Prophylaxis against venous thromboembolism

EDITOR,—We are disappointed that the review article on the risk of and prophylaxis against venous thromboembolism in hospital patients does not mention the advantageous effects of afferent blockade for patients undergoing surgical procedures.¹ The stress response to surgery increases angiotensin and catecholamine concentrations, which increase platelet aggregation.^{2,3} Hepatic synthesis of fibrinogen is increased,⁴ while synthesis of antithrombin III is reduced.⁵ Attenuated stress responses can be shown with epidural and spinal analgesia for lower abdominal and more distal surgery⁶ and with paravertebral analgesia for thoracic surgery (J Richardson *et al*, unpublished data). Proved benefits of afferent blockade include increased fibrinolytic activity⁷ and improved thromboelastography.⁸ In addition, lower limb blood flows are raised with epidural analgesia.⁹

These pathophysiological advantages are supported by an improvement in outcome after major surgery in patients managed with regional analgesia as well as, or instead of, general anaesthesia. Not only are venous thromboembolic events significantly ($p < 0.05$) reduced^{7,10} but other events related to coagulation are also favourably affected, including perioperative myocardial infarction^{8,11} and vascular graft patency.⁸

We do not take issue with the value of the mechanical and pharmacological methods described in the review, but the additional benefits offered by regional analgesia for surgical patients deserve emphasis.

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EDITOR,—The Thromboembolic Risk Factors (THRIFT) Consensus Group gives precise recommendations for preventing venous thrombosis.¹ For medical patients the need for prophylaxis with low dose subcutaneous heparin is shown in those with acute stroke and myocardial infarction, but these results cannot be extended to the other groups of medical patients with moderate risk.

In this population—which represents most of our patients—the risk of venous thrombosis is low:

9% for elderly medical inpatients² and 4% for patients without cardiac or pulmonary diseases.³ In this large group of patients the benefit of prophylaxis has never been established by a randomised controlled trial (Halkin *et al*'s study showing a reduction of mortality was an open, non-randomised trial⁴). We recently participated in a double blind, controlled randomised study of 2474 elderly bedridden non-surgical inpatients that compared nadroparin 0.3 ml and placebo. There was no significant difference between the low molecular weight heparin and the placebo in mortality (10.1% and 10.3% respectively) and overall incidence of thromboembolic events (2.9% and 3.9%).

In medical patients without stroke or cardiopulmonary disease the risk of venous thrombosis is probably too low to be significantly reduced by heparin. There is no scientific reason for proposing systematic prophylaxis with low molecular weight or standard heparin in all these medical patients. This costly recommendation has not been correctly evaluated.

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AUTHOR'S REPLY,—Jonathan Richardson and Sabaratnam Sabanathan suggest that venous thromboembolism is less common after major surgery performed with regional anaesthesia compared with general anaesthesia. A recent critical review of comparative studies in hip surgery agreed that "there is good evidence that in patients who do not receive prophylaxis, the incidence of postoperative deep vein thrombosis is lower with regional anaesthesia than with general anaesthesia. However, the incidence is still substantial (29-40%) in the unprotected patient and therefore regional anaesthesia should not be regarded as a substitute for effective primary antithrombotic prophylaxis. Whether the findings in unprotected patients can be extrapolated to patients who receive effective primary prophylaxis remains an open question which can only be answered by performing an appropriately designed randomized controlled trial."¹ We agree with this.

Jean-François Bergmann and colleagues question the use of prophylaxis in medical inpatients who do not have acute stroke, myocardial infarction, or other cardiopulmonary disease. We agree that these three groups make up most patients in acute medical wards in whom prophylaxis is indicated (in the United Kingdom they also make up most patients in acute medical wards). But other groups of patients immobilised in medical wards also merit prophylaxis—those with non-terminal malignancy, infections, inflammatory bowel disease, nephrotic syndrome, or paralysis of the lower limbs (for example, those with Guillain-Barré syndrome). Published work strongly suggests that these groups of patients have a significant risk of venous thromboembolism, and hence prophylaxis is rational.

