

# CORRESPONDENCE

Correspondents are asked to be brief

<b>Seat Belts and Head Rests</b> W. Gissane, F.R.C.S. .... 288	<b>Congenital Absence of Carpal Scaphoid</b> P. Papanikolaou, M.D., and M. A. Haddadin, M.B. .... 292	<b>Bullous Lesions in Poisoning</b> B. I. Hoffbrand, D.M., and Constance M. Ridley, M.R.C.P. .... 295
<b>Craniopharyngiomas</b> H. J. G. Bloom, F.R.C.P., and C. L. Harmer, M.R.C.P. .... 288	<b>Coalminers' Pneumoconiosis</b> P. D. Oldham, D.Sc., and G. Berry, M.A. .... 292	<b>Chinese Burn</b> B. S. Milner, M.B. .... 295
<b>Research in Psychiatry</b> E. J. M. Bowlby, F.R.C.P., and others; W. McC. Harrowes, M.D., F.R.C.P.E.D.; N. B. Malleon, M.D. .... 289	<b>Interviews for Prospective Students</b> T. C. Dann, M.D. .... 293	<b>Cardiac Arrhythmias during Laparoscopy</b> T. R. Morley, F.F.A. R.A.C.S. .... 295
<b>Chondromalacia Patellae</b> H. J. Burrows, F.R.C.S. .... 289	<b>Endocrine and Metabolic Manifestations of Cancer</b> E. J. Ross, F.R.C.P. .... 293	<b>Erythropoietin</b> R. D. Lange, M.D. .... 296
<b>Intelligence and Fertility</b> T. McKeown, F.R.C.P.; M. A. Simpson, M.B.; E. Alwyn Smith, Ph.D., M.R.C.P. .... 289	<b>Cytomegalovirus Oesophagitis</b> P. J. Toghill, M.D., and May McGaughey, M.B. .... 294	<b>Methotrexate Hepatotoxicity in Psoriasis</b> T. J. Ryan, M.R.C.P., and others .... 296
<b>Leucocyte Migration in Chronic Hepatitis</b> C. G. Vergani, M.D., and others .... 290	<b>Tropical Splenomegaly, Sickle-cell Trait, and P. falciparum Infection</b> D. I. K. Evans, M.R.C.P.E.D., and others .... 294	<b>Pulmonary Disability in Coal Workers' Pneumoconiosis</b> G. B. Murray, D.P.H. .... 296
<b>Publication of Conference Proceedings</b> H. S. Gear, M.D. .... 290	<b>Duodenal Ulcer and Gastric Cancer</b> M. Macleod, M.B. .... 294	<b>Sustained Relief of Hemiballismus</b> I. P. Rowlands, M.R.C.P. .... 296
<b>Hazard of Immunosuppressive Therapy</b> N. Manny, M.D., and others .... 291	<b>Multiple Crashes on Motor Ways</b> Mary S. Christian, F.R.C.S. GLASG. .... 295	<b>Alginate Casting Method for Recording Dermatoglyphs</b> Margaret E. Sands, B.Sc. .... 296
<b>Diagnostic Abdominal Paracentesis</b> S. F. O Beirn, F.R.C.S.I. .... 291	<b>Monitoring Heparin Infusions</b> M. R. Klaber, M.B. .... 295	<b>Hospital Staffing</b> J. J. Shipman, F.R.C.S., and others .... 297
<b>Payment by Colour</b> J. K. McKechnie, F.R.C.P.E.D.; A. S. Trus- well, M.D.; Yolande M. Friedl, M.B. .... 291	<b>Need for Continued Oral Therapy in Diabetes</b> J. W. Todd, F.R.C.P. .... 295	<b>Superannuation</b> F. W. Johnson, M.B. .... 297
<b>Imipramine in Pregnancy</b> E. V. Kuenssberg, M.B., and J. D. E. Knox, F.R.C.P.E.D. .... 292		<b>Cash for Hospitalization</b> P. A. Draper, M.B. .... 297
		<b>A Junior View of the S.H.M.O.</b> R. W. Griffiths, M.B. .... 297

## Seat Belts and Head Rests

SIR,—Surely the available evidence shows that seat belts and head rests have two distinct protective functions, and either one does not distract from the value of the other (15 April, p. 163)?

In car collisions occupants are thrown towards the area of the initial major impact of their vehicle—that is, backwards in rear end collisions and forwards in frontal collisions. People examined following rear end collisions generally remember the violent contact of their backs against their seats' back rests, but no-one so far has been able to tell me what happened to his head or neck. All but two were in seats without head restraints and all had various degrees of soft tissue neck injuries. Two, in a car with built-in head restraints, were wearing seat-incorporated three-point belts; both experienced heavy impacts as their backs hit the seat's back rest, but both were unaware of head impacts against their well-padded head restraints. Yet the metal supports of both restraints were bent at angles that under subsequent tests corresponded to a head impact force of over 150 lb (68 kg). Neither suffered neck injuries.

