The Lidköping Accident Prevention Programme — a community approach to preventing childhood injuries in Sweden

Leif Svanström, Robert Ekman, Lothar Schelp, Åke Lindström

Abstract

***Objectives*—In Sweden about 100 children 0–14 years die from accidental injuries every year, roughly 40 girls and 60 boys. To reduce this burden the Safe Community concept was developed in Falköping, Sweden in 1975. Several years later a second programme was initiated in Lidköping. The objectives of this paper are to describe the programme in Lidköping and to relate it to changes in injury occurrence.**

***Setting*—The Lidköping Accident Prevention Programme (LAPP) was compared with four bordering municipalities and to the whole of Skaraborg County.**

***Methods*—The programme included five elements: surveillance, provision of information, training, supervision, and environmental improvements. Process evaluation was based mainly on notes and reports made by the health planners, combined with newspaper clippings and interviews with key people. Outcome evaluation was based on information from the hospital discharge registry.**

***Results*—In Lidköping there was an on average annual decrease in injuries leading to hospital admissions from 1983 to 1991 of 2·4% for boys and 2·1% for girls compared with a smaller decline in one comparison area and an increase in the other.**

***Conclusion*—Because the yearly injury numbers are small there is a great variation from year to year. However, comparisons over the nine year study period with the four border municipalities and the whole of Skaraborg County strengthen the impression that the programme has had a positive effect. The findings support the proposition that the decrease in the incidence of childhood injuries after 1984 could be attributed to the intervention of the LAPP. Nevertheless, several difficulties in drawing firm conclusions from community based studies are acknowledged and discussed.**

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Keywords: community based programme, surveillance, Safe Community.

Karolinska Institute, Department of International Health and Social Medicine, Division of Social Medicine, Sundbyberg, Sweden
L Svanström

Karolinska Institute, Department of International Health and Social Medicine, Division of Social Medicine, Sundbyberg and Skaraborg Institute, Division of Public Health, Skövde, Sweden
R Ekman
Å Lindström

Karolinska Institute, Department of International Health and Social Medicine, Division of Social Medicine, Sundbyberg and National Institute of Public Health, Stockholm, Sweden
L Schelp

Correspondence to:
Professor Leif Svanström, Social Medicine Kronan, S 172 83 Sundbyberg, Sweden.

During the 1950s as many as 400 children were fatally injured annually in Sweden. This number has since fallen to include about 100 children 0–14 years who die from accidental injuries every year — roughly 40 girls and 60 boys.[1] For every child killed there are about 100 children whose injuries are serious enough for them to receive inpatient hospital care. Among the fatalities in the preschool age group home and leisure injuries dominate, while most teenagers are killed in traffic.

In the middle of the 1970s the Safe Community concept was developed in Sweden and was first put into practice in Falköping in 1975.[2] Subsequently, in 1984, a similar programme was initiated in Lidköping, Sweden. As with most other safe community programmes both began by establishing a local injury surveillance system. The purpose of this surveillance was to give information that would both help shape the intervention and assist in its evaluation.

The objectives of this paper are to describe the Lidköping Accident Prevention Programme (LAPP); its possible effect on injury incidence; and to discuss how the processes might serve to reduce injuries over time.

The Lidköping Accident Prevention Programme

In the early seventies, a community health unit was established to plan and coordinate health and safety promotion for Skaraborg County, including the Falköping and Lidköping municipalities. A fall of 34% in the incidence of injuries among preschool children was attributed to the Falköping Accident Prevention Programme[3] — from 48·6/1000 in 1978 to 32·2 in 1981/2. This inspired the local health authority in Lidköping to start a similar safe community programme : LAPP.[4] To raise the initiative's profile and to draw as much as possible on local knowledge and experience, an extensive intersectoral network was created.

The interventions agreed on including five elements: surveillance of injuries, provision of information, training, supervision, and environmental measures. The intervention started in 1984 and dealt with injuries affecting children and the elderly. This paper only addresses the former — injuries involving those under age 14 years.

