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The future of coronary heart disease prevention

Editor – It is high time that some of the many wildly incorrect and dissembling assertions currently being made about the Joint British Societies' second (JBS2) extremely reasonable recommendations for cardiovascular disease prevention,[1] which are entirely consonant with the National Institute for Health and Clinical Excellence (NICE) assessment of statins,[2] begin to be corrected before they cause real harm.

It was particularly distressing to find in the recent article by Wald a major inaccuracy (*Clin Med* August 2007 pp 392–6). He claims that JBS2 recommends that statins are introduced when serum cholesterol is 6 mmol/l or above. In reality it recommends that statins are introduced on the basis of cardiovascular disease risk. Generally high cardiovascular disease risk is identified by the presence of an atheromatous complication, diabetes, a genetic syndrome such as familial hypercholesterolaemia or a ratio of total serum cholesterol to high density lipoprotein cholesterol of 6 or more. Only some 5% of men and less than 1% of women would be in the latter catergory,[3] most of whom would develop premature cardiovascular disease.

One is also left to wonder why he believes that a Polypill containing three low-dose blood pressure lowering drugs, a statin (presumably low-dose), aspirin and folic acid is safer than atorvastatin 80 mg or rosuvastatin 40 mg when aspirin 75 mg daily alone is about 100 times more likely to cause a serious side effect than a statin even at its maximum licensed dose.[4-7] Atorvastatin or rosuvastatin in these doses will lower low-density lipoprotein cholesterol by 2.6 mmol/l as Wald correctly states. This translates into a 55% decrease in coronary heart disease and stroke risk.[6]

The Polypill concept is an interesting way to focus debate, but is never likely to be a reality except perhaps in secondary prevention. JBS2 on the other hand provides clear, albeit aspirational, guidance which makes use of paradigms in current clinical practice.

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In response to Warren and Durrington

Warren and Durrington seem to have missed three of the main points in my article:

1 That use of the cardiovascular risk factors as screening tests is an inefficient method of estimating a person's risk of a coronary heart disease (CHD) event. Combining different risk factors to calculate a person's risk over a specified time period, adds only a little precision to simply basing the risk estimation on age alone, and is not worth the extra complexity. While Durrington is correct in indicating that it was not total cholesterol concentration alone that was the basis for risk estimation in the guidelines, this observation does not overcome the problem that risk factors make poor screening tests. The cardiovascular risk prediction charts presented in the JBS2 guidelines[1] (and reproduced in the back of the British National Formulary) confirm this – the change in risk with increasing age greatly exceeds that with increasing serum cholesterol: high-density lipoprotein ratio or blood pressure. Current risk factor based screening should be simplified and replaced by using combinations of treatments to simultaneously lower several causal risk factors, in all people above a certain age or with a prior history of vascular disease regardless of the level of the risk factors.
2 The evidence supporting this approach and the efficacy of the individual Polypill components is substantial.[2] The evidence on the combined effect of all the components includes trial evidence that the effects of each are independent of the others,[3,4] a randomised controlled trial of multifactorial intervention[5] and an analysis of multifactorial intervention in general practice.[6] Polypills have been made for both primary and secondary prevention and are being used in trials, for example in India, New Zealand and Iran.
3 Using the Polypill is, of course, not an alternative to healthy dietary and lifestyle practices such as the avoidance of smoking. The two are complementary. The problem is that in practice only relatively modest changes in risk factors are achieved through changes in diet and other lifestyle habits. I agree with Warren that reducing the salt content of our food to less than 3 g/day is important; it requires a reduction in the salt content of manufactured foods by the food industry. This, too, is complementary to the Polypill approach. I also agree with Durrington that statins are safer than aspirin; I never claimed otherwise in my article.

In rejecting the Polypill concept Warren and Durrington are missing a major public

health opportunity that would have a significant impact in reducing CHD and stroke throughout the world.

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Atrial fibrillation – all change!

Editor – In the valuable review of atrial fibrillation (AF) by Savelieva and Camm (*Clin Med* August 2007 pp 374–9) I was delighted to see emphasis on the value of control of the ventricular rate, presumably to augment ventricular filling, particularly in older subjects where the ventricle is less compliant (the physiological third of ventricular filling sound disappears quite early in life).

