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Public safety legislation and the risk compensation hypothesis: the example of motorcycle helmet legislation

J G U Adams
Department of Geography, University College London, 26 Bedford Way, London WC1H 0AP, UK
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Abstract. The 'experiment' in the United States of America in which twenty-eight states in the latter half of the 1970s repealed their laws which made the wearing of motorcycle helmets compulsory is widely believed to have proved conclusively that helmet legislation is a highly effective public health measure. The principal statistical foundations of this belief are found in a report to Congress by the National Highway Traffic Safety Administration and in a study by Watson et al. These foundations are examined and found to be open to criticism.

The evidence surveyed here suggests that the effect, if any, of helmet legislation on motorcycling fatalities is perverse. The 'risk compensation theory' is proffered as a possible explanation of the available evidence.

The hypothesis

The risk compensation hypothesis suggests that if you alter a person's circumstances in such a way as to make him safer, if he is aware of this change, and if his attitude toward risk remains unchanged, he will modify his behaviour in a way that tends to restore the level of risk with which he was originally content (see Peltzman, 1975 and Wilde, 1976 for variations on this theme).

An experiment

The 'experiment' conducted in the USA during the latter half of the 1970s in which twenty-eight states repealed laws that made the wearing of motorcycle helmets compulsory is unlikely ever to be improved upon. It comes as close to being an ideal 'controlled' experiment as any scientist could wish for; over a few years a geographically diverse sample of states, containing about 47% of the country's motorcycle population, repealed their motorcycle helmet laws—a measure which it was widely predicted would cause a substantial increase in the numbers of motorcyclists killed. How have the predictions fared?

Readers of the *American Journal of Public Health* (*AJPH*) and the *British Medical Journal* (*BMJ*) who have not had a chance to examine the data for themselves are probably of the opinion that the predictions have fared extremely well. The *BMJ*, in an editorial entitled "A grim experiment", told its readers on 9 August 1980 that:

> "The immediate effect was a drop in the proportion of motorcyclists using helmets from 100% to 50%. Deaths from motorcycle accidents rose by an average of 38% in the states which had repealed their laws, while remaining constant in the other states ... . Deaths and injuries on the road are one of the few subjects where preventive medicine can be based on reliable statistics on the effects of intervention ... . The refusal by successive governments to take action on those data is a continuing disgrace" (*BMJ*, 1980, page 406).

The immediate cause of the indignation of the *BMJ* was an article in the June 1980 issue of the *AJPH* by Watson et al entitled "The repeal of helmet use laws and increased motorcyclist mortality rates in the United States, 1975-1978". An editorial in the same issue of the *AJPH* described the evidence of Watson et al as 'new and impressive' and called the widespread repeal of helmet laws 'tragic'. This editorial noted that the conclusions of Watson et al are consistent with those of a

previous study by the National Highway Traffic Safety Administration (NHTSA) on the same subject (NHTSA, 1979).

The studies by the NHTSA and by Watson et al bear the stamp of authority and the seal of approval of prestigious journals. Together they appear to settle the debate about the efficacy of helmet legislation once and for all. This appearance is deceptive. Both studies commit elementary statistical mistakes that completely invalidate their results.

The mistake in the NHTSA study

The mistake made by the NHTSA (1980) will be considered first because it is the simplest and easiest to explain. Figure 1 is reproduced from the front cover of a report by the NHTSA (1980) to Congress. The graph was taken from the body of the report and placed on the cover because it was considered such compelling evidence in support of legislation. The report concludes (page VIII-4) that the decline in helmet use associated with helmet law repeal is "the single most significant factor" responsible for the dramatic increase shown in figure 1 in the motorcyclist death rate after 1975. And Watson et al consider the figure such compelling evidence for the efficacy of helmet legislation that they reproduced it a year later in the *AJPH* in a second article entitled "Helmet use, helmet use laws, and motorcyclist fatalities" (Watson et al, 1981).

