BRITISH MEDICAL JOURNAL

SATURDAY 12 MAY 1973

LEADING ARTICLES

PAPERS AND ORIGINALS

MEDICAL PRACTICE

SUPPLEMENT

NO. 5862 BRITISH MEDICAL JOURNAL 1973 VOLUME 2 319-374 ASTM CODEN: BMJOAE 2 (5862) 319-374 (1973)

BRITISH MEDICAL ASSOCIATION TAVISTOCK SQUARE LONDON WC1H 9JR TELEPHONE 01-387 4499 WEEKLY PRICE 42p

CORRESPONDENCE

Correspondents are asked to be brief

Advice on Heart Transplants

SIR,—Several contentious points were raised in your leading article on this subject (17 February, p. 374), but one of them deserves particular mention. Why is it that, of all transplantation procedures, cardiac transplantation was singled out in what appears to be a somewhat negative way? The Chief Medical Officer's letter (p. 431) arose, of course, from the findings of a group of distinguished experts, but I am puzzled as to why transplantation of the pancreas, liver, and lung (which has given worse results than that of the heart) and even cadaver kidney transplants (of which the results are no better than recent results of cardiac transplantation) do not call for the same investigation by experts in these respective fields.

The experts agreed that "special resources should not at present be made available in Britain for cardiac transplantation in man (still considered to be largely experimental) at this stage." Is it then correct to say that any procedure with a 40% one-year survival and a 20% two-year or longer survival (my own experience), and even better recent results in Dr. Norman Shumway's experience, is still experimental, compared with certain death within a short time without transplantation? The surviving patients whom I have met seem to differ from this viewpoint. I did understandably omit to point out to most of them what the cost of this survival means in terms of taxes paid by the community. The question of resources being made available, and costs, could certainly have been countered by pointing out the considerable sum of money spent on the defence budget in most countries, and in Great Britain the cost to the taxpayer of maintaining a naval blockade against Rhodesia.

The group of experts whose opinions prompted this letter of advice on heart transplants was obviously not required to comment on, for instance, all the implications of mutilating surgery in the management of certain malignant diseases. Take, for example, the treatment of a rhabdomyosarcoma (sarcoma botryoides) of the vagina in a girl aged 5 years. The surgical management of this patient is the removal of the vagina, uterus, ovaries, rectum, and bladder, leaving the child with a colostomy and artificial urinary conduit, with a much slimmer chance of surviving for two years than a patient who has undergone heart transplantation. Was it right that the experts were not requested to give advice on this sort of surgery but concentrated rather on cardiac transplantation?

My own experience and that of Dr. Norman Shumway in the survival rate of cardiac transplantation is not only a statistical fact but also a very important personal consideration to each patient who is dying from incurable heart disease. If one compares these results with the heroic surgery mentioned above and all the human implications that go with it, one may have doubts about the resources, costs, and ethical and general advisability of certain surgery other than heart transplantation that is being carried out today without a murmur of protest.

Finally, I have been in heart surgery long enough to remember when, during the latter half of the 1950s in the United States and other countries, palliative closed heart surgery for congenital heart disease was abandoned, and with the use of the newly discovered heart-lung machine open corrections were suggested. British surgeons at that time adopted a very conservative approach. I remember very well how one famous British heart surgeon talked about the "honeymoon period" of cardiac surgery, which he said would be short-lived. This conservative attitude put a damper on the progress of cardiac surgery in Britain for several years. We must just hope the same thing does not happen with cardiac transplantation.—I am, etc.,

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Enuresis Again

SIR,—Your leading article (14 April, p. 69) gives good information about the treatment of enuresis, but your conclusions about the origins of enuresis need revising.

You quote with approval Miller's conclusion that "a slow pattern of maturation" is the cause of bed-wetting in children over the age of 5. But Miller[1] himself reported that of children wetting at 5, 29% had earlier been dry for three months or more and the rest dry at times. So for nearly a third of his enuretics maturation had certainly been fully completed and in the others had occurred to some extent. There is much other evidence that by the age of 5 maturation has occurred in nearly all children.[2 3]

You refer to Lovibond's theories about enuresis, that it is due to faulty learning or conditioning, to difficulty in conditioning, or to breakdown of acquired habit. It has become increasingly apparent to me that the evidence that nocturnal dryness is taught or conditioned is weak. Seven or eight per cent of children have night-time dryness by the age of 1 year.[4 5] Piglets have day- and night-time bladder control from the moment of birth.[6] Lovibond and Coote[7] suggest that cortical inhibition of micturition is "transferred to the sleeping state," but this cannot apply to the sizeable proportion of children who are dry at night before they are dry in the day. It is unlikely that learning or conditioning can occur in a sleeping child. The fact that most enuretic children can be conditioned with the bell does not prove that the night-time bladder control which in