Wheezy bronchitis was originally used to describe preschool children who wheezed only after viral respiratory tract infections and who seemed relatively resistant to sympathomimetic and steroid treatment. It was thought that only a minority of these children went on to develop 'asthma'. With the increasing awareness of the underdiagnosis of asthma in childhood the term was re-examined.² Wheezy bronchitis was attacked as an over used euphemism for childhood wheeze. Far from avoiding the diagnosis 'asthma', implying a chronic illness, the use of 'wheezy bronchitis' led to undertreatment and inappropriate use of antibiotics in many true asthmatics. It was suggested that all who wheezed should be lumped together as asthmatic unless proved otherwise. In keeping with this concept most doctors and many parents now perceive asthma as a common condition with a wide range of severity and symptoms throughout childhood.

Last year Dr Nicola Wilson re-examined the idea of splitting up wheezy disorders in childhood.³ Like Professor Margaret Turner-Warwick, who described different patterns of airflow obstruction in chronic adult asthmatics,⁴ she has argued that difficulties in treating asthmatic preschool children related to a failure in identifying clinical subgroups with different patterns of illness. Lacking a better term, she described a splinter group of 'wheezy bronchitics' as children who wheezed only in response to viral infections with little or no atopy compared with asthmatics of the same age. If the term is to be used at all it should refer to these kind of children. This is clearly not the group of patients studied in Finland. Forty three percent of these were highly atopic and atopic features were common in the rest. We are given no information about a family history of atopy. Although viral infections were identified in 45% of respiratory episodes, wheezing occurred in 73% of respiratory episodes in which no virus was found. These children might have had typical asthma with wheeze secondary to precipitants unrelated to viral infections. Thirty nine percent of children were receiving regular prophylaxis. This might have prevented persistent asthma symptoms that would otherwise have been present.

If childhood asthma is to be split up careful attention must be paid to clearly defining separate subgroups. This is particularly important when considering pathogenesis and treatment of airway narrowing in our young wheezers.

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Drs Mertsola, Ruuskanen, Vanto, et al comment: We appreciate the comments of our colleagues and agree that many of our patients can be regarded as asthmatics. However, according

to our present practice, the inclusion criteria used in the study (two or more wheezy bronchitis attacks) do not permit an asthma diagnosis in all these children. At present many authors regard wheezy bronchitis and asthma as the same disorder and children with wheezing attacks are treated with the principles of asthma therapy (as was the case in our study). The use of a term 'wheezy bronchitis' should not lead to undertreatment of patients.

About one third of the children in our study had regular prophylaxis and we agree that this might have prevented wheezing attacks in some children, as discussed in the paper. Recently we re-evaluated the importance of allergen exposure as a cause of wheezing in these children.¹ Although many of the patients were atopic, allergen exposure was not often suspected or proved as a precipitating factor for wheezing. This was probably due to the effective allergen avoidance routinely advised to parents of young children in child health centres in Finland. The early preventive measures are helpful for the patients but make the analysis of these kind of studies even more complex.

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Spontaneous resolution of congenital nephrotic syndrome in a neonate

SIR,—We were interested to read the article by Banton *et al* referring to a baby with congenital nephrotic syndrome whose condition resolved.¹ We too have observed a child with congenital nephrotic syndrome who made a spontaneous recovery, and whose case raises important ethical considerations.

The mother had a raised serum α foeto-protein concentration on routine screening in pregnancy and amniocentesis, performed at 21 weeks' gestation, showed an α foetoprotein concentration of 325 mg/l (normal <20) with normal cholinesterase. An ultrasound scan showed no gross fetal abnormalities. A diagnosis of congenital nephrotic syndrome was made and the mother was offered termination of her pregnancy but declined on religious grounds. A boy was born at term weighing 3750 g, the placenta weighing 1000 g. He had gross proteinuria (Albustix 4+) at birth but no haematuria. He also became hypocalcaemic, with a serum albumin of 18 g/l at 2 weeks. He had mild pitting oedema in the first week but none subsequently and at no time did he become hypertensive. The selectivity index (IgG urine : serum as a percentage of transferrin urine : serum ratio) was initially <7% (highly selective proteinuria) but rose to 17% at 7 weeks. The level then fell back to 9% at 10 weeks of age and the proteinuria had resolved by 10 months. A renal biopsy specimen at 2 months of age showed normal glomeruli by light microscopy with the exception of one obsolescent glomerulus. The tubules, interstitium, and blood vessels appeared normal. Immunofluorescent stains for complement components and immunoglobulins were all negative, and these findings were compatible with 'minimal change' nephrotic syndrome. He was treated with a high protein diet (6 g/kg/day), replacement immunoglobulin, and penicillin prophylaxis up to 5 months of age; neither steroids nor diuretics were used. The child is now 6 years old, has not relapsed, and is entirely well.

