suggestion of Körner¹ that Homer described the first case in history in the *Odyssey*² where Ares and Aphrodite are described as not being able to separate after illicit intercourse.

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¹ Körner, O, *Die ärztlichen Kenntnisse in Ilias and Odyssee*, München, Bergmannverlag, 1929.

² *Odyssey*, 8, 267-361.

Antenatal prediction of fetal sex

SIR,—I would like to report another interesting case. In 1978 a multiparous woman who had previously given birth to a male child with X-linked muscular dystrophy had an amniocentesis during her third pregnancy and the fetal karyotype was 46,XY. As this fetus was apparently a male, with an even chance of being afflicted with muscular dystrophy, and the mother was unprepared to accept the risk, the pregnancy was terminated. The fetus delivered was phenotypically female but further investigations revealed testicular feminisation syndrome to be the cause.

It is certainly a strange coincidence that the parents are possibly at risk of yet another X-linked condition, but I am pleased to report that there has been a further pregnancy with a happy ending, with the birth of a normal girl this year.

I am grateful to Dr Marina Seabright, regional department of cytogenetics at the Salisbury group of hospitals, and Dr Martin Bobrow, department of medical genetics at Oxford, for their expert help.

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Screening for thyroid dysfunction in diabetics

SIR,—We were interested to read Drs John Feeley and T E Isles's report on the prevalence of thyroid dysfunction in diabetics (23 June, p 1678) and have recently completed a similar study on 347 randomly selected diabetics with no previous history of thyroid disease.¹

We too have found the prevalence of primary thyroid failure as indicated by a raised serum thyrotrophin concentration to be surprisingly high at 10%, but, in contrast to Drs Feeley and Isles, showed that insulin-dependent diabetics accounted for the majority of patients with thyroid failure, of whom 13% had a raised serum thyrotrophin concentration, usually accompanied by thyroid microsomal antibodies (88%). Most (83%) of the insulin-dependent diabetics with raised serum thyrotrophin were aged over 50 years. Only 6% of insulin-independent diabetics of a similar age distribution had a raised thyrotrophin concentration, of whom 60% had thyroid microsomal antibodies. The higher prevalence of an elevated thyrotrophin concentration in insulin-dependent than in insulin-independent diabetics is in accordance with the prevalence of thyroid microsomal antibodies in these two conditions.²

Interpretation of Drs Feeley and Isles's findings would be helped by provision of details of serum thyrotrophin concentrations in patients of group 2, in view of the low upper limit of their normal range (4.5 mU/l). How

many patients in group 2 had a serum thyrotrophin concentration greater than 10 mU/l? The age distribution of the three groups of patients categorised on the basis of diabetic treatment is also omitted and would probably indicate that the insulin-dependent are considerably younger than the insulin-independent diabetics. Furthermore, the criteria for selecting patients to be studied are unclear. Patients who have been treated for hypothyroidism or who have been previously treated for hyperthyroidism must surely be "suspected of having thyroid disorders."

Some of the patients in group 3 may have underlying renal insufficiency to account for a low free thyroxine index without coincident elevation of serum thyrotrophin or the presence of thyroid microsomal antibodies. Many patients in group 2 who have an elevated thyrotrophin concentration in the absence of microsomal antibodies (which we uncommonly saw) may be found at follow-up to have normal thyrotrophin concentrations.³

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¹ Gray, R S, *et al*, *Journal of Clinical and Laboratory Immunology*, 1979, 2, 221.

² Irvine, W J, *et al*, *Lancet*, 1970, 2, 163.

³ Tunbridge, W M G, and Clark, F, *Annales d'Endocrinologie*, 1978, 39, abstract 115.

* * * We sent a copy of this letter to the authors, whose reply is printed below.—ED, *BMJ*.