The elementary mistake of the NHTSA was to base its conclusion on data that were too highly aggregated to support any defensible inferences about the efficacy of helmet legislation. Figure 2 shows that when disaggregated the data do not support the NHTSA's conclusion[1]. Plotting the death rates for the period after 1975, for repeal and nonrepeal states separately, we can see that the blame for the increase in the death rate cannot be placed on helmet law repeal, since, for most of this period, the increase was greatest in the states that did not repeal their laws. In 1976, nine states repealed their helmet laws, in 1977 a further fourteen, and in 1978 a further four. Between 1975 and 1978 the death rate in repeal states increased by 46.7%, while in nonrepeal states it increased by 48.2%. In 1979, one further state, Maryland, repealed its law. In 1979 the death rate decreased by 2.6% in the twenty-eight

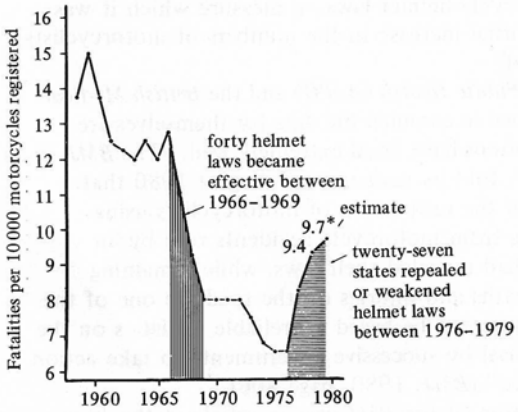

Figure 1. Fatalities per 10000 motorcycles registered, 1958–1979 (source: NHTSA, 1980).

[1] Data for figures 2 and 8 were provided by the National Centre for Statistics and Analysis, US Department of Transportation, 400 Seventh Street SW, Washington, DC. The relevant reports are "Motorcycle fatalities by state for 1975–79", submission 95 (9-2-80), Fatal Accident Recording Systems 9-5-80, and "Comparison of state total motor vehicle registrations 1975–79".

repeal states and by 7.8% in the nonrepeal states. Throughout the period, the death rate of the repeal states was on average 19% lower than that of the nonrepeal states.

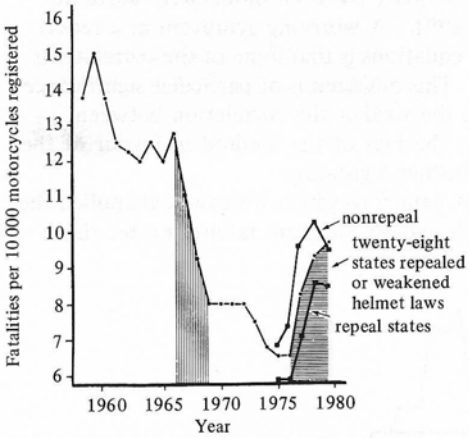

Figure 2. Disaggregated data for fatalities per 10000 motorcycles registered, 1975-1979. Figure 1 gives the number of repeal states by 1979 as being twenty-seven. Watson et al (1980) list Nebraska as a repeal state in 1977, but the NHTSA does not. Actual fatalities in 1979 were substantially below the estimate recorded in figure 1. Source: see footnote (1).

The mistake in the study by Watson et al

Watson et al conclude their 1980 paper on the same note of indignation as that found in the editorials quoted already. They write (page 583):

> "The repeals of motorcycle helmet laws have been one of the most tragic decisions made recently in the USA from the standpoint of public health ... . The retention of existing laws and the reinstatement of repealed laws should be an urgent issue for public health workers and everyone else concerned with lowering unnecessary mortality and morbidity, and the huge medical and other economic losses that result."

