just on the results of radioimmunoassay. Since a proportion of Addisonian patients will go on to develop permanent hypothyroidism and since the return to normality in the remainder may be delayed by many months, it would seem wisest to give thyroxine initially to all who are biochemically hypothyroid, and to attempt cautious withdrawal in some of them at a later stage.

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Guillain-Barré syndrome associated with campylobacter infection

SIR,—Drs K M Rhodes and A E Tattersfield (17 July, p 173) reported the first published case of Guillain-Barré syndrome in association with campylobacter infection. We have seen a patient with Guillain-Barré syndrome and some similar features to those of their case.

A 42-year-old lorry driver developed the initial symptoms of Guillain-Barré syndrome nine days after the onset of four days' diarrhoea, which he experienced abroad. The weakness of the extremities peaked on the fourth day after the appearance of the first symptoms of the syndrome. At that time the patient was unable to walk but was not brought to hospital until the eighth day. He did not need artificial respiration and his condition started improving during the next few days. The cerebrospinal fluid showed a protein content of 830 mg/l. Routine blood investigations were normal, and the stool cultures for *Salmonella*, *Shigella*, and *Yersinia* were negative. No antibodies compatible with acute viral, salmonella, yersinia, brucella, or chlamydia infection were found. Stool was cultured only once for *Campylobacter* (23 days after the onset of diarrhoea), and *Campylobacter fetus* subspecies jejuni was isolated. Electroneuromyography 24 days after the onset of Guillain-Barré syndrome showed pronounced exaggerated insertion activity throughout the muscles, reflecting axonal involvement. The motor and sensory nerve conduction velocities were normal. Two months later considerable denervation activity was present as a sign of extensive axon damage. After six months' follow-up there was atrophy of the distal muscles and weakness in all extremities, and, unexpectedly, the patient also had a high-frequency tremor and slight rigidity of the extremities.

The common features of our patient and that of Drs Rhodes and Tattersfield were the clear-cut period of acute diarrhoea, the short incubation period of Guillain-Barré syndrome, and the extensive axonal damage with considerable residual neurological signs even six months after the onset of the disease. *Campylobacter* appears to be the most common bacterial cause of diarrhoea.¹ We agree with Drs Rhodes and Tattersfield that at least some cases of Guillain-Barré syndrome developing after "non-specific" enteric infection are connected with the occurrence of campylobacter infection. In the light of our patient's tremor we also noted that *Campylobacter* has been suggested as a causal factor in some diarrhoea-associated convulsions,^{2,3} and involvement of the nervous system has also been reported.⁴ The pathogenesis of

Guillain-Barré syndrome might be different in various cases,⁵ and tissue damage in the nervous structures might itself be related to the causal agent. In these two cases our attention was particularly drawn to the extensive axonal damage.

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Impact of maternal serum alpha-fetoprotein screening on antenatal diagnosis

SIR,—We are forced to take issue with the conclusions drawn by Dr David J H Brock (31 July, p 365). Dr Brock fails to indicate: (a) the number of terminations in which no significant abnormality was found; (b) the number of neural tube defects not identified by a raised maternal serum alpha-fetoprotein estimation; and (c) the morbidity induced by amniocentesis performed for alpha-fetoprotein estimation. Our experience¹ is that this test is insufficiently sensitive and insufficiently specific. It is usually positive in the presence of a gross fetal neural tube defect, when prospects for survival of the fetus are low. It is much less reliable in the presence of lesser degrees of neural tube defect, when survival is likely and may lead to long-term institutional care.

Dr Brock, reiterating the well-known inconclusive association between moderately raised maternal alpha-fetoprotein and fetal neural tube defect, does not provide sufficient evidence to justify national adoption of the screening test.

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Left-handedness

SIR,—I would like to comment on the reply given to the reader who wished to know if there was any association between left-handedness and a variety of dextrocardia and transposition of thoracic and abdominal viscera (10 July, p 118). Your expert's answer, that "there is no association between the two," may not be correct. In a recent study on "Left-handedness: association with immune disease, migraine, and developmental learning disorder,"¹ we found the frequency of autoimmune diseases, migraine, and developmental learning disorders to be increased in left-handed subjects. In obtaining this information we did not ask

about dextrocardia or transposition of viscera, but unsolicited information from several families with left-handedness and autoimmune disease provided multiple examples of transposition of great vessels and of thoracic and abdominal viscera. The theoretical relation between developmental brain abnormalities, laterality, autoimmune disorders, and learning disorders is discussed in our paper.

