

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.,

H. J. G. BLOOM
C. L. HARMER

Radiotherapy Department,
Royal Marsden Hospital,
London S.W.3

- 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.,

JOHN BOWLBY
C. MURRAY PARKES
R. D. SCOTT

School of Family Psychiatry
and Community Mental Health,
Tavistock Centre,
London N.W.3

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.,

W. MCC. HARROWES

President,
Scottish Association for Mental Health
Clifton, Bristol

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.,

NICOLAS MALLESON

Research Unit for Student Problems,
University of London,
London W.C.1

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.,

H. JACKSON BURROWS

London W.1

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 Hogben¹ nearly 40 years ago and more recently by Waddington² 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

families . . . will tend to be those with the greatest gifts for parenthood."—I am, etc.,

THOMAS MCKEOWN

Birmingham

- Hogben, L., *Nature and Nurture*, London, Allen and Unwin, 1933.
- Waddington, C. H., *The Strategy of the Genes*, London, Allen and Unwin, 1933.

SIR,—In an otherwise useful survey of studies of the relationship between intelligence and fertility, your leading article (15 April, p. 125) gives a misleading impression of the relationship between intelligence as measured by standard intelligence tests and academic success, claiming that such test results have a "strong predictive value . . . especially in the more difficult subjects and for later achievement." This is simply not so; university selection would be a great deal easier and more fruitful if it was.

In fact, as I have pointed out at greater length elsewhere,¹ we have no measures of "strong predictive value" for academic success. Even the best available measures of ability are unable to account for as much as half of the variance in academic performance. Eysenck² surveyed 34 carefully designed studies, and found an average correlation of 0.58 between ability-test results and a number of academic criteria—this would account for hardly 35% of the variance in academic results. More studies have reported correlations as low as 0.20. While statistically significant, this would account for only 4% of variance in university achievement. Schwartzman *et al.*³ compared a group of failing medical students with a successful group, and found them to have approximately equal intellectual abilities despite their scholastic disparity. As many as one-fifth of all failures at university may have I.Q.'s of 130 plus.

There is little evidence to suggest that intelligence tests have any greater predictive value in "more difficult subjects," whatever those are, and none to suggest that they have any correlation whatever with "later achievement," however one measures that. The predictive value of all available measures is highest for first-year performance, and falls steadily with successive years at university. Performance later in life is not usefully predictable; as a study of historical biographies will make clear. It is important to realize that although I.Q. and previous academic performance have some modest predictive value, this value is very seldom high enough to justify the touching, and at times pathetic, faith often placed in them.—I am, etc.,

MICHAEL A. SIMPSON

Guy's Hospital,
London S.E.1

- Simpson, M. A., *Medical Education: A Critical Approach*, London, Butterworths, 1972, in press.
- Eysenck, H. J., *British Journal of Educational Psychology*, 1947, 17, 20.
- Schwartzman, A. E., Hunter, R.C.A., and Prince, R. H., *Journal of Medical Education*, 1961, 36, 353.

SIR,—The correlation of measured I.Q. with family size, or with social class, or even with colour of skin, seems always to alarm those who observe and report it. However, whatever the explanation of such correlations the more important question is whether they matter. If intelligence were a

uniquely valuable human resource currently being maximally exploited one might understand anxiety about its possible maldistribution or potential diminution, but all the evidence is that it is grossly underexploited in all prevailing social and educational systems and its value alongside such other human attributes as courage, humility, kindness, and a sense of justice, seems, to say the least of it, poorly established.—I am, etc.,

ALWYN SMITH

Department of Social and Preventive Medicine,
University of Manchester

Leucocyte Migration in Chronic Hepatitis

SIR,—Changes in hepatic structure¹ and the existence of immune lymphocytes able to recognize autologous liver cells in cases of chronic active hepatitis² support the hypothesis that cell-mediated immunity (delayed-type hypersensitivity) may be involved in the pathogenesis of the disease.

We employed the leucocyte migration test described by Sjøborg and Bendixen³ for measuring delayed hypersensitivity responses to liver antigen in vitro. Homogenate of human fetal liver has been centrifuged at 1,500 g and the supernatant has been used as antigen. To avoid the stimulatory effect of low antigen concentration⁴ 400 µg of protein per ml of culture medium was added to the migration chamber. Each experiment was performed in quadruplicate.

