PostScript

CORRESPONDENCE

Ischaemic stroke in chagasic patients

We read with interest the paper by Carod-Artal *et al*[1] that showed the relevance of Chagas' disease as a stroke risk factor in patients of South American origin. They also confirmed a textbook view[2] (not hitherto demonstrated) that cardioembolism is the main cause of stroke in Chagas' disease, in 52% of cases. This reflects in part the underlying chagasic cardiomyopathy, characterised by congestive heart failure and arrhythmias, present in 46% of chagasic patients as compared with 25% of non-chagasic patients.

Despite the lack of comparison of stroke characteristics between both groups, one very interesting finding was the significant percentage of chagasic patients who developed stroke without any known vascular risk factors or cardiopathy. As the authors stated, undetected cardiovascular disease could account for at least part of this finding. The indeterminate form of the disease is defined by the presence of infection confirmed by serological tests, in the absence of symptoms or of electrocardiographic or radiological abnormalities. Twenty five % of subjects with the indeterminate form of the disease may present significant structural and/or functional abnormalities when they are fully evaluated by more sensitive methods, such as ergometry and autonomic tests.[3]

Another possible explanation proposed by the authors would be the vasculitis phenomenon. Although there is good experimental evidence to suggest that changes in the microvasculature may contribute to chagasic heart disease,[4] much less is known about the possible involvement of central nervous system microvasculature in Chagas' disease. Indeed, most studies point to an important role for endothelin in the pathogenesis of microvascular changes in the chagasic heart,[4 5] but we are unaware of any similar studies of the central nervous system.

The authors also suggest the need for primary prevention in all patients with Chagas' disease cardiomyopathy. This is a strong recommendation, as most chagasic patients derive from poor social economic backgrounds and have poor access to the health system.[1] Chronic oral anticoagulant therapy is known to cause frequent clinical complications, especially bleeding; an alternative approach could be use of the low dose anticoagulant therapy that has been recently suggested for the treatment of deep vein thrombosis.[6] However, further studies are still needed to investigate this possibility specifically in Chagas' disease.

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Authors' reply

In our recent study, we demonstrated that at least 52.2% of chagasic strokes are due to cardioembolism and 36.8% are of undetermined cause.[1] A significant proportion of these cryptogenic chagasic strokes may also be cardioembolic in origin.[2 3] We therefore encourage the use of transoesophageal echocardiography in all patients with chagasic stroke, especially for the better definition of aetiology in the 36% having strokes of undetermined cause.

The monitored administration of warfarin is remarkably effective in the reduction of stroke recurrence in persons with cardioembolic stroke.[4 5] Thus, we recommend oral anticoagulation for all individuals with chagasic stroke, who have demonstrated risk factors for cardioembolism. To our knowledge, no case control study analysing these factors has yet been carried out.

Stroke of arterial origin in Chagas' disease seems much less common than in the general population of stroke patients.[1] Evidence for secondary prevention of stroke of arterial origin with oral anticoagulation as in the Warfarin-Aspirin Recurrent Stroke Study (WARSS)[6] and European/Australasian Stroke Prevention in Reversible Ischaemia Trial (ESPRIT)[7] clinical trials is controversial. Our follow up experience with the administration of secondary anticoagulation to persons with chagasic stroke, whether cardioembolic or not, has been encouraging in the lack of significant complications. We are therefore currently investigating the efficacy of antithrombotic therapy in chagasic stroke patients, using either low or moderate dosages of anticoagulants. Until the results of this or similar investigations are available, antithrombotic prophylaxis should be individualised in persons with chagasic stroke of undetermined cause, on the basis of the estimated risk of recurrent stroke *v* the risk of complications during anticoagulation.

We thank M M Teixeira and A L Teixeira for their comments and interest in our article.

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Efficacy of methyprednisolone pulse therapy on neuroleptic malignant syndrome in Parkinson's disease

I was astonished to find that Sato and colleagues were able to identify 40 cases of neuroleptic malignant syndrome (NMS) in patients with Parkinson's disease from a single institution over three years.[1] At a recent neurosciences grand round in Birmingham, UK, which has an interest in Parkinson's disease research, we could only recall two such cases in living memory.

