intubation (with considerable difficulty) and ventilation for 48 hours. Airway pressures at the onset of ventilation were low (29 kPa). Recovery was otherwise uneventful. Bronchoscopy 14 days after admission showed minor swelling of the arytenoid cartilages, but the trachea and main bronchi were normal. Cytomegalovirus was isolated from upper respiratory tract secretions obtained on admission.

This case shows that inflammation confined to the region of the arytenoid cartilages can produce life threatening upper airflow obstruction in the presence of a normal epiglottis, suggesting that supraglottitis might be a more accurate name for the condition. The previous intubation might have contributed to this illness, but the acute onset with fever and lack of features of previous airway trauma suggest that this was a primary infective condition, possibly due to cytomegalovirus.

Upper airway obstruction may be extremely difficult to diagnose in the presence of coexisting lower airway narrowing, as occurs in asthma and chronic obstructive airways disease. Upper airway obstruction should be considered as a cause for lack of improvement in severe exacerbations of asthma and chronic obstructive airways disease and when severe respiratory distress persists despite evidence to suggest diminishing bronchospasm. Indirect or direct fibreoptic examination of the larynx and upper airways should be performed.

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SIR,—Following the cases described by Dr S Gerrish and others I would like to report on a patient in whom microbiological investigations were of interest.

A 46 year old man presented in September 1987 with a 24 hour history of sore throat and progressive difficulty in breathing. He had returned 11 days previously from two months in Kenya. On examination he was feverish with inspiratory stridor. Indirect laryngoscopy confirmed acute epiglottitis and an emergency tracheostomy was performed. Treatment was begun with ampicillin 1 g intravenously four times a day and 100 mg hydrocortisone. A throat swab taken before treatment grew *Haemophilus influenzae* type B biotype I (Oxford Public Health Laboratory) resistant to ampicillin (β lactamase positive), chloramphenicol (chloramphenicol transacetylase positive), trimethoprim, and tetracycline but sensitive to cefuroxime, cefotaxime, and rifampicin (blood cultures were negative). The patient had remained feverish while receiving ampicillin but made an uneventful recovery after 10 days' treatment with intravenous cefotamine 2 g three times a day.

Because ampicillin and chloramphenicol are extremely valuable for life threatening invasive *H influenzae* type b infection such as meningitis (usually in children) and epiglottitis the multiple antibiotic resistances of this probably imported strain were of concern. When there are children aged under 4 at risk rifampicin prophylaxis (20 mg/kg (600 mg maximum dose) a day for four days) is recommended for household contacts of patients with type b *H influenzae* infection[1]; it eliminates carriage of *H influenzae* type b in over 95% of contacts. Antibiotics given to treat acute illness are usually less successful in eradicating *H influenzae* type b, so rifampicin may also be given to the index case after antibiotic treatment with other agents.[1] Our patient had no household contacts under 4 years of age, but because of the multiple resistances of his isolate rifampicin was advised in an attempt to eradicate carriage of the organism.

Blood cultures should be taken in epiglottitis, even though the yield of positive results may be as low as 23%.[2] Examination of the throat in children is traditionally not advised owing to the risk of provoking acute respiratory obstruction. Indirect laryngoscopic visualisation of the glottis is permissible in adults,[2] and practices probably vary regarding the taking of swabs for culture. It would be of interest to know whether laboratories overseas are encountering multiple antibiotic resistance in *H influenzae* type b and if secondary cases of invasive infection have occurred in household contacts of patients with adult epiglottitis.

I thank Mr C J Randall for permission to report this case.

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Psoriatic science

SIR,—Dr C M E Rowland Payne (7 November, p 1158) makes the assumption that keratinocytes cannot be defective in psoriasis because they proliferate at the same rapid rate in vitro as keratinocytes from normal subjects. This is not sufficient evidence, however, on which to base this conclusion; psoriatic and normal keratinocytes may respond differently to agents that enhance or inhibit cell growth.

Recent work carried out in our laboratory suggests that psoriatic keratinocytes do indeed differ from normal keratinocytes in their response to γ interferon, which has previously been shown to inhibit the growth of keratinocytes in a concentration dependent fashion.[1] Keratinocytes from psoriatic lesions, unlike those from normal skin, do not express HLA-DR antigens in the presence of γ interferon, nor is their growth inhibited. Proliferation of keratinocytes from unaffected psoriatic skin is likewise unaffected by γ interferon, although these cells can be induced to express HLA-DR antigens. This abnormal response to γ interferon, a cytokine produced by activated T lymphocytes and present in psoriatic lesions,[2] may contribute to the increased epidermal cell proliferation seen in this disease.

