we started using only intramuscular Metrodin, followed by subcutaneous Metrodin high purity in the latter months of 1993. The case mix, treatment protocol, and stimulation remained unchanged, as did the indications for oocyte retrieval or in vivo fertilisation over that time.

We noted two significant changes. Firstly, the number of patients ready for oocyte retrieval on the prearranged day fell, and the duration of stimulation and dosage required increased. In 1992 only 9% of patients required additional stimulation and oocyte retrieval to be delayed by seven days or more, whereas in 1993, 22% of oocyte retrievals took place more than seven days after the scheduled date.

The second change we noted was an appreciable drop in the pregnancy rate. In 1992, while human menopausal gonadotrophin was used, the pregnancy rate per cycle started was 25%, whereas in 1993 the pregnancy rate fell to 11%. The pregnancy rate did not improve on changing from Metrodin to Metrodin high purity. Since the beginning of 1994 we have reverted to human menopausal gonadotrophin (Pergonal). Of the completed in vivo fertilisation cycles from which we know the outcome, the pregnancy rate is 29%.

Our data suggest that exogenous luteinising hormone may have an important role in follicular development when pituitary desensitisation has been induced, or that the extraneous proteins highlighted recently may include various growth factors,² which may have an important direct role in follicular development. We suggest that there are clinical indications as well as cost factors¹ to support the use of human menopausal gonadotrophin rather than follicle stimulating hormone in ovarian stimulation in all patients receiving assisted conception, and not just the small group of patients with hypogonadotrophic hypogonadism.

The role of exogenous luteinising hormone and extraneous proteins in ovarian stimulation will be answered only by the development and meticulous clinical and biological evaluation of recombinant follicle stimulating hormone and luteinising hormone preparations.

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Alcohol histories taken from elderly people

EDITOR,—We wish to respond to D Thompson's comments on our study.¹ Thompson states that we claim that elderly people are more likely than younger people to hide their drinking and that there is no evidence to support this. This information is stated in our introduction and is based on earlier literature.²

Thompson also emphasises that studies have shown that older people drink less than younger ones. But declining alcohol intake does not imply that it has no clinical importance. Compared with young people, elderly people are much more vulnerable to the effects of alcohol.² Also, Curtis *et al* point out that as the population ages there may be many more older people who are dependent on alcohol.³

Thompson implies that failure to record histories does not necessarily indicate an under-

estimation of the importance of the ingestion of alcohol by elderly people. But failure by doctors to take an adequate history must lead to the diagnosis being missed and to underestimation of the problem. Several previous studies—for example, that by Barrison *et al*⁴—in young people have shown that doctors record inadequate alcohol histories. Rowland *et al* state that failure by junior doctors to take an adequate alcohol history relates to the patient's condition, pressure of time, and the doctor's attitude towards alcohol problems.

For several reasons, doctors are less likely to take an adequate alcohol history from elderly people.^{2,3} This seems amply confirmed by our study, in which the quality of history taking was significantly poorer ($P < 0.001$, $\chi^2 = 28.559$, $df = 2$) than that found in Barrison *et al*'s study.⁴

Alcohol problems may present in elderly people in many different ways. Curtis *et al* state that alcohol problems in elderly people may have replaced syphilis as the "great masquerader."⁵ Hence our recommendation—that a quantitative alcohol history, which should include our question on alcohol in tea or coffee, should be taken routinely from all elderly people—seems well justified. Caution is often sensible, but it seems incautious, even without our evidence, not to implement this precaution. It is simply good practice to take a good alcohol history.

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Correlation, regression, and repeated data

EDITOR,—J Martin Bland and Douglas G Altman's note on correlation, regression, and repeated measures draws attention to the problem of calculating a correlation coefficient based on repeated observations of the same subject.¹ But they fall prey themselves to the common misjudgment of placing too much importance on the significance of the correlation coefficient.

