alcohol on young people. We, like Mary-Ann McKibben, believe that a voluntary code is an inadequate means of regulating a multimillion pound industry with multiple producers. In addition to tighter controls on alcohol producers there needs to be stricter enforcement of existing laws on the sale of alcohol to young people. Increasing taxation on particular products may offer a further way of influencing young people's alcohol buying behaviour. Such a move should not be confined to the alcopops but should include white ciders, fruit wines, and vodka, which our report showed to be the drinks of choice of many schoolchildren.

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Algorithm to predict radiological erosions in early rheumatoid arthritis

Messages from paper are incorrect

EDITOR,—Paul Brennan and colleagues found that rheumatoid factor, disease lasting longer than three months, the involvement of at least two large joints, and male sex were significant predictors of radiological erosions in patients with arthritis in primary care.¹ The first two findings are consistent with, but the latter two conflict with, previous data. Is this important, and why has it arisen?

Entry criteria—The authors used the American College of Rheumatology's criteria for rheumatoid arthritis. These criteria, however, were not devised or intended to be used for early presentation in hospital, let alone in the community.

Study design—Baseline observations were correlated with radiological findings at roughly one year; thus an existing erosion (which in a community setting may be asymptomatic and unrelated to disease²) was equated with a new erosion. Radiological examination was also performed a variable time after the start of drug treatment (which influences the development of erosions). Finally, clinical features were not validated: in early disease repeated observation may be required to confirm a diagnosis.

Male sex—In table 5 of the authors' paper the entry "No" in the column headed "Male sex" means that there was a positive association with female sex. The one association with male sex shown in this table is incorrect as the paper showed increased radiological damage in women. The paper is said to include all recent studies of over 100 patients. However, the largest paper is cross sectional and cannot relate to prediction, several papers are not confined to early disease, and the well recognised genetic association with erosions is not discussed—nor is a longitudinal study that, consistent with all subsequent longitudinal studies, showed associations between rheumatoid factor, genetic factors, and erosions⁴ (and validated the previous original observation⁵).

The new element in the algorithm proposed by the authors is the involvement of two large joints. Misleadingly, the authors do not discuss the pathogenic relation in rheumatoid arthritis whereby small joint disease leads sequentially to large joint involvement. "Isolated" large joint disease has a good prognosis; the paper shows that only two of 10 patients with this developed erosive disease.

The new messages from this paper are not only incorrect but possibly harmful to patients. Doctors may think that women have a good prognosis and delay referral until large joints are involved. The more relevant question of whether disability is truly preventable will be answered by interventional studies currently in progress.

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Authors' reply

EDITOR,—Paul Emery and colleagues' letter shows several misunderstandings of our study. We agree that the American College of Rheumatology's criteria for rheumatoid arthritis were not devised for early disease and have ourselves questioned their use in epidemiological surveys.¹ In the absence of an alternative, however, we used these criteria to enable comparability with previous studies. Indeed, Emery and colleagues used the criteria in their study.²

The authors suggest that misclassification of new erosions and clinical features might have occurred but do not discuss how this would affect our conclusions. If any substantial misclassification did exist it would tend to be non-differential and to disguise an association—that is, the true relation between clinical features and outcome would be stronger than we detected. Given the design of our study and the training of the metrologists, the extent of any misclassification is likely to have been minimal. Drug treatment was treated as a possible confounder in our study and had no effect on our results.

Emery and colleagues criticise our review of previous studies because many of them are flawed. It was the weakness of previous work that stimulated us to conduct this prospective study in a well defined population. We are aware of the authors' contribution regarding the role of the HLA DRB1 genotype in predicting radiological outcome.² Our aim, however, was to identify clinical predictors that are routinely measurable at presentation. The HLA DRB1 genotype is not routinely assessed. The reference to male sex is not relevant as this variable was not an independent predictor of erosions or included in the final algorithm.

In two independent samples large joint involvement predicted erosions more strongly than other patterns of joint involvement. We commented on the relation between large joint involvement and small joint erosions in our discussion. We did not refer to "isolated" large joint involvement but instead referred to large joint involvement in the absence of positivity for rheumatoid factor and delayed presentation.

