Response to "Intervention Analysis for the Impacts of the 65 mph Speed Limit on Rural Interstate Highway Fatalities," by Chang, Chen, and Carter

William J. Rauch
Paul L. Zador

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INSURANCE INSTITUTE FOR HIGHWAY SAFETY

1005 N. GLEBE ROAD, ARLINGTON, VA 22201 (703) 247-1500

ABSTRACT

In "Intervention Analysis for the Impacts of the 65 mph Speed Limit on Rural Interstate Highway Fatalities," Chang, Chen, and Carter claim that the 65 mph speed limit initially increased highway fatalities but that this effect decayed after one year. These conclusions are contrary to Chang et al.'s own results as well as the vast majority of the published literature on the effects of raising and lowering the national maximum speed limit. The approaches and assumptions in this work are flawed and the conclusions should be dismissed.

"Intervention Analysis for the Impacts of the 65 mph Speed Limit on Rural Interstate Highway Fatalities," by Chang, Chen, and Carter (1993) in the *Journal of Safety Research* claims that the 65 mph speed limit initially increased highway fatalities but that this effect decayed after one year and that the increase in fatalities was far less than reported in other studies. The results presented by Chang et al. do not support these conclusions, which are contrary to those of numerous other published studies.

Although, Chang et al. list many studies based on multistate data that found increases in crashes, fatalities, fatality rates, and/or serious injuries on rural interstate highways following the raising of the speed limit to 65 mph, they also cite studies based on the experience of some states that apparently found no significant changes in fatalities. Chang et al. ignore the random fluctuations inherent in individual state fatality data and argue that, overall studies have produced inconsistent findings and the true impact of increased speed limits on fatalities is unknown. However, the evidence is overwhelming from nationwide studies that raising the speed limit increases highway deaths (e.g., Baum, Lund, and Wells, 1988, 1989; Baum, Wells, and Lund, 1990, 1991; Garber and Graham, 1990; NHTSA, 1989a, 1989b, 1990).

TIME SERIES ANALYSIS

Chang et al. used time-series based intervention analysis methods for 32 states that raised speed limits by June 30, 1987. After first modeling the monthly fatality pattern in Fatal Accident Reporting System (FARS) data from January 1975 to March 1987, they compared observed fatality data from April 1987 to December 1989 with the pattern predicted by their model to determine the effect of speed limits.

A model fitted to time series data before an intervention (e.g., adoption of the 65 mph speed limit) may be used to estimate what might have happened after the intervention had it not taken place. This requires first, that the intervention itself be described in the language of the model, and second, that the pre-intervention model, combined with a term describing the intervention, fit the data both before and after the intervention. If both conditions are met, parameters of the intervention term may be interpretable as describing the time course of the intervention.

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Finding the right description for data of this type, that is, fitting the correct model to the data, is both an art and a science; there is room for judgment in deciding how to proceed given certain diagnostic statistics. There are usually many models that fit a given time series, and researchers need to choose among them on the basis of both common sense and statistical test results. Even inappropriate models may fit the part of the time series that was modeled. However, if the model is inappropriate, it will not predict the future. The parts of the time series not used for fitting the model and various model parameters will have no valid interpretation in terms of time series behavior. Chang et al. provided no evidence that their model adequately describes the data both before and after the speed limit increase.

The procedure used by Chang et al. completely fails the test of common sense. For example, according to the terms included in Chang et al.'s model the fatality count in one month, say April, would predict the fatality count better two months ahead, in June, than one month ahead, in May. No explanation is given as to what this type of month skipping represents. In another example, there is a term in the model that suggests that a random event, such as inclement weather (e.g., snow, flood), that happens to affect fatalities in April of some year would directly affect fatalities in September of the same year, February of the next year, July of that year, and so on.

