because it seems that to focus on the presence or absence of dementia in Parkinsonism may in fact distort the picture through oversimplification.

The assessment of dementia can be simply a subjective, clinical evaluation, which is, unfortunately, most often how it is made in studies on Parkinsonism. It can also be made by looking at the actual degree of cortical atrophy. An example of this is Selby's² study of 250 Parkinsonism patients where he found 57.2% had such degeneration. Finally, and probably most usefully, is to carry out appropriate psychometric assessment. I have had occasion to review the studies where full assessment of level of intelligence of patients with Parkinsonism has been made using such well standardized tests as the Wechsler Adult Intelligence Scale (W.A.I.S.), the Mill Hill Vocabulary Scale, and Raven's Progressive Matrices. There is no argument that individual cases may have dementia but what is outstanding is that in none of these studies (seven in number) was the mean I.Q. for the Parkinsonism patients below the average range—namely, I.Q. 90 to 109.

In two of the studies^{3,4} the performance I.Q. on the W.A.I.S. was in the low 90s, but this could be attributed to direct impairment of motor ability affecting speed of performance on those subtests where motor activity is necessary. In some studies the verbal I.Q., though in the average range, was well above the population mean of 100. The overall deduction from these studies seems to be that a group of patients with Parkinsonism may be no more intellectually impaired than a random group of people of comparable age from the general population. This does not mean that they have no cognitive deficits. Recent carefully designed psychological investigations have produced evidence of cognitive deficits which operate much more selectively than a general intellectual deterioration.⁵⁻⁷ These studies have all controlled the effects of motor impairment per se on level performance. The findings concur in implicating the basal ganglia as having a role in perceptual organization.

Apart from interesting, theoretical considerations about the role of subcortical brain centres in higher mental processes, there is pragmatic value in such studies. If areas of subtle deficit can be carefully mapped then therapeutic techniques can be evaluated by measuring changes in these areas. I have already some preliminary evidence from a small pilot study that treatment by levodopa not only improves actual motor performance but also reduces the extent of the "central processing" deficit, at least over a period of up to five months after the start of treatment.—I am, etc.,

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Hemifacial Spasm

SIR,—After reading your leading article (16 December 1972, p. 624) I have waited to see, from your correspondence columns, whether any of your readers had hit on a method other than surgical for the relief of hemifacial spasm. To my surprise I appear to be able to teach my grandsons how to suck eggs.

When I started private eye practice over 30 years ago I saw in my first two years three patients with this distressing and socially embarrassing condition. As the twitching orbicularis was the chief offender, often causing sleeplessness, I wondered if one could perhaps damp down the impulses in the most distal branches of the facial nerve without causing actual paresis or paralysis of the particular muscles. I explained to the patient that my idea was to try injections of weak alcohol after local anaesthesia. Their reaction was willingness to "try anything." I started first with 10% and gradually worked up to 90% alcohol, as I found that fewer injections were needed with the latter and that the danger was minimal. In all, I have treated 15 patients, all of them with complete success.

For the orbicularis three prominent bony points are chosen: one just outside the margin of the bony orbit at the level of the outer canthus, the second just above, and the third just below the prominence of the malar bone. All are in a vertical line and the point of the needle should strike bone at all three points. The points are infiltrated with procaine and after five minutes a total of 0.5-0.75 ml of 90% alcohol is injected in divided doses. The patient should be warned that a hot, burning sensation may be felt in spite of the anaesthetic, but this soon passes off. A further warning should be given that at the first injection there may be a moderately severe reaction of redness and swelling, which, however, passes off in 2-3 days. Further injections are given at intervals of 2-3 weeks, until there is no further evidence of spasm. The orbicularis should be tested by asking the patient to "screw up" his eyes and then trying to force them open with one's fingers. Injections should not be carried to the point of paresis, and any relative weakness on the injected side should be a warning to stop treatment. The rest of the face can be treated similarly by injections along an extension of the same line into the cheek below the malar bone, opposite the corner of the mouth, and on the lower bony margin of the mandible.

One of my patients had violent contractions of the platysma, which was similarly successfully treated. In two patients there was a slight localized recurrence after 2-3 years. A single injection at the necessary points cured this. There is some temporary loss of sensation over the areas injected, and in one case there appeared to be a slight thickening of the tissues below the skin.—I am, etc.,

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Anaesthetic Contamination

SIR,—In reply to the letter from Drs. A. A. Spence, R. P. Knill-Jones, and D. D. Moir (21 April, p. 174), we agree that direct proof of any effect of long-term exposure of man to low concentrations of inhalational anaesthetic agents is at present lacking and that incrimination solely of halothane is particularly speculative. However, Allison and Nunn¹ have demonstrated gross effects,

from brief exposure to inhalational anaesthetic agents, on microtubules, which are known to be related to the motile system of sperm tails.² Reproductive disturbances, including an increased proportion of female live births, have been demonstrated in laboratory animals.³ A larger version of our pilot study might provide a simple, statistically acceptable method of demonstrating an effect upon anaesthetists in an area less sensitive to questioning than the incidence of stillbirth and spontaneous abortion and assist in deciding whether large-scale expenditure on "anti-pollution" devices is desirable.