The example cited to illustrate that the primary defect in psoriasis is in the skin—that is, transplanting psoriatic skin to nude, athymic mice—is almost certainly inappropriate. We have recently transplanted psoriatic skin to nude mice and found that four or five weeks after transplantation the skin could no longer be classified as psoriatic as defined by histological criteria. Interestingly, T lymphocytes disappeared from the graft only two days after transplantation. Thus any defect of the skin is not sufficient by itself to maintain a psoriatic lesion; T lymphocytes and probably humoral factors are implicated in the disease process.[3,4] Indeed, in the apparently normal, non-lesional skin of psoriatic subjects both epidermal cell proliferation and the number of T lymphocytes in the dermis are increased.[5]

Our findings suggest, therefore, that the pathogenesis of psoriasis may be explained by an altered response by keratinocytes to mediators produced by activated T lymphocytes, subsequent to their interaction with antigen presenting Langerhans cells in the epidermis.[3] Such T cell factors may alter fibroblast function, and this may in turn stimulate keratinocyte proliferation. Although it is probable that the primary defect in psoriasis is to be found in the skin, it cannot be claimed to reside in the dermal fibroblast on current evidence.

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Compulsory treatment in the community for the mentally ill?

SIR,—Ms Clare Dyer (17 October, p 991) raises urgent issues for the future practice of psychiatry in Britain. In many of the United States such practices are already a reality.

In 26 states and the District of Columbia there are explicit laws governing compulsory outpatient treatment. Eligibility may be under the same condition of dangerousness as inpatient commitment (Michigan); even so, there may be no provision for enforcing treatment for non-compliers (Pennsylvania). Alternatively, a lesser requirement may be set for outpatient treatment. In North Carolina, for example, the probability of dangerousness without continuing treatment is sufficient to warrant compulsory treatment in the community.

In a further 20 states unratified ad hoc arrangements provide alternative models. In Rhode Island an informal order binds the patient to outpatient follow up, failing which admission follows automatically. In Wisconsin a lawyer negotiates a contract, which the patients and treatment team sign. Massachusetts provides for a judge to find a patient "incompetent" to consent to treatment and to merit compulsory outpatient treatment until competence can be re-established. Indeed, the American Psychiatric Association recommends coercion outside hospital only for those patients deemed incompetent.

Practitioners working within these legal provisions describe enforcement as the central issue. When patients refuse treatment can they be forcibly medicated at home? Predominantly the course taken is to admit such patients to hospital and enforce medication there.

Evaluative research in this area is sadly meagre, and case reports[1] do not allow us to judge which model can best deliver treatment to these patients. These legal provisions have come to the fore now that patients are being taken out of institutions and attempt to balance the patient's rights to treatment and to privacy. They carry the danger of becoming overrestrictive responses to the needs of the seriously mentally ill where inadequate

community services are provided. They may degenerate into a form of probation, with brief statutory home visits to supervise medication. In their favour, community treatment orders cut both ways. They both commit the patients to treatment and commit psychiatric teams to providing continuing care for these most disabled and challenging patients.

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1 Geller J. Rights, wrongs and the dilemma of coerced community treatment. *Am J Psychiatry* 1986;143:1259-64.

Secretor state of patients with insulin dependent or non-insulin dependent diabetes mellitus

SIR,—Like Dr Caroline Blackwell and colleagues (24 October, p 1024), we compared the secretor state of 105 diabetic subjects with that of 97 age and sex matched non-diabetic individuals. Our study group comprised 55 patients with type I diabetes and 50 with type II diabetes, and the secretor state was assessed by a standard haemagglutination method using mixed saliva.[1] Statistical analysis was by χ^2 test. There was no significant difference in the distribution of ABO blood groups between those with type I and those with type II disease (table).

Proportions of secretors and non-secretors. Figures are numbers (and percentages) of patients

Study groups	Secretor state	
	Secretor	Non-secretor
Type I diabetes (n=55)	30 (54·5)	25 (45·5)
Type II diabetes (n=50)	35 (70)	15 (30)
Controls (n=97)	65 (67)	32 (33)

Our data therefore cannot confirm those of Dr Blackwell and colleagues but are consistent with previous reports that the occurrence of diabetes mellitus is independent of ABH (O) secretor state.[2,3]

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1 Periera M, Dodd BE, Marchant JV. The detection of A, B and H group specific substances in stains from body fluids by absorption-elution and mixed agglutination techniques. *Med Sci Law* 1969;9:116.
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Hormonochemotherapy in advanced breast cancer

SIR,—The report by Mrs Margaret W Ghilchik and others (7 November, p 1172) produces yet another regimen in which a high response rate is claimed in advanced breast cancer. Hitherto such claims have turned out to be overexaggerations, and there is no reason to think that this is the exception.