Finally, Emery and colleagues misinterpret our algorithm by suggesting that treatment should be delayed until large joints are involved. This is incorrect. We identified subgroups of patients without large joint involvement but still at high risk of developing erosions. We did not imply that treatment should be withheld from women without large joint involvement. Instead we discussed how decisions about treatment should be made on the basis of current symptoms (for example, disability) and future prognosis. We welcome discussion of the potential limitations and benefits of this algorithm. Emery and colleagues' criticisms are minor and do not affect its validity.

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Health workers and the baby food industry

Subject deserves much more serious consideration

EDITOR,—I was concerned to read the editorial about the baby food industry by R K Anand.¹ I have listened to Anand at two scientific meetings, one about 10 years ago in Bombay and the other just some weeks ago at the Royal Society of Medicine. Neither his discourse nor his subject have changed noticeably in that time, and his editorial is along the same lines. His utterances and writings, including this editorial, are devoid of scientific content and emanate from a narrow, quasipolitical obsession with the infant food industry; he shares this obsession with a fanatical band of activists, a few of them doctors, many of whom came along to the Royal Society of Medicine to cheer.

Ten years ago, in Bombay, I questioned him on the important matter of breast feeding by malnourished mothers. What I said seems to have found its target, since 10 years later at the Royal Society of Medicine he mentioned me by name as one who had "gone round India" saying that malnourished women could not breast feed. Of course I said no such thing. Neither have I "gone round India," except on some pleasurable holidays. The same criticism of me was repeated, I am told, at a lecture at the Institute of Child Health during Anand's recent trip.

Anand's editorial contains 11 references and two personal communications, one from an activist journalist. As far as I can tell, only one of the 11 references would have been peer reviewed, and this was written by fellow activists. One reference is to one page in a book by Swami Vivekananda entitled *Raja-Yoga* and published in Calcutta by Advaita Ashram; I have not been able to trace it. Another is a newspaper article, and another a political pamphlet.

It is pointless trying to answer this editorial in any detail, in the same way that one would make no progress in talking to a Jehovah's Witness about blood transfusion. My sadness that it was

published is accompanied by disappointment that Anand is thereby encouraged to continue to muddy the waters of what is, after all, a profoundly important and disturbing subject, which deserves much more serious consideration.

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1 Anand RK. Health workers and the baby food industry. *BMJ* 1996;312:1556-7. (22 June.)

Author's reply

EDITOR,—John Dobbing fails to address the questions that I raised in my editorial about the World Health Assembly's resolution aiming to discourage financial and other help for health workers from the infant food industry. Instead he seeks to dismiss the issue by using the term "activist" in a pejorative way to describe those who agree with the principles of the resolution and not with his own views.

My challenge to Dobbing on both occasions was in response to his implication that poor women cannot breast feed adequately to meet the needs of their babies after two to three months. I have never quoted him as saying that malnourished women could not breast feed.

Dobbing's paper in India is recorded in the proceedings of a workshop.¹ In it he says, "Exclusive breastfeeding in communities as indigent as this will often adequately support good growth for two or three months." He claims that at this workshop he questioned me. It was the other way round. I questioned him after his presentation and quoted Gopalan, the former director of the National Institute of Nutrition, and others about the remarkable ability of poor women to breast feed and the dangers of early introduction of complementary food.^{2,3} Industry takes advantage of statements such as the above from Dobbing and from others and promotes the introduction of cereals from an inappropriately early age.

Statements about the inadequacy of breast feeding by poor mothers have been made on the basis of theoretical calculations of infants' energy requirements. They have sometimes led to inappropriate recommendations. Butte *et al* reported that adequate growth can occur in 4 month old infants exclusively breast fed with an intake of 297 kJ/kg/day, which is substantially less than the daily energy intake of 480 kJ/kg/day recommended in 1971 for infants of 3-5 months.⁴ The relevance for breast fed infants of the standards used by the National Center for Health Statistics (NCHS) for evaluating growth have also been questioned by the World Health Organisation's working group on infant growth as these standards were derived from a predominantly bottle fed population. The working group concludes from data on Indian infants that "declining status in weight-for-age is likely to be identified beginning at 3 months using the current NCHS-WHO reference, but not until 5 months using the 12-month breast-fed pooled data set."⁵

Dobbing seeks to counter my editorial by criticising the references. Three of the references are from peer reviewed journals, and the others are entirely appropriate and necessary to the subject. *Raja-Yoga* is available from the Ramakrishna Mission, Mumbai 400 052.