The statistics upon which their indignation rests are much more sophisticated than those illustrated by figure 1. This is how they describe their method:

> "The states that repealed or weakened their helmet laws were matched with one or more states from the same geographic region that either did not have helmet use laws or did not change such existing laws in this four-year period. The effect of weakening the law in each state was then estimated in three steps: 1) the mortality data from each state for the period prior to repeal were regressed on smoothed data from the matched states; 2) these equations were used to predict the numbers of motorcyclist deaths that would have been expected in each state in the period following the repeal or weakening of the law if the laws had not been changed; 3) these numbers of expected deaths were then compared with the actual numbers of deaths that occurred" (Watson et al, 1980, page 579).

They concluded that:

> "It is estimated that the repeals or weakening of motorcyclist helmet use laws were typically followed by almost 40 per cent increases in the numbers of fatally injured motorcyclists" (Watson et al, 1980, page 579).

As in the NHTSA's graph in figure 1, at first sight their evidence looks like compelling support for legislation—so compelling that it was reproduced in the NHTSA's own report. Also like the NHTSA's graph, it is completely misleading.

The method adopted by Watson et al involves prediction based on regression equations and is crucially dependent for its plausibility on the 'matching' states being a

good match. To quote just one authority, "unless the correlation is reasonably high (say .7 or above), it may be rather misleading to make use of prediction equations", and "correlations of a very high order are necessary for even moderately accurate prediction" (Blalock, 1960, pages 285 and 299). A worrying symptom in a report which is dependent on the use of prediction equations is that none of the correlations between repeal and control states are given. This omission is of particular significance for this study, because, as explained below, the weaker the correlation between repeal and control states, the greater will be the bias of the method in favour of the authors' conclusions about the efficacy of helmet legislation.

Figures 3(a) and 3(b) are graphs of the raw data[2] to which Watson et al applied the method that led to the conclusion that Utah had 95.5% more fatalities after the

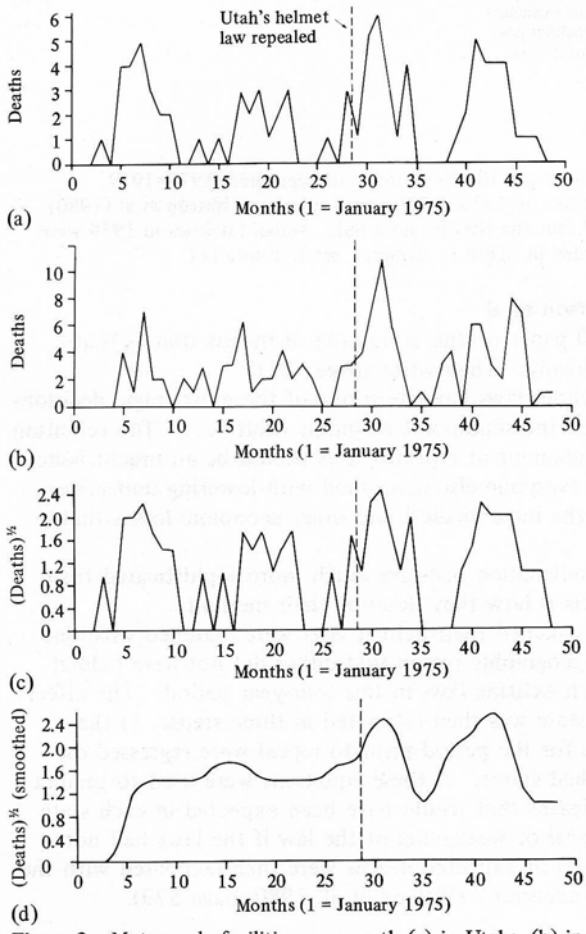

Figure 3. Motorcycle facilities per month (a) in Utah; (b) in the control states of Nevada and Wyoming; (c) in Utah (transformed data); and (d) in control states (transformed data). Source: see footnote (2).