There are two possible explanations for this high incidence of NMS. Firstly, in Japan the Parkinson's disease population may be more prone to developing NMS when their antiparkinsonian medication is reduced. This could be due to genotypic differences between Japanese and Western populations. Whereas a higher prevalence of the Parkin mutation has been noted in Japan,[2] judging from the age range and duration of disease given in table 1 of Sato's report,[1] these were not all young onset patients as one would expect with the Parkin gene. Nevertheless, it would be of interest to know if this high incidence of NMS has been seen in other Japanese centres and whether any genotypic reason can be found.

The second possible explanation is that the reductions in anti-parkinsonian medication that precipitated NMS were substantial. NMS has been recorded in Parkinson's disease in the past in association with so-called 'drug holidays', which have now been abandoned in most countries owing to the high fatality rate. Against this explanation is the fact that three patients had no change in their medication in Sato's study.[1]

The interest of this paper lies not so much with the proven benefits of methylprednisolone therapy in NMS in Parkinson's disease, as in the high incidence of NMS in the Japanese patients treated in this unit. I would value the author's further comments.

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Authors' reply

We greatly appreciate Dr Clark's comments about our work on the efficacy of methylprednisolone pulse therapy in neuroleptic malignant syndrome (NMS) in Parkinson's disease.[1] We agree with him that 40 cases of this syndrome identified from a single institute over three years in patients with Parkinson's disease is a large number. The study institution—the Futase Social Insurance Hospital—is a specialised centre for neurological diseases, particularly among elderly patients, and we have treated several hundred patients with Parkinson's disease. Furthermore, half the NMS patients were transferred from other non-specialised hospitals and private offices in the area. The large number of patients and inappropriate treatment in some patients resulted in an accumulation of NMS cases in our hospital. Physicians from other non-specialised hospitals and private clinics in the area are not always aware the risk of NMS on withdrawal of antiparkinsonian drugs. Indeed, in 30 cases of Parkinson's disease, physicians stopped antiparkinsonian drugs because of psychiatric symptoms, dyskinesia, and the on-off phenomenon. These factors may have resulted in the accumulation of NMS cases in our hospital.

We were not aware that Japanese patients with Parkinson's disease are more prone to developing NMS when antiparkinsonian drugs are reduced. However, the possibility of genotypic differences between Western and Japanese populations is interesting,[2] and comparisons could usefully be made on the prevalence of mutations in parkin or other genes between these populations.

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Mesencephalic ischemia and Parkinson's disease

I read with interest the paper by Abe *et al*[1] on occipital and posterior parietal hypoperfusion in 28 Parkinson's disease (PD) patients without dementia. These findings suggest that there was a reduced regional cerebral blood flow (rCBF) in the intraparenchymal territory of the posterior cerebral arteries (PCAs) probably due to the presence of atheromatous plaques located in the distal end of the basilar artery.[2] Atherosclerotic changes are of considerable importance because they can cause stenosis and/or occlusion at the origin of the terminal (PCAs) or collateral (superior cerebellar arteries) branches, as well as of the posterior perforating arteries (PPAs).

Based on the fact that in situ the donor tissues of catecholamines are normally highly vascularised and by contrast in PD the rCBF is reduced in the neostriatum, from February 1988 to December 2002 we have used two surgical procedures to treat PD:[3–5] (1) transplantation of adrenal medulla into the putamen by a transinsular pathway, and (2) omental transplantation on the interpeduncular fossa, anterior perforated space, and insular cortex in 16 patients with moderate or advanced stages of PD. Thus, omental tissue revascularises to the catecholaminergic (dopaminergic and noradrenergic) nuclei, as well as to the surrounding structures, and moreover prolongs the survival of the graft implanted in the putamen. In all patients, neurological improvement was better during the first weeks after surgery than in the following months or years. Our third patient is the same case previously reported by us.[3] At present, 15 years postoperatively, she has only slight tremor on the left leg and does not require anti-parkinsonian medication. She occasionally receives 1 mg of clonazepam at night. Her quality of life is good and she manages the daily living activities similar to any normal woman of her age.

In conclusion, the vascular impairment described by Abe and colleagues supports the autopsy findings[2] and neurosurgical results.[3–5] Clinical data suggest that PD is initiated in the intraparenchymal territory of the PPAs caused by atherosclerotic plaques located at the mouths of these arteries. Therefore, we believe that Parkinson's disease is wrongly classified as a neurodegenerative disorder.