This is not a sequential series, and a group in which response is likely to be excellent can be defined by case selection.[1] If only those patients predicted to have good survival were treated then a very good response rate would be achieved with any therapy used. The criteria for admission to the study are not described.

The study is not a trial of hormonochemotherapy against some standard therapy such as hormone therapy alone. Furthermore, the judgment of response lacks external review.

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1 Williams MR, Todd JH, Nicholson RI, *et al*. Survival patterns in hormone treated advanced breast cancer. *Br J Surg* 1986;73:752-5.

SIR,—The results of Mrs Margaret W Ghilchik and colleagues in 40 patients with advanced breast cancer are most impressive. This is so even when the six patients dying from progressive disease before completing the course are included in the denominator to give an overall response rate of 78%. Also impressive is the high proportion of patients who completed several weeks of daily intramuscular medroxyprogesterone. What I cannot understand is the table, where the figures do not seem to fit with the text.

Firstly, 38 sites are listed for the 34 patients assessed so presumably only four patients had more than one site affected. Sixteen patients (text) had a complete response (all disease sites back to normal for at least four weeks by the definition of the International Union Against Cancer) yet only 12 completely responding sites are listed. The 15 patients who had a partial response (text) have 22 sites listed with average durations of six, five, seven, and six months. Do these figures represent the durations of complete response which occurred in partially responding patients as suggested by the text? If so, then they cannot be "means" since by my calculations only four patients had more than one site affected. They certainly cannot represent the durations of partial remissions by site since for the 15 patients in this category the mean duration is given as 11·6 months. An explanation of these discrepancies should be given in view of the excellent overall results reported.

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AUTHORS' REPLY,—We could not include all the details we would have liked within a 600 word short report. The criteria for entry to this study (we did not claim to be carrying out a trial) was advanced breast cancer as defined in the paper. Most of the patients were moribund; hence the deaths of six before they could complete one treatment cycle. Our case selection was of a group who were expected to do badly and the results are not an overexaggeration but an account of the results we obtained.

Regarding points raised by Ms Stewart, eight of the 34 patients had disease at more than one site in addition to large tumour size, with all sites being used to assess response. The average duration of disease free interval for patients with disease at different sites who showed a partial response does represent the mean duration before progression of disease. The mean disease free interval of 11·6 months for patients showing a partial response included the results from two patients who had a long term arrest of disease (five and three years) but who were not disease free. The average duration of disease free interval calculated without these two patients was 6·5 months.

Current results of treatment of disseminated breast cancer are not so good that we should not explore new treatments, particularly in the light of recent scientific advances in our understanding of the disease and in the progress in treatment of other solid tumours. Our results are encouraging and warrant a comparative trial. We would welcome collaboration from other centres in Britain in such a trial and agree with Professor Blamey that external review of response would be required.

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SIR,—The encouraging report from Mrs Margaret W Ghilchik and colleagues described a response rate for metastatic breast cancer of 91% to cyclical sequential hormonochemotherapy (7 November, p 1172).

Response to treatment in the 34 patients who were evaluated was assessed using the International Union Against Cancer guidelines[1] described a decade ago, when perhaps the most important criterion laid down was that of external review of all clinical and radiological data. Too often this criterion is not fulfilled. It is most important when reporting such dramatic improvements in overall responsiveness of advanced disease that a statement should be included to allay fears that external review had not been obtained.

We are also concerned that the duration of response should be sufficient to confirm real benefit for the patients studied. We note that in this report the mean duration of partial response was not more than seven months in any subgroup of patients studied. In those patients with bone metastases (the commonest site for distant metastasis) the mean duration of partial response was five months only. The British Breast Group[2] has suggested that response should be defined as any objective remission of at least six months, and the fulfilment of this stipulation would promote consistency when reporting therapeutic trials.

The enormous variation in reported response to similar treatment will continue until a truly objective assessment of therapeutic benefit is possible by the use of biochemical markers.

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Euthanasia in The Netherlands

SIR,—Dr Mary Bliss and others state that "voluntary euthanasia has been effectively legalised in The Netherlands for several years" (14 November, p 1276). This information is not correct.

Euthanasia is still a criminal offence in The Netherlands. Under article 293 of the penal code any person who terminates the life of another person at the latter's request is liable to up to 12 years' imprisonment.

For the past 10 to 20 years the public debate has focused on the question of whether the criminal law on the termination of life on request should be amended so that a doctor who performs euthanasia on a patient who is undergoing intolerable (not necessarily merely physical) suffering is no longer liable to punishment. In July 1985 the State