Matched Cohort Methods for Injury Research

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INTRODUCTION

This article reviews the design and analysis of matched cohort studies of injuries where exposed study subjects are matched to others not exposed. We focus on the situation in which data are available for the matched groups with at least one member who had the study outcome, but data are absent or incomplete for matched groups that have no members with the outcome.

When matching is done in a case-control study, those with the outcome are matched to those without the outcome on certain confounder measures; this distorts the exposure status of the controls to be like that of the cases in regard to the matching variables (and perhaps other variables as well) (1). As a consequence, the selected controls may not represent the exposure experience of the entire population from which the cases were derived. Therefore, matching is a source of selection bias in a case-control study. The bias it produces can be removed in the analysis by accounting for the matching since, conditional on the values of the matching variables, controls will be representative of the source population.

The consequence of one-to-one matching in a cohort study is different. A variable can be a confounder only if, in the study cohort, it is associated with but not affected by the exposure and is independently predictive of the outcome. If each exposed study subject is perfectly matched to an unexposed subject on the value of some variable, and if there is no subject loss or missing data, there will be no association of exposure with the matching variable in the data, and confounding by the matching variable will be eliminated (2). Confounding by the matching variable could still occur, however, if an imbalance arose between the exposed and unexposed study subjects; this might happen, for example, if follow-up were less complete for one group compared with the other, or if some records were omitted from the analysis because of missing data.

Despite the potential of matching to prevent confounding in a cohort study and the potential of matching to sometimes increase study efficiency (2), it appears that this design is rarely used. For many cohort studies, matching exposed persons to one or several unexposed persons would be laborious. Furthermore, it might be wasteful in that matches might be unavailable for some potential cohort members. Today, cohort studies usually avoid matching, and the data are analyzed using regression methods that make it relatively easy to adjust for potential confounding factors that might otherwise be used for matching.

USEFUL FEATURES OF MATCHED COHORT STUDIES

Matched methods (in cohort studies or clinical trials) are attractive if the matching can be done within a person. To study whether a padded hip protector can decrease the risk of a hip fracture, we could randomize the two hips of each study subject; one hip would be allocated to wear a hip protector and the other hip would serve as a control. If a study subject fractured one hip, the other hip would be considered to be censored at that time. This design would avoid imbalance due to dropouts or missing data. It would control well for both the propensity of a person to fall and the likelihood of fracture if a fall occurred. This design would be more efficient than the cluster randomized designs used in some clinical trials of hip protectors (3, 4). It might also be useful for studies that attempt to evaluate the relative benefit offered by two types of hip protector or by two types of wrist protector for in-line skaters. The design assumes, however, that the devices being studied do not influence the side to which a study subject might fall. Factors that are constant within a person, such as age, sex, or whether the person is right or left handed, could be examined as modifiers of any effect of the studied device.

A second feature of matched cohort studies makes this design especially attractive for the study of certain injuries:
Risk ratios, relative rates, or hazard ratios may be estimated using information from only those matched sets with at least one study subject who had the outcome. For example, Walker et al. (5–7) studied the association between vasectomy and subsequent myocardial infarction. Using computerized data from a health maintenance organization, they matched men who had a vasectomy with other men on year of birth and length of follow-up in the health plan. Of 4,830 matched pairs, 36 pairs had one man with a myocardial infarction. The authors analyzed these data using a Cox proportional hazards model stratified on the pairs (8). Because this technique uses information from only the matched pairs with at least one outcome event, chart review for potential confounding variables was limited to the records of 36 men who had a heart attack and their 36 matched cohort members; chart review for the other 9,588 men was not necessary.

Matched cohort methods have been applied to studies of traffic crashes because data on all vehicles with a death are sometimes available. In the United States, the National Highway Traffic Safety Administration maintains the Fatality Analysis Reporting System; these data, collected since 1975, contain information about all crashes on public roads in which someone died within 30 days (9, 10). Because over 99 percent of all crash-related deaths of pedestrians or vehicle occupants occur within 30 days of the crash and (for legal reasons) vital status is diligently ascertained, follow-up regarding death is nearly complete. The available data contain over 100 variables with information about the crash and all involved vehicles and individuals in the crash event. There were 41,611 deaths and 56,668 vehicles in the 1999 data, compared with the estimated 11,232,000 vehicles involved in all crashes in that year in the United States (9).

