during the procedure unless contraindicated, but we could not see how many patients were thus treated in the conventional PCI group and the thrombus-aspiration group and whether there are any significant differences between groups. Could they provide these data?

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- 1 Vlaar PJ, Svilaas T, van der Horst IC, et al. Cardiac death and reinfarction after 1 year in the Thrombus Aspiration during Percutaneous coronary intervention in Acute myocardial infarction Study (TAPAS): a 1-year follow-up study. *Lancet* 2008; **371**: 1915–20.
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## Authors' reply

We appreciate Laurent Bonello and colleagues' comment with regard to the difference in direct stenting between the two treatment groups. This issue is often brought up when the results of TAPAS are discussed.

Our interventional strategy always starts with establishment of brisk antegrade flow in the infarct-related vessel (followed by intracoronary nitroglycerine) to allow selection and placement of a stent of appropriate length and diameter. In TAPAS, thrombus aspiration reduced the source of distal embolisation by removing atherothrombotic material exposed to the lumen. After aspiration, coronary flow was established in most patients. Consequently, balloon predilatation