SIR,—The recent study by Dr R S Gray and others¹ supports our contention that screening for thyroid dysfunction in a diabetic population is worthwhile. Their main findings (almost 3% prevalence of previously unrecognised mild clinical hypothyroidism and a 7% at-risk group) are similar to ours but occur in different groups of patients. Many of the apparent discrepancies may be explained by the differences in the diabetic populations studied.

In the Edinburgh study 53% (and almost half of the over-50 group) of diabetics were treated with insulin as opposed to 43% in our study, where the insulin-dependent patients were considerably younger than our insulin-independent group. In the study by Gray *et al* the population was evenly divided between the sexes, whereas 62% of our population were female. More important, our diabetics were on average eight years older and there is evidence² that the prevalence of thyroid antibodies in insulin-dependent diabetics decreases with advanced years. Undoubtedly some of our elderly diabetics would have better physiological blood sugar control on insulin (and thus become classified as insulin-dependent) but the benefits of such therapy for this age group remains to be established.

The upper limit of our normal serum thyrotrophin (TSH) range (4.5 mU/l) is based on 350 control subjects, and although this is not greatly different from the Edinburgh limit of 5.7 mU/l it must be stressed that TSH values are not normally distributed but skewed to the right; thus mild elevation of TSH may reflect suboptimal thyroid function. In group 2 of our study 11 patients had TSH levels greater than 10 mU/l. We agree that TSH levels may vary from time to time and we stressed that it is those patients with both

raised TSH and antibodies who are at particular risk of developing hypothyroidism. Our patients were selected as not suspected of having overt thyroid disorders and we thus included patients who had previously been treated for thyroid disease. Similarly Dr Gray and his colleagues, although selecting patients with no previous history of thyroid disease, must have suspected some thyroid disease in the insulin-dependent population, where they had previously shown thyroid antibodies occurring commonly.³ We feel that patients with previous thyroid disorders should be included in a screening programme and that it is both unwise and impractical to exclude such groups.

It has long been recognised that insulin-dependent diabetics are at risk of developing thyroid disorders. Our screening has also identified elderly women (in part reflecting this group's propensity to develop hypothyroidism in the non-diabetic community), whether insulin-dependent or insulin-independent, as an additional at-risk group.

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¹ Gray, R S, *et al*, *Journal of Clinical and Laboratory Immunology*, 1979, 2, 221.

² Whittingham, S, *et al*, *Lancet*, 1971, 1, 763.

³ Irvine, W J, *et al*, *Lancet*, 1970, 2, 163.

Diagnosis of multiple pregnancy

SIR,—Mr G J Jarvis (8 September, p 593) considers in his summary that two ultrasonic examinations are necessary to confirm the clinical suspicion of multiple gestations, but in the text he suggests that two ultrasonic examinations may be necessary. He also states that since 1964 little has been reported on problems about diagnosing twins, and that no published reports state the percentage of twins diagnosed at various stages of the antenatal period. Taylor¹ and Sundén² reported ultrasound to be a good method of detecting twins in selected populations—that is, in the patients referred for screening as clinically suspected of carrying twins.

Since 1973 ultrasonic routine screening has been included in the antenatal care of all pregnant women attending the department of obstetrics and gynaecology at the University Hospital, Malmö.³ Of 155 examined twin pregnancies from 1973 to 30 September 1979, two were mistakenly interpreted as singletons at the first screening (the faulty diagnosis being made by untrained operators in the beginning of the screening period). No false-positive diagnoses were made. This makes a methodological error rate of 1.3% compared with 21% (17/83) reported by Dr Jarvis. During the last two and a half years we have detected all twin pregnancies (54) by a single ultrasound examination in the 17th week. Thus our results do not support the apprehension of (among others) Powers⁴ that accuracy might be reduced if these techniques were applied to a general population of pregnant women.

