The recent work on the pathology and genetics of prion disease in humans has several implications for the epidemiological proposals at the end of Professor Matthews's editorial. Firstly, because of the clinical and pathological variability the true incidence of transmissible dementias (prion disease) in humans can be established with certainty only by a combination of genetic screening and immunocytochemical detection of the abnormal isoform of prion protein. Secondly, on the basis of information already available the incidence of the range of prion diseases in humans is (and probably always was) significantly higher than the figures quoted in the editorial. Thirdly, because of this improved rate of detection any attempts at ascertaining the possible impact of foodstuffs contaminated with bovine spongiform encephalitis on the incidence of transmissible dementias must take this apparent rise in the number of cases into account. Finally, the number of familial cases being identified raises the possibility that in humans the prion disease range has more of a genetic basis than hitherto realised.

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SIR,—Drs Christopher Fear and Manikkarasa Devakumar[1] take issue with Professor W B Matthews over his statement that all patients with Creutzfeldt-Jakob disease must be seen by a neurologist who is familiar with the disease[2]: instead they would substitute electroencephalography as a baseline investigation. Those following this recommendation are more likely to miss the diagnosis than a physician or psychiatrist prepared to review the diagnosis at each stage in the light of clinical evidence.

In practice the electroencephalogram is rarely helpful early in the disease, and the best a neurophysiologist reading it can do is to suggest that the test be repeated at intervals. Brown *et al* observed that the characteristic periodic electroencephalogram complexes were found comparatively late in the illness.[3] Although a pathognomic electroencephalogram is eventually seen in about 75% to 80% of patients, this may not be the case in certain groups of patients with Creutzfeldt-Jakob disease —for example, those receiving human growth hormone therapy.[4]

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Non-invasive mechanical ventilation for acute respiratory failure

SIR,—In their paper Dr M W Elliott and colleagues describe the use of nasal intermittent positive pressure ventilation in patients with acute hypercapnic respiratory failure.[1] Of their group, three patients had chronic obstructive pulmonary disease. We have recently managed four patients with this condition with associated acute hypercapnic respiratory failure. Two patients were treated using nasal intermittent positive pressure ventilation, and in the other two patients we used nasal continuous positive airways pressure. The table gives the patients' details. All four patients had known severe chronic obstructive pulmonary disease and were admitted with severe hypercapnic respiratory failure; two of them had associated respiratory acidosis. None of the patients were considered suitable for endotracheal intubation and assisted ventilation because of end stage chronic disease.

All four patients had supplemental oxygen at a flow of between 0·5 l/min and 2·0 l/min entrained through a port in the nasal mask. Arterial oxygen tensions increased in all patients from a range of 3·9-4·7 kPa to 6·3-8·0 kPa with nasal intermittent positive pressure ventilation or continuous positive airways pressure. In three patients there was a fall in arterial carbon dioxide tension; in the other patient, who was treated by nasal intermittent positive pressure ventilation, the arterial carbon dioxide tension was maintained (table). Nasal continuous positive airways pressure was well tolerated, but both patients in whom ventilation was assisted by nasal intermittent positive pressure ventilation required chin straps to prevent excessive air loss during the inspiratory phase.

Two patients survived to be discharged from hospital: one was alive and well several months after discharge, and one died of a bronchogenic carcinoma four months after discharge. Of the two who died in hospital, one died of progressive renal failure despite correction of arterial gas imbalance and the other died of progressive respiratory failure three weeks after admission despite an initial response.

In agreement with Dr Elliott and colleagues we have found nasal intermittent positive pressure ventilation to be of benefit in two patients with severe hypercapnic respiratory failure, but in addition two other patients benefited from the use of nasal continuous positive airways pressure, which has been used previously to manage patients with hypoxia but without hypercapnia[2] and in the treatment of obstructive sleep apnoea.[3] In the two patients described we achieved improvement in arterial oxygen tension with an associated fall in arterial carbon dioxide tension with this technique.

In summary, we believe that both nasal intermittent positive pressure ventilation and continuous positive airways pressure may be useful adjunct treatments in patients with hypercapnic respiratory failure, particularly those who are not suitable for assisted ventilation through an endotracheal tube. In our hands this treatment still requires close supervision by medical, physiotherapy, and nursing staff, and at present we prefer to treat these patients in high dependency settings.

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Details of four patients with chronic obstructive pulmonary disease admitted with severe hypercapnic respiratory failure

Case No	Age and sex	FEV_1 (l)	Method of ventilation	At presentation (with air)		Assisted		During convalescence		Outcome
				P_aO_2 (kPa)	P_aCO_2 (kPa)	P_aO_2 (kPa)	P_aCO_2 (kPa)	P_aO_2 (kPa)	P_aCO_2 (kPa)	
1	68 F	1·1	NIPPV	3·9	8·4	8·0	8·6	6·8	5·0	Alive at four months
2*	73 F	0·6	NIPPV	4·7	8·4	6·3	7·8	6·6	6·3	Died after four months
3	70 M	0·6	NCPAP	4·5	7·3	6·4	5·3			Died after three weeks
4	72 F	0·8	NCPAP	4·1	9·0	6·9	5·7			Died after five days

FEV_1 = Forced expiratory volume in one second.
NIPPV = Nasal intermittent positive pressure ventilation.
NCPAP = Nasal continuous positive airways pressure.
*The patient also had kyphasis.

Vasectomy and testicular cancer

SIR,—I agree with Dr A R J Cale and colleagues that the possibility of vasectomy affecting testicular malignancy is important in view of the current trend towards male sterlisation.[1] I disagree, however, that a large prospective study is the best approach at this juncture.

The finding of a significantly higher standardised incidence ratio of testicular tumour for men who had a vasectomy and the short average interval (1·9 years) between vasectomy and malignancy have led Dr Cale and colleagues to suspect "an association" and that "vasectomy accelerates the tumour."[1] Studies on the relation between vasectomy and testicular cancer have been few and the findings tenuous. Vasectomy was not considered as a potential risk factor in a review of the epidemiology of testicular cancer published in 1989,[2] and Moss *et al* did not find an association between vasectomy and testicular cancer among American men aged 18-40.[3] A case-control analysis performed specifically to test this hypothesis did find an association, but it was restricted entirely to Catholic men.[4] The investigators suspected that the finding may have been spurious owing to selective underreporting of vasectomy among the Catholic controls.[4] The finding by Dr Cale and colleagues of an excess of observed number of cases in only the 30-35 age group presents a problem similarly deserving an explanation. An association in time between two variables can be due to many reasons. More convincing evidence is needed before an expensive, logistically difficult, and time consuming prospective approach is to be undertaken.

The incidence of testicular cancer is very low (5·6 per 100 000 white men aged 20-69 in the United States).[4] As Strader *et al* pointed out, even if data of four previous large multicentre studies involving 114 000 men (with an average follow up period of five years) were pooled for analysis the study power would only be 0·2 (for the detection of