[2] Data for figures 3, 4, 6, and 7 were provided by the National Centre for Statistics and Analysis [see footnote (1) for address]. The relevant report is "Motorcycle fatalities by state and month for 1975-79", submission 95 (9-2-80), Fatal Accident Recording Systems 9-24-80. It has not been possible to confirm that this is precisely the same data set as that used by Watson et al (1980). A request to Dr Zador for his data met with the reply that they were not in a 'readily available format'.

repeal of its helmet law than expected on the basis of its relationship with the control states, Nevada and Wyoming. Utah is used in the following critique of the method because, according to Watson et al, it is the state which experienced the greatest postrepeal increase in fatalities. It is, in other words, the case that most strongly supports the indignation that Watson et al direct at those states that have repealed their helmet laws.

Figures 3(c) and 3(d) display the results of the data transformations that Watson et al employed before proceeding to the regression stage of their method. First they took the square roots of the data for both Utah and its control states. Then, in addition, they subjected the control state data to a procedure known as a '3RSSH twice smooth'. This has the effect of removing the peaks and troughs from the time-series data. It seems a curious thing to do to the data. The point of smoothing, according to Tukey (1977, page 205), the author of the method, is to give a "clearer view of the general once it is unencumbered by detail". But a comparison of figures 3(b) and 3(d) suggests that the smoothing operation performed by Watson et al gets rid of the general along with the detail. For the period before repeal, no trace of the seasonal fluctuations apparent in the raw data of figure 3(b) remains in figure 3(d). The main justification of the matching method is that the repeal and control states share a common pattern of seasonal fluctuation: "Since motorcyclist fatalities display strong seasonal variation, the similarity of such variation was an important part of the matching criteria" (Watson et al, 1980, page 581). There is a weak correlation between the raw data of Utah and the control states in the period before repeal (r^2 adjusted for degrees of freedom is equal to 25.5%). But the data transformations employed by Watson et al reduce this correlation almost to zero (r^2 adjusted for degrees of freedom is equal to 1.0%).

Figure 4 illustrates the relationship between the transformed data for Utah and that for its control states for the period before repeal. The most important thing to note about this correlation is that it means that any prediction based upon the 'matching' of Utah with its control states will not only be totally unreliable, but also very strongly biased. This is because the weaker the correlation between the variables, the flatter will be the regression line which describes their relationship. If, as is the case here, there is a pronounced increase in the average values of *both* variables in the period after repeal—the period for which the prediction is being made—most of the residuals from regression for the prediction period will lie above the flattened regression line; that is, the regression line will produce an underestimate of the number of deaths in the repeal state.

Figure 5 illustrates the nature of the bias injected by a weak correlation when a regression model is used for purposes of prediction in the way in which Watson et al have used one. It shows, for period 1, variable Y plotted on variable X. The slope

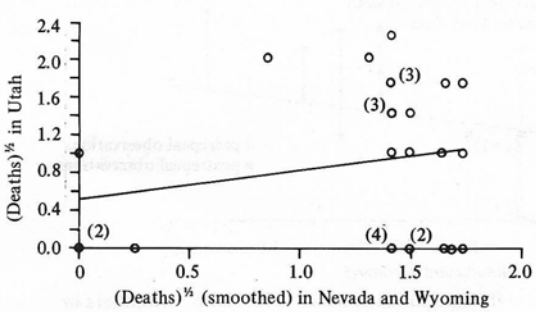

Figure 4. Regression of transformed data for Utah on that of its control states for the period 1975-1978. Numbers in brackets indicate multiple observations. Source: see footnote (2).

of the line on the graph will be determined by the standard deviations of the two variables, the graphing scale, and *the strength of the correlation*. If the correlation is zero, as in this illustration, the regression line will be horizontal, regardless of standard deviations and graphing scales.

Figure 5 assumes that both X and Y have means of one in period 1, and means of three and two, respectively, in period 2. It can be seen that if the regression line fitted to the data for period 1 were to be used to calculate the 'expected' values of Y in period 2 (using actual values of X in period 2), all the residuals would be positive, even though the increase in X was, on average, twice the increase in Y. Thus the method will be very powerfully biased in cases where there is a weak correlation and a significant increase in the average values of both variables between the period for which the regression line is calculated and the subsequent period for which 'expected' values are estimated. If the average values of both variables were to decrease, the bias would be in the opposite direction.