Occupants of the same car or riders on the same motorcycle can be thought of as matched in regard to crash- and vehicle-related variables, such as place, time, type of crash (e.g., hit by another vehicle, struck a tree), nearest hospital, ambulance response time, vehicle speed, vehicle model, and so on. Some variables, such as speed, may be difficult to measure or estimate; speed is missing in about half of crash records for a vehicle with a death, during years 1975 through 1998. Yet these variables are still matched for occupants of a vehicle. Other variables, such as vehicle make and model, may have so many values that categorizing them and controlling for them in an analysis may be difficult. By matching on vehicle, we can estimate the risk ratio associated with an exposure that does vary within a vehicle, such as seat belt use or occupant age, and the potential confounding influence of vehicle and crash characteristics that do not vary within a vehicle can be eliminated, even if some of these characteristics cannot be measured.

ANALYTICAL METHODS

The crude risk ratio

Imagine a hypothetical matched cohort study of 10,000 cars that crashed. Let us assume that each car had two persons in the front seat, and we are interested in estimating the risk ratio for death in the driver seat position compared with that in the passenger seat. Further, assume that these were severe crashes and that the average risk (incidence proportion) of death for passengers was 0.10, while that for drivers was 0.08; this would result, on average, in the data in table 1, which produce the average risk for drivers as \((A + B)/(A + B + C + D) = 800/10,000 = 0.08\), the average risk for passengers as \((A + C)/(A + B + C + D) = 1,000/10,000 = 0.10\), and the risk ratio of death for drivers compared with passengers as \([((A + B)/(A + B + C + D))/((A + C)/(A + B + C + D))] = (A + B)/(A + C) = 800/1000 = 0.80\). This estimate is just the count of dead drivers divided by the count of dead passengers; nothing special is needed to account for the matching. Information from cells \(A, B, \) and \(C\) in table 1 enters into this calculation; these are the vehicles, or matched pairs, in which one or both occupants died. However, information from cell \(D\), in which both persons lived, is unnecessary, because the count of exposed persons (drivers) and the count of unexposed persons (passengers) are equal by virtue of the matched design. With information from only vehicles with a death, 1,720 vehicles in this example, we can estimate the crude risk ratio that would have been obtained if we had information on all 10,000 vehicles. Although we call this the crude risk ratio, the matched design ensures that it is not confounded by vehicle or crash characteristics. Whether or not we use all 10,000 vehicles, our information about the risk ratio comes from the accidents where at least one occupant died.

If all crashes had identical characteristics, the crude risk ratio estimate would apply to the individual crashes. In reality, important characteristics such as severity will vary. If the risk ratio varies with severity or other characteristics related to risk, the crude risk ratio (being only a ratio of average risks) may not apply to any individual crash (11), and the data may not provide enough information to detect this variation. This drawback would arise even if we had data regarding all crashes, not just those in which one or more occupants died.

Advantages. The crude risk ratio is simple to calculate and controls for confounding by the matching variables, provided there is no loss to follow-up or missing data.

Disadvantages. The crude risk ratio fails to control for confounding by variables not used in matching, and it averages over crash characteristics, which may be misleading if the risk ratio varies with those characteristics.
Mantel-Haenszel method

Mantel-Haenszel methods for stratified summaries of odds ratios have been extended to summarize risk ratios using matched-pair cohort data (1, 12–16). The estimator of the risk ratio from the matched pairs reduces to the crude risk ratio described above. Using the notation from table 1, this estimator is \((A + B)/(A + C)\). The natural log of this estimator has a variance estimator of \((B + C)/(A + B)(A + C)\) (1, 17). This estimator requires no information from the pairs in which no one had the outcome, and in simulated data it provides the desired confidence interval coverage (18). This method has been applied, chiefly for pedagogic purposes, to traffic crash data (1, 18, 19).

Advantages. The log risk ratio and its variance are simple to calculate, as only three counts are needed. We are not aware of commercial software that implements this method, but a program that executes the method in Stata statistical software is available at the following website: http://depts.washington.edu/hiprc/prevmat.htm (20).

