Commentary: Causal pathways of relative motor vehicle crash fatality risk are hard to estimate from police records

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Lardelli-Claret et al.1 estimated relative driver and front seat (FS) passenger fatality risks as a function of age, sex and belt use (ASB). Four sets of relative fatality risk (RFR) estimates were derived from Poisson regression models based on Spanish traffic crash registry data for the years 2000–04 (N = 84 338 FS occupant pairs). Each of the models related the probability of an FS driver (Model 1), an FS passenger (Models 2 and 3) or an FS occupant (Model 4) fatality to some combination of covariates for occupant, vehicle, crash and environmental factors. The covariates varied by model. Depending on model specification, the RFR estimates of ASB were conceptualized as accounting for the causal RFR effects of these factors through some combination of two ‘causal pathways’ the authors identified as the ‘severity’ or the ‘resilience’ pathway.

Model 1 (‘Joint’) estimated the RFR of drivers due to driver ASB and a mixed group of variables through the severity and resilience pathways.

Model 2 (‘Resilience’) estimated the RFR of passengers due to all variables in Model 1 plus FS passenger ASB, crash severity and driver history through the resilience pathway.

Model 3 (‘Severity’) estimated the RFR of passengers due to all variables in Model 1 plus FS passenger ASB, crash severity and driver history through the resilience pathway.

Model 4 (‘Paired’) estimated the RFR of FS occupants due to FS occupant seating position and FS occupant ASB through the paired pathway.

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RFR estimates for confounders, that is for variables other than ASB, were not assigned causal interpretation.

(1) Why is it important to know the causal effect of a potential risk factor? Aside from pure scientific curiosity, we want to know that an RFR estimate measures not only a statistical association, but also a causal effect, because knowing that a factor is causal can prompt various stakeholders in highway safety (e.g., manufacturers, engineers, policy makers, physicians and others) to try to devise effective risk-reduction measures. For example, even though occupant gender cannot be changed, it might be possible to develop and adopt gender-specific safety technologies or procedures.

(2) Is the proposed ‘pathway decomposition’ a reasonable one? Given the extreme complexity of a motor vehicle crash, this reader is sceptical that police-reported crash data can lead to a valid decomposition of the effects of ASB on RFR into causal pathways. Haddon introduced a widely adopted grouping of crash risk factors into the nine cells of a $3 \times 3$ matrix, he defined by crossing a 3-level time-factor component (pre-crash, crash and post-crash) with a 3-level crash-factor component (human, vehicle and environment). Here are two examples of factors that could affect RFR that were left uncontrolled because of data limitations. (i) None of the models estimated by Lardelli-Claret et al. explicitly accounted for any post-crash factor. For instance, there were no variables measuring the timeliness and competence of emergency medical services although quickly delivered effective emergency help is critical to survival after a crash. (ii) Crash severity refers to both (a) characteristics of the physical contact between a vehicle and other vehicles or some object in the environment; and (b) characteristics of a ‘second collision’ between an occupant and some component(s) of the vehicle’s interior (e.g. seat belt, air bag, side door, steering wheel, etc.). Lardelli-Claret used variables that might be able to distinguish the severity pathway from the resilience pathway. However, although they could possibly describe some aspects of pre-crash driving behaviour and point to the overall level of crash severity, none of the covariates bore any relevance to the second collision ultimately responsible for occupant injuries. Thus, at least some of the factors causing between-crash outcome differences were left uncontrolled.

In addition to insurmountable problems with omitted variables, there are also methodological problems that might make a careful reader pause before accepting the model-based estimates as causal. Here are four: (i) the models were not tested for collinearity among the variables. This is an important omission because collinearity between an outcome on the one hand and its putative causal factors on the other could have resulted in biased RFR estimates. (ii) No justification was offered for not including any of the covariate interactions. (iii) A very large number of covariates with no significant effect and wide confidence intervals was included in the models. Such poorly identified variables can be a major source of collinearity. (iv) Model fit was not tested. In other words, we do not know how well model-based probability estimates corresponded to observed outcomes.

(3) Are the estimates obtained by Lardelli-Claret et al., and their interpretations, reasonably valid? Here are two reasons for why they might not be: (i) about 30% of all police records for occupant-pair data were discarded because one or more key data elements (e.g. outcome and ASB) were missing. No evidence was provided that missing data was random. Not correcting estimates for missing data can lead to substantial biases. The fact that the authors imputed missing values for non-key covariates cannot obviate the concern for potential missing value biases. (ii) According to estimates included in an Appendix, a positive driving under the influence (DUI) significantly reduced driver RFR in the joint model, significantly increased occupant RFR in the resilience model, and had no effect on occupant RFR in the severity model (Table 1). Given the well-known effect of even a low driver blood alcohol concentration (BAC) on RFRs, it is difficult to interpret these estimates. My best guess is that the apparent driver fatality risk-reduction due to driver DUI in the joint model was caused by some combination of collinearity, omitted interactions or excluded observations.

(4) What other methods are available for estimating causal effects from observational data? Before considering alternative methods, it would be useful to sharpen the research questions. For instance, ‘All other things being equal, what is the effect of gender on RFR?’ Here, the emphasis is on the term all, including ASB, and severity. Simply put, what would be the effect of replacing occupants by their opposite-sex twins? More formally, one needs to allow for interaction effects of ASB, as recent evidence showed that those characteristics have many large interactions, at least in the USA.

One option for estimating RFR is to combine methods of propensity analysis with the method of double-pair comparisons. For example, using all available information, one would estimate the probability that a driver of a specified age and gender is belted, stratify the data set on belt-use probability.

### Table 1 Effects of DUI, drugs and drowsiness on RFRs in three models

<table>
<thead>
<tr>
<th>Model</th>
<th>DUI (pos. test)</th>
<th>Drugs</th>
<th>Drowsiness</th>
</tr>
</thead>
<tbody>
<tr>
<td>1 (Joint)</td>
<td>0.45*</td>
<td>1.77</td>
<td>1.25</td>
</tr>
<tr>
<td>2 (Resilience)</td>
<td>1.35*</td>
<td>1.76</td>
<td>1.32*</td>
</tr>
<tr>
<td>3 (Severity)</td>
<td>1.06</td>
<td>2.25*</td>
<td>1.51*</td>
</tr>
</tbody>
</table>

*p < 0.01.

pos. = positive.
apply the double-pair comparison method to estimate belt-use effects within belt-use probability strata and aggregate estimates across the strata.

Of course, no method, no matter how carefully constructed, can produce unbiased estimates if critical variables affecting belt use that also affect outcome are not available for analysis. All in all, however, the paper by Lardelli-Claret et al. tackled a very difficult and important problem by taking a commendably innovative approach.

Conflict of interest: None declared.

References


