LETTERS

Designer drinks and drunkenness among schoolchildren

Study left several questions unanswered

EDITOR,—Neil McKeganey and colleagues report that 12-15 year olds who had recently consumed "new drinks" (white cider and fruit wines) reported having drunk most on the most recent occasion and were more likely to have been drunk than those who had consumed other drinks.[1] Their study begs several questions.

Firstly, was their sample made up of both sexes, and did they control for this?

Secondly, how recent was "recent" drinking?

Thirdly, how many of the children were occasional or regular drinkers, especially of new drinks?

Fourthly, drinkers of white cider and fruit wines ranked fourth and fifth, respectively, in the mean number of days on which they had drunk in the previous year. Drinkers of fruit wines ranked highest in the mean number of units consumed on the most recent occasion (marginally ahead of drinkers of premium lager), while drinkers of white cider ranked fourth. Yet, rather than compare new drinks with specific product types, the authors make comparisons with the aggregated category "old drinks," which includes products with low mean scores on these measures. Why?

Fifthly, why compare recent drinking with lifetime occurrences of drunkenness (undefined)? How many of the children had tried new drinks recently but had had their drunken episode several months or years previously or even before new drinks came on the market?

Finally, what products were consumed during drunken episodes?

The safest conclusion to be drawn from McKeganey and colleagues' study is that, among a group of 12-15 year olds of unknown sex who were infrequent or regular drinkers, those who consumed new drinks at some point during an unspecified recent period, depending on whom they were compared with, drank more or less on their last occasion (whenever that was) and reported fewer or more drinking days in the previous year, and were more or less likely to have been drunk (whatever that means) on an unknown number of occasions over their lifetime.

Considerable public concern has been expressed about young people drinking "alcopops" (and hence these drugs have been given free publicity). Yet our study of the brand preferences of 15-17 year olds found that curiosity about alcopops rarely translated into their becoming the drink of preference.[2] Perhaps older people drink alcopops for their youthful associations, while young people continue to prefer the more adult old drinks. The fact that most of the children reported no difficulties in obtaining alcohol suggests that easy availability is a major problem.

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1 McKeganey N, Forsyth A, Barnard M, Hay G. Designer drinks and drunkenness amongst a sample of Scottish schoolchildren. *BMJ* 1996;313:401. (17 August.)

2 Crawford A, Allsop DT. *Young people and alcohol in Scotland.* Glasgow: Scottish Council on Alcohol, 1996.

More "alcopops" have come on market since study was done

EDITOR,—Neil McKeganey and colleagues' survey of the drinking patterns of schoolchildren in Dundee highlights concerns that the alcohol industry's development of designer drinks such as white ciders and fruit wines is exacerbating an already worrying level of drinking among under 18 year olds.[1] From 1990 to 1994 the number of regular drinkers among the 11-15 age group rose considerably (from 13% to 17% in England, 15% to 19% in Wales, and 9% to 14% in Scotland).[2] The average weekly consumption of those in this age group who drank also increased appreciably in the same period—for example, from 5.4 units to 6.4 units in England.[2]

The authors' survey was conducted seven months before the launch of the first so called alcoholic soft drink or "alcopop" on to the British market in June 1995. Just over a year later there are about 80 alcopops on the market, ranging from alcoholic lemonades, colas, and strawberryades to alcoholic soda waters and spring waters. These drinks represent a dangerous development as regards young people: they are sweet, fizzy drinks with a high alcoholic content that are marketed in containers depicting cartoon characters and gimmicks such as labels that glow under ultraviolet light, and they are affordable.[3]

The voluntary code of practice launched by the alcohol industry in response to concerns about the appeal of alcopops to young people has so far made no difference to the way in which these drinks are being named, labelled, or marketed.[4] Stronger action is needed to ensure that producers of alcohol take their responsibilities towards young people seriously and that a mandatory code is established to which all producers are required to adhere; this code should be monitored by an independent body and not, as now, by the alcohol industry itself.

Further research is needed to assess the appeal to young people of the labelling and marketing devices being used by some sections of the alcohol industry, as well as to assess the impact that alcopops are having on young people's drinking patterns. As McKeganey and colleagues conclude, the continuing development of the new alcoholic soft drinks market is likely to worsen an already worrying situation.

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2 Goddard E. *Teenage drinking in 1994.* London: HMSO, 1996.

3 McKibben M. *Pop fiction? The truth about alcopops.* London: Alcohol Concern, 1996.

4 Portman Group. *Code of practice on the naming, packaging and merchandising of alcoholic drinks.* London: Portman Group, 1996.

Authors' reply

EDITOR,—A Crawford and D T Allsop raise several questions about our short report. Our sample was of mixed sex (49.9% boys, 50.1% girls); there was no significant difference in reported alcohol consumption between the sexes. We defined recent alcohol consumption as being consumption on the last occasion on which the schoolchildren consumed alcohol. With regard to the breakdown between "regular" and "occasional" drinkers, these terms are inherently ambiguous in this age group. We are criticised for comparing consumption of the new drinks with consumption of old drinks, but it seems perfectly reasonable to us to compare the two categories as a shorthand method; in addition, we included information on the mean number of units of alcohol consumed on the last occasion for both new and old drinks, enabling product comparisons to be made from our data.

We are not suggesting that it is only the new drinks that are being consumed by young people or that it is the new drinks alone that are leading to the high level of drunkenness that we identified. Vodka was also being widely consumed by the schoolchildren in our survey and was associated with high levels of reported drunkenness. In their survey Crawford and Allsop found that the "alcopops" are not the preferred choice of 15-17 year olds. Our report, however, was about white ciders and fruit wines, which are being widely consumed by schoolchildren.

The important question is what is to be done about the plethora of alcoholic drinks that are attractive to young people and are being widely consumed by them. The alcohol industry would have us believe that a voluntary code is sufficient to curb the worst excesses of the targeting of

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When deciding which letters to publish we favour originality, assertions supported by data or by citation, and a clear prose style. Wit, passion, and personal experience also have their place.

Letters should have fewer than 400 words (please give a word count) and no more than five references (including one to the *BMJ* article to which they relate); references should be in the Vancouver style. We welcome pictures.

Letters should be typed and signed by each author, and each author's current appointment and address should be stated. We encourage you to declare any conflict of interest.

Please enclose a stamped addressed envelope if you would like to know whether your letter has been accepted or rejected.

Letters will be edited and may be shortened.

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 disease[2]) 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 erosions[4] (and validated the previous original observation[5]).

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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5 Emery P, Salmon M, Bradley H, Wordsworth P, Tunn E, Bacon PA, *et al.* Genetically determined factors as predictors of radiological change in patients with early symmetrical arthritis. *BMJ* 1992;**305**:1387-9.

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