The congenital nephrotic syndrome is heterogeneous and various types can be recognised histologically. The Finnish type, and the forms exhibiting diffuse mesangial sclerosis, or focal segmental glomerulosclerosis all have a poor prognosis. A policy of prenatal diagnosis is therefore justified in families with a previously affected child whose clinical course or histological type is known to be adverse. However from time to time screening for neural tube defects by maternal α foetoprotein in serum and amniotic fluid will lead unexpectedly to the diagnosis of congenital nephrotic syndrome^{2,3} and the type of congenital nephrotic syndrome and the actual prognosis will not be known. In view of the benign course of our patient, and the one described by Banton *et al*, the counselling of such families can be extremely difficult.

A review of patients with congenital nephrotic syndrome at this hospital over a period of 24 years (S Yoshiara, RHR White, F Raafat, unpublished data) showed that, of 26 cases proved at biopsy, eight were of the Finnish type, three had the form exhibiting diffuse mesangial sclerosis, 12 had focal segmental glomerulosclerosis, and three the histological appearances compatible with minimal change nephrotic syndrome. Of the latter, two siblings were steroid resistant and continue to have mild proteinuria at 19 years of age, with normal renal function, although one is hypertensive. The other child responded to prednisolone 60 mg/m²/day and remained in complete remission until 5 years of age, when proteinuria recurred; a further biopsy specimen taken at this time showed type 1 mesangiocapillary glomerulonephritis. One of the 12 infants with focal segmental glomerulosclerosis was partially steroid responsive and is currently in remission after treatment with cyclophosphamide.

The distinction between the histological subtypes can sometimes present difficulties. Although the glomeruli in Finnish nephrotic syndrome show mild mesangial hypercellularity, this can also be a feature of minimal change nephrotic syndrome. Moreover the cystic tubular dilatation which characterises Finnish nephrotic syndrome is of variable severity and also occurs in focal segmental glomerulosclerosis, while segmental sclerosis in the latter condition may initially be so minimal as to escape detection in a renal biopsy specimen obtained during the first three months of life. Experience in differential diagnosis can only be gained if patients are referred to regional paediatric nephrology centres for evaluation.

If the differential diagnosis remains in doubt, it is reasonable to offer a trial of corticosteroid treatment, with antibiotic prophylaxis because of the increased risk of infection, rather than to dismiss the condition as untreatable. The counselling of a woman who had not previously had a child with congenital nephrotic syndrome requires an awareness that there is, perhaps, as much as a 10% prospect of steroid responsiveness if she produces an affected, liveborn infant.

The authors are grateful to Dr James Partridge for permission to report this patient.

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Retinal haemorrhages in falciparum malaria

SIR,—Kaur and Taylor, in their otherwise extensive review of retinal haemorrhages in children, omitted arguably the most common cause in the world—falciparum malaria.¹

Retinal haemorrhages are present on admission in 6-8% of children with cerebral malaria^{2,3} with haemorrhages developing in a further 4% of children after treatment (personal observation). Their incidence in uncomplicated malaria has not been established. In adults haemorrhages are associated with more severe disease, as manifest by higher parasitaemia, schizontaemia, anaemia, and increased mortality.⁴

The haemorrhages are often multiple and bilateral. Most are intraretinal, either flame shaped or dot and blot, often developing white centres (Roth's spots) with time. Some are subretinal.⁴