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Traumatic brain injury as a risk factor for Alzheimer's disease

In a recent systematic review of case control studies investigating head injury as a risk factor for Alzheimer's disease (AD), Fleminger *et al*[1] replicated the results of the meta-analysis by Mortimer *et al*[2] in males (OR 2.29; 95% CI from 1.47 to 2.00) but not in females (OR 0.91; 95% CI from 0.56 to1.47). Their findings support in males only an association between a history of previous head injury and the risk of developing AD, but the study could not review the relation between head injury and ApoE gene status as risk factors for AD.

The review by Fleminger *et al* was based on clinical studies alone and, as Wilson[3] emphasised, did not consider the nature or severity of the original head injury; and the results of the first retrospective autopsy study of the relation between closed traumatic brain injury (TBI), ApoE allele frequency, and AD[4 5] unfortunately were not mentioned. This present study has examined:

- the incidence of AD pathology in 55 consecutive autopsy cases (mean age 77.6 years, SD 7.1) with residuals of closed TBI lesions (old contusions in the frontal, temporal, or other brain areas)
- the frequency of TBI residuals in 53 age matched AD cases proven at autopsy.

In both series, ApoE was evaluated from archival brain material embedded in paraffin. The results were as follows.

In the TBI series, 12.7% (four males and three females) showed CERAD B (Consortium to Establish a Registry for Alzheimer's Disease) definite AD (Braak stages 5 and 6), and 9.1% showed CERAD B probable AD (Braak stages 3 or 4). TBI history dated back from 10 to 30 years before death; duration of AD ranged from four to seven years. Two of the subjects with AD showed ApoEε3/4, and the remainder 3/3 or 3/2; of the remaining 43 subjects without AD, three exhibited 3/4 alleles. The prevalence of AD (21.8%) in this small autopsy cohort was significantly higher than in either a recent large clinical series (3.3%)[6] or the general population over the age of 70 years (14%).[7]

In the AD cohort (all CERAD B or C, Braak stages 5 and 6), there was an ApoEε4 allele frequency of 30% (similar to other AD series). Residuals of TBI were seen in four brains (two males and two females, each 7.5% of the cohort), all four lacking the ApoEε4 allele. These data in small autopsy cohorts confirmed previous clinical studies suggesting that severe TBI is a risk factor for the development of AD, particularly in subjects lacking the ApoEε4 allele which is considered a risk factor for AD. No gender differences were found.

Irrespective of these data, we agree with others[1 3] that further work should consider population based cohorts and larger autopsy series of TBI and AD, in order better to elucidate the relationship between TBI, ApoE alleles, and the development of AD.

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The specificity of prescription patterns in secondary stroke prevention

We would like to comment on the important report by Landi and colleagues about the factors associated with a reduced likelihood of receiving secondary stroke prevention treatment[1] and present our own data. We have demonstrated that in community-dwelling patients with chronic atrial fibrillation, living alone or in rural areas, history of previous falls, and cognitive and functional impairments are independent factors that result in physicians prescribing aspirin instead of anticoagulants, thus disregarding the common guidelines for stroke prevention.[2 3] We have also shown that in some cases it does not mean malpractice.[3] In elderly patients, a geriatric assessment including a shrewd evaluation of the psychosocial conditions can guide physicians in the selection of the correct treatment, thus avoiding the risks related to anticoagulants in individuals at high risk of falls or with inability to comply with regular blood monitoring.[2–5]

Our data are only partially comparable with those of Landi and colleagues, since in their study a significant number of the reported undertreatment concerns aspirin and triclopidine, drugs that have an unfavourable risk–benefit ratio in comparison with anticoagulants, even when they are prescribed for individuals living alone, with a low education level and poor cognitive or functional performance. In these conditions, low compliance is not enough of a risk and does not justify undertreatment. As a matter of facts, in the clinical conditions described by Landi and colleagues, an ''ageist'' cultural background prevails without real clinical motivation.

The difference between the two sets of data suggests that physicians need to be taught to consider the complexity of the medical scenario and to distinguish incorrect prescribing patterns due to limitations imposed by cultural factors from the rational behavior of physicians who adopt a multidimensional model of care and avoid treatments commonly recognised as beneficial but burdened by a high cost–benefit ratio.