Disadvantages. This method has only limited ability to control for potential confounders or to examine risk-ratio variation. If we wished to estimate the risk ratio of death for occupants wearing a seat belt compared with those not wearing a seat belt, we might be concerned that occupant sex could confound this association. We could limit our comparison to those pairs composed of only men and only women, but this would waste potentially informative data by discarding pairs consisting of one male and one female. Furthermore, front seat occupants necessarily differ with regard to seat position. If the risk of death associated with being in the driver seat were different from that in the passenger seat, this might confound the association between seat belts and death; using the Mantel-Haenszel method, it is impossible to control for sources of confounding, such as seat position, that are always discordant within a matched pair. Finally, this method cannot examine the possibility that risk ratios differ by seat position.

In our experience, there has been so little confounding by seat position, age, and sex that Mantel-Haenszel estimates of risk ratios associated with seat belt use and motorcycle helmet use are fairly close to fully adjusted estimates (18, 19). However, because analytical methods that can account for confounding and effect modification due to characteristics that vary within a matched pair are available, we see little reason to use the Mantel-Haenszel method.

Double-pair method

In 1986, Evans (21) published a statistical procedure that he called the double-pair method. The method can be thought of as an adaptation of the Mantel-Haenszel method. To estimate seat belt effects, for example, we first select those vehicles in which both driver and passenger were not belted and one or both died (table 2); we use these records to compute the Mantel-Haenszel estimate of the risk ratio of death associated with seat position only. Next, we select vehicles with a belted driver and an unbelted passenger, at least one of whom died; we use these records to compute the Mantel-Haenszel estimate of the risk ratio of death associated with both belt use and seat position. To estimate the risk ratio associated with seat belts only, we divide the second of these risk ratios by the first. It is this division of one risk ratio by another that distinguishes this method from the Mantel-Haenszel method.

To use all the data, belted and unbelted drivers are also compared with belted passengers; the risk ratio estimate for belts only from this comparison is then summarized with the estimate from the comparison with unbelted passengers, using weights based upon the inverse of the variance for each estimate. Similar calculations are then made to estimate seat belt effects for passengers.

Evans proposed a variance estimator for the log of the ratio of two risk ratios. His formula and that in another recent paper (22) assume that the marginal counts \((A + B)\) and \((A + C)\) in table 2) are independent, which cannot be correct because these counts share the pairs with two deaths (\(A\) in table 2). We suggest two alternatives: one derived from the variance estimator for the Mantel-Haenszel method (1, 17) and one derived from an estimator based on the delta method (18). For the latter, one uses an asymptotic variance for the log of the ratio of two risk ratios, 

\[
\log[(A + B)/(A + C)]/[\{E + F\}(E + G)]
\]

given by

\[
\frac{\{\{[A \times (A + B + C) + (B \times C)] \times (F + G)] + [E \times (A + F + G) + (F \times G)] \times (B + C)\}}{[\{(A + B) \times (A + C) \times (E + F) \times (E + G)\]}
\]

For the data in table 2, the risk ratio estimate associated with seat belt use was 0.36 (95 percent confidence interval: 0.34, 0.39). In simulated data, these two suggested estimators produced virtually identical results, and both had desirable coverage properties (18).

The double-pair method has been applied several times to traffic crash data (18, 19, 23–38).

Advantages. Risk ratios and variances are simple to calculate with this method, and separate estimates for driver and passenger seat positions are produced. A program that implements the method in Stata statistical software is available at http://depts.washington.edu/hiprc (20). However, a variance estimator for the log of the average risk ratio for all

![Table 2](image-url)

<table>
<thead>
<tr>
<th></th>
<th>Died</th>
<th>Lived</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>Unbelted driver</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Died</td>
<td>5,596 (A)</td>
<td>11,721 (B)</td>
<td>17,317</td>
</tr>
<tr>
<td>Lived</td>
<td>11,825 (C)</td>
<td>11,524,183 (D)</td>
<td>11,536,008</td>
</tr>
<tr>
<td>Total</td>
<td>17,421</td>
<td>11,535,904</td>
<td>11,553,325</td>
</tr>
<tr>
<td>Belted driver</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Died</td>
<td>442 (E)</td>
<td>662 (F)</td>
<td>1,104</td>
</tr>
<tr>
<td>Lived</td>
<td>2,632 (G)</td>
<td>1,477,399 (H)</td>
<td>1,480,031</td>
</tr>
<tr>
<td>Total</td>
<td>3,074</td>
<td>1,478,061</td>
<td>1,481,135</td>
</tr>
</tbody>
</table>

* Cell counts are for pairs, not individuals. Cell counts \(A, B, C, E, F, G, H\) and \(D\) are from an actual study (61), while counts for \(D\) and \(H\) are estimates.
front seat occupants (drivers and passengers) has not been published; we have used bootstrap methods (18).

