that a long term trial is needed—hides a much more complicated situation than is suggested.

Hip sonography was introduced in Austria in 1980 and has been painstakingly followed up since then.² The results of examinations of many tens of thousands of neonatal hips are now available. From the beginning immediate vigorous conservative treatment was instituted for all dysplastic and unstable hips. This was followed by an extensive and eventually comprehensive ultrasound screening programme for all newborn infants in the maternity unit.³

This policy resulted in a dramatic reduction in the average age at diagnosis of hip dysplasia⁴—itself a strong prognosticator of improved outcome. Over the ensuing years the numbers of babies presenting with late dislocation of the hip and with hips failing to mature completely fell to almost zero. Practically all children have now finished their treatment before the age at which they start to walk. Operative reduction has not been necessary in the screened group for several years, and congenital dislocation of the hip has been completely eradicated among Austrian babies (R Graf, personal communication). The savings due to the reduction of the number of operations more than cover the cost of the entire screening programme.⁵

Thus the large long term trial that Clarke calls for was completed and reported on several years ago. Its results are staggering if true because they imply that many hundreds of children are still suffering from a crippling but completely preventable disease. Such results may need to be corroborated and confirmed, but at the very least the enormous Austrian and German experience needs to be taken into account.

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AUTHOR'S REPLY.—Peter M Dunn is concerned by my implication that clinical screening for congenital dislocation of the hip may itself increase the incidence of neonatal hip instability by provoking joint laxity. I believe that this is a genuine concern among some clinicians, and I referenced a paper that addressed the problem.¹ The article by Paterson *et al* draws attention to the dangers of clinical examination in the “extreme cases of an infant with osteogenesis imperfecta.”² Dunn will be aware that any screening test must be safe and that since the test is applied non-selectively small problems are magnified and rare events become important. Accordingly, such events must be recorded.

The clinical screening test for congenital dislocation of the hip has contributed in some centres, such as Dunn's, to a reduced incidence of the condition. There is, however, concern about the apparent increase in late diagnoses of the condition in other centres. Catford *et al* recorded that the incidence of cases established late increased in Southampton during 1965-78.³ The Malmö study of 96 891 infants found that the incidence of later cases increased from 0.07/1000 in 1956 to 0.6/1000 in 1987.⁴ The data are open to epidemiological scrutiny, but when there is any doubt about the validity of a screening test efforts to improve its universal sensitivity and specificity must be con-

tinued. I am sure that Dunn would condone such efforts if augmentation of the clinical test, say with ultrasonography, led to a further reduction in late presenting cases of congenital dislocation of the hip, which, as he says, is a “readily treatable but potentially crippling deformity.”

Brennan Wilson is correct that ultrasonography of the hip is used commonly in Europe, where Graf pioneered a technique that essentially relied on a morphological rather than dynamic assessment of the hip. The technique has been reported to have reduced the incidence of established dislocation of the hip, but published treatment rates for neonatal hip abnormalities thus detected have concerned some orthopaedic surgeons in Britain and the United States. Tonniss *et al* used the technique and reported an incidence of hip abnormality of 65.4/1000 and a treatment rate of 44/1000.⁵ Additionally, the treatment rate in their series doubled after they started to use ultrasound examination to show abnormal hip morphology.

I agree that the Austrian experience needs to be taken into account, but until published results are available for study and the epidemiological basis assessed, concern will remain about such high rates of “prescribed” treatment based on ultrasonographic diagnoses.

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Managing the persistent vegetative state

EDITOR.—As the general practitioner of the family of a young man in a persistent vegetative state for the past three years I have more than a passing interest in Keith Andrews's editorial.¹ I fully appreciate the need for immediate and focused skilled care for patients who are in a persistent vegetative state and realise that they might have to be cared for at regional or supraregional centres, but I have some reservations.

I have already witnessed the devastating physical, emotional, social, and financial effect on my patient's immediate family. How long would the parents be expected to uproot themselves while the optimum 11 hour day care was given—six months, a year, three years? In times of recession how long would any employer keep their jobs open for them? How would the parents cope away from their familiar social support—the working men's club where the father can go in for a couple of pints twice a week and find quiet comfort among friends; the house where they both find daily joy from their new grandson. These are emotional questions, but not idle ones. The cost for these families is already high and I am not sure that this plan would not increase it.

1 Andrews K. Managing the persistent vegetative state. *BMJ* 1992;305:486-7. (29 August.)

* The author's name has been withheld to protect the patient.

EDITOR.—Keith Andrews's optimistic review¹ avoids any reference to the implications for the

many recommendations in recent years that it may in some circumstances be appropriate to withdraw life sustaining treatment from these patients, who can survive 10-20 years, or more, without improvement. These have come from the President's Commission,² the Academy of Neurology,³ the American Medical Association,⁴ an international consensus,⁵ and, in Britain, from a working party of the Institute of Medical Ethics.⁶ Moreover, many American courts, including the Supreme Court, have sanctioned such action.⁷

Such an approach is a natural development of the acceptance of the ethical principle of patient autonomy: that competent patients can refuse or request the withdrawal of life prolonging treatment if they consider that its burdens outweigh its benefits. It is more difficult to make such a decision for incompetent patients unless they have made an advance directive—a course now widely advocated.⁸

A recent American study of responses of some 500 people about how they would frame advance directives for life prolonging treatments showed that 80% would not want such measures if they were vegetative, and half would not wish them if they were in coma with a small chance of full recovery.⁹ Several questionnaires have indicated that vegetative survival is regarded as a fate worse than death, and some consider that recovery of a limited degree of conscious awareness associated with continuing very severe mental and physical disability from brain damage may be even worse.¹⁰ When an international group of neurosurgeons was asked at what level of probability they would withhold or withdraw life prolonging measures from severely head injured patients, they quoted 90-95% certainty of a poor outcome (dead, vegetative, or severely disabled)—but, when asked to restate this figure if they themselves were injured, many of the surgeons wished treatment withdrawal at a much lower probability of poor outcome. They were not prepared to accept the risk of vegetative or severely disabled survival that they would impose on their patients.⁹

In this context it is therefore important to be able to estimate the probability of various degrees of recovery after various periods in the vegetative state. It would be very helpful if Keith Andrews's experience could be analysed to provide indicators to identify which patients have capacity for reasonable recovery. Without such data this review may reverse the trend towards compassionate withdrawal of life sustaining measures. This has hitherto seemed a reasonable response to the ethical dilemma created by the capacity of modern technology to save and sustain the lives of some patients with brain damage so severe that it would previously have proved fatal.

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