Methods

STUDY AREAS

Skaraborg County, the home of Lidköping, is situated between Gothenburg and Stockholm in southern Sweden. It is mainly an agricultural and manufacturing county with 40% arable land compared with 8% for Sweden as a whole. In 1991 the population was 278 162. For this study comparisons are made between the intervention area, the municipality of Lidköping (population 35 949), four bordering municipalities (population 42 078), and the whole of Skaraborg County. The 'border' municipalities use the same hospital as Lidköping but received no intervention.

PROCESS EVALUATION

The LAPP evaluation involved studies of both process and outcome. The process evaluation was based mainly on reports made by the health planners, combined with newspaper clippings and interviews with key informants.

OUTCOME EVALUATION

The outcome evaluation was based on data from Skaraborg County Hospital discharge register. Cases are patients discharged from hospitals with an injury diagnosis coded E807-929 according to the *International Classification of Diseases* (ICD-9). These patients are then identified by place of residence regardless of the location of the hospital in which they were treated.

STATISTICAL METHODS

Difference in annual injury rates and their 95% confidence intervals between the comparison areas of Lidköping, the four border municipalities combined, and Skaraborg County were calculated.[5] Linear regression was then used to estimate the annual change in incidence.

Results

THE INTERVENTION PROGRAMME

In 1984 an interdisciplinary group was established to administer LAPP. Representatives from the health care services included a district nurse, a paediatrician from the emergency hospital, a head nurse from the health centre, and the health planning officer. In addition the group included several representatives of the municipal administration — from the social welfare services responsible for preschools, a road engineer, a school nurse, a physical education teacher, and a consumer safety secretary. Later, other representatives were added from the police, the Red Cross, and a housewife.

LAPP started by jointly establishing priorities for intervention and as part of that process a special surveillance of school injuries was initiated. In addition, reporting of transport injuries, began earlier, as well as surveillance of all inpatient and outpatient injuries[6 7] continued.

Intervention activities focused mainly on providing information. Some examples included age related safety checklists, verbal information to parents from child health care staff, and an infant carseats loan programme. A campaign to make the snow ploughing system safer was also initiated, as was a special training course to prevent sports injuries.

During 1986–7 the programme became more intense. The interdisciplinary group met six times during 1986 to develop new information material. As well, a telephone 'hot line' was established to permit the public to call for advice about specific injury risks in different environments and about dangerous products. This information was disseminated to about 14 000 households through preschools, schools, and child health care units.

A campaign on bicycle safety, including helmet promotion, was also launched as a result of findings from the road injury study. The campaign included an exhibition and posters at the town hall. Another display, with a focus on all childhood injuries, was circulated to schools and health centres. Environmental changes were initiated, like improving gym floors to decrease slipping injuries.

During the next period, from 1987 to 1988, almost 250 mothers and staff in day care centres were trained in child safety and first aid, and more than 1000 parents received information on injury prevention. At the same time municipal safety rounds[8] were initiated to increase the safety of the physical environment. These inspections were performed by those concerned with the safety of children and the elderly. Subsequently, during 1988, a steering group was established for the whole programme and the same year LAPP hosted the first National Conference on Injury Prevention.

In 1989–90 the main emphasis was on traffic safety and included the establishment of a student safety guards' organization. From 1991–3 the main elements continued, following much the same structure. The principal components of the intervention are summarized in table 1.

OUTCOMES: INJURY RATES

Data from the Skaraborg County Hospital discharge register show that in the whole of Skaraborg the injury rate remained generally stable from 1983 to 1991 (table 2). There were no statistically significant differences between the mean annual differences of the three study areas and the confidence intervals overlap.