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1 Dobbing J. Maternal nutrition in pregnancy and later achievement of offspring. In: *Manual of proceedings of the pre-congress workshop on prevention of mental handicap.*

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New developments in Nestlé court case in India

EDITOR,—R K Anand's editorial about the World Health Assembly's resolution urging member states to prevent the baby food industry from providing financial or other support for health workers is timely.¹ Though Indian law has encouraged the major professional bodies to stop taking support from the infant food industry, Nestlé is now offering support to general practitioners in the guise of teaching about the dangers of unmodified bovine milk. This action is being challenged by consumer groups, and more and more health workers are becoming wary of getting any support.

It is over a year since the *BMJ* published an article about a court case in India involving Nestlé, concerning charges that it had violated the Infant Milk Substitutes, Feeding Bottles and Infant Foods (Regulation of Production, Supply and Distribution) Act 1992.² Nestlé subsequently claimed that it obeyed the Indian law. However, the criminal case against Nestlé is still before the court. The company has succeeded in further delaying the determination of this matter by filing a petition against the government of India challenging the provisions of the infant milk substitutes act. Consumer groups are infuriated by this and are considering a national boycott of Nestlé's products.

Nestlé also states that "it is noteworthy that none of the complaints against Nestlé are made by the Indian government or regulatory bodies." This ignores the fact that the Association for Consumers Action on Safety and Health, which has brought the case against Nestlé, is specifically mandated under section 21(i)(c) of the legislation to monitor the act and to make a written complaint in court against companies that violate it.

The action is being pursued according to due process, on the one hand by a multinational company determined to minimise constraints to its commercial operations and on the other hand by a small group with limited resources, concerned only that a law set up to safeguard the welfare of children is adhered to.

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Costs that would be incurred in establishing "difficult airway register" could be better spent

EDITOR,—J B Liban proposes that a national "difficult airway register" should be established to identify and store information on those patients in whom airway management has been problematic.¹ Several points should be considered before such a scheme is proceeded with.

Liban states that "about 41% of all deaths attributable to anaesthesia are related to difficulties with endotracheal intubation." These data, from the confidential inquiries into maternal deaths, refer only to obstetric anaesthesia. The overall proportion of anaesthetic deaths related to difficult intubation is not cited in recent reports of national confidential inquiries into non-obstetric perioperative deaths, but the actual numbers are likely to be small. In a 1987 British retrospective study of non-obstetric failed intubations six out of 13 380 patients could not be intubated, and all survived.² Since these studies were undertaken the laryngeal mask airway has been introduced into routine anaesthetic practice and has been used effectively in the management of difficult intubation.³

Accessing a patient register would be time consuming and would depend on patients identifying themselves, either orally or (as Liban proposes) by means of a Medic Alert bracelet. We suggest that sufficient information to identify the patient's problem could almost always be fitted into the bracelet itself—for example, "Only intubatable with fibreoptics, but mask ventilation easy—J Bloggs, consultant anaesthetist, Small-town General." Many anaesthetists already provide written details of critical incidents to patients and their general practitioners post-operatively. Further registration with a national information store would take time and involve additional paperwork, which would almost certainly lead to non-compliance.

We could find no published evidence that the use of a centralised agency in the United States has reduced mortality due to airway problems. There is evidence that use of a strategy or algorithm facilitates successful airway management and leads to fewer adverse outcomes.⁴ The time and cost involved in establishing, running, and accessing a national database might be better spent on increasing awareness of, and rigorous training in, strategies for managing patients with a difficult airway. Summoning senior or more experienced help is as important as knowing the exact details of previous attempts at intubation.

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Intravenous immunoglobulin in the Guillain-Barré syndrome

May cause severe adverse skin reactions

EDITOR,—R A C Hughes's editorial on the use of intravenous immunoglobulin in the Guillain-Barré syndrome concludes that this treatment is beneficial and is easier than plasma exchange to administer.¹ We agree with this but wish to draw attention to potentially severe adverse skin reactions to the treatment. We report here on four patients who each received a five day course of intravenous immunoglobulin at the standard dose of 0.4 g/kg/day. Three were given Sandoglobulin, while the other received intravenous immunoglobulin prepared by the Scottish National Blood Transfusion Service. All developed dermatological complications.