to likely ways in which heart disease could be prevented by alteration in diet. Drs. Ball and Turner give the impression, too, that there is a fairly broad consensus in the dietary recommendations. This is just not true. Some authorities but not all recommend a reduction in cholesterol; some but not all a reduction in dietary fat; some but not all an increase in polyunsaturated fat; some but not all a reduction in saturated fat. Even within the reports there are sometimes important differences in the opinions expressed by individual members of the committees producing them.

The truth is that very experienced and respected research workers continue sincerely to hold different and even contradictory views. Any statement that suggests otherwise, whether from those in the health profession or from the food industry, is unwarranted and misleading.—I am, etc.,

JOHN YUDKIN

Consistent Chromosome Abnormalities in Acute Leukaemia

Sin,—May I refer to the letter from Drs. K. P. Ball and R. W. D. Turner (21 September, p. 740)?

Since 1956,1,2 and more particularly in a recently published work,3 I have related coronary disease to the consumption of the refined carbohydrates, including such examples as white flour and even beer, all of which can raise the blood glucose to higher levels than would occur with natural, unrefined carbohydrates.4 If this is done the association of coronary disease with obesity, and especially with diabetes, falls into place naturally, and the whole subject becomes greatly simplified; whereas relating the disease only to the consumption of table sugar (sucrose) fails, just as it fails in the case of obesity, and for that matter diabetes.5

Dr. Ball and Dr. Turner would have us to consume much less animal fats, which (including milk fats) are of very ancient lineage, dating not only from biblical times6 but also from neolithic times even more remote; and the same holds true for us to eat, instead, much more polyunsaturated fats such as are present in sunflower seed and other vegetable oils, most of which were never seen as such on this planet until the invention of the hydraulic press and the new solvent procedures. This evolutionary incongruity presents a perpetual challenge to the logic of any substitution in fats on the above lines. No grass-feeding or other type of feeding in animals, as I try to show in my work, removes this incongruity either.

But quite apart from this, the present coronary explosion has not been preceded by any marked increase in fat consumption, least of all as regards animal fats, whereas it has been preceded by an enormous increase in sugar consumption, all of which I have also tried to show from many sources.

Dr. Samuel Johnson, in a well known passage, said: I.W. was nothing but the concentrated essence of a man's thoughts so much as the knowledge that he is going to be hanged in the morning. The incidence of coronary disease in westernized countries is now of epidemic proportions, and among these there is a quarter of all deaths, and, alas, our own profession is affected about as much as any other. It is therefore desirable for each one of us to concentrate his thoughts on the possible causative factors and use his own judgment thereon. For our lives are at stake on the decision. And I submit that the more closely we adhere to natural (=evolutionary) principles and the less we try to be clever at nature's expense, the safer we shall be.—I am, etc.,

T. L. CLEAVE

Catisfield, Fareham, Hants

2 Cleave, T. L., Fat Consumption and Coronary Disease, Bristol, Wright, 1957.
3 Cleave, T. L., The Sarcodise Disease, Bristol, Wright, 1974.
6 Deaveronomy, xxii, 14.

Consistent Chromosome Abnormalities in Acute Leukaemia

Sin,—Chromosomes of patients suffering from acute leukaemia have been studied in many different laboratories1,2 but no consistent abnormality has been found. Clonal cytogenetic abnormalities have been detected in only 30-45%, of our patients,3 and though particular abnormalities were characteristic of each patient in relapse in no case was there a correlation between the type of abnormality and the type of disease or with the length of the remission period. Cells of patients in remission always appeared to have a normal 46 chromosome complement.

Recently we have examined bone marrow samples by an adaptation of Seabright's trypan/Giemsa banding technique4 and have obtained more critical data. The chromosomes of three patients (A.M., T.K., I.W.) with acute myeloblastic leukaemia have been examined by this technique and the results are shown in fig. 1 and in the table. Cells of patients A.M. and T.K. were examined while they were in haematological remission and had been treated with cytotoxic drugs for two and 12 months respectively. The cells of I.W. were examined in his initial haematological relapse before any drugs had been administered. With banding techniques all three patients exhibited similar cytological pictures: trisomy 9 was apparent and one member of the chromosome 21 pair was missing or altered in all three patients. One chromosome 8 was missing in A.M. and T.K. Since I.W. showed similar abnormalities to A.M. and T.K. these abnormalities had not been induced by cytotoxic drugs.

