# SPECULATION VERSUS FACT: REPLY TO THE COHENS

## LEON S. ROBERTSON

The comment by the Cohens on my article, "An Instance of Effective Legal Regulation" (1976), consists mainly of speculation about the fatality distributions and questions regarding the adequacy of the statistical models that I used to test for deviation from random fluctuation. Such speculation is unwarranted, and particularly so when the issue is public policy affecting human life. A point by point examination of the Cohens' comments indicates that they are without merit.

The Cohens are concerned that I did not state the obvious fact that the variation among states in prelaw helmet use would result in variation in the effects of the laws. Perhaps, for some readers, I should have stated the obvious, although I did allude to the point in the first paragraph of the discussion section of my paper. The effect of prelaw variation only contributes to the magnitude of residual variance in the analysis of variance of the effects of the laws, and is of no importance since the results of the law were significant despite the size of the residual variance.

The Cohens speculate that "there may be an interaction in compliance when the two laws—helmet and daytime headlight— are examined simultaneously." This means that they think helmet laws may affect headlamp use and headlamp laws may affect helmet use.

Since helmet use was virtually 100 percent in the states with helmet laws whether or not they had headlamp use laws, there is no reason to believe that a headlamp use law has an effect on helmet use in states with helmet use laws. Helmet use in Los Angeles—without a headlamp use or helmet law—was higher than in Chicago where there was no helmet law but there was a state headlamp use law. However, one should not jump to the conclusion that the headlamp use law in Illinois somehow contributes to low helmet use relative to that found in California.

For a number of years I have conducted surveys of seat belt use by automobile drivers in four cities, including Los Angeles. Belt use has been consistently higher in Los Angeles than elsewhere. In the spring of 1976, 33 percent of Los Angeles drivers observed were wearing belts compared to 21 percent of drivers in both Houston and Baltimore and 25 percent in Detroit. But no

reasonable person would infer that automobile drivers in Los Angeles use seat belts more often because California has no helmet law for motorcyclists. Apparently both motorcyclists and drivers in Los Angeles avail themselves of crash protection more often than their counterparts elsewhere—perhaps because a greater proportion of their travel is on crowded, high-speed freeways. This does not detract from the fact that substantially more people use belts and helmets when required to do so by law (Andreassend, 1972).

With respect to a possible effect of helmet laws on headlamp use, it should be noted from the data in Table 2 of my original paper that the maximum possible magnitude of such an effect would be a difference of about 10 percentage points in daytime headlamp use, compared to a difference of about 40 to 75 percentage points in helmet use between states with and without helmet laws. Since the effect of helmet use in reducing deaths has been well established, but the effect of headlamp use has not, the highly dubious possibility that helmet laws have a small effect on headlamp use is not an argument against those laws.

In any research where statistical significance is tested, alternative statistical models are always available, and the validity of interpretations of the results of the tests selected depends on whether the assumptions of the model are met. The Cohens' assumptions regarding the model I used and the model they suggest are incorrect. Moreover, they stated what I found in such a way as to be misleading.

The Cohens assume that, "if the law is effective in reducing fatal involvement rates, we would expect rates to decline by year among law states, not to decline by year among nonlaw states, and we would expect law states to differ from nonlaw states only in the years of and after the enactment of the law. This is measured by the *law by year* interaction effect, a within-state effect, and an effect that is *not* significant in Robertson's analysis" (emphasis theirs).

The first sentence in that statement is incorrect because a decline in death rates in the states enacting laws is not necessary to infer effectiveness of the laws in preventing deaths that would have otherwise occurred. For instance, that inference could still be made if: (1) there were no significant difference between law and nonlaw states prior to the law; (2) states enacting laws maintained the same rates after enactment; (3) the rates in the comparison states increased significantly. Thus, change in rates in the states that enacted laws as revealed in split plot or other designs is not a necessary criterion for inferring an effect from the law. The pos-

sible patterns of association in quasi-experimental designs have been discussed in detail in a book I cited in my original paper (Campbell and Stanley, 1963) and should be consulted by the interested reader not familiar with such research designs.

The Cohens' statement that a significant law by year interaction effect is necessary to attribute the reduction of deaths to the laws is also not correct. A law by year interaction effect would be maximized if the rates showed a crossing pattern over the three years—from very high to very low in states enacting the laws and from very low to very high in states not enacting laws. Such a pattern would raise serious questions about the comparability of the states prior to passage of the laws and would thus raise serious questions about the effects of the laws.