The basic problem is the way in which significance is calculated as the correlation coefficient is strongly affected by the number of cases for which there are pairs of data. If, for example, you have about 500 cases the correlation coefficient needs only to be 0.088 to be significant at the 0.05 level; if, however, you have just 18 cases the correlation coefficient will need to be at least 0.468 to achieve the same level of significance. Yet it is patently absurd to say that a correlation of 0.4 is less important in the latter case than a correlation of 0.088 in the former simply because the correlation of 0.088 is more significant. The second coefficient is clearly larger and therefore of more interest, though our test of significance tells us that we have to be more circumspect in this second case than in the first in inferring that the relation could not have arisen by chance.

When the number of observations is small the sample correlation will be a biased estimate of the population correlation coefficient (if you have the smallest sample size possible—just two points—

these will by definition always fall on a straight line and therefore have perfect correlation). To correct for this problem most statistical packages will give an adjusted correlation coefficient known as r_{adj} .

Another important point about correlation, which Bland and Altman omit to mention, is that before correlation coefficients are calculated the scatter plot should be checked to see whether the same subject produces outlying data points for both variables measured. If this is the case it suggests that the subject's data, although possibly legitimate, might have a disproportionate influence on the resulting correlation. An inspection of the scatter plot that Bland and Altman provide for their hypothetical data suggests that this may be an issue with one of their subjects. A recommended method for dealing with this problem is to run the analysis including and excluding such extreme points to see what differences appear in the results.

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Authors' reply

EDITOR,—The brief for the statistics notes was to write a series of short pieces of around 600 words. Most of these will cover only one point concerning the design of a study or analysis. The piece that Rajendra Persaud comments on dealt only with the error of combining repeated observations from several subjects and then analysing the data as if they were a simple sample. It was not intended as a full treatise on correlation. We agree with Persaud about the importance of estimation rather than significance tests and the need to plot data, and have written of these elsewhere.¹⁻³ While it is true that the sample correlation coefficient is a biased estimate of the population correlation coefficient, this was not relevant to our topic. We do not know of any programs that make the adjustment that he mentions.

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Who cares for young carers?

EDITOR,—Sue Jenkins and Candida Wingate discuss the needs of children and young people who help to support adults with disabilities in the community,¹ emphasising "the strains of practical everyday caring." We believe that the emotional strains may need even greater consideration and that they vary, depending on the illness of the person being supported at home.

In 1988 we undertook a pilot study of the effects of multiple sclerosis on the children of sufferers.² In two boroughs we identified 12 families from hospital records and from the membership of the local branch of the Multiple Sclerosis Society. Three parents were fathers and nine mothers; the range of the degree of disability was wide.

Eleven of 15 children complained of extra chores,

as Jenkins and Wingate commented on. Twelve of the children were concerned by change in the affected parent's mental state or personality; nine expressed uncertainties and fears about the future and fears of getting the disease themselves; eight complained of a limited social life and limited communication in the family; six complained of constant nagging and trivial talk; six said that they could not turn to their affected parent for advice or decisions; and four complained that the affected parent had no idea of their feelings.

Our results highlight how even minor or subtle disabilities in a parent can cause children considerable anxiety. While the impact of disability on partners or adult carers is well recognised, the impact on different generations is neglected¹ and worthy of further discussion.

The complexities of meeting the varied needs of those suffering from multiple sclerosis is being increasingly recognised,⁴ although the needs of children and young people still fail to be addressed. Meeting the practical needs is straightforward through care attendant schemes.⁵ We still have great difficulties in meeting emotional needs. We wish to emphasise the emotional impact of a disabled parent on his or her children. This is particularly important with regard to cognitive impairments or if the parent is deteriorating (however subtly) from emotional, intellectual, or physical impairments.

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Prescribing antibiotics in general practice

EDITOR,—Conrad M Harris and David C E F Lloyd have tried to address an important issue for general practitioners considering their antibiotic prescribing behaviour.¹ Their work, however, is descriptive, and no firm conclusions should be drawn from it on the optimum treatment of infections (often respiratory) in general practice.

I was intrigued to read that a five day course of phenoxymethylpenicillin was more common than a seven day course. Can anyone tell me why? I have tried to trace the evolution of prescriptions for five days of phenoxymethylpenicillin and have found references to intramuscular treatment during an epidemic of infection with group A β haemolytic streptococci in American servicemen,² and the subsequent use of oral penicillin for 10 days,³ for which recommendations persist in the United States.⁴ The reason for the final transition from 10 to five days eludes me.

