detected for each individual. Very often, the noxious substances are cereals and/or milk and its derivatives.

46 adults with rheumatoid arthritis were submitted to our regimen for over one year (17 patients), over two years (17), or over three years (12). Every three months we evaluate painful joints, swollen joints, morning stiffness, nightly awakenings, Ritchie and Lee indices, ESR, and white blood cell count.

The nutrition change was a failure in 10 patients and induced a significant benefit for all tested indices in 36 patients (78%). Among the 36 responders, there were 17 clearly improved and 19 in complete remission for one to five years. 8 of these 19 patients stopped all medications and no relapse was noticed. The favourable action of diet appeared before the end of the third month in 32 responders. Improvement was progressive but often rapid and clinical signs were corrected before ESR. 7 responders who abandoned the dietetic prescription relapsed but improved again when the diet was restored.

We note that Kjeldsen-Kragh et al also suppressed gluten and dairy products for 3–5 months, which may explain their good results.

The development of rheumatoid arthritis could be due to a food or bacterial peptide that crosses the gut wall and proceeds towards joints.[1] The aberrant expression of HLA-DR molecules on synovial cells and chondrocytes would allow the presentation of this peptide to T cells that respond by an immune response against joints. The diet could act by elimination of a food peptide or by a change in gut flora eliminating a bacteria or by recovery of lesions of small-bowel mucosa.

Immunology Laboratory,
Hôpital Saint-Eloi,
34059 Montpellier, France

JEAN SEIGNALET

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SIR,—In the study of fasting and one-year vegetarian diet in rheumatoid arthritis, the test group stayed at a health farm while the controls were consigned to a convalescent home. The test group had intensive personal contacts with dietitians and others while the controls did not. These major differences in management must weaken the conclusion to be derived from this study since changes in central nervous system function, with secondary effects on inflammatory processes, could account for the changes seen. There is a need for studies on the effects of societal interactions on the function of the immune/neuroendocrine systems.

Rheumatology Unit,
Division of Medicine,
UMDS,
Guy's Hospital,
London SE1 9RT, UK

G. S. PANAYI

Laryngeal mask airway in acute cerebrovascular disease

SIR,—In your Oct 26 editorial, you review the pros and cons of the laryngeal mask airway (LMA). Although Davies et al[1] pointed out that the LMA might have a place in emergency airway management, it has not been used widely in emergency resuscitation. We feel that its use is appropriate in the management of airway problems in patients with acute cerebrovascular diseases. Pressor response to tracheal intubation may be harmful in such disease,[2] but LMA insertion is associated with a weaker pressor response than are laryngoscopy or intubation.[3] Additionally, LMA is better than the usual airway or a face mask because it allows safe mechanical ventilation if it is needed.[4]

We have used LMA in six patients (aged 59–71 years) within one hour of onset of acute cerebrovascular disease. To avoid extensive aspiration, patients who had eaten within two hours before onset were excluded. Three patients had subarachnoid haemorrhage, two had brainstem infarction, and one had hypertensive putaminal haemorrhage. All showed consciousness disturbance on admission (Glasgow coma scale [GCS] 4–12, mean [SE] 8·5 [2·8]), with signs of airway obstruction due to insufficient muscle tone. The LMA was inserted without difficulty in all patients. Cardiovascular monitoring including direct arterial blood pressure measurement and electrocardiograms were stable during LMA insertion. However, a patient with mild consciousness disturbance (GCS 12) rejected prolonged placement of LMA, possibly because of laryngeal discomfort. We believe that the LMA could be a useful device in patients with moderate to severe consciousness disturbance caused by acute cerebrovascular disease and in whom a sudden pressor response should be avoided.

NOBUSHIKO ITO
NAOKI AIKAWA
SHINGO HORI
YOTARO SHINOZAWA
KATUYUKI OBARA
RYOICHI SAITO
HIROKAZU TASHIRO

Department of Emergency Medicine,
School of Medicine,
Keio University,
Tokyo, 160 Japan

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Drugs for childhood fever

SIR,—Your Oct 26 editorial recommending that antipyretic agents be used in febrile children implies that ibuprofen is safer than paracetamol (acetaminophen). The only statement on the safety of ibuprofen is the comment that over 240 million doses have been given without ill-effect, with reference to a study in which 93 children received ibuprofen, or whom 18 were withdrawn (13 for ineffective antipyresis, 5 for possible adverse effects).[1] The statement quoted is not in the paper itself but in a reply from the authors to a comment in the journal and no references are given.

Death, renal failure, and metabolic acidosis have all been reported in children after acute ibuprofen overdose.[2-4] Ibuprofen, like other non-steroidal anti-inflammatory agents, decreases renal blood flow and may cause gastrointestinal bleeding, ulceration, and perforation.[5] Because its mode of action is similar to that of aspirin, the possibility that it too may result in Reye's syndrome has been postulated.[6]

Paracetamol is exceptionally safe when given in the correct dose. Prospective drug surveillance studies have found no adverse effects with paracetamol in inpatients.[7] After acute poisoning, hepatotoxicity is the main concern. This complication, however, is very uncommon in children and is thought to be due to the increased capacity for sulphation and increased levels of glutathione.[8] We recommend a total daily dose of paracetamol of 60 mg/kg, which can be given in 4–6 divided doses (ie, the drug can be given more than four times daily). In the cases of chronic paracetamol poisoning described in the review by Penna and Buchanan, the doses prescribed exceeded 300 mg/kg daily.[9] We feel that rather than risking the widespread use of a new drug with major and minor side-effects we should be informing doctors and parents how to use paracetamol appropriately. More consideration needs to be given to manufacturers' dose recommendations spanning wide age groups, which could lead to 100 mg/kg daily being given to some children. We are also concerned by the call from community pharmacists[10] for the easier availability of paracetamol suppositories

since these are an inflexible dose formulation likely to result in standard doses given across a wide age and weight range of children.