This evidence strongly suggests that the term "whiplash" is a misnomer, and that neck injuries following rear end collisions are caused by the initial and often severe degrees of neck hyperextension.

After extensive clinical experience I have yet to encounter a fracture or fracture dislocation of the neck or an injury to the cervical cord following uncomplicated rear end collisions, though I have little doubt they can occur unless prevented by head restraints. In addition a study of over 500 necropsy reports has not revealed such severe neck injuries following uncomplicated rear end collisions. Yet the same necropsy

evidence has shown (in addition to other injuries) a 12% instance of fractures and fracture dislocations of the neck with and without cord involvement following frontal collisions in non-belt wearers. These we have attributed to occupants being thrown forwards and their heads violently striking various car structures in front of them.

This evidence does not substantiate Dr. I. W. Caldwell's thesis that "in many situations . . . the wearing of seat belts is positively dangerous unless supported by . . . head rests." However, one must agree that these and other proved features in protective car design are long overdue for legislative action. The value of enforced legislation for protective car design has been proved in the United States, where the benefits have been shown to far exceed the costs. Indeed, if the estimated high costs of their proposed "clean-air" exhaust and air-bag restraint systems are discounted, the cost of damage limiting car design is indeed modest.—I am, etc.,

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## Craniopharyngiomas

SIR,—We read with great interest your leading article on craniopharyngiomas (25 March, p. 764), but would disagree with the writer's conclusion regarding treatment that "near total removal should be the aim . . . despite the subsequent endocrinological and electrolyte problems." In the series of 50 children which he quotes,<sup>1</sup> there were, in fact, 43 patients who were described as suffering from diabetes insipidus as a post-operative complication, 10 with persistent

visual loss, nine with persistent hypernatraemia, six with a temporary "lobectomy" effect, five with convulsions, four with meningitis, and one with C.S.F. rhinorrhoea; five died in the postoperative period. The series reported by Bartlett<sup>2</sup> is also quoted by your writer as confirming "that the best results are obtained after radical surgery." In this study there were only 20 of 73 patients (27%) surviving 10 years with the possibility of a maximum 10-year survival rate of 43%. In view of these results the alternative treatment to radical surgery—conservative operation and postoperative radical radiotherapy—would seem at least worthy of discussion.

All the article goes on to say is that "the efficacy of radiotherapy remains in doubt," quoting as a reference for this statement the publication of Kramer *et al.*<sup>3</sup> Kramer and his colleagues in fact conclude: "these tumours are eminently suitable for irradiation therapy in the treatment of craniopharyngioma"—which is hardly supportive evidence for your article's view point. Further, a more recent and important publication<sup>4</sup> goes unmentioned. In 26 previously untreated patients there was only one recurrence after radiotherapy; none of the long-term survivors have shown any added disability attributable to radiotherapy.

It is difficult to assess the value of treatment in the absence of precise survival rates. Matson, for example, refers to 44 of 57 children as being alive, but further details are not given. We are currently assessing the results of radiotherapy in a series of 100 patients of all ages with primary or recurrent craniopharyngioma treated at the Royal Marsden Hospital. Both the length of survival and quality of life appear to be best in those cases treated by a combination of conservative surgery (cyst evacuation and biopsy only) followed by

radical irradiation. The 5 and 10-year crude survival rates for cases treated in this way are 85% and 72%, respectively. Over 90% of the patients were in good general health with no serious neurological or mental disability at two years; vision was good or useful in 82%.

For patients treated in the most recent period using a 6 MeV linear accelerator, the 3-year survival rate for 31 patients was 94%, while at five years 11 of 12 cases (92%) were still alive and well. These early results for radiotherapy, together with those quoted above, appear to be more than a challenge to that reported for radical surgery.

We are concerned, not only that your authoritative columns should claim that radical surgery is the best treatment, but that it is said to be the only effective treatment; this is surely a question which now demands re-evaluation.—We are, etc.,

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- 1 Matson, D. D., *Neurosurgery of Infancy and Childhood*, 2nd edn, p. 544, Springfield, Illinois, Thomas, 1969.  
2 Bartlett, J. R., *Brain*, 1971, **94**, 725.  
3 Kramer, S., McKissock, W., and Concannon, J. P., *Journal of Neurosurgery*, 1961, **18**, 217.  
4 Kramer, S., Southard, M., and Mansfield, C. M., *American Journal of Roentgenology*, 1968, **103**, 44.

### Research in Psychiatry

SIR,—Though we are in agreement with most of what you say in your leading article on "Research in Psychiatry" (8 April, p. 61), it is surprising to find no reference whatever to the family.

As you rightly point out, man is not only a complex biological organism but also a product of society. To understand psychiatric stress, therefore, requires us to take variables from both these fields into account. At their interface lies the family. When things go badly there, owing to disruption or distortion of bonds of affection, individuals are exposed to heavy stresses which there is reason to believe can contribute greatly to many forms of psychiatric disorder. When, by contrast, family relationships are mutually supportive, potential stress on members, from whatever source, is mitigated.