When injury rates before and since the onset of LAPP in 1984 until 1991 were calculated

Table 1 Activities in the LAPP

Surveillance	Routine (hospital discharge register)
	Special (school and transport injuries)
Information	Child car seats
	Bicycle helmets
	Bicycle safety exhibit
	Dangerous toys exhibit
	Red Cross safety caravan
	Media
	Broadcasts in Skaraborg
	Local press coverage
Training	Coaches and athletes
	Day and child care staff
	Parents of preschool and school aged children
Supervision	Age related checklists
	Municipal safety rounds
Environmental	Safe playgrounds
	Gravel pits
	Gymnasiums
	Bicycle lanes
	Transport routes for child care

Table 2 Incidence of hospitalized injuries (rates/1000 under 14 years) in Lidköping (intervention area) and comparison areas by year and gender (n = area population)

	Intervention area		Comparison areas			
			Four border municipalities		Skaraborg	
	Girls	Boys	Girls	Boys	Girls	Boys
1983	10·5	17·6	8·3	12·0	8·7	12·7
n	3247	3356	4118	4318	26202	27464
1984	15·0	13·5	6·9	13·8	8·8	13·2
n	3200	3271	4046	4203	25818	26998
1985	10·2	12·6	8·4	11·3	8·4	13·0
n	3140	3265	4059	4175	25519	26690
1986	8·4	16·9	6·7	13·7	8·7	13·7
n	3092	3252	4018	4146	25296	26470
1987	12·4	12·2	9·5	15·9	9·5	14·2
n	3056	3208	4018	4162	25151	26325
1988	7·3	13·4	4·2	12·5	7·8	12·4
n	3016	3204	4049	4163	25079	26391
1989	13·3	19·2	10·7	14·1	8·8	12·4
n	3006	3232	4113	4163	25153	26602
1990	9·1	10·3	11·0	13·7	9·3	12·8
n	3072	3287	4179	4304	25506	27047
1991	9·5	11·0	7·1	12·2	7·9	11·4
n	3160	3378	4235	4355	25977	27552
Mean	10.6	14.1	8·1	13.2	8·7	12·9
95% CI	8·7 to 12·5	11·5 to 16·7	6·4 to 9·8	12·1 to 14·3	8·3 to 9·1	12·3 to 13·5
β_1	−0·3	−0·4	0·2	0·1	−0·1	−0·2
% Change/year	−2·1	−2·4	2·2	0·6	−0·3	−1·0

Source: Skaraborg County inpatient discharge register 1983–91.
CI = Confidence interval.

using linear regression, however, a different picture emerges. The beta values expressed as per cent changes show an average annual decrease for boys of 2·4% and 2·1% for girls in Lidköping. In comparison, in Skaraborg County there was an average annual decrease of only 1·0% for boys and 0·3% for girls. Furthermore, the four 'border' municipalities had an average annual *increase* of 0·6%/year for boys and 2·2% for girls during the same period.

Discussion

In theory, intervention programmes should be based on systematically researched models and should be carefully monitored and evaluated.[9] Evaluation is intended to be an objective, rational process[10 11] in which the effects of policies or programmes on their targets (individuals, groups, institutions, or communities) are revealed, undistorted by prejudice or preconception. It is assumed that the findings of such evaluations will help decision makers to make wiser choices about future courses of action than they would otherwise. In practice, however, prior beliefs and paradigms of those involved colour everything, from how the intervention is conceived, to the language and scope of the evaluation, and the interpretation of the findings.

Consequently, the designs available for evaluating community intervention programmes are, in general, rather weak. One such design involves before and after test comparisons in one area. This can sometimes be strengthened by using a series of observations before, during, and after the intervention. Another approach is the quasiexperiment. In this geographical areas are compared on the basis of pre-existing, unplanned, known contrasts in exposure to an intervention. Effectively, this was the strategy chosen for this evaluation of the LAPP programme.

For LAPP we used records of all injury related hospital admissions from 1983 to 1991. Although a valuable source of information, the data from this register is mainly intended for administrative and economic use and the diagnoses recorded have not been validated. Thus, the interpretation of trends using these data are complicated by possible changes in admission policies, therapeutic technology, or diagnostic coding practices.