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Authors' reply

The data presented by Bellelli and Trabucchi confirm our findings suggesting that many older adults do not receive secondary stroke prevention treatment.[1] However, we really do not believe that our results indicate only an ''ageist cultural background without real clinical motivations''. Indeed, in our article we recognised that the decision of not to treat could not be considered as ''undertreatment'', but it may be related to the uncertainty about the cost-effectiveness of the treatment in a frail population. These doubts are not always unrealistic, especially among frail post-stroke elderly individuals, who characteristically have a high number and complexity of associated diseases, with a concomitant higher risk of drug interactions and adverse drugs events.[2] Furthermore, the reduced rate of treatment observed in our study is not only explained by potential risks in frail elderly patients, but also by uncertainties about the potential benefits.[3 4] In fact, the most important evidence of antiplatelet or anticoagulant medications after cerebrovascular accidents is substantially based on non-disabling ischaemic stroke. Evidence about the benefits of secondary stroke prevention is much more limited in the frail elderly population with severe physical and/or cognitive impairment. In this respect, it is important to underline the fact that the data presented by Bellelli and Trabucchi are based on a sample of community dwelling patients with atrial fibrillation, that ''per se'' is an indication to treat. In contrast, our study sample, which was based on patients receiving home care programmes indicating that an important and disabling health problem was in place, included a frailer population.[5] In this respect our results can not be generalised to all healthy community dwelling elderly individuals. However, we acknowledge that studies addressing the efficacy of secondary prevention treatment are needed, especially for frail and functionally impaired older individuals who have suffered a stroke.

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Relationship between stridor and sleep apnoea syndrome: is it as simple as that?

We read with interest the article by Hirayama *et al*[1] in which the authors, using an original imaging method, low field magnetic resonance fluoroscopic study, proposed that upper airway obstruction precedes laryngeal occlusion causing the stridor in patients with multiple system atrophy (MSA). This issue of nocturnal stridor in MSA is of great importance since it is a common cause of sudden death and a recognised prognostic factor in this disease.[2] It affects about 19% of patients as shown in our series and by others.[3] We feel that the relationship proposed between obstructive apnoeic respiratory events and stridor is not as simple as suggested by the authors and must be considered in light of classical standardised polysomnographic (PSG) data.

In our own series, 18 consecutive patients with MSA were assessed for night-time disturbances by all-night standard PSG with continuous synchronised audiovisual recording. Nocturnal stridor occurred in 10 patients and, except in one patient, was always accompanied by breathing disorders, mostly apnoeic, with or without significant oxygen desaturation. In four patients, obstructive sleep apnoeas (OSA) occurred without stridor, and one of these patients presented predominantly with central apnoea that also occurred while awake. Among the patients with stridor, four presented predominantly OSA and one mainly central apnoea. Mixed and prolonged apnoea, up to 53 s, was seen along with stridor in five patients and was isolated in two others. Episodes of mixed apnoea were typical in their occurrence as

they were always preceded by heavy, prolonged inspiratory effort and stridor, indicating upper airway obstruction. Such episodes were not detected in OSA patients without stridor. Apnoeic events of any type were in most cases followed by the recurrence of snoring and not by an inspiratory stridor sound.

Thus, there seems to be a wide variety of combined sleep-related breathing disorders ranging from a majority of obstructive apnoeas to stereotyped mixed apnoeas of very long duration and sometimes preceded by stridor in MSA.

Nocturnal breathing disturbances in MSA are due to the complex involvement of multiple brainstem nuclei, leading to a defect in the respiratory control system independently of the occurrence of stridor.[2] Among these breathing disorders, OSA are the most common and may occur in non-obese MSA patients even in the absence of stridor, thus indicating that the mechanism underlying the two events is different. The higher incidence of OSA observed in MSA patients may also be due to the severity of bradykinesia and the fact that patients with severe MSA lie predominantly, if not always, in the supine position while asleep. The reduction of nocturnal obstructive events during lateral position in patients with OSA has already been reported.[4] In our patients, who were audio monitored, stridor was not followed by typical obstructive apnoea nor was the apnoea ended by a stridor. Thus, we believe that stridor and OSA in MSA are different and independent events. We also found that mixed apnoea occurred stereotypically and was very prolonged and often preceded by a harsh sound typical of stridor, as documented by audio monitoring.

Non-invasive continuous positive air pressure (CPAP) should be proposed for relief of sleep breathing disorders. It has been used successfully to treat stridor and OSA in MSA patients.[5] In our series, nine patients accepted CPAP treatment (six with stridor and sleep apnoea and three with isolated OSA). One patient died before initiation of the treatment and two patients without sleep complaints dropped out after one week because of lack of tolerance despite having a severe apnoea/hypopnoea index. Since the onset of CPAP treatment, both patients and their spouses reported better sleep, improved daytime alertness and wellbeing. For some patients, getting used to CPAP took up to a month, after which it was generally well tolerated. After a mean follow up period of 10 months, the patients' compliance with the continued use of CPAP was satisfactory and their relatives did not report any recurrence of stridor.

