On the plus side, the growing and identifiable international community of those with a specific interest in child safety, illustrated by the attendances at this journal's editorial board meeting and the joint Kidsafe/ISCAIP meeting, was most heartening. It is remarkable that we have moved so far in the three years since Atlanta. There is no doubt in my mind that the journal and the society are filling a niche. They are pulling us together and ensuring that the subject receives the attention it demands.

So where does Rangiora fit into all this? I was staying in Melbourne with friends from New Zealand’s Plunket Society. When one heard that I was heading for 10 days’ holiday in New Zealand after the conference, she asked me if I was going to be in Christchurch on a particular day. I am a bad liar and am in Plunket’s debt so I confessed that I was. ‘Oh good’, she said, ‘you can come to the meeting of our Safe Rangiora group’. My holiday was going to end a day earlier than planned!

Not deterred by being shaken by what I subsequently learned was only a ‘minor’ earthquake in New Zealand’s mountains, I turned up in Rangiora. The small, informal meeting brought a small, informal meeting brought about discussions between those who were absent. We have to translate the output of Melbourne into lessons for the Rangioras of the world — and equally we have to ensure that the child safety problems facing Rangiora are addressed in — or better still, before — Amsterdam.

I visited Amsterdam to discuss the next world conference with its organisers. Rangiora is in my thoughts as its a long way from this small New Zealand town to Amster-
daam and the Safe Rangiora worker could not make the meeting herself. Thank you to those who organised both meetings — Melbourne’s and Rangiora’s.

MICHAEL HAYES
Associate editor
capt, 4th floor,
Clarks Court,
Farrington Lane,
London EC1R 5AU, UK

LETTERS TO
THE EDITOR

NEISS and child injury data

EDITOR,—NEISS is a valuable system for injury surveillance that has served as a worldwide model for national consumer product safety agencies for several decades. Many important regulations and voluntary changes have a direct impact on its data and numerous tragedies have been averted by its judicious use. The classification error studied in my March paper was quite low. It suggests that NEISS encoding procedures, staff orientation and training, and coder motivation is quite good. However, like any surveillance system, there is room for improvement to ensure the highest reliability, stability, representativeness, and efficiency for the purposes for which it was intended.

In response to Mr McDonald’s comments I would like to address the three issues he raised. First, he is concerned with the poor sensitivity in NEISS for a product with the potential for causing a number of incidents such as baby walkers does not reflect a weakness of NEISS’ overall sample design. In any sample survey, the smaller the incidence rate in the population, the greater the uncertainty. In my judgment, the uncertainty surrounding baby walkers is important as at the level of reporting NEISS would not show a statistically significant (two sided) change had occurred even if the rate increased or decreased by 50% from one year to the next. Mr McDonald is accurate and perceptive in asserting that different statistical tools could be applied over many years to improve the robustness of assessing statistical significance. The application of different tests to the same data as the one on varying claims over the significance of the apparent increase in rates over time have been made, at different times, even by the Consumer Product Safety Commission.

The letter’s second comment, I believe, highlights a key point of my paper, though our conclusions appear to differ. NEISS stratifies by geographic and size strata, but has not stratified by type of hospital (children’s hospitals or trauma centers). For all age product related injuries, the current design provides reliable estimates. However, for children’s injuries the picture is not as clear. The geographic and sex stratification in the NEISS paper is derived not so much from the numbers of hospitals randomly chosen in one state as from the fact that currently, both of the only two children’s hospitals contributing to NEISS are the only two children’s hospitals in the state. Enough children’s hospitals in the sample and in both in one state may be the consequence of a random sampling procedure, conducting a probability sample, in and of itself, does not ensure representativeness. Especially when the subpopulation of interest (children) is clearly not distributed randomly among the individual hospitals in the sample frame. Hulley and others have long pointed to the possibility that an unrepresentative sample may occur in a probability sample from ‘random error’ (chance). Because the sample is randomly chosen, there is a probability (commonly estimated by using the Poisson approximation to binomial density when n = 100 and np < 10) of specific disproportions (either too many or too few) that can occur by chance. In reality, the situation is more complex because children’s hospitals are probably not randomly distributed among the size or geographic strata either.

The number of children’s hospitals in the current sample is not as important as the fact that this number has varied in the past and can vary in the future. It is the issue of stability of estimates over time that is raised by the paper. If the current two children’s hospitals are ‘perfectly acceptable’, does that imply about previous periods when twice as many children’s hospitals were in the sample, the period in which only one was in the sample, or the future when none may be in the system (as could occur if current children’s hospitals drop out and are replaced randomly by non-children’s hospitals)?

Finally, it is largely because children’s hospitals treat a small proportion (<5%) of all children’s emergency department visits that their random inclusion and exclusion over different study periods can have the effects noted in my paper. They treat a small proportion of children’s injuries, but as shown, they account for an atypical contribution to the baby walker related injury weighted estimates. If the sample was completely unbiased, shouldn’t the children’s hospitals contribute less than 5%, of the cases? As pointed out, over almost three times this proportion (14%) of baby walker cases (in 1991) even though they made up less than 2% of the sample frame.

To ensure optimal representativeness and stability, stratification of the NEISS sample by children’s hospitals at least at the aggregate statistical adjustment, as NEISS is considering, is warranted.

HAROLD B WEISS
Research assistant professor,
Center for Injury Prevention Research,
University of Pittsburgh,
230 McKee Place, Suite 400,
Pittsburgh, PA 15213, USA

3. Hulley SB, Cummings SR. Designing clinical research, an epidemiologic approach. Bal-

Road Danger Reduction Forum

EDITOR,—The Road Danger Reduction Forum represents road safety professionals committed to reducing danger on the roads at source, as part of a sustainable transport and public health policy. This commitment includes a willingness to reveal the failures of much of traditional policies that have been pursued in the name of road safety. It involves acceptance of the fact of compensa-
tory or adaptive behaviour by road users, and in particular interest in the so-called to ‘road safety’ initiatives and the consequent implications for all road users. We like to think that we, and the 40 local authorities that have signed our Road Danger Reduction Charter in our two years, are a small but enthusiastic alternative to complacency about danger on the road.

We looked forward to coverage of the debate occurring around the movement for road danger reduction, pursued by us and others such as RoadPeace, the road crash victim’s charity, in Injury Prevention, but, on the evidence of the first issue, we were to be disappointed. The article on bicycle crash helmets indicated that reasoned debate might be difficult: I found that not only was it an example of the kind of approach we are trying to oppose, but that it was of a distinctly poor standard of supposedly evidence-based argument. In particular there was no reference to the two fullest discussions of bicycle helmets at that time by Davis and Hillman. We were then sent a copy of page 233 of the December 1995 issue (‘More on bike helmets’) with the invited response. The point that confused you was that a measure which dissuades people from cycling will have health disbenefits. If the dissuasion also results in people continuing to drive motor cars, it will be associated with the health disbenefits caused by car use as well. This should be quite simple to understand, and I would suggest that it is simply such a basic truth, but unwilling, to follow such a logic.

I’m sorry if this seems impolite, but in my experience views on road safety are fre-
quently determined more by ideology than science — and I think this point needs to be made where it applies. I do hope the possibility for rational presentation of views might exist.

ROBERT DAVIS
Road Danger Reduction Forum,
3AU, Lane,
London NW10 2AX, UK