In addition, it must be acknowledged that the intervention did not begin suddenly, nor was it narrowly defined. It build up steadily and changed its character (organisation, scope, and intensity) gradually from 1984 onwards. Moreover, Lidköping Municipality is not absolutely identical to the 'four border municipalities' or to the whole of Skaraborg County in terms of all predisposing factors. But the comparison areas do control somewhat for several possible biases, and because of the limitations above, it seems safe to assume that any differences are conservative estimates. Moreover, the county contains both intervention communities, Falköping and Lidköping, and in addition, other preventive activities occurred over the entire county — a fact that cannot be avoided in a study of this kind. Despite all these limitations, the results suggest a decrease in the incidence of childhood injuries since 1983 in Lidköping.

Evaluations based on processes alone also present challenges. Above all, there is the risk of bias — for example, recording what is hoped for or what seems socially desirable. One way to limit this is to set up a team of evaluators that is independent of the organizations involved in the intervention. The burden of ensuring validity then falls not only on the interviewer and the respondent, but also on the researchers. The latter must devise measures, provide protocols, analyze the data, and submit their reports to external critique.

In spite of the many methodological problems there is, nevertheless, support for the conclusion that the decrease in the incidence of childhood injuries was the result of the LAPP. This conclusion is based on the fact that the four border municipalities, which had no programme, showed an increase during the same period. As stated previously, the fact that the whole of Skaraborg was also part of the intervention area makes this comparison conservative.

The fact that Schelp also showed a decrease of child home injuries in Falköping lends support to the conclusion of a relationship between the LAPP and decrease in injury incidence.[3] The average decreases in Schelps study after two years of intervention was 34% among those under 14 years of age.

In view of the problems with an administrative register, like the Skaraborg County inpatient hospital discharge register, there are many reasons to support the creation of a specific surveillance system to assist future evaluations of this nature. The system established by the end of the 1970s did not function after the beginning of the 1980s, but a new surveillance system was introduced in 1989 and will in the future provide an improved opportunity to analyze the rates in the 1990s.

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Injury prevention research wins award

The 1995 Charles C Shepard Science Award went to injury prevention scientists studying the relationship between arrests for drunk driving and the risk of dying in an alcoholic crash. Their winning study, 'The risk of dying in alcohol-related motor vehicle crashes among habitual drunk drivers', was published in the *New England Journal of Medicine* (25 August 1994). This is the first time an injury prevention topic has been the recipient of this pre-eminent award.

In accepting the award, Dr Brewer, a medical epidemiologist in CDCs Division of Unintentional Injury Prevention, said, 'This study is a clear indication that injury is being recognized as a significant public health problem and is an example of how epidemiology can be used to scientifically define an injury problem and its causes while also proposing specific interventions to prevent injuries and save lives. The study also demonstrates the importance of collaboration between CDC and public and private organizations in conducting injury prevention research'.

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CRITIQUE

Lidköping Accident Prevention Programme: what was the impact?

John D Langley, Jonathan C Alsop

Community interventions have been widely embraced by the injury prevention community after the purported success of the safe community project in Falköping.[1] Much has been promised and public agencies have high expectations, yet there have been few published evaluations. The paper by Svanström *et al* in a recent edition of this journal is thus of considerable interest.[2]

Svanström *et al* described the Lidköping Accident Prevention Programme and its possible effect on injury incidence. A variety of intervention activities of different intensities and duration occurred between 1984 and 1991. Childhood injury rates (per 1000 persons), for girls and boys, were examined for the period 1983–91 for Lidköping and two comparison areas: four border municipalities and Skaraborg County. The authors report 'There were no statistically significant differences between the mean annual differences of the three study areas and the confidence intervals overlap' (p 170). Such an analysis is not informative. For example, one might reasonably expect different mean rates of injury between various communities as not all communities are the same. A fundamental question is: was the intervention in Lidköping associated with a significant change in injury rates over time relative to the comparison groups. The authors sought to answer this using linear regression. They presented 'beta values expressed as per cent changes' (p 171) and concluded that the results suggest that the intervention had an effect.