Figure 6 illustrates this biasing effect with the example of Utah. It should be noted that most of the postrepeal observations lie beyond the range of the data to which the regression line was fitted. The line in figure 6 illustrating the regression of the control state data on Utah data shows that the method, allied to a weak correlation, can be used to prove anything. If the 'expected' fatalities for the control states after repeal are predicted from this regression line, the location of the residuals for the postrepeal period indicates that Nevada and Wyoming had many more fatalities than 'expected'.

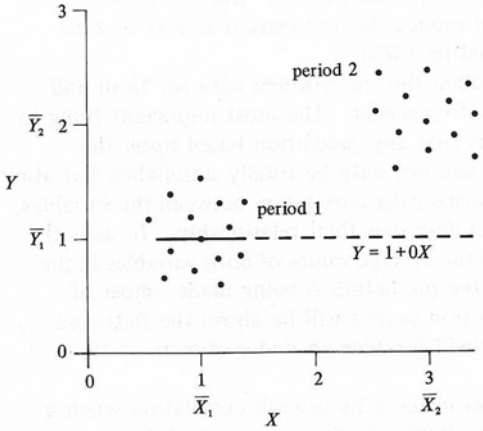

Figure 5. Bias caused by a weak correlation.

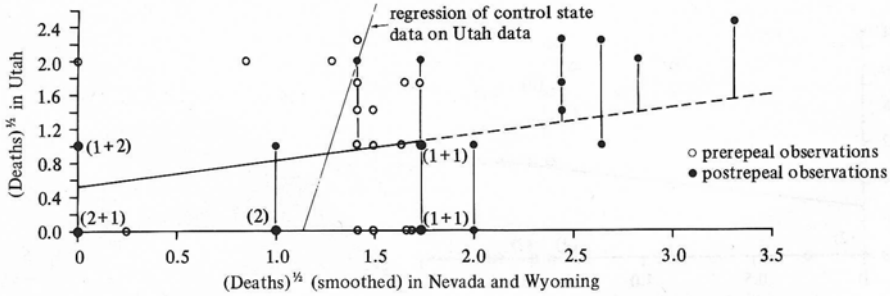

Figure 6. Regression of Utah data on control state data for the period 1975–1978. Numbers in brackets indicate multiple observations; where there are two numbers the second number indicates postrepeal observations. Source: see footnote (2).

Figure 7 illustrates a more simple and direct method for comparing the time-series fatality data for the repeal and control states. For each month, the fatalities in the control states have been subtracted from the fatalities in the repeal states. Hence the two time-series are reduced to one, which represents the difference between them. This has the effect of removing any common trend or shared pattern of seasonal fluctuation, leaving only variation attributable to random or other causes. If helmet law repeal is a factor effecting a 95.5% increase in the fatalities of the repeal state, then it should produce in figure 7 a large and sudden *increase* in the average level of the graph after month twenty-eight, the time of repeal. In fact, after month twenty-eight the average level of the graph *decreases*, from −0.54 to −1.5. Table 1 summarises the data for Utah, Nevada, and Wyoming, from which it can be seen that the number of deaths in the control states increased by almost twice as much as in the repeal state in the period after repeal.

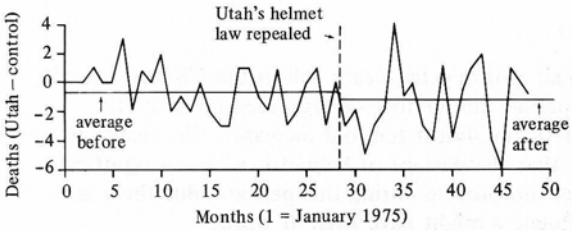

Figure 7. Motorcycle fatalities per month, Utah data minus control state data (1975–1978). Source: see footnote (2).

