The Medical Effects of Seat-Belt Legislation in the United Kingdom: the statistical arguments

A. T. GREENFIELD

A careful reading of the report should demonstrate that the pitfalls and mistakes that creep into many multicentre studies have been avoided. Sound findings can only be expected from good methodology.

The most important single decision regarding the analysis of the data was the prior statement of 17 hypotheses which were to be tested statistically. With over 14,000 patients and with 210 items of information for each patient, there was scope for testing an immense number of combinations of variables. It would not be surprising if every twentieth such table showed a significant result at the 5% level. When reading published reports of research, it is always necessary to be alert for spurious claims which are consequences of such analysis. This is called data dredging. Admittedly, the number of findings that can be accepted as substantiated was limited by restricting the report to pre-stated hypotheses. However, in the tables called 'Profit and Loss' tests of all frequently occurring major and minor injuries have also been reported, and some of these show changes which would normally be accepted as significant. In drawing attention to this group of injuries it was felt that a new set of hypotheses which should be subjected to further study is being highlighted. Some of them may prove to be genuine changes caused by wearing seat-belts, but without further study this cannot be claimed.

At many points in the analysis, counts were compared. For example, in the first year 1329 front-seat occupants were admitted to hospital wards, and in the second year 924 were admitted. The question was, had there been a real change, or was the difference the result of random variation within our particular sample of hospitals from one year to the next? If they had been counted in a different 14 hospitals, the numbers would not have been exactly the same. The sample was approximately 5% of the casualties in the country. If figures were available for 20 different groups of hospitals, considerable variation between the groups would be expected. Even with the same group of hospitals, and even if there had been no changes in any conditions, the same number of patients admitted every year would not be expected in the count. However, with a sample count of any particular size, the probability of any other sample counts being within certain limits, provided the conditions have not changed, can be estimated. Then, when the number of admitted casualties in the second year is counted and that number is discovered to be outside the limits, it can be argued that the conditions have probably changed.

Counts of the type which are dealt with here usually vary much in the same way. They are called Poisson counts, or counts with Poisson distributions. When the figures were first tested it was assumed that the counts did have Poisson distributions. Discussions with Mr Peter Scott of the Transport and Road Research Laboratory, drew attention to work by Hutchison & Mayne which showed that the counts of road casualties had a higher variation than Poisson counts. This was accepted, and pages 60 and 61 of the report explain how the calculations were adjusted to take this increased variation into account. Throughout the analysis the variation has been multiplied by a factor which makes any test more stringent and more suitable for studying traffic statistics. In the text it is called the HM adjustment (for Hutchison & Mayne).

The legislation applied only to people in the front seats and so some differences were expected in what was found to happen to the occupants of the front and rear seats. It was also possible that

Correspondence: Mr. A. Greenfield, 6 Pinewood Close, Seaford, East Sussex, BN25 3AH.
the increased wearing of belts might have somewhat different results upon the driver and the front-seat passengers. For any particular aspect which was examined, after seeing if there was a difference between the total numbers in the 2 years, the relative proportions of drivers, front-seat passengers and rear-seat passengers were examined to see if they had changed. In most cases it was clear that the improvement affected only those who were in the front seats.

This in itself made it seem more likely that the result was due to the legislation. It was not enough, though: it could be, for example, that at about the same time as the legislation was introduced, far more cars were fitted with laminated glass in their windscreen. This might also cause improvement in certain injuries for front seat occupants only.

In trying to test whether or not the change was caused by the belting, those casualties who were wearing belts and those who were not were identified first. It might be thought that the next step would be simply to compare the percentage of belted casualties with the percentage of unbelted casualties, but this would be fallacious because there would be no distinction between the change apparently caused by increased belting and the change caused by other unmeasured factors that altered from the first year to the second year. It is necessary to separate the influence of belting from any other influences that occurred over time. The method used to do this was log-linear analysis. Anyone interested in the mathematical argument underlying this should refer to a modern statistical textbook. What the method shows, however, is that the number of casualties and the change in their distribution relative to seating position are both strongly associated with the wearing of belts, and are not merely caused by other time-related but unrecorded factors. As the report shows, this is the case for many different injuries.

Is there any greater or lesser risk of having a lung injury when wearing a seat-belt in a road accident, and can the relative risk be quantified?

In section 9.13.5 the relative risks of receiving a lung injury when belted and unbelted were examined. The important point about this calculation is that it takes into consideration not merely the changes of distribution of lung injuries among our patients who have already been involved in accidents, but also the changes of belt-wearing rates among motorists in the community. These are the great majority who have not yet been injured in accidents, but are nevertheless at risk of being injured some time in the future. It is only when this is done that the correct figure can be estimated for the changing risk of having a lung injury.

The conclusion is clear. In both years there was a greater risk of lung injury for the unbelted than the belted. Furthermore, the relative risk was very much greater in the second year than in the first. This supports the belief that the 5% of front-seat occupants who persist in flaunting the law are more prone to injury than others.

Statistical analysis never proves anything. It merely indicates how likely or unlikely it is that the evidence could have arisen by chance. The cumulative effect of all the very low probability figures never totally extinguishes the possibility that the whole picture might have arisen through chance. Nevertheless, the sensible response to this study is to continue using a seat-belt.

POINTS RAISED IN OPEN DISCUSSION

The original hypotheses were based more on previous clinical experience rather than previous trends known in this country. There was some question as to whether the Null hypothesis should have been ‘no change’ or ‘continued change’. The report did consider some aspects of previous trends but a companion study by the Transport and Road Research Laboratory looked at monthly casualty figures from 1979 onwards. The analyses revealed very clear reductions in casualty frequencies among car and van occupants. For car drivers and van occupants there were (in round figures) 20% reductions in fatal and serious casualties, and 10% reductions in slight casualties. For car front-seat passengers the corresponding reductions were larger, at 30 and 20% respectively.