In their discussion the authors state 'It is assumed that the findings of such evaluations will help decision makers to make wiser choices about future courses of action than they would otherwise' (p 171). Regrettably they failed to translate their findings into the number of injuries prevented.

It is unclear to us how to translate the statistical results they report into the number of injuries prevented. Given the degree to which community intervention programmes have been promoted and the resources they have attracted, in the absence of significant body of research on their effectiveness, we sought to reanalyse the data using different methods, with view of determining what impact the intervention had made on the incidence of injuries.

Injury Prevention Research Unit, Department of Preventive and Social Medicine, University of Otago Medical School, PO Box 913, Dunedin, New Zealand
J Langley
J Alsop

Correspondence to:
Dr Langley.

Table 1 Poisson regression of outcome over time

Area	*Year (β_1)*	*p Value*	*Deviance/df*
Intervention			
F	−0·0036	0·679	1·04
M	−0·0116	0·093	1·18
Four bordering areas			
F	0·0199	0·374	2·67
M	0·0059	0·730	0·68
Skaraborg County			
F	−0·0261	0·243	1·82
M	−0·0316	0·095	2·11

Methods and results

We begun by taking a similar approach to that reported by Svanström *et al*. That is we analysed the data within each sex and within the three areas separately. Rather than using linear regression, however, we chose to perform a Poisson regression (under the assumption that injuries per person follow a Poisson distribution, that is data obtained from a count process). The outcome variable was chosen as the actual numbers injured each year (x_i), evaluated by multiplying the injury rate by population size. The logarithm of the population size was then used as an 'offset' variable, so that the actual outcome after the log link is taken into account is the logarithm of the injury rate. The model is thus:

$$\log(x_i) = \log(N_i) + \beta_0 + \beta_1 Year_i + e_i$$

where x_i denotes the *i*th observation in an area and for a sex category, N_i is the population size, $Year_i$ the original untransformed year of hospitalisation, and e_i the error term. Results for the six area/sex combinations are presented in table 1.

We can see from table 1 that none of the trends in hospitalisation rates are statistically significant at the 5% level. Large values of deviance divided by degrees of freedom (df) given an indication of lack of fit. Two of the six models appeared not to fit particularly well. These high ratios of deviance to df indicate over dispersion, which may be a result of the lack of explanatory variables included in the model. Also, each of these models had only 7 df so it was thought that a better approach would be to combine the information into an overall model.

We therefore undertook a second stage of modelling. Again a Poisson regression model was used, but with additional terms,

$$\log(x_i) = \log(N_i) + \beta_0 + \beta_1 Year_i + \beta_2 Area_i + \beta_3 Sex_i + \beta_4 Area_i \times Year_i + e_i$$

thus we also included sex, area, and an area-year interaction term. This interaction term enabled us to model possible differences in the slopes of rates over time in separate areas. The main focus of the paper was to compare the intervention area with the two control areas. For this reason we decided to compare the intervention area first with the four bordering areas, and second with Skaraborg County. For these comparisons the models had 31 df, an improvement upon the original six models. The results are presented in table 2.

One of the important findings was that there were no significant time trends in any of the comparisons. This compares well with the first analysis (table 1). The differences in average injury rates were significant between the intervention area and the other two comparison areas. Also, the differences between sexes were highly significant in all three comparisons (we would expect that a similar sex effect is to be found in all three areas). One crucial finding, however, was that there was a significant difference ($p = 0{\cdot}041$) in time trends between the intervention and four border areas. We can relate this in terms of a significant difference in slopes (that is non-parallel), even though the slopes themselves were not significantly different from zero. This difference was not significant ($p = 0{\cdot}174$) when the intervention area was compared against Skaraborg County, though this may have been due to dependence of the areas because of geographical overlap. Note that all models fitted adequately, as the deviance divided by the number of degrees of freedom were not greatly different from 1. As an additional check, the two 'control' areas were compared. No significant difference in injury rate was found between these two areas ($p = 0{\cdot}224$), and also no difference in the time trends ($p = 0{\cdot}182$).

