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Antiemetics, Prolactin, and Breast Cancer

SIR,—The letters from Dr. H. W. C. Ward (20 July, p. 169) and Dr. M. O. Thorner and others (17 August, p. 467) which recommend that drugs raising the circulating plasma prolactin level should not be given to patients with breast cancer seem premature. There is no evidence that prolactin levels are raised in patients with breast cancer,¹ nor that this hormone causes an increase in the rate of growth of breast cancer in human beings. In certain animal models prolactin does increase the rate of growth of the tumour,² but results obtained with these models are not necessarily applicable to human disease.³ Attempts to control the rate of growth of breast cancer by suppressing prolactin secretion have for the most part been unsuccessful,^{4,5} and patients on long-term phenothiazine therapy do not have a higher incidence of or death rate from breast cancer.⁶ The association of breast cancer with reserpine therapy has by no means been established.⁷

For these reasons we doubt that the evidence is at hand to justify predictions about the effect of antiemetics on the growth of breast cancers. The question may be worthy of study by an appropriate trial to compare cyclizine with chlorpromazine. Until evidence is at hand which relates directly to this question, however, it seems inadvisable to discontinue a drug of proved value with which we have had many years of experience in favour of a drug whose potential influence on cancer of the breast in humans is totally unknown.—We are, etc.,

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Larrey and Débridement

SIR,—I am afraid that I must maintain that the extracts I gave from Larrey which summarized his principles of treatment of war wounds are fair and accurate (14 September, p. 686) and that Dr. R. G. Richardson's letter (28 September, p. 806) pointing out that he excised the bruised edges of wounds of the face before suturing or that he cut away bits of tendons and muscle sticking out of wounds in no way weakens my thesis that Larrey did not, and indeed could not be expected to, practise debridement as we understand it today.

Modern debridement (the word has been part of English surgical language long enough to justify the dropping of the French acute accent) was born in the first world war out of the discovery that the then current antiseptic treatment of wounds was useless in wounds resulting from modern high explosives and that, to take the most striking example, wide excision of dead and dying muscle was necessary to prevent the disastrous results of infection by the anaerobic bacteria of gas gangrene. French surgeons naturally knew the origin and traditional surgical meaning of the term; British surgeons in general did not, but not surprisingly tended to derive it from the already anglicized "debris." This was a fortunate error since the two derivations together covered in one word both aspects of modern surgical practice—incising to relieve tension and the removal of tissue debris serving as a potential culture medium for bacteria.

Between the wars the principles of debridement became somewhat obscured, largely from a tendency to substitute the term "excision." This, by stressing the act of cutting away rather than what should be cut away, led to unnecessary sacrifice of healthy tissue, particularly skin; it also led to the idea that the object of the procedure was the excision not just of dead but of potentially infected tissue, and from this developed a variable but fallacious series of rules limiting the procedure to certain time intervals after the receipt of the wound. The full surgical implications of the principle were indeed not appreciated until the antibiotic era, when it was found that even specific antibiotics might not deal with infection in the presence of a focus of necrotic tissue.

May I make two final points? The greatness of Larrey is beyond dispute and is in no way diminished by the recognition that he was also a child of his time. Secondly, though as a surgeon I have queried one sentence, I read Dr. Richardson's book with great profit and pleasure.—I am, etc.,

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Effects of Posture on Limb Blood Flow in Late Pregnancy

SIR,—We thank Professor J. W. Downing and Mr. A. Singer (17 August, p. 470) for their interest in our article (15 June, p. 587).

We are aware that some degree of compression of the lower aorta may occur in the supine subject in late pregnancy but feel that the circulatory consequences are much less than those of inferior vena caval occlusion. The patients studied by Eckstein and Marx¹ (who were all in labour) had only mild reductions in femoral arterial pressure in the supine position and it is unlikely that a reduction of perfusion pressure by this means alone would lead to the marked reductions in leg flow reported by us.

Flow through a limb during partial venous occlusion is reduced by a rise in venous pressure that leads to a lowered perfusion pressure. Inflation of a venous occlusion cuff will allow a further temporary venous occlusion until the venous pressure exceeds the occlusive pressure exerted by the cuff. When the venous pressure reaches

this point venous outflow will recommence, further venous distension will cease, and no further increase in volume will occur. This state would be achieved more rapidly if venous compliance were less at these pressures or if some venous congestion were already present. In all our measurements an identical cuff inflation pressure was employed and it is thus most unlikely that flow measurements would be affected by alterations in venous compliance unless the volume-pressure relationship of the vessels is markedly non-linear for pressures below 40 mm Hg. In the course of our experiments, an increase in limb girth was often evident when the patient moved into the supine position, but the rate of increase of limb distension was as linear as that observed in the lateral position.

It has been shown² that substantial falls in cardiac output may occur in late pregnancy when the patient lies supine, and large increases in femoral venous pressure are found in this position. These falls in cardiac output are usually not accompanied by evidence of increased sympathetic activity, such as tachycardia. If cardiac output and lower limb blood flow are reduced *pari passu*, why is it necessary that a fall in cardiac output should be accompanied by a reduction in blood flow to the upper limbs, as Mr. Singer suggests?

In spite of our differences with your correspondents, we are pleased that we all agree on the benefits of avoiding the supine position, especially in operative obstetrics.—We are, etc.,

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¹ Eckstein, K. L., and Marx, G. F., *Anesthesiology*, 1974, 40, 92.

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How Significant is Persistent S-T Segment Depression?

SIR,—It has been accepted, perhaps too readily, that S-T segment depression can be used to allow a linear quantitative assessment of the severity of myocardial ischaemia.¹ However, this concept has never been proved and, because of this and also the poor reproducibility of such changes, its use in the investigation of the efficacy of antianginal agents was rejected by the King's College Hospital report last year.² The factors which predispose to S-T segment depression and its perpetuation in susceptible anginal patients are ill-understood. Support for the rejection of the use of this parameter in the quantitative assessment of anginal patients is provided by the following observations.

Before coronary sinus catheterization and femoral arterial sampling an anginal patient was asymptomatic and the modified V₅ E.C.G. record³ was normal (fig. 1a). The onset of spontaneous, typical anginal symptoms was paralleled by plane S-T segment depression (fig. 1 b) and a complete reversal of the normal myocardial lactate extraction to -0.9 mg/100 ml, confirming the presence of myocardial ischaemia.³ Sublingual trinitrin (0.5 mg) was administered to relieve symptoms. Five minutes