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Inaccuracy of London School of Hygiene sphygmomanometer

SIR,—Dr Desmond J Fitzgerald and others (2 January, p 18) showed recently that the London School of Hygiene (LSH) sphygmomanometer, when compared with a Hawksley random-zero sphygmomanometer, underestimated the systolic blood pressure by 7.1 mm Hg and the diastolic pressure by 3.6 mm Hg (average of 20 patients in whom simultaneous duplicate readings were obtained by connecting two stethoscopes via a connection to the cuff). Dr J G Evans (13 March, p 823) pointed out that the random-zero sphygmomanometer in turn has been shown to underestimate systolic pressure by 1.2 mm Hg and diastolic pressure by 2.2 mm Hg compared with an ordinary sphygmomanometer.¹ Thus, the LSH sphygmomanometer can be expected to show pressures around 8 mm Hg lower for systolic and 5.6 mm Hg lower for diastolic than ordinary clinic readings made with a mercury sphygmomanometer.

We have compared an ordinary mercury sphygmomanometer with the Auto-Manometer,² an electronic device manufactured by Southern Computers Ltd (PO Box 6091, Dunedin North, New Zealand) which operates in a manner similar to the LSH sphygmomanometer. It was originally developed in the hypertension clinic of Professor Simpson at the Wellcome Research Foundation in Dunedin, New Zealand. We used the same methods as Dr Fitzgerald and others with the modification that we let the two observers make two recordings in each patient, between which they changed manometers. We picked 20 patients at random from those making routine visits to the hypertension clinic.

The results are shown in the figure. The linear regression lines based on the average values of the two observations for each apparatus were Auto-Manometer = 1.10 mercury manometer (systolic)—19.3 and Auto-Manometer = 1.18 mercury manometer (diastolic)—21.9. The average values showed a mean underestimation by the Auto-Manometer of 3.8 mm Hg for systolic pressure and 4.4 mm Hg for diastolic pressure (both $p < 0.001$) at a pressure level of around 150/90 mm Hg. The regression lines are not quite parallel to the lines of identity, so the error is probably smaller in higher blood pressures. When one trained observer (GN) checked Auto-Manometer readings against self-read mercury readings in 16 patients, Auto-Manometer readings were also consistently lower than mercury readings by 1.0 mm Hg systolic and 2.9 mm Hg diastolic (both $p < 0.01$).

At 10 calibrations with the cuff around a cylindrical object the mercury readings were higher by 0.5 mm Hg. When the two observers were checked against one another, although average differences were small, the difference between the two occasions could be very large in individual cases. For the mercury manometer, systolic pressure

Comparison of recordings of blood pressure in 20 patients with the Auto-Manometer and with an ordinary mercury manometer. Unbroken lines indicate linear regressions; broken lines are the lines of identity.

differed on average ($\pm$ SD) by 0.05 ± 7.08 mm Hg (range +20 to -12) and diastolic pressure by 0.95 ± 5.09 mm Hg (range +10 to -4) (both insignificant). For the Auto-Manometer systolic pressure differed by 0.6 ± 6.21 mm Hg (range +10 to -13) and diastolic pressure by 3.0 ± 5.66 mm Hg (range +16 to -10; $p < 0.05$). This presumably reflects a systematic error of one of the observers in reading the Auto-Manometer diastolic pressures too low. When the readings of the two occasions were corrected for each observer using the regression lines calculated, there were no significant differences in the mean values for any one observer. There was no significant correlation between the differences of the two manometers and the heart rate.

The use of the Auto-Manometer apparently also involves a systematic error of underestimation of the indirect blood pressure when compared with the "non-blind" use of an ordinary mercury sphygmomanometer. This is about 4 mm Hg for both systolic and diastolic pressures and consequently smaller than for the LSH machine as reported by Dr Fitzgerald and others. We think that this error is due to the way the blood pressure value is registered in the mind of the observer when the Korotkoff sounds are heard: it takes one or two heart beats to determine that the sounds heard are not artefacts; in the case of a "non-blind" measurement the brain extrapolates back to the value of the first rather than the last sound, while at a blind measurement the delay cannot be rectified. The Auto-Manometer does not, of course, have the calibration error of the LSH, which no doubt contributes to the greater error of that machine. In our hands, the Auto-Manometer has proved to be a valuable instrument for measuring the blood pressure in a blind way. This is of particular importance in, for example, blood pressure studies with non-blinded treatment periods or when serial measurements are performed during interventions such as isometric exercise² or mental arithmetic,³ when previous knowledge of the direction of changes usually seen is a powerful stimulus to observer bias.

