

LEADING ARTICLES

Death after bereavement BRIAN R McAVOY	835
Hyperplastic anaemia and parvovirus infection D P BENTLEY	836
Normal pressure hydrocephalus MILNE ANDERSON	837

CLINICAL RESEARCH • PAPERS AND SHORT REPORTS • PRACTICE OBSERVED

Splenic irradiation in treating warm autoimmune haemolytic anaemia H MARKUS, J C FORFAR	839
Metabolic abnormalities in children of non-insulin dependent diabetics R D G LESLIE, H P VOLKMANN, M PONCHER, I HANNING, H ORSKOV, K G M M ALBERTI	840
Asthma and climatic conditions: experience from Bermuda, an isolated island community M J CAREY, I CORDON	843
Mortality of workers at the Sellafield plant of British Nuclear Fuels P G SMITH, A J DOUGLAS	845
Right superior vena cava draining into the left atrium with left superior vena cava draining into the right atrium D LEYS, J MANOUVRIER, T DUPARD, P KASSIOTIS, C REY, P MARACHE, G DUCLOUX, H PETIT	855
Munchausen's syndrome by proxy: a warning for health professionals LAWEH AMEGAVIE, OMNIA MARZOUK, JOHN MULLEN, JOHN SILLS, JEAN B M GAUTHIER LE TENDRE	855
Blowing hot and cold: "near miss" sudden infant death and episodic hypothermia K P DUNNE, M MCKAY, T G MATTHEWS	856
Correction: Cigarette smoking BARON ET AL	856
Effect on prescribing of the limited list in a computerised group practice W G IRWIN, K A MILLS, K STEELE	857
Role of general practitioners in the care of disabled young adults F S W BRIMBLECOMBE, D L KUH, C J LAWRENCE, R C SMITH	859

MEDICAL PRACTICE

Injuries of the spine sustained during gymnastic activities J R SILVER, D D SILVER, J J GODFREY	861
Sir Harry Platt: 100 not out RICHARD SMITH	864
Private nursing homes: contribution to long stay care of the elderly in the Brighton Health District J BENNETT	867
Cytotoxic chemotherapy for common adult malignancies: "the emperor's new clothes" revisited? JOHN H KEARSLEY	871
Any Questions?	866, 870, 876
Clinical curio—Spurious ventricular tachycardia M B BUCHALTER, E T WILLIAMS	866
Medicine and Books	877
Personal View BENJAMIN LEE	880
Correction: Use and misuse of a digoxin assay service GIBB ET AL	876

CORRESPONDENCE—List of Contents

881

OBITUARY

893

NEWS AND NOTES

Views	889
Medical News	890
BMA Notices	891
One Man's Burden MICHAEL O'DONNELL	892

SUPPLEMENT

The Week	895
Survey of recently appointed consultants in geriatric medicine WILLIAM H BARKER, JAMES WILLIAMSON	896
LMC scheme to help sick doctors	899

CORRESPONDENCE

Immunity and depression					
J C C Chase, MRCPsych	881	A complication of capillary glucose monitoring		Salaries of clinical academic staff	
AIDS: act now, don't pay later	882	I N Scobie, MRCP; I Martin-Scott, MD; N I Jowett, MD	884	J P Payne, FFARCS	886
M W Adler, FRCP; D Bhugra, MB	882	Clinical judgment rather than administrative directives	884	Points	
W F G Tucker, MB, and Sheila MacNeil, PhD	882	C R Hart, FRCP	884	Skin reactions to terfenadine (H C Masheter and M St G Wheeley); The perfect rabies vaccine (D T Langford and W M Shepherd); A case of scurvy (S Vallance); Obstetric anaesthetic services (W G Dawson; P F Franks and others); Obstetric care, social class, and maternal mortality (G J A Walker); Crisis in hospital pharmacy (J Maxwell); Epidemics of fractures during periods of snow and ice (S P Cembrowicz)	887
Discrimination against singlehanded general practitioners	882	Continuous ambulatory peritoneal dialysis fluid: another fluid positive for HIV antibody?	885	Routine preventive measures and failure to attend appointments (J A C Winter); Heart disease in China (D C Snashall); A rise is a rise is a rise (I R Fletcher); Women in medicine (Neena Modi); Effect of biofeedback on patients' tolerance of fiberoptic bronchoscopy (C R Swinburn); Staying one jump ahead of the resistant <i>Staphylococcus aureus</i> (B S Azadian and D J M Wright; T H Pennington); Treatment of high blood pressure: should clinical practice be based on results of clinical trials? (G Rose); Medical defence subscriptions (I U Haq)	888
M Levy, MRCP, and R Pietroni, MRCP	882	Incidence of rhesus immunisation after genetic amniocentesis	885	Correction: AIDS (Van de Perre, Kestelyn, Sprecher)	888
Programme for early detection of gastric cancer		L A D Tovey, FRCPATH	885		
P W J Houghton, FRCS, and others	883	New method of typing <i>Staphylococcus aureus</i> resistant to methicillin	885		
Manpower and women		Georgia Duckworth, MRCP	885		
Susan M Benbow, MRCPsych	883	Medical housing "lines"	885		
Epidemics of fractures during periods of snow and ice		H S Kohli, MRCP	885		
Jill Meara, MB, and others	883	Recommendations on blood pressure measurement	886		
Bias in awarding research grants		I R Starkey, MRCP	886		
J G Ball, FRCP	883	Severe head injury: the first hour	886		
A case of scurvy		M Adiseshiah, FRCS	886		
M W P Carney, FRCPsych	883	Obstetric anaesthetic services	886		
The perfect rabies vaccine		D A Buxton Hopkin, FFARCS	886		
G Harverson, FRCR	884	Use of captopril in insulin dependent diabetics	886		
When things go wrong		D B Northridge, MRCP, and D M Fraser, FRCPE	886		
F J Darby, MRCP	884		886		
Prognosis of patients discharged from a coronary care unit					
M C Bateson, MD	884				