Since observations are interesting for two main reasons—firstly, because they indicate that acute leukaemias do manifest consistent cytogenetic pictures and, secondly, because they show that patients in haematological remission can still retain cytogenetically aberrant cells. They thus suggest that acute leukaemia is not "cured" when the patient is in haematological remission but that, as in chronic granulocytic leukaemia, the disease still persists.

In one patient with acute myeloblastic leukaemia Jonasson et al.6 demonstrated a clone of cells which were 47+8 and pointed out that such 47+8 clones have also been found in sideroblastic anaemia, a condition often considered to be pre-leukaemic. We have not found any +8 clones in our material, and yet such clones might be generated if a 46—8+9 cell was formed by non-disjunction (fig. 2). In their study of an adult patient with myelomonoblastic leukaemia Rutten et al. observed a clone of cells which were 47+9. Neither of these authors, however, studied cells with only 46 chromosomes by banding.

NORMAL CELL

(46) 8,8,8,9

SINGLE

NON-DISJUNCTION

(46) 8,8,8,9,9

(46) 8,8,8,9,9,9

DOUBLE

NON-DISJUNCTION

(46) 8,8,8,8,9

(46) 8,8,8,8,9,9

(46) 8,8,8,9,9,9

(46) 8,8,8,9,9,9,9

Fig. 2.—Abnormal ratios of number 8 and 9 chromosomes derivable by single or double nondisjunction events.

Our results and the others mentioned suggest at this stage that the important cytogenetic picture may be one of an imbalance between the number 8 and number 9 chromosomes in the leukaemic cell, and it is interesting that the number 9 chromosome has also been implicated in chronic myeloid leukaemia.7 It may also be significant that chromosome 21 appears to be involved in acute leukaemia and also in Down's syndrome (trisomy 21), which shows a much increased incidence of leukaemia.—We are, etc.,

JUDITH H. FORD

SALLY M. PITTMAN

FREDERICK W. GUNZ

Karyotypic Cells from Derivatives of Bone Marrow or Peripheral Blood in Three Patients with Acute Myeloblastic Leukaemia

<table>
<thead>
<tr>
<th>Patient</th>
<th>Diagnosis</th>
<th>Condition</th>
<th>Karyotype (Sample)</th>
</tr>
</thead>
<tbody>
<tr>
<td>A.M.</td>
<td>A.M.L.</td>
<td>Remission</td>
<td>45.XY, 8, 8, 9, 9, 21</td>
</tr>
<tr>
<td>T.K.</td>
<td>A.M.L.</td>
<td>Remission</td>
<td>45.XY, 8, 8, 9, 9, 21</td>
</tr>
<tr>
<td>I.W.</td>
<td>A.M.L.</td>
<td>Remission</td>
<td>45.XY, 8, 8, 9, 9, 21</td>
</tr>
<tr>
<td></td>
<td></td>
<td>Remission</td>
<td>46.XY, 8, 8, 9, 9, 21</td>
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<td></td>
<td></td>
<td>Remission</td>
<td>46.XY, 8, 8, 9, 9, 21</td>
</tr>
</tbody>
</table>

*Estimate: 30-50% of metaphases.
Antiemetics, Prolaclin, 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 surgically removing the 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 depression 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 cancer. This 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.,

PHILIP K. BONDY
TREVOR J. POWLES
Institute of Cancer Research,
Royal Marsden Hospital,
Sutton, Surrey


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 débridement as we understand it today.

Modern débridement (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 year 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 débridement 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 put away, led to unnecessary sacrifice of healthy tissue. This may not only have been due 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 times 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 quenched one sentence, I read Dr. Richardson's book with great profit and pleasure. I am, etc.,

Hythe, Kent

DAVIDPATEY

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 circumstances concerned are not 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 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 to increase 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 diastolic venous pressure 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 capacitance was 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 shown3 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 correspondent, we are pleased that we all feel strongly in the interests of avoiding the supine position, especially in operative obstetrics.—We are, etc.,

G. B. DRUMMOND
D. B. SCOTT
MARTIN M. LEES
University Department of Anaesthetics,
Royal Infirmary,
Edinburgh

3 How Significant is Persistent S-T Segment Depression?

Sir,—It has been accepted, perhaps too unreservedly, that S-T segment depression can be used to allow a limited quantitative assessment of the severity of the 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 is 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 asymmetrical and the modified V5, E.C.G. record3 was normal (fig. 1d). The onset of spontaneous, typical anginal symptoms was provoked 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.4 Sodium nitritin (0·5 mg) was administered to relieve symptoms. Five minutes