Chang et al. make the flawed assumption that changes in monthly fatalities between 1975 and 1989 could be accounted for with *no reference* to outside (exogenous) factors. Their models included no variables for any of the factors (e.g., vehicle miles of travel, seat belt usage, unemployment rate, etc.) known to be associated with fatalities. Because of the undisputed prior evidence for the importance of such factors, Chang et al. would need to show that their models fit the data, but they report no model fit statistics. Without regard to common sense or scientific validity, it appears that Chang et al. choose their model only because it resulted in the lowest residual mean squares. A small improvement in an unadjusted R^2 is not a valid reason for including theoretically implausible terms in a model. Thus their model cannot be accepted as adequate. (See Ljung and Box (1978) and Judge et al. (1988) for a discussion of model fit criteria.)

Six different shapes are presented for the hypothetical effect on fatalities of raising the speed limit. According to Chang et al., significant effects were identified due to the intervention for two of the six shapes. One shape represents the effect as a step function (Type 2) and another represents it as a step function followed by exponential decay (Type 5). Using the Type 5 shape for the intervention function, Chang et al. estimated a larger initial increase in rural interstate deaths (30.5 per month) for the period of April 1987 to August 1988, than when using the step function estimate (26.5 deaths per month). But it was also estimated that this initial rise declined slightly at an exponential rate to 26.2 deaths/month.

Chang et al. interpret the period between April 1987 and August 1988 as a "learning period." The date for the end of the learning period was obtained by trial and error: Chang et al. kept fitting their model with the end of the "learning period" set equal to each month between June 1987 and August 1988 until some parameters became significant. This trial and error procedure is unacceptable for two reasons. First, the type intervention methods used by Chang et al. were designed to estimate the effect of a known intervention at a specified time and not to try to determine when the intervention occurred. Second, when more than one statistical test is performed, the cutoff levels for significance testing must be appropriately adjusted to take into account the number and type of repetitions. Without such adjustments, significance tests yield misleading results.

In any case, the results show that large and statistically significant increases in deaths took place in April 1987 and that this declined only slightly over time.

In a followup sensitivity analysis, Chang et al. determined that, when estimated using the step function (Type 2) description of the effect, the intervention was statistically significant as long as it was assumed to occur during any of the months between July 1986 and December 1987. From this, Chang et al. conclude "... such a consistent impact pattern in nationwide highway fatalities may be attributed to factors other than the increase in speed limits, such as the increase in the driving population or the average travel mileage. The identification of exact reasons for such a pattern is beyond the scope of this paper and should be addressed in further studies." This is a clear misinterpretation of their "sensitivity analysis." If their model were correct, which it is not, then the results from the sensitivity analysis could only mean that the statistical power of their test was inadequate for pinpointing the exact beginning of the intervention. This would not be cause for surprise since the test used was inappropriate.

If their model is not correct, (i.e., if some factors known to affect fatalities are left out of the model) then no conclusions can be reached about the effect, or lack of effect, of raising the speed limit on fatalities. The only valid conclusion in this case is that the model needs to be improved before it is put to use.

TIME SERIES -- CLUSTER ANALYSIS

Chang et al. repeated the same types of analyses on five clusters of states that had been grouped using three variables related to state population size: vehicle registration count, total mileage of public roads and streets, and annual vehicle-miles of travel. The only justification provided for using this clustering technique was that the authors wanted to use time-series intervention analysis. It is unclear why states of relatively similar population size should be affected similarly by increased speed limits. There were two groups comprised of only one state each (California in one and Texas in the other). One group contained only two demographically and geographically dissimilar states (Illinois, Florida), and no evidence was offered as to why the states in the two other clusters would be expected to respond similarly to speed limit changes. The same methodological errors were repeated for the clustered analyses as were performed on the nationwide study.

CONCLUSION

In the framework adopted by Chang et al., the effect of raising the speed limit could best be specified by the assumption that it coincided with a change in monthly fatalities at about the same time. Chang et al. estimated such a model and found that raising the speed limit coincided with a statistically significant increase in fatalities. That finding is consistent with the overwhelming bulk of the published literature on the effects of raising and lowering the national maximum speed limit. However, instead of accepting this finding, they argued that this was not the real effect of raising the speed limit because alternative and far less plausible statistical models also fit the data. Chang et al. made no attempt to bolster their reasoning with statistical tests and their arguments are nonsense. The approaches and assumptions underlying Chang et al.'s conclusions are both technically and logically flawed and should be dismissed.

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