In view of the differences between different

methods of measurement, it seems reasonable to demand that the exact method of measurement should always be stated. Otherwise, comparisons between different sets of experiments may be misleading.

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Electrocardiographic abnormalities and autonomic dysfunction in Guillain-Barré syndrome

SIR,—On 24 April (p 1231) you published a case report by Dr T G Palferman and others about electrocardiographic abnormalities and autonomic dysfunction in Guillain-Barré syndrome. Recently we have seen two patients with Guillain-Barré syndrome and tachycardia and at the same time a fall in blood pressure.¹ Both patients had pulmonary emboli.

Case 1—A 44-year-old woman had definite Guillain-Barré syndrome with weakness in both arms and legs; her breathing was unaffected. After two weeks in hospital she suddenly developed a fall in blood pressure and sinus tachycardia. The diagnosis was uncertain, but we considered her to have autonomic dysfunction due to her Guillain-Barré syndrome. Over the next week she improved, but two weeks later she had a sudden cardiac arrest and died. Necropsy showed thrombosis in the right femoral vein and also multiple, bilateral pulmonary emboli of different ages.

Case 2—This man was already having assisted ventilation because of his Guillain-Barré syndrome. He had a sudden fall in blood pressure, tachycardia, and a sudden fall in arterial oxygen saturation. We made the clinical diagnosis of pulmonary embolus and treated him with heparin. He improved and is now doing well.

We want to point out that patients with Guillain-Barré syndrome are at high risk of developing thrombosis and pulmonary emboli and that this might be a differential diagnosis to consider when these patients show electrocardiographic abnormalities or sudden changes in blood pressure or heart rate.

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¹ Hagman A, Malmberg K. *Läkartidningen* 1982; June 30.

Fees for cervical smears

SIR,—It was interesting to note Dr R M E Stone's observation (31 July, p 381) on the increase in abnormal cervical smears in his practice. We would like to reassure him that he is not alone in his observations and that we, too, question the validity of the DHSS schemes. At the Middlesex Hospital we have noticed a threefold increase in the number of cases of cervical intraepithelial neoplasia grade 3 during the last five years. During the two years 1980-1, 89 cases of cervical intraepithelial

neoplasia grade 3 and four cases of invasive carcinoma in women under 35 years were diagnosed from a single source, the Margaret Pyke Centre. It has been suggested that this increase in young women may be a disease variant and not typical of the natural history of the majority of cases with cervical carcinoma. If this is the case then it is a variant which is increasingly common, which appears to select our (and Dr Stone's) practice, and which would not be covered by DHSS guidelines.

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Coronary disease

SIR,—We are encouraged to read Dr Julian Tudor Hart's positive strategy for identifying subjects in general practice at risk for coronary disease (31 July, p 347). We hope this will become widespread in general practice because the results of prospective studies, not often as well controlled as he would like, demonstrate the benefit of his philosophy.

There are two points that we would like to make from our extensive experience. The first is that doctor's time is wasted on the detection process, which can be done efficiently by any suitably trained observer, including school leavers. Doctors should concentrate on counselling and following up the group thus identified. Secondly, we disagree with the "illusion of the computer." The computer has a well tried place in the identification of high-risk groups, the quality control of the observers, and the structuring of the record so that it becomes a useful research and evaluation tool. Our own system actually does this and it was designed more than six years ago. We are, at present, translating and modernising the system to run on smaller, cheaper units suitable for use in smaller screening operations and, of course, general practice.

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Managing cerebral malaria

SIR,—Your three correspondents Drs N J White and D A Warrell (7 August, p 439) and Dr D G Dawson (p 444), have raised questions concerning the use of adrenaline injections for treatment of malaria. This hoary subject is periodically revived by those not conversant with its history. In the 1930s Maurizio Ascoli of Palermo described his "humoral" treatment of chronic malaria associated with relapses, splenomegaly, anaemia, and cachexia by intravenous administration of adrenaline.^{1,2} He and his followers claimed remarkable effects of this treatment, which apparently eliminated from the spleen the parasites lingering in this organ when it contracted as a result of the injection. The initial daily dose of adrenaline was 0.01 mg at high dilution; this dose was increased gradually until after 20 days a dose of 0.1 mg was reached. When necessary quinine treatment was added. Oddly enough, the success of this treatment was confirmed