

**Review of “Cell Phone Use and Crash
Risk: Evidence for Positive Bias”
by Richard A. Young**

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ESTIMATES OF CRASH RISK ASSOCIATED WITH CELLPHONE USE

There currently is a lot of debate about the risks associated with various distractions while driving. At the center of this debate is the crash risk associated with cellphone use. Studying this is difficult because police crash reports do not reliably indicate whether or not drivers were on the phone at the time of the crash. Thus, other means are required to establish whether or not crash-involved drivers were using phones, and suitable controls are needed.

Two important epidemiological studies verified crash-involved drivers' cellphone use from cellphone company billing records. In a study conducted in Western Australia, McEvoy et al.¹ examined cellphone use in crashes serious enough to injure the drivers. Cellphone use in this study was defined as incoming calls, outgoing calls, and outgoing text messages. The Insurance Institute for Highway Safety funded and co-authored the McEvoy et al. study. In Toronto, Canada, Redelmeier and Tibshirani² examined cellphone use in crashes with substantial property damage but that did not involve injury. In this study, cellphone use consisted of incoming and outgoing calls. Both studies used case-crossover designs to compare drivers' cellphone use during a time interval leading up to a crash (hazard window) with their own cellphone use during a comparable time interval at some point before the crash (control window). Although these studies were conducted in different countries and nearly 10 years apart, both found that cellphone use while driving was associated with a four-fold increase in crash risk (RR = 4.1, 95% CI = 2.2-7.7;¹ RR = 4.3, 95% CI = 3.0-6.5²). Both also found no significant differences in the elevated crash risk associated with handheld and hands-free phones.

Two recent "naturalistic driving" studies continuously monitored drivers in vehicles instrumented with video cameras, accelerometers, and other technology.^{3,4} These studies employed case-control designs and compared the prevalence of different distracting behaviors

during a 6-second time interval surrounding a “safety-critical event” to their frequency in a random 6-second time interval during normal driving. Unlike the epidemiological studies described above, phone use in the naturalistic driving studies was based on observed use in video recordings rather than on cellphone company billing records. Furthermore, the data used to estimate risk in the naturalistic driving studies were not limited to crashes.

Klauer et al.³ found that hand-held phone conversations were associated with a non-significant increase in the likelihood of an at-fault crash or near-crash among 109 drivers of passenger vehicles ($OR = 1.29$, 95% CI = 0.93-1.80). Dialing using a hand-held phone was associated with more than a two-fold increase in at-fault crash and near-crash involvement ($OR = 2.79$, 95% CI = 1.60-4.87). At-fault crashes were rare (49 crashes), and most were not serious; the majority of the dataset was comprised of at-fault near-crashes (439 near-crashes). Using naturalistic driving data from 203 drivers of large trucks, Olson et al.⁴ found that conversations on handheld phones did not significantly increase the risk of being involved in a safety-critical event ($OR = 1.04$, 95% CI = 0.89-1.22). Dialing using a hand-held phone, however, was associated with a significant increase in the likelihood of a safety-critical event ($OR = 5.93$, 95% CI = 4.57-7.69). Safety-critical events were defined as crashes, near-crashes, crash-relevant conflicts, and unintentional lane departures. About two-thirds of all safety-critical events were crash-relevant conflicts, defined as circumstances that required a crash-avoidance maneuver.

Young's⁵ study focused on the discrepant estimates of crash risk associated with cellphone use in the epidemiological studies and crash/near-crash risk and risk of safety-critical events associated with cellphone conversation in the naturalistic studies.

SUMMARY OF YOUNG

Young⁵ argued that the discrepancy between the estimates is due to bias in the epidemiological studies. McEvoy et al.¹ and Redelmeier and Tibshirani² both assumed that participants were driving during the entire control window. If participants were driving during only part of the control window, then Young asserts that their exposure to crash risk would be lower in the control window than in the corresponding hazard window, thereby inflating the estimated crash risk associated with phone use.

To estimate this bias, Young⁵ developed a “driving consistency index” based on the average percentage of overlap in minute-to-minute driving on two consecutive days. Driving consistency was calculated using 100 days of GPS data from 439 household vehicles included in a Traffic Choices study sponsored by the Federal Highway Administration.⁶ Neither the characteristics of the drivers nor the sampling methods were described. Driving consistency was calculated for all 439 vehicles across 100 consecutive day pairs, and the average driving consistency index was 26.4%. In other words, driving time on consecutive days overlapped a little more than a quarter of the time, on average.