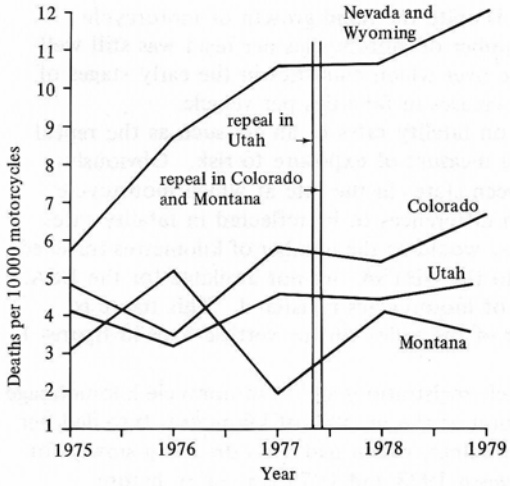

Figure 8. Deaths per 10000 motorcycles in the control states, Colorado, Utah, and Montana. Source: see footnote (1).

Table 1. Number of deaths in Utah and in the control states, before and after repeal.

State	Average monthly deaths		Percentage increase
	before repeal	after repeal	
Utah	1.46	2.15	47.3
Nevada and Wyoming	2.00	3.65	82.5

Registration data are not available on a monthly basis. Figure 8 compares annual fatalities per 10000 registered motorcycles in Utah, Montana, and Colorado with those in Nevada and Wyoming. Nevada and Wyoming also served as control states for Colorado and Montana. Montana and Colorado are the repeal states which, according to Watson et al, experienced the second and third, respectively, greatest increases in fatalities following repeal. They calculated that these states had, respectively, 86.7% and 82.3% more fatalities than 'expected' following repeal. Thus figure 8 shows that the three states that are purported to support the conclusions of Watson et al most strongly, all have substantially lower death rates than the control states and did not experience significant increases in their death rates relative to the control states at the time of repeal.

The large percentage increases from expected to actual deaths reported by Watson et al would therefore appear to be entirely artifacts of an inappropriate statistical method.

The data

Between 1958 and 1975, the annual motorcyclist death toll in the USA increased from 720 to 3189. In the same period the number of registered motorcycles increased from 521290 to 4964070—an almost ten-fold increase. The reader is invited to conclude from figure 1 that the passage of helmet laws was a significant cause of the decrease in deaths per motorcycle during this period. But there is evidence to suggest that other influences might have been at work.

The early stages of the motorization process have been characterized, almost everywhere in the world, by rapid increases in the numbers killed, even more rapid increases in the numbers of vehicles owned, and sharp decreases in the numbers killed per vehicle (Adams, 1981, page 121). Despite the rapid growth of motorcycle ownership in the USA, by 1975 the number of motorcycles per head was still well under 0.1, that is, well within the range over which countries in the early stages of motorization experience the greatest decreases in fatalities per vehicle.

Ideally, when looking for the effect on fatality rates of an act such as the repeal of helmet legislation, one should have a measure of exposure to risk. Obviously, if there are significant differences between states in the rate at which motorcycle use is changing, one would expect such differences to be reflected in fatality rates. The most appropriate measure in this case would be the number of kilometres travelled by motorcycle. Such data, according to the NHTSA, are not available for the USA. A much cruder measure is the number of motorcycles registered. This figure *is* available and serves as the denominator of the index on the vertical axes in figures 1, 2, and 7.