The Royal Liverpool Children's Hospital is one of the largest children's hospitals in western Europe and we have not found a clinical need for an alternative to paracetamol for the management of fever in children.

Institute of Child Health,
Royal Liverpool Children's Hospital,
Liverpool L12 2AP, UK — I. CHOONARA

Pharmacy Department,
Royal Liverpool Children's Hospital — A. J. NUNN

Mersey Region Health Authority — C. BARKER

1. Marriott SC, Stephenson TJ, Hull D, et al. A dose ranging study of ibuprofen suspension as an antipyretic. *Arch Dis Child* 1991; **66:** 1037–42.
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SIR,—Your Oct 26 editorial illustrates the ability of experts to read into scientific articles what they wish to believe. Having cited one paper on "sudden febrile infants deaths"[1] to jangle the alarm bells for the outcome of febrile convulsions, your editorial cites guidelines from the Royal College of Physicians and British Paediatric Association[2] on the ineffectiveness of anticonvulsant therapy in reducing the incidence of febrile convulsions. The next sentences imply that the same article advises antipyretics to control febrile convulsions yet in the paper I read "The working group knew of no evidence that antipyretic treatment influences the recurrence of febrile seizures". The next paragraph suggests that antipyretics make a child feel better when febrile but in a previous paragraph on the lack of evidence that fever carries a biological advantage you cite a paper that also points out that there was no evidence that the antipyretics made the child feel better.[3] Further, to write "In the absence of evidence that antipyresis is harmful" sits uneasily with the citation of a paper on paracetamol poisoning and hepatotoxicity in children.[4] When the delivery of pharmaceutical information is put on one side, what is left in the editorial? A traditionalist view that wringing one's hands is no use and that some drug or the other must be given, even if it is more for the parent's sake than the child's. That may be pragmatic but the arguments are not scientifically sound.

Department of Paediatrics,
Faculty of Medicine and Health Sciences,
United Arab Emirates University,
PO Box 17666,
Al-Ain, United Arab Emirates — PETER DEBUSE

1. Parks YP, Sunderland T Sudden febrile infant death *Lancet* 1986; i: 774–75
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4. Penna A, Buchanan N Paracetamol poisoning in children and hepatotoxicity *Br J Clin Pharmacol* 1991; **32:** 143–49

SIR,—Your Oct 26 editorial states that "antipyresis does not seem to prolong the illness or adversely affect outcome". Two articles cited do indeed show that treatment with "antipyretic" drugs had no affect on duration of illness.[1,2] However, in neither are temperature data presented. Were the doses high enough to lower body temperature? In the cited study by Doran et al,[3] paracetamol was given to children with chickenpox, but the dose of this drug was stated as not being sufficiently high to produce antipyresis. To my knowledge, there have been no prospective studies with children to determine whether fever shortens or prolongs illness.

You state that "Fever is a symptom not a diagnosis". It is important to add that fever is a response to pathogens, not just a symptom,[4] and there is a large body of evidence supporting the hypothesis that this response is adaptive.[5,6] You also state that "Patients and professionals are also alarmed by the threat of febrile convulsions". I know of no data indicating that convulsions associated with modest fevers are in any way related to fever. One study showed that the recurrence rate of febrile convulsions in fact decreased nine-fold in those children who had the highest temperatures during their "febrile" convulsions.[7] Whether this means that high fever (>40°C) is protective during convulsions is unclear.

A child with a temperature of over 39°C is likely to be "uncomfortable, irritable, and anorectic" but not because of the presence of fever. The magnitude of a fever is probably just an indicator of illness severity (and thus a crude indicator of the host's response to the pathogen). Drugs that reduce fever are probably making the child less uncomfortable and less irritable as a result of the analgesic (and perhaps anti-inflammatory) effects, not their antipyretic properties.

Department of Physiology,
University of Michigan Medical School,
Ann Arbor, Michigan 48109, USA — MATTHEW J. KLUGER

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SIR,—Paracetamol is thought to be a drug with many benefits and few risks,[1] but in Italy and some other European countries general practitioners and paediatricians still need to be convinced to use it rather than a non-steroidal anti-inflammatory drug, whose side-effects have lately been analysed.[2,3] Ibuprofen probably is a very good drug but a prospective evaluation of side-effects in children needs to be done before it can be advised as a routine antipyretic. Its anti-inflammatory effect is not a reason for preferring it to paracetamol. Is the purpose to lower the body temperature and relieve the child's discomfort or to reduce an inflammation that we often cannot detect? We should be very conservative when advising on drugs that a million children will take every day; the main task currently is probably to teach doctors and mothers to use paracetamol correctly.

Department of Paediatrics,
Federico II University of Naples,
80131 Naples, Italy — ALFREDO PISACANE
CLAUDIO SIMEONE

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CORRECTION

Mesalazine alveolitis.—In this letter by Dr T. Welte and colleagues (Nov 16, p 1273) the first sentence of the penultimate paragraph should have read "Mesalazine alveolitis is rare, with only one report[8] of three suspected cases".