Thus we feel that the relationship between stridor and sleep apnoea is far from clear, especially considering the polysomnographic association of stridor and mixed apnoea that we found. Complex supranuclear neurological dysfunction may account for this association, but further studies are however needed to clarify this issue and better establish the indications for CPAP.

Acknowledgements

The study was supported by a PHRC 97 grant and a CHU de Bordeaux-CNRS fellowship.

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Authors' reply

We would like to thank Dr Ghorayeb *et al* for their interest in our paper and their comments. We agree with the view that the relation between stridor and apnoea in MSA is very important.[1] Unfortunately, in our experimental procedure, we could not establish a correlation between the image of the vocal cords and upper airway and stridor symptoms because it is difficult to record airflow and vocal sound simultaneously in a magnetic resonance imaging (MRI) recording.[2]

In obstructive sleep apnoea syndrome (OSAS), even when the upper airway was obstructed completely, the vocal cords were not obstructed. Therefore, stridor does not develop although snoring may occur in OSAS. In contrast, MSA patients had an obstructed upper airway, which was frequently accompanied by stenosis of the vocal cords. However, we did not find stenosis of the vocal cords without stenosis of the upper airway. If the stridor is produced by stenosis of the vocal cords and snoring is produced by stenosis of the upper airway, snoring should be accompanied by stridor in all MSA patients. In fact, we observed that the initial narrowing of the larynx and pharynx produced snoring. Ghorayeb *et al* point out that OSA (SAS with upper airway obstruction) can commonly occur even in non-obese patients with MSA without the presence of stridor. We agree with this observation, but in this study, we did not find stenosis of the vocal cords without upper airway stenosis, so none of our patients developed stridor without snoring. We observed the patients in the MRI room to identify the sleep state and the presence of snoring and stridor, and we found that the highest pitch vocal sound appeared after heavy and prolonged inspiratory effort. This phenomenon is very similar to Ghorayeb *et al*'s observation of apnoea and stridor. We also suppose that the phenomenon of apnoea in MSA patients occurs with confinement of stenosis of the upper airway. Therefore, we think that some patients in MSA with SAS can be treated with CPAP similar to OSAS patients. However, the effect of CPAP could be diminished, since the respiratory centre is eventually involved with the progression of disease in MSA, and central apnoea and abnormal respiration may appear. Further study is required to clarify the indication of CPAP in patients with MSA.

The mechanism of SAS in MSA is unclear. Our study showed complete obstruction of the upper airway and vocal cords occurred in MSA even with the presence of tongue atrophy and without narrowing of the larynx. Thus, we suggest that there is another mechanism involved distinct from that of OSAS. Some reports have stated that a dystonia-like phenomenon was present in the vocal cords in the stridor through electromyographic study, suggesting a similar mechanism to be present in the progression of upper airway obstruction.[3 4]

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BOOK REVIEWS

Biological psychiatry, Vol 1 and 2

Edited by Hugo A H D'Haenen, Johan A den Boer, and Paul Willner. Published by John Wiley & Sons, Chichester, 2002, pp 1404, £345.00. ISBN 0-471-49198-5

The European editors of these two volumes have brought together contributions from all over the world and from a range of relevant specialties. Although the majority of the authors work in psychiatry, the other disciplines represented include neurology, psychology, physiology, and pharmacology. Guided by clear concepts regarding the anatomy of the book overall as well as the individual chapters, the editors have succeeded in providing an integrated and comprehensive review of biological psychiatry.