The authors' comments that five days' treatment could save money have received attention in some general practice magazines.⁵ But the authors make no reference to the effectiveness of the treatments they describe. To conclude that the most commonly prescribed treatment is the most appropriate treatment is based on a false premise—that the majority knows what it is doing.

I have spent the past 12 months trying to contribute to the question of efficacy by asking general practitioners in Avon to take part in a randomised controlled trial comparing antibiotic with placebo for the treatment of sore throats. Interestingly, we have used a five day course of phenoxymethylpenicillin. I hope to report whether this is effective later this year.

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Guidelines

EDITOR,—J D Swales¹ has misconstrued Gene Feder's message² about the importance of ensuring that the way in which guidelines are developed is clear to potential users. Guidelines can change medical practice and lead to improved outcomes for patients.³ To achieve this, however, people who use guidelines (including clinicians, purchasers, and providers) need to identify those that are scientifically valid⁴ and disseminate and implement them appropriately.⁵ Guidelines are more likely to be scientifically valid if systematic reviews or meta-analyses are used to identify and synthesise evidence; they are developed by a nationally based multidisciplinary group that includes representatives of all key disciplines; and explicit links are made between their recommendations and the strength of evidence.⁶ The development of guidelines, however, is subject to many potential biases that reduce their validity. It is important that people who develop guidelines provide adequate documentation to allow potential users to appraise the guidelines validity and to make an informed judgment about whether to adopt them in their clinical practice. The lack of documentation about the methods of development provided with most published guidelines means that potential users are unable to do this.

Feder observed differences in the evidence used to develop two guidelines on hypertension and in the recommendations made. Swales suggests that the differences are probably due to the different value judgments of the people who develop guidelines. An alternative explanation, however, is that the development of one or both of the guidelines was flawed and that the observed differences reflect differences in the validity of the guidelines. Without further documentation about the methods used in their development it is impossible for potential users to judge why the guidelines differ.

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Notifications of measles

EDITOR,—David W G Brown and colleagues' finding that only 17% of sporadic cases of measles were confirmed serologically is not surprising.¹ From February to July 1993 we carried out a survey to determine whether notifications of measles in Tameside Metropolitan Borough were based on reasonable clinical suspicion. We defined a notification as reasonable if the patient had had fever; a rash over the face and body lasting at least three days; and one of cough, coryza, and sore eyes.² For notified cases we interviewed the parents and asked about symptoms and signs; vaccination history; history of measles; and whether, before becoming ill, the patient had been in contact with someone known to have measles. Vaccination history was also checked against child health computer records.

The parents in 35 of 42 cases were available for interview. None gave a history of contact with someone with measles. Only 10 patients fulfilled the criteria, of whom two were under 1 year old and four had been immunised against measles. Of the remaining four patients, three were young men aged between 20 and 22. Ten patients had not had a fever, and 18 had not had a rash of appropriate distribution or duration. Nine were under 1 year old, the youngest being 6 weeks old.

The criteria used in this study were fairly non-specific and would have resulted in the notification of many illnesses other than measles. This makes the notification system impracticable for detecting outbreaks or monitoring vaccination programmes. In addition, many parents in this study expressed anxiety at the diagnosis of measles, particularly in young babies, so care should be taken to ensure that the diagnosis is correct.

Measles is now relatively rare in Britain, and some general practitioners seem to be uncertain of the clinical signs and symptoms. We look forward to the widespread availability of salivary testing and to the day when laboratory confirmation of the diagnosis before notification becomes routine.

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Human rights in Turkey

EDITOR,—As a member of both the Turkish Medical Association and an ethnic minority group from the Balkans, I feel compelled to reply to Paul Mansour's letter about the presence of doctors during executions in Turkey and his comments concerning the Kurdish problem.¹ I have never experienced or witnessed institutionalised racial discrimination against any ethnic minority in Turkey. Since 1980 increasing terrorist attacks by Kurdish separatists have led to a warlike situation in south east Turkey, through which innocent locals have suffered. Terrorism also caused delay in further democratic reforms.

Turkey has a democratically elected parliament in which all ethnic minority groups, including Kurds, are represented. This parliament holds