I disagree, however, that the awareness of asymptomatic or silent AF has only been recognised recently since 'lone atrial fibrillation' as it was called by Sir John Parkinson and William Evans at the London Hospital in the 1940s was well recognised and regarded as a benign condition if there was no evidence of underlying heart disease, and particularly no enlargement of the left atrium on X-ray screening in the right oblique position. Of course this has been superseded by the echocardiogram. Thus I have had a lifelong interest in lone AF and never used warfarin in such cases because the risk is appreciable though small. In 50 years of special interest in lone AF I have never seen an embolus and have never had to start warfarin in this special group. It seems that there is little risk of clotting from stasis in the left atrium when both the left ventricle and left atrium are completely normal. Sadly, I do not have statistical evidence for this view but I think it would be shared by those who have had a long day-to-day experience of clinical cardiology. Furthermore, I doubt whether such a careful selection of cases is possible in most large statistical studies. Of course the decision not to give warfarin can only be made after a very careful investigation by a cardiologist and a careful echo study. Sadly, I have seen junior cardiologists starting warfarin in a patient with unexplained AF without even performing an echo.

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In response

Editor – Dr Leatham correctly points out that lone atrial fibrillation (AF) (ie, AF associated with no structural heart disease) has long been recognised by the medical community. Indeed, cases of lone AF were described by Parkinson and Campbell in 1930 who found no associated heart disease in 15% of their 200 cases of paroxysmal AF.[1] William Evans and Peter Swann proposed the term lone AF in 1953 in their publication on 20 patients with no evidence of cardiac enlargement, mitral stenosis, hypertension, or thyrotoxicosis.[2] After reviewing the patients' history for over 10 to 20 years, they concluded that 'the condition did not jeopardize life in a single instance, and did not even prove a handicap to the majority'. The 1987 report from the Mayo Clinic concerning 97 patients with lone AF from Olmsted County, who were 60 years old or younger at diagnosis, confirmed that lone AF in this setting is associated with a very low risk of stroke and that routine anticoagulation may not be warranted.[3] In this cohort, four patients (1.3%) had strokes and the overall survival rate was 95% at 15 years. Conversely, lone AF occurring after age 60 years in the Olmsted County population was a risk marker for a substantial increase in cardiovascular events (5.0% *v* 1.3% per person-years) as well as rates of stroke and transient ischaemic attacks (0.9% and 1.1% *v* 0.2% and 0%) compared with non-AF patients that warrants consideration for antithrombotic therapy.[4]

Subsequently, the Framingham study investigators reported significantly greater rates of stroke (more than fourfold) in 43 patients with lone AF compared with matched controls without AF.[5] The most striking evidence that lone AF may not be entirely a benign condition came from the Paris Prospective Study which reported a nearly twofold increase in risk of death and particularly cardiovascular mortality in individuals affected by AF but with no structural heart abnormality.[6]

In 1930, Parkinson and Campbell described AF associated with reversible cardiomyopathy after restoration of sinus rhythm by quinidine[1] and since then several reports have suggest that uncontrolled AF may cause frank congestive heart failure in the absence of any structural heart disease and that upon cessation of the arrhythmia, complete recovery of left ventricular function may follow.[7] Hence the recent National Institute for Health and Clinical Excellence guidelines on AF emphasise the role of echocardiography as an essential part of the clinical investigation of a patient with newly discovered AF.

While lone AF is not associated with any heart disease by definition, silent (or asymptomatic) AF often can occur in association with almost any cardiovascular pathology. The hazard of silent AF lies in the fact that it poses the same risk of stroke or tachycardia-induced cardiomyopathy if untreated, but often goes unrecognised both by the patient and the physician. As a result, many patients may be denied potentially life-saving therapy such as anticoagulation. In the Framingham study, 21 (18%) out of 115 patients with acute stroke had AF discovered for the first time on admission for stroke and five were admitted with sinus rhythm but developed AF after admission.[8] Whether AF in an asymptomatic form was present before stroke or developed as its consequence remains