The introductory chapters address conceptual and measurement issues in biological psychiatry. The next section comprises a series of chapters on basic principles, reviewing key topics such as animal models, monoaminergic transmitter systems, neuroendocrinology, immunology, psychophysiology, neuropsychology, brain imaging,

genetics, and gender issues. The bulk of the book covers a series of clinical syndromes: cognitive disorders, substance related disorders, schizophrenia and other psychotic disorders, mood disorders, anxiety disorders, eating disorders, sleep disorders, and personality disorders. Adopting an approach that virtually defines the term ''biological psychiatry'', each disorder is systematically addressed in a series of chapters covering the areas discussed in the basic principles section, as well as a review of current pharmacotherapy. This structure avoids overlap between chapters, and also generates several intriguing reviews that consider less commonly addressed topics, such as the psychobiology of somatoform disorders, the neuroanatomical bases of eating disorders, and the neuroendocrinology of personality disorders. Despite the relatively limited amount of research data in such areas, these chapters are no less authoritative than the chapters dealing with more established subjects. With few exceptions, the chapters in these volumes are well organised, focused, and succinct, with thorough reference lists.

This book is primarily orientated towards postgraduates and researchers within psychiatry, neurology, psychopharmacology, and psychology, and there is no doubt that they will value it highly. But clinicians in these fields who dip into it this book will find many useful insights into the neurobiological aspects of the conditions with which they work.

T R E Barnes

The bereitschaftspotential movement-related cortical potentials

Edited by Marjan Jahanshahi and Mark Hallett. Published by Kluwer Academic/Plenum Publishers, New York, 2003, pp 315, £150.00. ISBN 0-306-47407-7

The Bereitschaftspotential (BP; readiness potential, although the sense of the German word is rather more imperative) was discovered in 1964 and named in 1965 by Hans Kornhuber and Lüder Deeke. In their original description it was a negative going wave of cortical potential that was first detectable 1–1.5 s before the movement occurred. Like the demonstration of evoked potentials, it was a technological advance (the computer of average transients or signal averager) that permitted detection of these minute waveforms. The discovery (with its implications for volition and free will) acted as a considerable stimulus to research. This book brings up to date the state of knowledge concerning the BP and other brain potentials occurring around the time of a motor act.

It is an expensive text. What does the reader get for the money? The book consists of 17 chapters in 7 sections. There is a brief introduction by the editors that states the aims of the book. These are: to explain the processes that the BP reflects, to quantify the number of components responsible for the BP, to explore the anatomical substrate for the BP, and to flag up areas for future research. These are commendable aims, and, to the extent that much information on all these aspects is contained in the book, they are achieved.

However, the arrangement of the material into the 7 sections of the book does not neatly reflect these aims and it is left to the reader to pick out the information where it occurs. This approach of presenting a number of unpublished papers with little sub-editing has its pros and cons. In its favour there is a mass of new data from experts in the field and an element of historical and personal background and critical commentary that one would not otherwise find in the scientific literature. Against it is the difficulty of obtaining a coherent overview of the subject. This is compounded by inconsistent terminology (a glossary of abbreviations is a serious omission, the more so as the abbreviations are often inconsistent), and poor proof reading (there are very many minor errors). The general reader will find the introduction useful and will enjoy the chapters by Libet and by Deeke and Kornhuber but, as a whole, the book is strictly one for the specialist.

M Lakie

Classic cases in neuropsychology, Vol 2

Edited by Chris Code, Claus-W Wallesch, Yves Joanette, and AndreRoch Lecours. Published by Taylor & Francis Books Ltd, Hove, 2002, pp 340, £39.95. ISBN 0-86377-891-7

Why read the classics? If you're still not sure why it might be worth bothering, this book would really be wasted on you. Better that it should fall into the hands of someone who really appreciates that modern neurology and neuropsychology owes an enormous amount to the careful descriptions of single cases. And, despite the whizz and bang of functional imaging, this is likely to continue to be the case.

In this volume, you will find discussion of Babinski's cases of anosognosia for hemiplegia, Wernicke's case of conduction dysphasia, Goldstein and Gelb's description of form agnosia, Dejerine's case of alexia without agraphia, and many other gems from the distant past. But, in addition, you will also be pleasantly surprised to see more recent ''classics'' such as Bisiach and Luzzatti's descriptions of right hemisphere Milanese patients who, when recalling from one imagined vantage point their famous Piazza del Duomo (the city's central square), reported places that would appear to their right, neglecting those to their left. But, when asked to imagine turning round, they failed to report locations they had previously mentioned and described instead places that now fell to their right from this new viewpoint. This description of ''representational neglect'' has had a profound impact both on stimulating research into the neglect syndrome and understanding the nature of mental representations of space.