The migration index (M.I.) was calculated dividing the mean area of migration in the

antigen-containing chamber by the mean area of control migration. An example of the migration after 24 hours incubation is shown. Twelve normal patients, six patients with persistent chronic hepatitis, and 12 with aggressive chronic hepatitis were tested. The diagnosis of chronic hepatitis had been established on the basis of clinical course, biochemical evidence, and liver biopsy.

The migration index of normal subjects was 1 ± 0.3 (mean ± 2 D.S.). Six cases of chronic aggressive hepatitis showed migration inhibition (M.I.: 0.67, 0.59, 0.60, 0.45, 0.51, 0.69). A similar reactivity was not seen in patients with chronic persistent hepatitis. These results agree with the data of Smith *et al.*⁵ The difference between the former two groups is significant ($P < 0.01$).

In conclusion our findings suggest that in a proportion of patients with chronic aggressive hepatitis cellular hypersensitivity against components of normal fetal liver is present. With more purified antigenic preparations the specificity and sensitivity of the test might well be increased and more conclusive evidence probably achieved.

We thank Misses A. Valcavi and E. Oberti for technical assistance.

—We are, etc.,

CARLO G. VERGANI
PAOLO R. CROCCHIOLO
EUGENIO F. REDAELLI

Istituto di Patologia Speciale Medica,
Università di Milano,
Italy

- Sherlock, S., *American Journal of Medicine*, 1970, 49, 693.
- Tobias, H., Sanfran, A. P., and Schaffner, F., *Lancet*, 1967, 1, 193.
- Sjøborg, M., and Bendixen, G., *Acta Medica Scandinavica*, 1967, 181, 247.
- Sjøborg, M., *Acta Medica Scandinavica*, 1968, 184, 135.
- Smith, M. G. M., and Williams, R. eds., *Immunology of the Liver*, p. 135, London Heinemann, 1971.

Publication of Conference Proceedings

SIR.—Dr. David Pyke's "Personal View" (5 February, p. 371) in looking at the costs and encumbrances of publishing the proceedings of conferences, etc., deserves notice.

There is a conditioned reflex in all audiences at the final euphoric gathering of a conference, symposium, or seminar to demand publication, usually in their bulky, indigestible entirety, of the proceedings. However, the International Diabetes Federation is not the only pioneer to attempt control of this urge.

The editorial committee of the Third World Conference on Medical Education, New Delhi, 1966 decided against full publication of its discussions, papers, etc. It did, however, accept that not everything was light weight. Some items deserved a permanent record. Partly for this reason, partly to overcome the problem of library classification and indexing, and partly to give as full distribution as possible, it was decided to seek a journal willing to publish the worthy features of the conference in a routine number. After negotiations, two journals accepted this responsibility. The resultant issues together gave the selected conference reports world distribution, at low cost and in a form acceptable to readers, reference seekers, and librarians alike. These journals were the *Journal of Medical Education* (U.S.A.), February 1968, and the *Indian Journal of Medical Education*, May 1968.

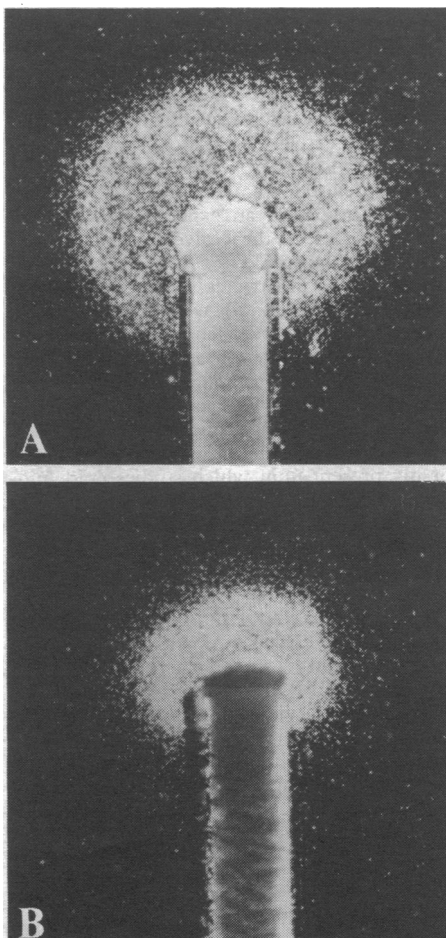


FIG.—Migration of leucocytes from a positive patient. A no antigen added; B with antigen.