In Britain, estimates both of motorcycle registrations and of motorcycle kilometreage are available. This permits the calculation of the number of kilometres travelled per year by the average motorcycle. This number, which had been dropping slowly for many years, increased very sharply between 1973 and 1978 (by 43%), before dropping again in 1979. If a similar phenomenon occurred in the USA it could account for most of the increase that the NHTSA attributes to helmet law repeal. But apparently even the US registration data is of dubious accuracy. In their 1980 paper, Watson et al use their doubts about its accuracy as their reason for using a method of analysis that takes no account of exposure to risk at all: "Because credible registration data were not available, we felt that no such data could be used in this study" (page 581). They are not, however, consistent in their rejection of evidence based on registration data. In their 1980 paper, they cite, as evidence supporting their conclusions, a study by L Robertson based on registration data (page 580), and in their 1981 paper they actually reproduce the graph in NHTSA

(1979) (figure 1 above) and assert without any caveats at all that it shows that "the repeal of 27 state helmet laws between 1976 and 1979 coincided with a rise in the fatality rate from 6.7 to 9.7, a 31 per cent increase"[3]. Their clear implication is that repeal was the cause of the post-1975 increase shown in figure 1.

The NHTSA (1980) also admits to reservations about the reliability of the registration data (page V-25). But these reservations were apparently not sufficiently strong to dissuade it from using figure 1 as the cover of its report to Congress—a gesture which the unwary might reasonably construe as a manifestation of confidence in the evidence.

Either the data are not to be trusted and the NHTSA's case collapses, or they are to be trusted and the NHTSA's case collapses.

The risk compensation hypothesis

The NHTSA (1980) report contains other evidence about the efficacy of helmets in addition to that contained in figure 1, the main plank of its case. This other evidence consists of a review of a number of studies which show quite convincingly that *if one is in a motorcycle accident* one's chances of escaping death or serious injury are greatly improved by the wearing of a helmet. This evidence can be reconciled with the evidence presented in figures 2 to 8 by means of the 'risk compensation hypothesis'. This hypothesis suggests that the added sense of security provided by a helmet, and by the safety propaganda promoting the use of helmets, might lead riders to take more risks. This hypothesis is considered in the NHTSA report, but rejected with the following arguments:

> "An argument that required helmet use causes a false sense of security assumes that helmet use creates a sense of invulnerability which would not exist if helmets were not required. Yet data from the USC [University of Southern California] study show that the most frequent reason (38 percent) given by accident-involved riders for *not* wearing a helmet was that there was no expectation of being involved in an accident, i.e., invulnerability" (NHTSA, 1980, page IV-20).

Without access to the precise form of the question put to the surviving accident victims interviewed in the USC study, one is left to choose between the possibility that a nonsense question was asked, and the view that a very strange construction has been placed on the answers. Despite the moronic 'Hell's Angels' reputation attaching to some motorcyclists, one strongly suspects that all motorcyclists know that motorcycling is dangerous and that helmets provide a degree of protection *in an accident*. If we assume that their attitudes toward risking death or injury are not changed by the act of putting on a helmet, it would seem distinctly possible that the sense of added protection afforded by helmets in accidents might result in motorcyclists taking greater risks of being involved in accidents.

A second argument advanced by the NHTSA against the hypothesis is that it does not apply to other safety precautions:

> "If one were to accept the proposition that helmets create a false sense of security among motorcyclists one would have to logically extend this reasoning to other areas of daily activity in which safety devices are used on a voluntary or compulsory basis. Is a false sense of security created for industrial workers who must wear hard hats, safety glasses, safety shoes etc? Or do boaters and water skiers feel invulnerable because they wear flotation devices?" (NHTSA, 1980, page IV-20).

[3] An increase from 6.7 to 9.7 is an increase of 44.7%, not 31%. But as figure 2 indicates, the actual number of fatalities per 10000 motorcycles in 1979 was considerably below the estimate shown in figure 1. For all states together, it was 8.8. Thus the increase from 1976 to 1979 was, coincidentally, 31%.

The answer to both these questions is frequently almost certainly *yes*. Trapeze artists, steeplejacks, and window cleaners clearly attempt manoeuvres with their safety nets and harnesses that they would not attempt without them. Similarly, boaters and water-skiers who are poor swimmers will attempt exploits with flotation devices that they would not attempt without. Whether or not the sense of security provided by safety devices is 'false' depends on whether the safety benefit perceived by the user exceeds the benefit they actually provide.