Of course, the qualities of the contributing chapters do vary considerably but the subject matter that is covered in this collection is wide ranging, and also entertaining. The chapters that work well are those that place their case study in their historical, as well as scientific context. There are important lessons here, for instance, about the dedication and obsessional nature of some neurologists. Dejerine, for example, himself carried out the postmortem on his patient with pure alexia within 24 hours of his death—at the patient's home. He clearly wanted to find out how the lesion location differed from that of a patient he had reported on the previous year who had alexia with agraphia, and no administrative difficulties at any hospital were going to prevent him!

This is a great book, well worth reading for pleasure and for learning about some of the most important cases that have shaped our understanding of higher cortical function.

Why read the classics? Why be a neurologist.

M Husain

Assessment of aphasia

Edited by Otfried Spreen and Anthony H Risser. Published by Oxford University Press, Oxford, 2003, pp 320, £32.50. ISBN 0-19-514075-3

This book surveys existing tests for people with aphasia, with particular emphasis on reporting studies that address their reliability and validity. In this it is admirably comprehensive, at least for the tests it covers. These are exclusively tests published in English, with, within this, a strong bias towards those originating in the United States. Many tests widely used in the United Kingdom with people with aphasia, in both research and clinical practice—for example the Graded Naming Test[1]—do not warrant consideration. Those used in Europe, including the Aachen Aphasia test that has the best psychometric properties of any aphasia test, get only the briefest mention.

For the tests they do consider, Spreen and Risser are admirably comprehensive in surveying the literature on reliability and validity. But, as they point out, in the development of these tests ''psychometric development has been less than optimal in many instances and neglected in others''(p 33). These weaknesses are serious. They report a number of tests where the reported test-retest reliability is around 0.7 or even less. The authors do not point this out, but any test with a test-retest correlation coefficient this low is seriously compromised. The implication is that around 50% of score variance is error. As a result the test will be almost useless in monitoring change, and any of its scores will need to be treated with real scepticism.

Many of the interesting issues in the assessment of aphasia are not really addressed. Many clinicians assess people with aphasia in order to identify the nature of both their impairments and their intact processes because that provides the basis for devising treatments that are directed at the impaired functions and use the strengths of the intact processes. This book provides no guidance on how that might be done.

Many existing aphasia tests, including the two most widely used, the Boston Diagnostic Aphasia Examination and the Western Aphasia Battery, aim to classify people with aphasia into ''diagnostic groups''—for example those with Wernicke's aphasia and Broca's aphasia. Sadly Spreen and Riser never explore whether assigning syndrome labels in this way has any impact on patient management.

This book is an excellent source of references addressing the validity and reliability of American aphasia assessments, but less satisfying on the many complex issues that surround the uses of assessments for different purposes with people with aphasia.

D Howard

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Intraoperative imaging in neurosurgery MRI, CT and ultrasound

Edited by R L Bernays, H-G Imhof, and Y Yonekawa. Published by Springer Wien, Vienna, 2003, pp 144, €88. ISBN 3-211-83835-X

This book forms part of a series of symposia reported by Springer-Verlag. The topics covered are important and timeous. Most neurosurgical units would be evaluating the need for intraoperative imaging, the options, and possibilities. A factor that emerges is the importance of structure, and indeed then the need for courses and refresher symposia on modern operative anatomy and in particular to fully understand the fibre tracts of the brain. The first section, on interventional MRI, covers systems currently in clinical use and some background development and potential that would be valuable and necessary reading for a unit contemplating the introduction of such technology. The authors are experienced and the contribution significant. The second section deals with the role of intraoperative MRI and glioma surgery. It appears that data are emerging that the use of intraoperative MRI allows for more complete resection and probably a better outcome for patients with this devastating disease. It also gives a description of some of the difficulties that will be encountered when using neuro navigation together with the MRI system. The third section provides thoughtful reflections on the use of intraoperative ultrasound for cranial surgery and a chapter on the use of intraoperative CT scanning for navigation in spinal surgery. The final chapters provide some personal reflections on intraoperative MRI imaging technology and the use of functional MRI, together with a chapter on the cost benefit ratio of the technology. In the postscript Dr Yonekawa again highlights the need for training in basic micro-neurosurgery to continue parallel to the learning of innovative technical developments.

The book will provide essential reading for heads of service, neurosurgeons with an interest in neuronavigation and intraoperative imaging, and managers who will be faced with requests for the introduction of such equipment. It is a comprehensive and well balanced collection of views and information on this important and emerging topic.