We are not alone in regarding the study of family interaction and its effect on the development and functioning of personality as one of the most promising of all fields for future research in psychiatry.—We are, etc.,

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SIR,—May I congratulate you on your leading article (8 April, p. 61) on research in psychiatry? I began the practice of psychiatry in 1924 under Sir David Henderson, who later sent me to his own former chief at the Henry Phipps Psychiatric Clinic in the Johns Hopkins. This was Professor Adolf Meyer, a Swiss and the son of a

Swiss pastor. The thousand words or so of your excellent article are almost completely summed up by Meyer's oft repeated and unaltered declaration that "We study the whole man and his setting, and not only the parts of man."

At that time for over half a century Meyer was the leading figure in American and indeed world psychiatry. When he died after the end of the war in 1948 his views and teaching vanished almost overnight. They were swept away by the rush of the irresistible financial tide of psychoanalysis, for it was a saleable article in a way that Meyer's "psychobiology" was too undramatically formulated to be.

I confess that today I am wondering if psychiatry is a useful word. I am afraid we are regarded with inordinate expectation, with flat disbelief, or as rather a poor joke. It is my belief that the average man would agree more happily to see a neurologist than a "trick-psychist."—I am, etc.,

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SIR,—Myths—being re-told often, as in your leading article "Research in Psychiatry" (8 April, p. 61)—die hard. It is not true that one can always find support for good research projects in psychotherapy.

In 1960 I changed my clinical commitment to half-time, intending to undertake research in psychotherapy. I carefully prepared two alternative projects. One involved a then new technique, now known as "flooding," as part of a total psychotherapeutic regimen for phobic subjects; the other involved short periods of partial sensory deprivation preceding psychotherapeutic sessions to see whether these could heighten affective transactions in the subsequent consultation. This second project was not devised by me alone but was presented jointly with an experimental psychologist (a reader in the University of London) who was responsible for the statistical design, and a psychiatric colleague (a consultant in a regional hospital) who had agreed to undertake the blind scoring of interview transcripts. In spite of application through all the standard channels, including the Medical Research Council, neither project was funded. Having children to support I put proposals to the University of London for my present unit, which studies epidemiological aspects of student problems, and had no difficulty in getting the financial backing required. Indeed the unit is established as a permanent U.G.C.-financed commitment to this day.

The organizing and funding of psychiatric research is for the most part in the gift of good men and true but men whose belief structures and whose clinical practice incorporates the view that psychotherapy is simply well-meaning bumbledom. For them only the research methods appropriate to psychopharmacological or epidemiological work can produce real science. Of course they prove themselves right.

One-third of sickness is predominantly psychiatric. Roll on Rothschild!—I am, etc.,

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### Chondromalacia Patellae

SIR,—In your leading article (15 April, p. 123) you imply that the pain in this condition is "worst . . . notably when descending stairs. . . ." In practice, it will be found that this is one of the few conditions of the knee in which discomfort is characteristically worse going up stairs than going down. It is useful to ask about this diagnostic point as well as the generally recognized characteristic symptoms, such as discomfort with prolonged sitting with the knee flexed and inability to kneel. Another aspect of chondromalacia patellae is that it can become manifest with overtraining for competitive running and jumping, and, as an iatrogenic condition, from repetitive resisted extension of the knee from the flexed position—for instance with weights and pulleys as a misconceived aid to "rehabilitation."—I am, etc.,

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### Intelligence and Fertility

SIR,—A long letter would be needed to explore fully the confused, unsubstantiated, and erroneous statements in your leading article on "Intelligence and Fertility" (15 April, p. 125), but here are a few examples.

(1) That the variance of intelligence test scores can be attributed meaningfully to inherited and environmental influences. The objections to partitioning variance between nature and nurture were discussed clearly by Hogben<sup>1</sup> nearly 40 years ago and more recently by Waddington<sup>2</sup> among others.

(2) That a mean family size of 2.1, which it is suggested is required in Britain, will be achieved by assisting couples to have the number of children they choose and without attempting to influence their decisions. There are no grounds for thinking that an optimum mean family size (which would change with changing economic and social circumstances) will occur fortuitously, and it is questionable whether family size will continue to fall as conditions improve unless people can be persuaded that it is socially irresponsible to have large families, as it is to smoke or spit.

(3) That it is the more capable parents who will feel able to have large families. This has certainly not been true in the past, when there was an inverse relation between mortality and intelligence of children and family size. Nor is it obvious that in future, when many irresponsible parents will have large families and many responsible parents will choose to have small ones.

(4) That natural selection will operate more effectively if there is greater rather than less variation in family size. For a given mean family size, genetic variability of offspring will be greater when families are more rather than less uniform in size (since in the latter case more children will have the same parents).

The conclusion that "a uniformity of two-child units would virtually limit natural selection to that from death in utero" is therefore based on the assumption that with greater variation in family size there would be greater postnatal mortality, presumably in large families, a requirement not mentioned and hardly consistent with the claim that "couples who have larger-than-average