Discussion

Svanström *et al* reported a significant effect using linear regression. In our first analyses we failed to show an effect using Poisson regression. We chose to perform Poisson regression over linear regression due to a variety of reasons. The numbers of injuries may not follow a normal distribution, as they can be thought of as a count process within each individual of the population. As they are rare events, they can instead be assumed to follow a Poisson distribution. In addition, the Poisson regression constrains the number of injuries to lie between zero and infinity, while linear regression has no lower bound. Finally, a non-constant variance in the number of injuries may be present also.

Our second analysis also showed no reduction in injury rates over time. However, it showed that the intervention area had a statistically significant reduction in injury rate compared with the four bordering areas. This means that the reduction in the intervention area, when examined in isolation, was not significant, but when offset against the injury rate rise in the four bordering areas, we found to be significant. Somewhat unusually, this effect was not present in the comparison of the intervention area with Skaraborg County.

Table 2 Poisson regression of factors for various area comparisons

Comparison	*Factor (p values)* *Year*	*Area*	*Sex*	*Area-year*	*Deviance/df*
Intervention *v* four bordering areas	0·417	0·001	<0·001	0·041	1·79
Intervention *v* Skaraborg County	0·117	0·002	<0·001	0·174	1·47
Four bordering areas *v* Skaraborg County	0·116	0·224	<0·001	0·182	1·33

From our final model we estimate that in 1991 there were 38 female and 58 male hospitalisations for the four border areas. If the intervention had taken place in this area also, we would have expected only 28 female and 42 male hospitalisations. This means that we could expect to save a total of $10 + 16 = 26$ hospitalised injuries. Note that the implicit assumption here, is that the injury rate in the Lidköping area would have risen similarly to the four border areas without the intervention. This is a very modest return on what must have represented a substantial input of resources. What might have been the return if similar resources had been concentrated on one or two very specific issues (for example helmets for cyclists and impact absorbing surfaces under playground equipment)?

The foregoing discussion assumes that the reduction in injury in the intervention area was due to the intervention and not to some other extraneous factors. Regrettably Svanström *et al* fail to provide any quantitative data on the impact of the intervention on injury risk and protective factors. We are thus presented with a 'black box intervention'. In other words we have no process evaluation. For example, although Svanström *et al* briefly describe a campaign on bicycle safety, including helmet promotion, the reader is not provided with any data on changes in bicycle safety behaviour over time. The failure to present this is a serious deterrent to understanding and progress in the field.

A recent evaluation of the Shire of Bulla Safe Living Program, a community intervention programme in Victoria, Australia, failed to show any statistically significant changes in overall injury rates or hospital bed days.[3] This, coupled with our findings, suggests greater caution should be exercised in promoting these broad, multifaceted intervention programmes. Building up community expectations but failing to achieve significant reductions in injury could well result in disenchanted communities, concluding that there is, after all, nothing that they can do which can make a difference. Nothing could be further from the truth. As investment in community injury interventions grows so too has the importance of rigorous evaluation. A critical component of such evaluations is determining the impact of specific interventions (for example promotion of helmets) on specific behaviours (for example helmet wearing) and relevant injury outcomes (for example head injuries to cyclists) and seeking to determine if a range of specific interventions have had a more wider impact by influencing injury prevention behaviour. Given the resources safe communities have attracted to date a comprehensive critique of the evaluations published to date is warranted.

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Commentary: statistical perspectives on the Lidköping papers

We have examined the critique by Langley and Alsop and the original Lidköping paper and re-entered the data in order to replicate both sets of analyses. As we see it, the authors of the original paper (Svanström *et al*) do indeed lose statistical power by breaking the data down too far and performing two separate analyses (one for boys, one for girls). For each of the two genders, they find the difference in the trends over time between the intervention and neighbouring area to be non-significant ($-0{\cdot}3 - 0{\cdot}2 = -0{\cdot}5$/year for girls, and $-0{\cdot}4 - 0{\cdot}1 = -0{\cdot}5$/year for boys). The p values are thus 0·33 and 0·25. Instead of reporting these p values, they use words like 'strengthen the impression' and 'support the proposition'.