TABLE 1

ANALYSIS OF VARIANCE OF WEIGHTED MOTORCYCLIST
DEATH RATES PER 10,000 REGISTERED MOTORCYCLES IN
STATES WITH AND WITHOUT HELMET USE LAWS

| Source | One Year prior to Helmet Use Laws | | | |
|---|---|---|---|---|
| | Sum of squares | df | F | Significance |
| Law | 439390 | 1 | 0.31 | None |
| Degree days | 10514744 | 1 | 4.47 | 0.02 |
| Law × degree days | 238523 | 1 | 0.17 | None |
| Residual | 16886192 | 12 | 0.17 | |

| Source | Two Years with Helmet Use Laws | | | |
|---|---|---|---|---|
| | Sum of squares | df | F | Significance |
| Law | 6664625 | 1 | 6.69 | 0.02 |
| Degree days | 6213686 | 1 | 6.23 | 0.02 |
| Law × degree days | 2173782 | 1 | 2.18 | None |
| Residual | 27910368 | 28 | | |

My analysis found no significant difference between the states prior to enactment of the laws but significantly fewer deaths over the three years. Since the average number of deaths was slightly but not significantly lower in states that enacted laws, prior to their enactment, than in states that did not, and 30 percent lower after passage of the laws, the effect can only be attributed to reduction in deaths occurring during the year of and the year subsequent to enactment. The data in Table 1 should put to rest any doubt about statistical significance of these differences. Using the data weighted to reduce the effect of sample size on variance,

and controlling for climate, there is no difference in death rates between law and nonlaw states prior to enactment of the laws and a highly significant difference after enactment. The effects of climate were the same during both periods, and were independent of other effects, as shown in the original analysis, and again in Table 1. Thus, there is no justification for the Cohens' speculation that some interaction of climate with the introduction of the law would explain my results. An analysis using unweighted fatality rates gives the same results.

The Cohens' discussion of the fact that some of the helmet laws were in force for only part of the initial year ignores two considerations. First, the months during which the laws were not yet in force were mainly winter, when motorcycle use is at a minimum in all but the warmest states. Second, press coverage at the time of legislative debate may contribute more to informing motorcyclists about the law, and securing their compliance with it. Thus helmet use may increase before the official date of enforcement.

The selection of states and the determination of a time span were severely limited by the lack of any data in some states and of monthly data in many states. Also, states that enacted both helmet laws and headlamp laws in the time periods under consideration were not used because of the impossibility of attributing any observed effect to one law or the other. It was not possible to match states on multiple criteria, as the Cohens suggest, but I believe that objective observers of the American scene would agree that the sets of states used are as comparable as any that could have been chosen had the restrictions I noted not prevailed.

It became evident in compiling the data that states in the so-called sunbelt had higher motorcyclist death rates so the control for heating-degree days was introduced to reduce the variance produced by that factor. Climate was very stable during the period studied and each state considered was on the same side of the median climate throughout the period studied as it had been in the year prior to enactment. The climate factor was not used in analyzing the headlamp law because it does not affect the proportion of deaths between states—the statistic used in the headlamp analysis.

Because of the limited number of states, the limited data in the period subsequent to passage, and the fluctuation in fatalities prior to passage, I was extemely cautious in my statements regarding the effects of headlamp laws, as a careful examination of the original paper will indicate. The Cohens' suggestion that a simple comparison of proportions of fatalities before and after enactment would be more appropriate ignores the importance of the design I

used in detecting the possibility of different trends in the states prior to enactment. They do not state which statistical significance test they used to test the difference between proportions, but it is very likely that the power of any such test to detect possible differences is less than that of analysis of variance.

I believe that objective observers will find that the Cohens' commentary consists mainly of speculation and sophistry, and is not in line with the facts. Comparisons of injuries to motorcyclists in crashes with and without helmets have repeatedly discovered substantially less frequent and less severe injury to the head when helmets were used (Cairns and Holburn, 1943; Lewin and Kennedy, 1956; Foldvary and Lane, 1964; Jamieson and Kelly, 1973). My study found much greater helmet use in states with helmet laws compared to states without such laws and substantial reductions in deaths in states that enacted such laws compared to contiguous states that did not. It does not require complex logic to conclude that helmet use laws are a major factor in reducing deaths of motorcyclists. Anyone who uses the Cohens' commentary to justify further repeal of helmet use laws does so at the jeopardy of human life.

## REFERENCES

ANDREASSEND, D.C. (1972) "The Effects of Compulsory Seat Belt Wearing Legislation in Victoria," in *Papers Presented at the National Road Safety Symposium*. Canberra: Australian Government Publishing Service.

CAIRNS, H. and H. HOLBURN (1943) "Head Injuries in Motorcyclists with Special Reference to Crash Helmets," *British Medical Journal* 591.

CAMPBELL, Donald T. and Julian C. STANLEY (1963) *Experimental and Quasi-experimental Designs for Research*. Chicago: Rand-McNally.

FOLDVARY, L.A. and J.C. LANE (1964) "The Effects of Compulsory Safety Helmets on Motorcycle Accident Fatalities," 21 *Australian Road Research* 7.

JAMIESON, K.G. and D.S. KELLY (1973) "Crash Helmets Reduce Head Injuries," 2 *Medical Journal of Australia* 806.

LEWIN, W. and W.F.C. KENNEDY (1956) "Motorcyclists, Crash Helmets, and Head Injuries," 1 *British Medical Journal* 253.

ROBERTSON, Leon S. (1976) "An Instance of Effective Legal Regulation: Motorcyclist Helmet and Headlamp Laws," 10 *Law & Society Review* 467.