**Disadvantages.** The double-pair method has limited ability to control for potential confounding by occupant variables such as age or sex. The method cannot produce estimates unless there are some pairs concordant on the exposure; that is, both are belted or not belted. Furthermore, bias may arise if a variable related to the vehicle (such as make or weight) or the crash (such as speed or rollover) modifies the association of a within-vehicle varying confounder, such as seat position, with the risk of death (18). Despite these problems, estimates of seat belt effects and motorcycle helmet effects from this method are fairly close to more fully adjusted estimates (18, 19, 39). Given the limitations of the double-pair method, we prefer the methods described below.

**Estimating equations**

Davis (40) and Liang (41) derived regression generalizations of the Mantel-Haenszel matched pair odds ratio based on the theory of estimating equations. Greenland (39) extended these methods to modeling the paired Mantel-Haenszel risk ratio and illustrated the approach in an analysis of the association of motorcycle helmet use with death. This approach has been extended to accommodate more than two persons in each matched set; one study estimated the risk ratio of death for persons occupying a truck bed compared with those in the truck cab (42).

**Advantages.** This method can accommodate both confounding and effect modification (interaction terms). It can be programmed in software that can iteratively solve an equation. A program written for GAUSS software (43) is available at http://darwin.epib.cwru.edu/~witte/gauss.

**Disadvantages.** Because the method is not implemented by specific commands in a commercial software package, it may not be convenient for some users. The results appear to be nearly identical to those from other risk ratio methods that may be more convenient and are described next.

**Conditional Poisson regression**

Poisson regression can be used to estimate risk ratios (44–46). If we condition additionally on the number of deaths in each matched set, the Poisson likelihood becomes a product multinomial likelihood, and we can estimate risk ratios for occupants within the same vehicle; for a single binary covariate, this becomes a product binomial likelihood. In deference to the terminology used by others who have published this conditional likelihood expression, we call this condi-

<table>
<thead>
<tr>
<th>TABLE 3. Hypothetical data for a matched-pair analysis of an exposure that reduces the risk of death by 50%*</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Exposed Died</td>
<td>Not exposed Died &amp; Lived</td>
<td>%†</td>
</tr>
<tr>
<td>Died 0 (A)</td>
<td>5,000 (B)</td>
<td>0</td>
</tr>
<tr>
<td>Lived 10,000 (C)</td>
<td>985,000 (D)</td>
<td></td>
</tr>
<tr>
<td>Died 750 (A)</td>
<td>4,250 (B)</td>
<td>5</td>
</tr>
<tr>
<td>Lived 9,250 (C)</td>
<td>987,500 (D)</td>
<td></td>
</tr>
<tr>
<td>Died 1,500 (A)</td>
<td>3,500 (B)</td>
<td>10</td>
</tr>
<tr>
<td>Lived 8,500 (C)</td>
<td>986,500 (D)</td>
<td></td>
</tr>
<tr>
<td>Died 2,250 (A)</td>
<td>2,750 (B)</td>
<td>15</td>
</tr>
<tr>
<td>Lived 7,750 (C)</td>
<td>987,250 (D)</td>
<td></td>
</tr>
<tr>
<td>Died 3,000 (A)</td>
<td>2,000 (B)</td>
<td>20</td>
</tr>
<tr>
<td>Lived 7,000 (C)</td>
<td>988,000 (D)</td>
<td></td>
</tr>
<tr>
<td>Died 3,750 (A)</td>
<td>1,250 (B)</td>
<td>25</td>
</tr>
<tr>
<td>Lived 6,250 (C)</td>
<td>988,750 (D)</td>
<td></td>
</tr>
<tr>
<td>Died 4,500 (A)</td>
<td>500 (B)</td>
<td>30</td>
</tr>
<tr>
<td>Lived 5,500 (C)</td>
<td>989,500 (D)</td>
<td></td>
</tr>
</tbody>
</table>

* There is no confounding. There were $1,000,000$ crashes involving pairs, the average risk of mortality was 1% among the unexposed and 0.5% among the exposed, and one member of each pair was exposed. Cell counts are for matched pairs.