Only children conceived while the father was engaged in full-time anaesthetic practice were included in our results. In their paper⁴ Dr. Knill-Jones and his colleagues do not state that this criterion was applied to their finding of 50% female children (compared with 48.5% in controls) born to female anaesthetists nor do they mention it with regard to the subgroup under 30 years, in which they now report 53.6% of male children. The proportion of female live births found in our survey was compared with the birth pattern in the Sheffield Hospital Region, in which region the anaesthetists we surveyed were employed, and not only with the pattern in England and Wales as a whole, as they have inferred.—We are, etc.,

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- 1 Allison, A. C., and Nunn, J. F., *Lancet*, 1968, 2, 1326.
- 2 Satir, R. P., in *The Contractile Process*, ed. A. Stracher, p. 241. Boston, Little, Brown, 1967.
- 3 Fink, B. R., Shepard, T. H., and Blandau, R. J., *Nature*, 1967, 214, 146.
- 4 Knill-Jones, R. P., Rodrigues, L. V., Moir, D. D., and Spence, A. A., *Lancet*, 1972, 1, 1326.

School Eye Clinics

SIR,—Mr. J. A. E. Primrose's letter (14 April, p. 117) is most important because he describes the time-honoured view of school eye clinics and the division between them and the hospital clinics. Some rationalization really is necessary.

Doubts have been raised on the effectiveness of treating squint and amblyopia if they are not discovered before school age. Attention has concentrated on hospital cases (usually of preschool age) of squint and amblyopia and there is a conspicuous lack of knowledge and method in treating the school child with these conditions—largely, I believe, because of this division into hospital and school clinics. I tried to foresee developments in the ophthalmic care of children in order to arrange the back-up organization to meet any changes, principally regarding the problems of squint and amblyopia. Two separate services treating variations of the same condition often occurring in the same families is illogical. Furthermore, the public health service, as we know it, will shortly disappear.

The next "development" may be a completely different screening programme either aimed at truly preventive ophthalmology, if that is found possible, or at least better directed to revealing treatable conditions at an age when they can be treated. The obvious defect in the present school screening programme is that by the time it detects squint and amblyopia they have probably been estab-

- 1 Celesia, G. G., and Wanamaker, W. M., *Diseases of the Nervous System*, 1972, 33, 577.
- 2 Selby, G., *Journal of the Neurological Sciences*, 1968, 6, 517.
- 3 Perret, E., Kohenof, M., and Siegfried, J., *Neuropsychologia*, 1969, 7, 79.
- 4 Asso, D., *British Journal of Psychiatry*, 1969, 115, 555.
- 5 Talland, G. A., and Schwab, R. S., *Neuropsychologia*, 1964, 2, 45.
- 6 Teuber, H. L., and Proctor, F., *Neuropsychologia*, 1964, 2, 85.
- 7 Horne, D. J. de L., *Journal of Neurology, Neurosurgery and Psychiatry*, 1971, 34, 192.

lished for some time. The school eye clinic, therefore, has not been significantly involved in truly preventive medicine.

I did not know of Gardiner's work¹ when I wrote my paper (3 February, p. 278). He has, however, shown that the school eye clinic can be used for the detection of squint and amblyopia in the preschool children. Examination of younger siblings of those already attending hospital clinics for squint has been carried out for some time, but the search has been for a definite squint or a tendency to break down to one.² Gardiner has gone one step further and searched for refractive errors in those cases who have not developed strabismus. He uses the two separate clinics for this, while I have merely joined them together under one organization and with one set of records. I strongly believe that one organization is necessary to deal with the whole range of children's ophthalmic problems, and it should be under the active direction of a consultant. Its precise location is immaterial.

Kramar³ found that detection of certain refractive errors offered the best chance of identifying children at risk for developing a squint. Publication of her findings was delayed until they had been confirmed in a different sample.⁴ We now examine clinically and refract all preschool siblings of children with a squint or amblyopia, or a positive family history or a refractive error which we think is significantly associated with these conditions. Our findings will be reported.

I disagree with Mr. Primrose over the problem of defaulters. I believe it is better to follow-up a defaulting preschool child than one of school age, because it is more likely that we will achieve permanent visual improvement (in cases of amblyopia) in the younger child. We should, however, concentrate on those we think will really benefit rather than follow-up all defaulters. I think the parents should be responsible for myopic school children.

To conclude, I believe that more efficient treatment of squint and amblyopia will come from (1) making an accurate prognosis of the possible results of treatment when we first see the child, and (2) from better screening procedures. I cannot be persuaded that this can be achieved better with two separate services when I know it can be done through one.—I am, etc.,

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- ¹ Gardiner, P. A., *Community Medicine*, 1972, 127, 117.
- ² Pratt-Johnson, J. A., and Lunn, C. T., *Canadian Journal of Ophthalmology*, 1967, 2, 50.
- ³ Kramar, P. O., *British Orthoptic Journal*, 1973, in press.
- ⁴ Ingram, R. M., *British Orthoptic Journal*, 1973, in press.