Because we receive many more letters than we have room to publish we may shorten those that we do publish to allow readers as wide a selection as possible. In particular, when we receive several letters on the same topic we reserve the right to abridge individual letters. Our usual policy is to reserve our correspondence columns for letters commenting on issues discussed recently (within six weeks) in the *BMJ*.

Letters critical of a paper may be sent to the authors of the paper so that their reply may appear in the same issue.

We may also forward letters that we decide not to publish to the authors of the paper on which they comment.

Letters should not exceed 400 words and should be typed double spaced and signed by all authors, who should include their main degree.

Immunity and depression

SIR,—I am concerned that Dr A M Denman concludes that there is little evidence of appreciably altered immunity in depressed subjects (23 August, p 464).

Physicians and psychiatrists who suspect that the mind and body may be more closely linked than we have yet been able to show will no doubt find this interesting. It is therefore surprising that there are only three studies of cellular immunity in depressed subjects. Of these, Dr Denman has quoted the work of Schieffer *et al* and Albrecht *et al*^{1,2} but not the findings of Kronfol *et al*.³ This is especially important as Kronfol and workers did find decreased proliferative responses in subjects who were less depressed than the patients of both Albrecht *et al* and Schieffer *et al*. This would argue that severity of depression alone cannot explain the findings. Any inference that might be drawn from the negative findings of Albrecht *et al* should be tempered by an awareness that their subjects were drug free for only one week.

A further criticism levelled at these studies was that they did not take into account diet, treatment, and life style. However, as with the earlier work of Bartrop *et al* and Schieffer *et al* on bereaved subjects,^{4,5} they did consider drug intake. The part that diet, sleep disturbance, and exercise play in immunocompetence has been poorly tested. When done it was concentrated on extremes,^{6,8} which are bound to cause physiological responses quite different from the disturbances that might be

expected in a moderately depressed subject. The problem is undoubtedly complex, but when found, lymphocyte depression has not correlated with dexamethasone treatment.² Recent work on the relation of immune function to sleep in man describes changes in natural killer cell and interleukin activity,¹³ both of which seem to be independent of the diurnal cortisol rhythm. We clearly need to know a great deal more about the impact that sleep disturbance would have on subjects with morbid mental states.

Dr Denman's insistence that future study should analyse the responses more closely linked to relevant host defence is justified. To date no one has looked at more specific antimicrobial measures of immunity. In depressed subjects there has been only one study of subset ratios,² and its findings were negative. However, several studies have shown alteration in natural killer cell activity and altered ratios of helper to suppressor cells in times of stress.⁹⁻¹² Although anxiety and depression were not measured in these studies, one might well speculate about the Hamilton rating scale scores of such subjects.

It is clearly correct to question the pathophysiological relevance of depressed proliferative responses to mitogens. To imply that "there are those who believe that altered immunity predisposes to depression," however, is to misrepresent the views of those working in this subject. The work of Murphy and Brown¹⁴ amply demon-

strates the caution with which psychiatrists approach the subject, and few of them suggest specious explanations for psychosomatic illness.

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