A third argument against the hypothesis is that unhelmeted riders have more accidents than helmeted ones:

> "Additionally, FARS [Fatal Accident Recording System] data in combination with observational data from four State studies supported by NHTSA and the USC study show that the rate of helmet use by accident-involved riders is lower than the rate of helmet use by non-accident involved riders. These data indicate that persons who do not wear helmets, whether required by law or not, appear to be more frequently involved in accidents than helmet wearers" (NHTSA, 1980, page IV-21).

The NHTSA report itself explains why this should not be construed as evidence refuting the risk compensation hypothesis:

> "This ... suggests that failure to wear a safety helmet may be a basic characteristic of a subgroup of motorcyclists who, in all likelihood, are high risk takers in other areas of their lives, not merely those related to motorcycles or motorcycle helmet use" (NHTSA, 1980, page IV-21).

Finally, the NHTSA report argues that there is simply no evidence:

> "It is argued that requiring helmet use creates a false sense of security among motorcyclists and thus causes them to take risks which they would not take were they not required to wear helmets. No statistical evidence has been offered to support this argument" (NHTSA, 1980, page IV-20).

Figures 2 to 8 display statistical evidence which supports the latter argument. Watson et al (1980) report that in states with helmet laws, wearing rates are over 98%, but in states without helmet laws, the rates average 48%. Further, they report that in crashes of comparable severity, the death rate of unhelmeted riders exceeds that of helmeted riders by 52%. *If* there is a large decrease in numbers wearing helmets following repeal, *if* helmets provide a significant measure of protection *in crashes*, and *if*, as figure 2 suggests, the increase in fatality rates since 1975 has been greatest in states that have not repealed their laws, then the expectations of the advocates of helmet legislation have been confounded. Some other factor which they have previously not considered must be at work, either increasing the fatality rate in nonrepeal states or decreasing it in repeal states. A possibility deserving serious consideration is that the driving behaviour of motorcyclists has been influenced by the sense of security associated with the wearing of helmets.

Two other studies in the realm of road safety provide support for the hypothesis that driving behaviour is influenced by the perception of risk. A Swedish study (Rumar et al, 1976) compared driving speeds of cars fitted with studded tires with those without. In dry road conditions there was no significant difference. In icy conditions the cars fitted with studded tires were driven significantly faster. And a recent international survey of the efficacy of seat belt legislation has shown that the decrease in road deaths in the mid-1970s associated with the energy crisis was greater in countries that did not have effective seat belt laws than in those that did (Adams, 1982).

Conclusion

The foregoing review of the statistical case for helmet legislation suggests that the faith of the *BMJ* in the reliability of the statistical evidence on the effect of helmet legislation is misplaced. But debates about sophisticated statistics should not be allowed to drown out the voice of common sense. The risk compensation hypothesis is essentially common sense. There is a wealth of evidence from everyday experience that suggests that people's behaviour is influenced by their perception of risk. People tend to be more cautious when up high ladders than when up low ladders. They tend to take more care when standing on the edge of a high precipice than when standing on a low kerb. They tend to slow down when they encounter bends in the road or patches of fog and to speed up when the road becomes straight or the visibility good—and so on. The possible illustrations of the phenomenon are countless.

There is an impressive amount of propaganda designed to encourage people to believe that they are very much less vulnerable when using seat belts and motorcycle helmets, and common sense suggests that driving behaviour will be influenced by safety devices that diminish the user's sense of vulnerability. What unaided common sense cannot predict is whether the behavioural changes induced by a safety device will partly, completely, or more than completely nullify the intended effect of the device.

The statistics which the NHTSA considers the most compelling evidence in support of helmet legislation are displayed in figure 1. The state which Watson et al (1980) consider has had the greatest increase in deaths as a result of helmet law repeal is Utah. When the distorting effects of the analytical methods employed by the NHTSA and Watson et al are removed, their data (with the caveats discussed above) suggest that the effect, if any, of helmet legislation on motorcycling fatalities is perverse.

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