The pathogenesis of the haemorrhages is undetermined. Although they are associated with the development of anaemia in adults with cerebral malaria they are present early in the disease when haemoglobin concentration is relatively high. Intracranial hypertension has been documented in children with cerebral malaria,² but there was no correlation between opening lumbar puncture pressure and haemorrhages in this study or a larger adult series.⁴ Raised central venous pressure secondary to convulsions may be a cause, but haemorrhages can develop in the absence of seizures. The pathognomic histopathological feature of cerebral malaria is the sequestration of parasitised erythrocytes in small vascular beds including the retinal vessels. We have documented the appearance of haemorrhages at around the time of sequestration on several occasions. Recently it has been suggested that the phenomenon of 'rosetting' of uninfected cells around the parasitised cells plays a part in the pathogenesis of cerebral malaria,⁵ and it is possible that the lodging of these agglutinates in the retinal vessels leads to haemorrhages. Haemorrhages were not associated with thrombocytopenia or disseminated coagulation in adults.⁴

Whatever the mechanism, falciparum malaria probably causes, at a conservative estimate, a quarter of a million new cases of retinal haemorrhage a year in children, and as such is worthy of a mention.

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Establishment of working definitions in nocturnal enuresis

SIR,—The recent paper by Dr Butler highlights the need for consistent use of nomenclature when reporting studies of nocturnal enuresis.¹ However there are several points worthy of comment. Why include only those wetting more than 50% of nights during the baseline when this will exclude many enuretics equally in need of help who he admits are 'more difficult to treat'. Furthermore, by selecting only the wettest patients from your baseline period you risk the phenomenon of 'regression towards the mean' biasing your results towards subsequent improvement irrespective of treatment.

The working definitions proposed by Dr Butler would provide only limited information on the effects of drug treatments for enuresis. The application of these definitions would satisfactorily highlight the generally low rates of 'initial arrest' of enuresis and the high subsequent relapse rate,² but they would fail to describe the lesser degrees of improvement that many children achieve while taking desmopressin or tricyclics. Although the gold standard for treatment is a 'cure', many fail to achieve this with conditioning therapy and for these children the temporary improvement that drugs may effect, such as an increase in the number of dry nights or a reduction in the size of the 'wet patch', are perceived as worth while.

It is important that these benefits are not overlooked when considering drug treatments for nocturnal enuresis.

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- 1 Butler RJ. Establishment of working definitions in nocturnal enuresis. *Arch Dis Child* 1991;66: 267-71.
- 2 Meadow SR, Evans JHC. Desmopressin for enuresis. *BMJ* 1989;298:1596-7.

Dr Butler comments:

Dr Evans's comments regarding my article on working definitions in nocturnal enuresis¹

raise some interesting issues, which are predominantly concerned with the model's applicability to groups of children (for example, those who have infrequent bed-wetting accidents) and methods of treatment (for example, medication) not originally the prime focus of the initial survey.

In an attempt to establish some consistent definitions of nocturnal enuresis, which Dr Evans agrees are badly needed, I embarked on a survey of published reports of conditioning methods, given that these can be regarded as 'easily the most successful specific treatment available'.² The method of arriving at the proposed definitions is contained within the article and they were discussed at length at the National Enuresis Research Steering Group, the body which originally invited me to pursue this work. The suggested baseline inclusion criteria of '50% or more wet nights in a two week period' was offered as a means of encouraging homogeneity of samples in future studies of treatment effectiveness. My intention was clearly not to detract from studies of children who wet infrequently, who as Dr Evans suggests are a very interesting and deserving population. Rather I am concerned that where such a population are studied, for reasons of clarity, the authors might be encouraged to state the degree of severity of wetting. Indeed, I believe the article stated as much.

I would argue that the proposed definitions of initial success, drop out, relapse, etc, although derived from studies on conditioning methods, are equally applicable to assessing the effectiveness of other interventions be they medication, diet, hypnosis or whatever, as they outline, reasonably unambiguously, the criteria to be met.

Dr Evans stresses the absence of measures of improvement for those children who failed to meet the initial success criteria. I would agree that monitoring of progress is essential, whatever the treatment interventions, and indeed measures of improvement such as size of wet patch, time of accident, and so on have been outlined in detail elsewhere.³ Given such measures, however, the objective either in clinical terms or research methodology must remain the achievement of an initial success criteria.

Dr Evans's final point regarding the probability of regression to the mean is clearly one variable, among many others, that would be controlled for within an appropriate experimental design. However as the regression to mean phenomena tends to describe changes on improvement measures rather than explain complete remission, it further emphasises the importance of establishing criteria for initial success as advocated in the article.

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