promise which helps us retain that distinction is "anaemic hypoxia.""

In summary, precise definition is important because, as Medalia, et al, of corrections our out, different aetiologies can result in different patterns of symptoms such as the immediate versus delayed encephalopathies associated with "pure circular collapse" versus carbon monoxide poisoning respectively. We must also be alert to ways in which different aetiologies lead to overlapping pathologies and pathologies and pathophysiology. The terminologies which we have found helpful is "neurogenic fascicular atrophy". In the absence of giant type fibres which are typical for SMA in early infancy, muscle biopsy revealed the presence of denervation, the extent of reinnervation and the chronicity of the process, but did not conclusively disclose the precise anatomical localisation of the denervating event. There is no comment on whether there were any giant type fibres in the muscle biopsies.

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Dr Gautieres et al reply: We appreciate the interest of Dr Valaz et al in our paper. We do not think that the cases we reported have much in common with those described by Darwish et al. In their three patients, muscle atrophy was already present at birth and the condition remained static, in marked contrast with the progressive disease with a postnatal onset seen in our patients. We agree that MRI of the cord is desirable but was not available to us at the time we saw these patients. We did not feel it justifiable to perform contrast myelography as the clinical features, clearly indicated anterior horn cell disease. In particular, the absence of sensory changes, diffuse abolition of deep tendon reflexes, normal spinal function and electrodiagnostic picture, all evidence extending to clinically normal muscles distant from the cervical region, all favour spinal muscular atrophy. The progressive extension of the disease in the lower limbs is a well known phenomenon and its occurrence in two siblings born to consanguineous parents are also strong arguments against a malformative, vascular or tumoural process. Indeed, the late clinical picture in our patients was very similar to that of the classic type of spinal muscular atrophy. Our aim was to draw attention to an unusual clinical variant rather than describe a new disease entity.

Defining prognosis in medical coma

I appreciated David Bates' well written editorial on the process and limitations of establishing prognoses in patients with medical coma. Since we collaborated on the international non-traumatic coma study, perhaps I may make one or two points not strongly emphasized by Dr Bates.

The editorial generally summarises accurately the international study's results but omits emphasizing a dimension which may not be self evident. In the study excluded all patients whose coma did not result either from known organ failure, or from known exogenous causes such as a deprivation of oxygen supply or an excess insulin dose. All cases with self-induced coma-causing drug poisoning as well as all cases in which aetiologic diagnosis was uncertain were automatically excluded. The reasons are straightforward: nearly all such patients survive intact with intensive care including some with a flat EEG and fixed pupils lasting for a day or more.

I regret that I disagree with Dr Bates in his contention that since only a small number of several thousand patients can reduce the theoretical error of 5% in predicting poor outcome, one cannot make decisions based on unfavourable early signs, however bad they may be. What about the patient whose coma did have a record, but which they certainly would not do so if they did have a record and inedentify sufficiently a pained and crippled being if that was the cost of losing the Bates don't allow an escape therapy that will do any damaging downside statistical feature anywhere in their editorial. Many Americans are becoming increasingly apprehensive about being rescued from an early death by critical care measures only to face lives permanently blighted by intractable pain, severe physical disability, cognitive impairment or some combination of all three. When we advise patients of how their lives may have a 50-50 chance of being saved if they survive and their families are aware of the possibility that the child may have a poor quality of life, we are not leading parents to believe that if everything is done it will be good for the child. In my own experience, a few will say, "Please do everything, doctor". In my own experience, most will want to have "please be merciful—he/she couldn't stand living as a permanent cripple, much less being a hopeless burden on the family".

The humane decision of who and when to treat and how long to do so is necessarily delicate, difficult and sometimes painful for the physician; it is an even greater burden for the family. At least by the US Constitution, the doctor is neither the only party nor the major decider in this situation, the patient is. Evidence in this country, is that we physicians are under-fulfilling our responsibility on this critical matter.

In 1984, Dr Ben Humphrey was the author of a small monograph entitled "Final Exit", which offers direct advice on how to commit suicide for those who, for whatever reasons, wish to consider this option. It has now become the national bestseller. The accompanying New York Times news story included comments from bookellers, journalists and potential patients that implied that physicians often overses or unduly

A predominantly cervical form of spinal muscular atrophy

We have reservations about the paper by Dr Goutieres et al, though not the concept itself. Documenting the clinical and histological findings in five infants, they describe a condition "A predominantly cervical form of spinal muscular atrophy". Cervical spinal muscular atrophy (SMA) is a real entity and has been reported previously. It is probably due to abnormal development of the lower cervical segments of the spinal cord resulting in muscle wasting and contractures of the upper extremities and normal lower limbs.

