Table 1—Estimated prenatal and postnatal prevalences (per 100 000) of diagnosis of Turner's syndrome karyotype by test method (amniocentesis or chorion villus sampling) and karyotype (45,X and others). Figures in parentheses are 95% confidence intervals

Karyotype	Prenatal prevalence	Postnatal prevalence among untested girls	Relative risk
Amniocentesis without ultrasonography			
45,X	64.6 (42.2 to 94.7)	16.0 (13.1 to 19.3)	4.0 (2.7 to 6.0)
Others	84.5 (58.5 to 118.1)	15.6 (12.7 to 18.8)	5.4 (3.9 to 7.7)
Total	149.1 (113.8 to 191.6)	31.5 (27.5 to 36.1)	4.7 (3.6 to 6.1)
Chorion villus sampling without ultrasonography			
45,X	271.2 (165.8 to 418.6)	16.0 (13.1 to 19.3)	17.0 (12.0 to 24.0)
Others	40.7 (8.4 to 118.9)	15.6 (12.7 to 18.8)	2.6 (0.9 to 7.9)
Total	311.9 (197.8 to 467.7)	31.5 (27.5 to 36.1)	9.9 (7.0 to 14.0)

false positive prenatal diagnosis, for both 45,X and other karyotypes.

We agree that the supplementary evidence from the study of the 24 liveborn infants with a prenatal diagnosis of Turner's syndrome is weak, especially for the predictive value of the 45,X karyotype. We nevertheless carried out this study to see if it was possible to refute the surprising results of the prevalence study. This was not the case.

Three possible explanations exist for our finding of a much higher prevalence of Turner's syndrome karyotypes at karyotyping after amniocentesis than at postnatal clinical diagnosis: (a) most cases of Turner's syndrome among live-born children remain undiagnosed; (b) the risk of late spontaneous abortion of fetuses with Turner's syndrome after amniocentesis is much higher than estimated from the study; and (c) the specificity of prenatal diagnosis is less than perfect. In our discussion we concluded that (a) and (b) are unlikely to explain the entire discrepancy between the prevalences, especially for amniocentesis, and this led us to question the predictive value of prenatal karyotyping.

Both letters about our study point to the need for more information. We agree, and have corresponded with the board of the Danish Cytogenetic Central Register to try to get access to more information; we have not succeeded so far.

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Minocycline for acne

Food reduces minocycline's bioavailability

EDITOR,—I disagree with R E Ferner and Celia Moss's claim that the bioavailability of minocycline is not influenced when the drug is taken with food.¹ Colleagues and I studied 12 healthy volunteers (students aged 21–27) and found that after a standard meal the bioavailability of the drug was reduced by 14% on average compared with preprandially. This seems a low percentage, but as the interindividual scatter was large (2–51%) the effect on individual subjects cannot be predicted. Thus minocycline should be taken with water about one hour before a meal. After 200 mg minocycline the maximum blood concentration was diminished from 3.2 to 2.4 µg/ml.^{2,3}

If different tetracyclines are to be preferred to minocycline it should be emphasised that meals reduce their bioavailability even more: the bioavailability of doxycycline is reduced by 26% (range 3–49%),² of tetracycline by 46% (13–73%),³ and of oxytetracycline by 41% (4–77%). Milk results in an even greater decrease in bioavailability: that of minocycline is reduced

by 27% (8–61%), of doxycycline by 30% (9–53%), and of oxytetracycline by 83% (45–96%).^{2,4} These findings are relevant to treatment.

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Doctors should not change the way they prescribe for acne

EDITOR,—Andrew Gough and colleagues' paper and the accompanying editorial by R E Ferner and Celia Moss draw the attention of doctors to the uncommon hepatitis and lupus erythematosus-like syndrome induced by minocycline.^{1,2} Since 1973, 30 cases of hepatitis and 11 cases of lupus erythematosus-like syndrome have been reported to the Committee on Safety of Medicines, and in most cases the patients recovered fully.

Two deaths have been reported in association with minocycline. One patient was taking multiple treatment, and the death is more likely to have been due to an antimalarial. The second death was from pancytopenia. I cannot find any other case of pancytopenia occurring in association with minocycline. Thus, considering the extensive use of minocycline and the incidence of idiopathic pancytopenia (1 in 100 000), it does not seem unreasonable to think that the association was coincidental.

It is important to put the facts into clinical perspective. Colleagues and I have treated 25 000 patients with acne over 26 years, many of whom (>85%) have received minocycline. Three patients developed hepatitis and one a lupus erythematosus-like syndrome. Three patients recovered fully, but the patient with the lupus erythematosus-like syndrome had a residual arthritis. We have had no deaths among patients taking minocycline, but five patients have committed suicide because of depression associated with acne; a further 51 patients have been admitted to hospital with a drug overdose due to depression.

Blood taken from over 3000 patients receiving minocycline showed no abnormalities in their liver function or serum aspartate aminotransferase activity.

I do not believe that doctors should change the way they prescribe for acne. I agree with Gough and colleagues that, as with any drug, doctors should be aware of the risk of iatrogenic disease, which with minocycline seems to be reversible in most cases. I agree that tetracycline should be the first line oral treatment and minocycline used as an alternative when patients fail to respond. I also agree that, in the light of Gough and colleagues' findings, new cases of hepatitis and lupus erythematosus-like syndrome will be reported. Nevertheless, acne needs to be treated, by the most appropriate and effective treatment, sooner rather than later to minimise its immense physical and psychological impact.³

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Magnesium is underused in acute atrial fibrillation

EDITOR,—In their series on atrial fibrillation Gregory Y H Lip and colleagues have mainly concentrated on the management of chronic atrial fibrillation.¹ We wish to draw attention to the problem of acute atrial fibrillation and other atrial tachyarrhythmias in critical illness and to the role of magnesium in the treatment of these conditions.

Atrial arrhythmias commonly occur in critical illness; causes include treatment with inotropes, electrolyte imbalances, and hypoxia. Unfortunately, the correction of these abnormalities may not result in reversion to sinus rhythm, and in this situation magnesium is helpful. Magnesium is an important cofactor in the myocardial NaKATPase enzyme, which regulates electrical activity in the heart.^{2,3} Treatment with magnesium may correct rhythm disturbances in patients with both low and normal magnesium concentrations.^{2,3} Its efficacy in ventricular arrhythmias has been well documented.⁴ It is less commonly recognised that magnesium may also be used to correct supraventricular arrhythmias⁵ and has been shown to be superior to amiodarone in treating atrial tachyarrhythmias in critically ill patients.³ Magnesium is a relatively safe drug,² provided that patients are observed for signs of toxicity and the serum concentration is monitored.

It is our impression that magnesium is underused; should we not use it more widely?

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