Since 1964 many authors have reported on physical⁵⁻⁶ and biochemical methods⁷⁻⁹ to detect twins. I reported in 1976 that the introduction of ultrasonic screening of pregnant women in Malmö increased the antepartum detection of multiple pregnancy and dimin-

ished the average gestational age for detection.⁵ I agree with Dr Jarvis that the early diagnosis of twin pregnancy is a prerequisite for adequate antenatal care and management of delivery. In my opinion ultrasound, if available, is the diagnostic method of choice. With modern technology and with trained operators the method is highly reliable as a screening method. More than one scan should not be necessary—certainly not if twins are suspected and specially sought. With the new real-time scanners it is tempting to make it everyone's tool. However, I wish to stress that ultrasound diagnostics, similar to radiology diagnostics, should be performed by specially educated and trained operators if the method is not to be brought into disrepute.

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- ¹ Taylor, E S, *et al*, *American Journal of Obstetrics and Gynecology*, 1967, **99**, 671.
- ² Sundén, B, *Journal of Obstetrics and Gynaecology of the British Commonwealth*, 1965, **72**, 952.
- ³ Grennert, L, Persson, P-H, and Gennser, G, *Acta Obstetrica et Gynecologica Scandinavica*, 1978, suppl 78, p 5.
- ⁴ Powers, W F, *Obstetrics and Gynecology*, 1973, **42**, 795.
- ⁵ Grennert, L, *et al*, *Lancet*, 1976, **1**, 4.
- ⁶ Westin, B, *Acta Obstetrica et Gynecologica Scandinavica*, 1977, **56**, 273.
- ⁷ Mägisté, M, *et al*, *American Journal of Obstetrics and Gynecology*, 1976, **126**, 697.
- ⁸ Lidbjörk, G, Kjessler, B, and Johansson, S G O, *Acta Obstetrica et Gynecologica Scandinavica*, 1977, **69**, 45.
- ⁹ Vince, J D, *et al*, *Lancet*, 1977, **1**, 43.

Disinfection with glutaraldehyde

SIR,—I am afraid that Dr A C Mair's answer (15 September, p 673) to my letter (18 August, p 444) contains numerous inaccuracies which ought to be corrected. This gentleman wrote that I was wrong when I stated that alkaline glutaraldehyde has a maximum "use life" of 14 days.

My statement was that "under normal hospital use the alkaline compositions have a short life (maximum 14 days)." I am here talking of the continuous use and reuse of the same solution in the Arbrook Cidematic machines, which account for at least 60% of the Cidex market in US hospitals. I am not talking of the activity of a solution stored for 28 days as described by Miner *et al*.¹ Everyone knows that all cidal solutions in the USA have to show that they are still active after storage for 28 days. This has nothing to do with the activity of a solution in normal repeated use in hospitals in Cidematic machines.

Dr Mair should know that the only published data from Arbrook on this subject were released by Masferrer *et al*.² They showed that after 15 days (or 69 cycles) of continuous hospital use the Cidex content in glutaraldehyde was down to 0.87%. To reach the same level with Sonacide in the same machine one needed 26 days (or 124 cycles). Dr Mair should also know that Bageant from the University of Virginia hospital recently wrote³: "Arbrook is now recognising this problem [drop in glutaraldehyde concentration]. On their new Cidematics, they will be installing counters as well as timers. After 40 cycles or two weeks, whichever comes first, the glutaraldehyde will be discharged. The counters will also be installed on existing Cidematics in hospitals that want them."

In other words, Arbrook-USA had to recognise, under the supervision of the Environmental Protection Agency, that the active glutaraldehyde content of its alkaline

glutaraldehyde solutions drops faster under continuous hospital use than that of potentiated acid formulas. This indeed is in full agreement with the theoretical laws of physics (mechanical dilution) and chemistry (polymerisation of aldehydes).

By clearing the confusion created over the standard 28 days' storage life (referred to in Dr Mair's comments) one can see from the above figures and statements that in the USA Arbrook scientists themselves recognise the shorter two weeks' continuous use in hospital practice.

Cidex and Sonacide have long been proved excellent products in the US hospital market. Each one obviously has its merits and limitations, but it is important both from the scientific and from the practical viewpoint to distinguish between storage life and use life.