Before commenting on the method suggested by Langley and Alsop, we suggest two ways in which the original authors could have made more efficient use of their data. The first of these is to aggregate the genders and calculate one rate per year for the two genders combined. When we do this (either by a 'rougher' straight average of the two rates, or in a fancier way by adding numerators and adding denominators), we get somewhat stronger evidence of a difference in the two slopes. One way to test for a difference in slopes is to include an year × area term in the model in addition to area and year — just as Langley and Alsop did. The single p value is 0·18, corresponding to a 'difference in slopes' of $-0{\cdot}47$/year. In the critique from New Zealand the authors give a p value of 0·041 for this interaction in the Poisson model. They fail, however, to tell the reader in which direction this 'difference of slopes' goes. We hope that the journal will set a firm tone by asking for magnitude and direction first, confidence interval (CI) second, and p values last (if at all).

The second approach, still staying with rates, is to treat the rates in boys and girls as one data set, but to account for (that is, take out), the variation in the rates between the two genders by including gender as a term in the regression. In PROC GLM in SAS for example, one can model the rates, rate = gender year area year × area.

If one does this with the 36 data points in the first four columns of the table in the paper, and if one codes area as 1 if intervention and 0 otherwise, and gender as 1 if boys and 0 if girls, and if we center the years by subtracting 1987 so that 1983 is -4 and 1991 is $+4$, we get the following fit (SEs in parenthesis):

rate = 8·5
+ 4·3 (0·78) gender
+ 0·12 (0·21) year
+ 1·69 (0·78) area
− 0·48 (0·30) year × area.

Notice that the coefficient for area is $+1{\cdot}69$, indicating that, in general, rates are higher in the intervention area. The key is the $-0{\cdot}48$ (very close to the average of the two gender specific differences in slopes in the authors' analysis. The *t* statistic for this is $-1{\cdot}6$ so the p value (from a *t* with 31 df) is 0·12.

Before dealing in depth with the submission from New Zealand, two preliminary comments. First, we believe that — from a purely data analytic viewpoint — they are wise to treat the 36 observations (intervention *v* neighbouring area) as one data set rather than two (one for each gender). Second, however, we would have liked to see coefficients, not just p values.

Now, to the main issue, which is the use of a Poisson model. There is a paradox here in that the authors find evidence of some extra-Poisson variation when the genders are considered separately, yet mysteriously these problems 'go away' when all 36 data points are considered together. The issue of whether the Poisson distribution is — in principle, and in this data set — the 'correct' model to use is an important one. The Poisson model appears to 'bring out the signal' better than the analysis that treats the variations around the lines as Gaussian. If we were dealing with counts that were unlikely to be influenced by factors such as weather, 'local' short term interventions, clustering because of injuries to multiple persons from the same source/cause (for example, an incident with a school bus), or any other such perturbations, then the Poisson distribution would make sense. But *a priori* we would have expected extra-Poisson variation and suspect that the reason the authors don't 'find' it is that it is buried in the large number of degrees of freedom.

Indeed, when we look more closely, the ratio of deviance/df of 1·79 tells us that the deviance is around 55, which is beyond the $p = 0{\cdot}01$ point of the χ^2 distribution with 31 df. So, in fact, the Poisson model does not fit that well: there is more variation than the Poisson model would predict. Thus, the standard errors obtained for the coefficients for the model in table 2 are too small, and the p values too extreme. Indeed, it appears that the more naive model in the original paper, which considers the variance around the line to have a magnitude independent of (bigger than) the mean, is more appropriate in this case. In general, it makes more sense to use a model that allows the variation to be estimated from the data rather than from an assumption that is not fulfilled by the data. The model used in our analysis above is the same as the one used by the original authors, except that we use all the data in one analysis.