Safer Motoring

SIR,—Your leading article (28 April, p. 195) on this subject was most timely, and so was your warning that the present type of British lap-and-diagonal seat belt was satisfactory for the average-sized wearer but less so for the very tall or very small, to which in many instances one would have to add the big or people wearing bulky fur coats. At a time when there is increasing pressure for legislation to make the wearing of seat belts compulsory, we must first ensure that the standard belt can be worn comfortably and safely by all car occupants whatever their shape.

While the search for even better belts must continue apace, there is one simple measure that should be taken by car manufacturers at once to make the existing type more adaptable to the majority of users. The critical factor is the height of the pillar mounting of the diagonal strap. If it is too high (or with a short wearer), the webbing chafes uncomfortably across the neck and cheek, with possible risk of injury in an accident. If too low, it may slip off the shoulder; this too often is the unavoidable position in two-door cars or coupés which lack a centre pillar.

Ideally, the pillar mounting should be level with the lobe of the wearer's ear. To achieve this for the greatest number of users, it would only be necessary to provide a choice of, say, three alternative mounting points in the upper portion of the pillar.

Because they always tension and position themselves correctly on the wearer automatically, as well as for the freedom they give in reaching all the controls and other advantages such as neater stowage, retractable belts should be the only approved type, but care must be taken to provide enough webbing even to extend across a portly woman in furs.—I am, etc.,

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Anomalous Origin of Vermiform Appendix

SIR,—A 37-year-old man was admitted to hospital with a history of epigastric pain in the previous eight hours, associated with vomiting. The pain later settled around the umbilicus and a provisional diagnosis of appendicitis was made.

Diagrammatic representation of anomalous origin of appendix.

At operation there was no sign of the appendix in the ileocaecal region along the line of the anterior taenia coli. No other abnormality was found in any other organ. The caecum and ascending colon were freed and an injected appendix was found lying proximally retrocolic and distally in the paracolic gutter. When followed proximally it was found to arise from the ascending colon; its base was lying near the lateral taenia about 10 cm from the ileocaecal region. The distal end of the appendix was lying near the neck of the gall-bladder. The appendicular artery was arising from the

right colic artery, and small branches from the right colic artery were supplying the distal part of the appendix. Routine appendectomy was performed and the patient's postoperative progress was satisfactory. The pathologist's report was: "Appendix, length 10 cm, contains liquid faeces. Microscopically, no histological abnormality."

Every surgeon is familiar with the various positions of the appendix, including the rare intramural,¹ intramesenteric,² and retrocaecal intramesenteric³ positions. In uncommon circumstances one may find the appendix on the left side⁴⁻⁵ or in the subhepatic region owing to malrotation of the gut. A similar thing can happen in situs inversus.⁶ In the present case the appendix was originating from the ascending colon near the lateral taenia about 8-10 cm away from the ileocaecal junction and was mostly retrocolic. No previous report of a similar case could be found. If one remembers the development of the appendix,⁷ the anomalous origin in this case can be explained by accepting the differential growth of the caecum on the medial side of the appendix instead of the usual lateral side.

I thank Mr. J. C. Morris for allowing me to report this case.

—I am, etc.,

S. GAYEN

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Dartford, Kent

- ¹ McGladdery, W. F., *Central African Journal of Medicine*, 1972, 18, 54.
- ² DiGiuglio, V. S., *Illinois Medical Journal*, 1969, 136, 252.
- ³ Shapiro, N. M., Michels, L. M., and Hurwitz, S., *California Medical Journal*, 1963, 98, 158.
- ⁴ Owen-Smith, M. S., *British Journal of Surgery*, 1969, 56, 233.
- ⁵ Beneventano, T. C., Schein, C. J., and Jacobson, H. G., *American Journal of Roentgenology*, 1966, 96, 344.
- ⁶ Wyndham, N., *Medical Journal of Australia*, 1970, 2, 726.
- ⁷ Solanke, T. F., *British Journal of Surgery*, 1970, 57, 100.

Nutrition of the Pregnant Woman

SIR,—In your leading article (5 May, p. 255) you state that it is "a question of philosophy rather than of science" as to "whether the mother's natural surge of appetite which results in her storing fat should be taken as the best guide to her dietary intake". This assumption is based on the false premise that excess fat "is usually lost between pregnancies without any conscious effort", and Billewicz and Thomson's paper from Aberdeen¹ is quoted in support. Their figures show in fact that the average or mean weight gain of 531 women after three pregnancies was as much as 4.3 kg (9.5 lb). This, to my mind, is a significant amount of fat, and its significance should not be discounted by adjusting the figure downwards because all Aberdeen women were gaining weight with age during the years concerned (1949-64) and by taking the median figure instead of the mean, as the authors do.

Africans living a natural life and eating unprocessed food do not gain weight with age during adult life,² and this is now being accepted as the ideal towards which we all should aim. Obesity is a major health problem in the western world, mainly because we eat sweet foods and refined cereals for pleasure rather than to satisfy hunger, and the best time to alter bad eating habits is during pregnancy, when women are especially health-conscious. In a consecutive series of 100 of my own antenatal patients