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- ¹ Miner, N A, *et al*, *American Journal of Hospital Pharmacy*, 1977, **34**, 376.
- ² Masferrer, R, *et al*, *Respiratory Care*, 1977, **22**, 259.
- ³ Bageant, R A, *Respiratory Care*, 1979, **24**, 666.

The concept of disease

SIR,—The concept of disease (29 September, p 757) has generally been approached from the adult standpoint, particularly that of the professional adult—medical, sociological, or philosophical.

Alternatively, however, the way that the concept develops throughout childhood can be explored. This shows up developmental sequences and cultural differences, but it also emphasises the way that illness and concepts of disease get drawn into the arena of communication between individuals, their families, and professionals such as doctors and teachers. It is essential that this topic is further developed to help improve communication between caring agencies and their clients, and to clarify what are frequently conflicting expectations of each other. This puts "disease" in the category of a word which is used to communicate different things for different people at different times. In order to respond most appropriately to our patients we need to have some concept of what is happening for the individual in the context of our meeting with that person—which consequently limits our use as doctors to a particular situation; "disease and doctor consultation," and we must remember that this is only one of many possible contexts.

I am currently researching how children develop their concepts of illness, and would be interested to hear from those with a similar interest.

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*.*This correspondence is now closed.—ED, *BMJ*.

Anaesthesia

SIR,—Dr J Alfred Lee in his admirable paper (10 November, p 1192) tells us that "anaesthesia in the 1920s differed little from what it must have been in the 1850s"; but, to judge by his description, anaesthesia in the 1920s differed little from that of the 1940s when I was first exposed to it as a medical student—

except, perhaps, that the water-sight Boyle's machine was in regular use.

Dr Lee's paper reminds us that, though the specialty of anaesthesia was a late comer and a late developer in medical history, its progress within the span of one man's professional lifetime has been as exciting as that of aviation or atomic power and that, as a unique clinical service specialty, it has facilitated advances in almost all other branches of medicine and surgery.

Dr Lee is, however, characteristically modest about his own share in this dramatic story. He is internationally revered as an innovator and a teacher; as founder-author of the anaesthetists' "bible," *A Synopsis of Anaesthesia*; and as president of the Association of Anaesthetists of Great Britain and Ireland. His contribution to the field of local analgesia is renowned and his pioneer work in the introduction of postoperative (recovery) wards led the way in the development of intensive care. His school of anaesthesia at Southend in the 'fifties and 'sixties was of world-wide repute and surpassed that of most teaching hospitals of the time in its willingness to teach and guide its trainees—to have been a registrar with Lee at Southend is to bask in reflected glory, like a soldier who served with Wellington at Waterloo or a singer who was taught by Caruso.

Dr Lee still practices and teaches anaesthesia in his well-earned retirement. May he long continue to do so.

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Rubella immunisation

SIR,—A study I recently completed on services for the prevention of congenital rubella in an area of South-east England showed that the risk of rubella in pregnancy was widely appreciated but that most women did not know what practical steps should be taken to allay anxiety. Although most family practitioners were ready to provide serological screening and immunisation on demand, few women were aware that it was reasonable, before becoming pregnant, to ask a family practitioner about rubella prevention. Even women who had been screened for rubella antibodies as antenatal patients were mostly ignorant of the fact that they had been tested; the result was usually only mentioned to those considered to need immunisation.

Communication with the public must be improved if the policy statement that prevention is everybody's business is to lead to responsible action. The following three improvements would be valuable to the programme of congenital rubella prevention:

(1) General practices should display posters reminding patients to ask the GP about prevention. If the practice does not provide preventive services the poster should state the place and time of a public clinic which does. The DHSS should ensure that such posters are supplied to all practices.

(2) The makers of vaccines should include with phials information suitable for the adult patient. This information would reinforce verbal advice and give answers to the questions commonly asked.

(3) Women who have been screened for rubella and found seropositive should have "immune to rubella" stamped on a medical