J van Dellen

New frontiers of MR-based techniques in multiple sclerosis

Edited by Massimo Filippi and Giancarlo Comi. Published by Springer-Verlag, Italy, 2003, pp 107, €49.95. ISBN 88-470-0198-6

Magnetic resonance (MR) is the single most important laboratory technique for diagnosis and monitoring of patients with multiple sclerosis (MS). Although some may cringe at the thought of yet another review of MR methods, development of new MR based methodologies continues. This short book provides a succinct description of the "cutting edge" of the field in seven chapters written by acknowledged experts.

Dousset, for example, describes ways in which individual cells may be tracked in the central nervous system after labelling with new iron oxide based contrast agents. Filippi, Rocca, and Rovaris review applications of both magnetisation transfer and diffusion weighted MRI to defining early axonal injury in normal appearing areas of a white matter. There is a further discussion of methods of diffusion tractography, which allows axonal tracts to be mapped, giving both information on the anatomy of major tracts and their integrity. Rashid and Miller describe applications of arterial spin-labelling magnetic resonance, a technique for defining perfusion changes that potentially provide an absolute measure of brain activity. The importance of better understanding cortical functional changes is emphasised in a nice review of functional MRI demonstrating ways in which the organisation of brain systems may change adaptively with the progression of pathology in MS. Part of the future of MRI clearly lies in enhancing sensitivity and spatial resolution of the imaging. An exciting approach to this has been development of ultra-high (>5 T) field magnets. Kangarlu and his colleagues present images of human brain from an 8 Tesla (that is more than 5 times as powerful as a conventional clinical scanner) imaging system with individual plaques of MS shown.

This volume can be read quickly and the chapters are well written. It is highly recommended for neuroscientists and radiologists who want a brief, authoritative introduction to the current state of the art.

P Matthews

History of neurology in The Netherlands

Edited by J A M Frederiks, G W Bruyn, and P Eling. Published by Boom Publishers, Amsterdam, 2002, pp 401, €42.00. ISBN 90-5352-686-2

Pride of place in the first century or so of Dutch neurology must go to the basic sciences of anatomy and physiology. These were my first points of contact with the Dutch neurological tradition, now almost 50 years ago. To the fledgling investigator working on propriospinal reflexes, Ariens Kappers et al's *The comparative anatomy of the nervous system of vertebrates including man* (New York, Macmillan, 1936) was the place to turn for the structural background to physiological experiments. And in physiology, the work of Magnus, de Kleijn, and Rademaker were essential to understanding posture and rigidity. Studies on pathological peripheral and central nerve fibres were illuminated by the early pathological studies on Beri Beri by Winkler (1855–1941) and Pekelharing (1848–1922), and Hans van Crevel's work in the laboratory of Verhaart (1889–1983) in Leiden.

The book reviews the origins of this great tradition and charts its continuation into the late 20th century through Dusser de Barenne (who became professor of physiology at Yale) and Nauta (who also emigrated to the United States) and his student Hans Kuypers who was professor of anatomy successively in Rotterdam and Cambridge. Other aspects of neurology (the editors prefer the traditional use of the word to denote all aspects of the study of the nervous system, normal and pathological, including neurosurgery as well as clinical neurology) were later in achieving the well deserved international recognition they now have.

The book provides a wealth of detail about the evolution of the different neurological centres in The Netherlands and the contributions coming from them. As in Germany, psychiatry and neurology remained closely linked until well into the 20th century. The development of the subspecialties is considered in some detail. A special feature of the Dutch scene was the way in which high quality original work came not infrequently from non-university settings.

Of particular interest to the general neurological reader are the more detailed accounts of the life and work of a number of the major neurological figures in The Netherlands. Ariens Kappers emerges not only as the important contributor he was, but as a rather remote, self centred individual with his eye always to the main chance, and not especially appreciative of the work he got others to do for him. He, like most of the others in this section of the book, seems to have lived a rather austere live concentrated on his professional duties. There was tragedy for some, including Bernard Brouwer (1841–1949) who as Rector Magnificus did his best to limit the inroads of Nazism in the University; the authorities closed the university down. But after the liberation in 1945, Brouwer was judged not to have done enough in opposing the Nazis, and was refused an opportunity to return to the university. His colleagues, however, believed in his integrity and in 1947 he was appointed Director of Amsterdam's Central Institute for Brain Research, where he continued to work until his death.

The book is well produced and illustrated, with portraits and a number of scientific illustrations from both the early and the recent literature.

I McDonald