Studies have consistently shown that prophylaxis with low dose heparin is effective in medical patients who do not have myocardial infarction or stroke.^{2,3} The reduction in mortality in patients

treated with heparin in the study of Halkin *et al* is consistent with such a reduction in the incidence of deep vein thrombosis and was observed in patients with infection or malignancy as well as in patients with cardiopulmonary diseases.⁴ We agree that this study used an unconventional, non-randomised method of allocating cases (even versus odd medical record numbers), but mortality was analysed by intention to treat (being recorded for all even numbered patients whether or not heparin was given), and hence significant bias is unlikely to have resulted.⁵

We are interested to learn that the preliminary results of Bergmann and colleagues' study show a 26% reduction in thromboembolic events but no reduction in total mortality in the group treated with heparin. It is not clear whether patients with cardiopulmonary disease were included in this study: if they were not this may account for the different conclusion of their study from that of Halkin *et al*, in which such patients were in the majority.⁶

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Needs of black and ethnic minorities

EDITOR,—The analysis of the health needs of British ethnic minority groups presented in the 1991 report of the chief medical officer¹ is well meaning but it oversimplifies complex issues, reinforces stereotypes, misses key new ideas, and presents misleading interpretations of data. The report's benefits to ethnic minority groups are likely to be outweighed by the stigma arising from the associated publicity. Let me substantiate these strong criticisms.

The report justifies the term black and ethnic minority because "it underlies a common experience of life in this country among people whose skin colour is not white" (my italics). Why then is the principal focus of the report on cultural and genetic factors, which vary at least as much between ethnic minority groups, as between them and the white community, when the common experience is one of family migration, the struggle for respect, wellbeing, and social position, a too ready alienation from white society, discrimination, and relative poverty? Black makes sense only in the social, not biological or cultural, context. Why is so little attention paid to heterogeneity within the ethnic minority groups when recent work has emphasised the problems created by inappropriate terminology that encourages a false impression of homogeneity,² and has shown striking variation in socioeconomic, lifestyle, and disease patterns within even a Hindu community?³

The report is stereotyped for it reiterates established, much criticised, thinking. It also propagates the dangerous stereotype of ethnic minority groups being a problem. Without any notable

exception the emphasis is on ethnic minority groups having worse health. Even when there is good news—for example, higher immunisation rates^{5,6} or lower mortality from most cancers⁷—this is not acknowledged. The fact is that members of ethnic minority groups are alive, kicking, and whenever possible vigorously contributing to our ailing, recession-bitten community. The overall standardised mortality ratio of most ethnic minority groups is close to the national average,⁷ and the proper interpretation of this requires noting their greater socioeconomic deprivation. For British Punjabis living in Glasgow, in comparison with the general population, health status was better in several respects, comparable in most measures, and worse in others.⁸

The report ignores several powerful critiques of traditional thinking on ethnicity and health.⁹⁻¹¹ Why? The cynical view would be that an emphasis on personal behaviour, cultural factors, and genetics deflects attention from deep and disturbing issues about the structure of society; a strategy also seen in relation to the inequalities in health debate.

The misleading interpretations are too many to discuss so I shall merely observe that:

- The study of ethnicity and health is far from new in this country and a vast literature exists,⁹⁻¹¹ so ignorance is no excuse for past inaction;
- The lumping together of “Asians” to calculate standardised smoking and drinking ratios is inappropriate when there are huge variations between men and women, and between “Asian” subgroups; the result is not of theoretical or practical value;
- The problems of health education or health promotion is not simply one of appropriate presentation, but of availability of relevant materials¹⁵;
- Most importantly of all, an effect strategy for assessment of health needs cannot be achieved by concentrating on differences between groups but requires a sound understanding of the priorities of each group.¹⁶ The comparative analysis is of value only for fine tuning the development of services and for hypothesis generation.