† Percentage of vehicles with at least one person dead that is in cell $A$: $\% = 100 \times A / (A + B + C)$. 

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tional Poisson regression (46–53). (This is not the same likelihood, however, as a “conditional Poisson” likelihood published in one recent text (54).)

Poisson regression is often used to estimate incidence rate ratios, rather than ratios of mean event counts, by incorporating data on the person-time at risk into the model as an offset; that is, the log of person-time is entered with its coefficient fixed at 1 (55). In the case of pairs in traffic crashes, the potential follow-up time is identical within pairs (30 days), so risk ratios over the 30 days after a crash can be estimated by setting person-time to 1 month for all subjects, or by ignoring person-time altogether. This regression method has been applied to case-crossover studies of possible adverse outcomes after vaccination (56, 57) and a study of racial discrimination in housing rentals (58), and it has been used in a few studies of traffic crashes (19, 59–61).

Advantages. As with most regression methods, it is easy to accommodate confounding and effect modification in a flexible manner. The method can be extended to include more than two persons in each matched set. This method has been implemented in commercial software, for example, in the Stata procedure, xtpois (20).

Disadvantages. In a simulation study, the confidence intervals provided by this method were too wide; 95 percent confidence intervals covered 98 percent of the simulated results (18). Because the number of pairs that are typically examined in this kind of analysis is in the thousands, this may not be a serious drawback; confidence intervals tend to be quite narrow.

Cox proportional hazards regression

Proportional hazards models can be stratified on vehicle, generating estimates based on within-vehicle comparisons; if the time to death or censoring is set to some arbitrary constant value and if the Breslow or Efron methods are used to account for tied survival times (8, 62, 63), the results will be the same as those from conditional Poisson regression, as the likelihoods for these methods are identical when the data come only from matched pairs. This method has been applied for the purposes of illustration in one paper about traffic crashes (18). (If the exact marginal (64) or exact partial (65) likelihoods are used to account for tied survival times, the results will be the odds ratios; the likelihoods for these methods are the same as those for conditional logistic regression, which we discuss later.)

One could use these models to estimate hazard ratios, rather than risk ratios, by incorporating data on the actual survival time; time to death is included in US data for fatal crashes, although it is often missing. For a chronic disease outcome, such as time to death after a diagnosis of cancer, it makes sense to use actual survival time, which is often many years. Death is inevitable, but we are interested in postponing death. However, if death occurs after an injury, it is often nearly instantaneous and any delay in death is usually measured in a few days. Postponing death a few days may not be desirable if the additional survival time is spent in an intensive care unit on a ventilator. Therefore, ignoring time to death and estimating the risk ratio for death within a 30-day interval may be a good choice for studies of traffic crashes.

In some studies, however, we might wish to account for time to an injury event. Imagine that we wished to study how the hazard of a fall varied with shoe characteristics. Each study subject who wore shoes with a leather sole might be paired with someone who wore shoes with a rubber sole. Pairs could be matched on sex, year of age, home floor surface (carpeted or bare), and number of falls in the past 6 months. Cohort members would be followed until they fell or until they were censored by the end of the study or by dropping out of the study; a hazard ratio could be estimated using time to fall or censoring, stratified on the pairs.

Advantages. This method is available in many statistical software packages and is preferable when time to an event, rather than just whether an event occurred, is of interest.

Disadvantages. As with the conditional Poisson method, which provides the same estimates for matched-pair data, the risk ratio confidence intervals from this method appear to be too wide in simulations (18).

Conditional logistic regression

Conditional logistic regression is often applied to case-control studies in which cases and controls are matched on variables that may be confounders (1, 66, 67). This method has been used to analyze data for a few studies of traffic crashes in which persons were either matched within the same vehicle or were in different vehicles that were in the same crash (68–71). The odds ratio from conditional logistic regression will closely approximate the risk ratio if, in the study population from which the matched pairs arose, the proportion of persons who experienced the outcome was small (1, 72, 73) and, in addition, the baseline risk varies little across subgroups (11). Death is indeed rare in crashes: In 1999, there were an estimated 11,232,000 drivers in US crashes and 25,210 (0.2 percent) drivers died (9). However, crashes are not homogeneous in regard to the risk of death; many deaths occur in severe crashes where the risk of death is great. As a consequence, it is common that two persons die in the same vehicle and, in this situation, the odds ratio will be further from 1.0 than the risk ratio (11, 17–19, 39).