In spite of using all the data at once, and using the model which fits the data (rather than using standard errors calculated from a model that doesn't fit the data), we too are unable to show that the difference of slopes is significant at the 'magic' 0·05 level. Thus, even though we agree with translating betas (and differences of betas) into numbers of injuries, we wonder if these numbers shouldn't be taken with a grain of salt. After all, from our conclusion that the difference in betas isn't statistically significant at the 0·05 level, we can project that the 95% CI for the number of injuries prevented will include zero.

The paper from New Zealand argues that a Gaussian distribution may not be appropriate, whereas a Poisson one would. In fact, the two are not mutually exclusive. If one had a Poisson distribution with a mean of 33 or 50 events per year (or >80 if we combine the two genders), then the distribution is also, for all intents and purposes, Gaussian. The main point of a Poisson distribution is that it is a one parameter distribution in which the variance equals the mean — something that appears not to be the case with the data in this application.

A cautionary note: if we fuss too much about the distinctions between these two forms of error variation, or between linear and log linear regression, or indeed other issues in modelling, we run the risk of concentrating too much on the 'small picture'. A much bigger uncertainty in the inferences we can draw from this study stems from the ecologic nature of the data and the fact that it is an 'unrandomized' study of $n = 2$ units (areas), each of which is subject to many other influences beyond those allowed for, or allowable for, in the analysis. So we should keep these arguments about models and p values in perspective: the results in the county as a whole are another sobering reminder of what else might be going on that our 'model' cannot account for.

In summary, the New Zealand authors appear to squeeze too much 'statistical significance' from the data by using a model that artificially makes the standard errors too small and the p values too extreme. The original authors could have used the data to their full advantage. However, if they *had* tried harder (as we did), by analyzing all of the 36 data points at once, the result would have remained 'NS'. But even if they had found $p = 0·04$ rather than the 0·12 or some other such value, we would still need to be cautious in our interpretation.

JAMES HANLEY
SUNGSUB CHOI
Department of Epidemiology and Biostatistics,
McGill University,
Purvis Hall,
1020 Pine Avenue,
Montreal,
Quebec H3A 1A2
Canada

Believe it or not but this really happened . . .

A CCSN BBS correspondent writes: 'Because I am an in-line instructor who is very concerned with the issue of safety I tend to find myself in these situations. I was skating at an indoor rink when I saw this father readying his young daughter for skating. She was wearing all of the protective gear, a rarity for most indoor establishments. When I noticed them Dad was trying to put a helmet on the little girl's head. Part of the difficulty he was having stemmed from the fact that the helmet was backwards.

You've really got to learn the correct way to wear a helmet, and because it's not apparent to everybody, I always try to be helpful. I skated over to the father and told him that his daughters' helmet was on backwards. He told me that it was 'okay', I thought maybe he didn't hear me as the music sometimes gets quite loud. I tried again. I explained that the back of the helmet is the side with the most styrofoam, to cushion the base of the skull in a backwards fall. Dad thanked me very politely, and explained that it didn't matter, as his daughter was not capable of skating fast enough for it to be an issue. Well how do you argue with logic like that? Apparently he thought she'd be travelling fast enough to require a helmet, but not quite fast enough to require she wear it correctly.

Well Dad was right about one thing — the little girl didn't skate very fast — [and] the other kids were darting about her like 'bats out of Hell'.

Every now and then one of those kids would get a little too close and that little girl would be on the ground. It didn't take long for the helmet to slip off her head and dangle about her neck like the Red Baron's scarf. I watched Dad race out and reaffix the still backwards helmet several times. I tried to explain to him that helmets are designed to fit the head a specific way, and that maybe it would stay on better if he turned it around. He felt her hair was making it slip off and again he would be more concerned about it being on correctly if she could skate faster. (There was that wacky logic again.)

After about the ninth attempt to reaffix the helmet Dad decided to try turning the helmet the other way. After she skated the two laps without incident he announced that maybe it did fit better when it was on correctly' (CCSN BBS).