As one of the few “black” members of a health authority I shall not be recommending this report to my fellow officers. The principles which I shall recommend are not based on the flawed analysis of differences but on whether acceptable standards of care are achieved and on actual, not relative, priorities.¹⁶

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Serum cholesterol and mortality in Finland

EDITOR,—Minerva accepts too uncritically¹ Pekkanen *et al*’s view of the results of their 25 year follow up of the Finnish cohorts in the seven countries study.² She states that “in the end high cholesterol predicted both high mortality from heart disease and high overall mortality.” Pekkanen *et al* show, however, that over the 25 years men with cholesterol concentrations below the lowest quartile (below 5.8 mmol/l) had a lower risk of death due to coronary heart disease and a higher risk of death from other causes, giving an all cause mortality that was similar to that in men with higher concentrations. Furthermore, during the first 10 years men with cholesterol concentrations below the lowest quartile had the highest relative risk of death from causes other than coronary heart disease.

Minerva also comments that men with high serum concentrations of cholesterol had low overall death rates in the first 10 years because relatively few died of cancer, although the reported figures show that the most significant deficit in relative risk was for causes other than cancer, stroke, and coronary heart disease, which presumably includes murder, suicide, and accidental death.

In the same issue of the *BMJ* George Davey Smith and Andrew N Phillips’s persuasive paper on confounding in epidemiological studies recommends that “the first responsibility of [epidemiological] investigators should be to retain the proper degree of caution when interpreting and discussing their findings.”³ Pekkanen *et al* show no such caution, for despite the absence in their data of any evidence that altering serum cholesterol concentration improves the quality or the duration of life of men aged 40 to 59, they conclude that community wide efforts to lower cholesterol concentrations in a whole population are probably the most effective, safest, and cheapest way of reducing total mortality.

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Adolescent perpetrators of sexual abuse

EDITOR,—As the coordinator of a social services project working with adolescent perpetrators of sexual abuse, I was interested in the personal view written by someone whose daughter had been abused.¹ It is possible that, had the social services department mentioned in the article followed the guidelines set out in *Working Together*² and held a case conference about the perpetrator, more progress might have been made.

Our programme offers assessment and intervention work to a wide range of young abusers, sometimes after court appearances but more often after a case conference. The case conference gives us the chance to explain to the parents and the young person the concerns about a young perpetrator and to offer an assessment. In our experience (we have seen over 60 young people) many families welcome this and are willing to allow us to carry out a six to eight session assessment.

Surprisingly, perhaps, we are often able to break down at least some of the denial during this period, although the level of parental denial and avoidance is clearly crucial. We have had only one instance of refusal to undergo assessment after a case conference (in this case, for various reasons, court action was not possible). In the more relaxed atmosphere of the assessment sessions, as opposed to the formal atmosphere of a police interview, adolescents are much more ready to disclose their sexual behaviour. This then enables work to be done to help the perpetrator take responsibility for his or her actions and, at the least, enables the risk of further offending to be assessed and reported back to the case conference.

The article clearly points out the need to take adolescent offending seriously. Many adolescents continue to offend as adults, and we don’t yet know how to identify those who will stop. Common sense, as well as information from adult abusers themselves, suggests that the longer abuse continues without disclosure the more likely it is to become an addictive cycle of behaviour and thus to continue into adulthood. Decisions about whether young people who have abused others may pose serious risks later should be based on an assessment. This sort of information and discussion is best handled in the multiagency forum of a case conference.

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EDITOR,—I am a general practitioner and happily married with two children, and I wonder what healthy sexual development is. I recall, when I was 4-5 years old, my girl friend and I stuffing leaves into our “botties,” while another game as a child involved intricate investigations with a knitting needle. Overnight at 11 years of age the opposite sex became very interesting, and we played “kiss, cuddle, or torture.” We soon learnt that the torture was the most interesting—either a grope up the vest to find budding breasts or a poke down the pants. Nobody seemed to come to any harm, though I didn’t like being pursued by one particular older, bigger boy—I feared he might want more from this game. I describe these events as I think they must all be part of normal physical, emotional, and sexual development. But when does the normal become the perverse? An incident within my family makes me wonder.

One day my daughter—about 4 years old and already playing a healthy game of “doctors” under a rug with her girl friends—told me that her brother, 11 years older, had given her some sweets, looked at her bottom in the bath, and told her not to tell. I immediately told him that he shouldn’t be doing this.

A year or two later I left my son to babysit his sister. I returned home, and my daughter soon told me that her brother had given her a big bag of sweets and some money. When I asked why (it wasn’t usually his nature to be so generous) she described how he had looked at her again and had