Conditional logistic regression uses only the “discordant-pair” information in cells B and C of table 1, in contrast to the other methods we have reviewed, which also use the information in cell A, the pairs with two dead occupants. Because the proportion of all pairs in cells A, B, and C that fall within cell A typically ranges from 15 to 20 percent in actual crash studies (19, 61, 71), the odds ratio may poorly approximate the risk ratio and hence more poorly estimate the effects of factors on caseload (39).

Suppose that we wished to study an exposure that actually reduced mortality by an average of 50 percent in all crashes. One million vehicles crashed, and in each vehicle one occupant was exposed and the other was not. The average mortality was 1 percent among the unexposed and 0.05 percent among the exposed. Only the vehicles with at least one dead person are needed to produce odds ratios or risk ratios; table 3 illustrates that, as the proportion of pairs that are both dead increases, the odds ratio diverges from the risk.
ratio. In a matched cohort study of crash data, the risk ratio of death for those who wore a seat belt compared with those unbelted was estimated as 0.39 (95 percent confidence interval: 0.37, 0.41) using conditional Poisson regression; the odds ratio of death was 0.28 using conditional logistic regression (18, 61).

**Advantages.** There are none compared with other regression methods.

**Disadvantages.** Conditional logistic regression estimates the odds ratio, which is often further from 1.0 than the risk ratio and which more poorly represents the impact on case-load (39). The pairs in which both persons died are ignored.

**A POTENTIAL LIMITATION**

The matched cohort methods that we have described can produce estimates of risk ratios within all occupant pairs that crash. However, these estimates may not apply to the average solo driver who crashes, if those drivers differ from paired drivers with respect to characteristics that modify the risk ratio associated with exposure. For example, one study estimated that the risk ratio for using seat belts in rollover crashes was 0.23, while the estimate was 0.47 in other crashes (18). This difference may be attributed to the ability of a belt to prevent ejection in a crash, a common feature of fatal rollover injuries. If rollovers were more common for solo drivers, compared with drivers who crashed with a passenger, the average risk ratio associated with using a seat belt for a solo driver would be somewhat greater than that for drivers in a matched-pair study. This difficulty can be addressed if the matched-pair analysis produces separate risk ratio estimates for those features of solo drivers or their crashes that differ substantially from those of matched drivers.

**A POSSIBLE STRENGTH**

Misclassified data can distort any measure of association. Limiting a crash analysis to matched pairs with a death may use data that are less subject to misclassification than data for all crashes. Fatal crashes are investigated with more effort than “fender-benders.” Many states have specially trained police officers who collect information regarding a crash that involves a death. Data in the Fatality Analysis Reporting System are considerably more complete than data in the Generalized Estimates System, a systematic sample of police reports for all crashes in the United States (74, 75).

There is evidence that police data about seat belt use on routine crash forms may often be inaccurate (76). Some researchers have expressed concern that, if some crash survivors told police that they were belted when they were not, any benefit of seat belts would be exaggerated (37). Some survivors might be motivated to claim falsely that they were belted in order to avoid a fine for not using their seat belt. However, a recent matched-pair analysis of crashes in which someone died reported excellent agreement between police reports of belt use in fatal crashes and ascertainment of belt use by trained investigators for the National Accident Sampling System Crashworthiness Data System (60, 77). The trained investigators made their determination of seat belt use from actual belt inspection for over 90 percent of the occupants; risk ratio estimates for belt use were essentially the same when data based on inspection were used and when data based upon police reports were used.

**CONCLUSIONS**

Conceptualizing data regarding fatal crashes as a matched cohort study offers several advantages. First, risk ratio estimates for all crashes can be produced by using data from only those occupant groups in which one or more died. These data have been available in the United States since 1975. The expense of collecting these data is far less than the expense of collecting data for all crashes.

Second, creating estimates from occupants of the same vehicle provides protection against confounding by vehicle- and crash-related factors. Even if data were available for all crashes, the difficulties of controlling for differences in crash forces between vehicles are considerable; a within-vehicle analysis may be preferable.

Third, an analysis of vehicles with a death may use data that are more complete and more accurate than routine data for all crashes, thereby improving the accuracy of effect estimates.

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