They have not mentioned any necropy findings. They need other sources of confirmation since they attempt to describe a newly defined clinical condition. In atypical positions of the spine, descriptions of the authors should have ruled out a possible cervical hydromyelia or syringomyelia with amyotrophy or even a congenital cervical spinal tumour, and familial syringomyelia is well known. For this purpose a myelo-C1 or an MRI of the cervical spine should have been done. Imaging spinal areas by MRI has almost revolutionised our concepts. We have seen two children with cervical spinal pathologies, one with a dermoid tumour and one with suspected syringomyelia (unoperated upon yet, so no tissue diagnosis is available). Both cases had symptoms mimicking SMA. In each case, MRI was the greatest help and was the most straightforward method. In conditions running a more benign course such as monomeric amyotrophy, studies with T1 or metrizamide CT have demonstrated atrophy of the related spinal cord.

Muscle biopsy findings in the cases of Dr Goutieres et al were all consistent with "neurogenic fascicular atrophy". In the absence of giant type fibres which are typical for SMA in early infancy, muscle biopsy revealed the presence of denervation, the extent of reinnervation and the chronicity of the process, but did not conclusively disclose the precise anatomical localisation of the denervating event. There is no comment on whether there were any giant type fibres in the muscle biopsies.


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Matters arising
prolong the lives of those suffering, thereby providing the necessity for self-action by the patient.

Perhaps when we advise patients and families who face difficult decisions, we would be better to heed the available evidence about who will do well and who will do badly rather than withholding recommendations because unduly cautious statisticians choose to neglect the downside effects of their theoretical arguments. Neuropathologists are in an especially vulnerable position to understand the misery that accompanies chronic, overwhelming brain damage. This knowledge places them in a position to understand the humane needs in such cases and advise families accordingly.

Most of these problems are not taken into consideration in Sibley’s article.

It is generally pointed out that the “scientific” evidence of epidemiology and biostatistics overlooks the fact that such studies can at best only be estimates within a cohort and cannot be generalised; epidemiological studies are marking their place in providing aetiological clues but cannot be used to deny possible causal relationships. As was pointed out by Schoenberg, “Statistical significance (or lack thereof) does not equal biological significance.”

It is difficult to understand, on the basis of what is already known about the pathogenesis of MS, how trauma such as laceration of the hand, contusion of the finger, a root canal procedure, or a thorn in the foot can possibly lead to the appearance or recurrence of symptoms of MS. Sibley et al dismiss the alteration of the blood-brain barrier in the genesis of the MS lesion; in doing so they disregard the numerous recent publications which have amply confirmed this fact, including statements by such authorities as McDonald and Barnes who commented that “A consistent feature of nearly every MS lesion is breakdown of the blood-brain barrier which is largely repaired over weeks, leading to marked changes in the size of acute lesions” and by Comi et al, who wrote, “Blood-brain barrier damage is necessary early in the development of focal demyelination”.

One cannot expect that all, or even most MS patients will have exacerbations following trauma, since it is a common experience for MS patients to have repeated viral infections without changes in the course of their disease.

The relationship between trauma to the head, neck or back that I have supported, is based upon clinical, neuropathological, and experimental evidence which were reviewed in 1984 and based on the obligatory step of the alteration of the blood-brain barrier for the formation of the MS lesion. This has now been amply documented.

Additional problems seemed to have plagued this study: for reasons which are not explained, symptoms alone are not recognised as exacerbations. Nevertheless, diplopia, paresthesia and vertigo, classic symptoms of MS are quite often unaccompanied by objective neurological examination. It is curious that Sibley et al used 48 hours’ duration for an exacerbation as opposed to the more generally accepted 24 hours.\(^*\)

Finally, it is difficult to draw any conclusions from data given on the basis of mean annual exacerbation rates for the simple reason that exacerbations in MS are never uniformly distributed; periods of complete quiescence lasting several years not uncommonly occur after a series of several exacerbations within a single year.

A prospective study of physical trauma and multiple sclerosis

I read with great interest the article by Sibley et al.\(^*\) Because of my own involvement in this particular problem, I wish to make the following comments.

There can be no more difficult task than to conduct epidemiological surveys of multiple sclerosis (MS) because of its unpredictability and symptomatic variability. It is therefore unsuitable for measurements with the yardsticks currently at hand. Indeed, to design an epidemiologically valid study, it would be necessary to match MS patients (not just healthy controls) for age, sex, ethnic origin, duration and geographic sites of residence, living and social environment, duration and severity of disease, type of clinical course, stage of activity, and number and location of lesions using modern imaging techniques such as MRI. It would be necessary to decide upon the basis of objective neurological changes only if keeping with the Schumacher Committee criteria for definition of a relapse. The 48 hour minimum duration of new symptoms was an arbitrary departure from the Schumacher criteria to help ensure that fluctuations in symptoms due to fatigue and other physiological factors were not counted erroneously as exacerbations.

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