Young⁵ applies the same calculation used by Redelmeier and Tibshirani² to correct for inconsistent driving between the hazard and control windows in the epidemiological studies (see below). He multiplied the relative risk ratios by the driving consistency index (26.4%). This correction is based on the assumption that the risk factor (cellphone use) is not present without exposure (driving). In other words, the assumption is that reducing the amount of driving during the control window also reduces the probability of cellphone use during the control window.

After applying the driving consistency estimate to the two epidemiological studies, Young⁵ found no significant increase in crash risk associated with cellphone use while driving in

either study (adjusted RR = 1.1, 95% CI = 0.55-2.1;¹ adjusted RR = 1.1, 95% CI = 0.75-1.8²).

Young concluded that the “part-time driving hypothesis likely accounts for much of the discrepancy between recent and earlier [relative risk] estimates for conversation while driving” (p. 3).

CRITIQUE OF YOUNG

McEvoy et al. and Redelmeier and Tibshirani Accounted for Driving Inconsistency and Tested the Robustness of Observed Effects

Young's⁵ criticism raises a potentially valid concern about the two epidemiological studies. If drivers were not driving during the entire control window, then the crash risk estimates reported in these studies would be biased. McEvoy et al.¹ and Redelmeier and Tibshirani² took steps to account for this possible bias. Redelmeier and Tibshirani² asked a separate sample of 100 drivers if they drove during a “selected period” of time, and 35% indicated that they had not. To account for partial driving during the control window, Redelmeier and Tibshirani multiplied their crude risk ratio (6.54) by the percentage of drivers who reported driving in the selected period of time (65%), resulting in a relative risk ratio of 4.3 ($6.54 * 0.65 = 4.25$). As discussed above, this procedure was adopted by Young and makes the same assumption about the relationship between driving and cellphone use in the control window. It is worth noting that Young's adjustment for driving inconsistency was 2.5 times greater than Redelmeier and Tibshirani's² adjustment. Redelmeier and Tibshirani² also conducted a secondary analysis that recalculated the risk estimate using subjects who recalled driving during both the control window and the hazard window and found cellphone use was associated with increased crash risk (RR = 7.0, 95% CI = 3.7-15.5).

McEvoy et al.¹ accounted for driving consistency by asking about and then removing participants that did not report driving in the 10-minute control window. They also reduced the size of the hazard and control windows if participants reported driving fewer than 10 minutes before the crash occurred.

Both studies also conducted sensitivity analyses to ensure that their observed effects were robust to other inconsistencies between the control and hazard windows. Redelmeier and Tibshirani recalculated their risk estimate using five different control windows, ranging from 1 day to 1 week before the hazard window. McEvoy et al.¹ varied the length of the hazard and control windows from 10 minutes to as little as 5 minutes before the crash and also varied the latency between the hazard and control windows (24 hours, 72 hours, 1 week) based on self-reported driving. A comparable increase in crash risk was observed in each of these analyses.

Young's Driving Consistency Estimate May Not Be Comparable with Driving Consistency in Epidemiological Studies

Although McEvoy et al.¹ and Redelmeier and Tibshirani² tried to account for driving inconsistency, they ultimately relied on drivers' memories to verify that they drove during the entire control window. Human memory is fallible and can be influenced by recall bias, especially when recalling periods of driving more than a year later as in Redelmeier and Tibshirani's study. Young's⁵ driving consistency index is based on GPS recordings of actual vehicle use and provides an objective measure of day-to-day driving consistency, but it is unclear how this relates to drivers' recollections. Furthermore, this estimate characterizes only the driving consistency of the drivers in his sample and may not generalize to McEvoy et al. and Redelmeier and Tibshirani's study populations. For a number of reasons, the daily driving of his

sample of U.S. drivers may not be comparable with the Australian drivers in McEvoy et al. and the Canadian drivers in Redelmeier and Tibshirani.

First, Young's⁵ estimate likely does not accurately reflect driving consistency during the times of the day and days of the week of the driving studied in McEvoy et al.¹ and Redelmeier and Tibshirani.² His driving consistency estimate is based on the average percentage of overlap in day-to-day driving for an entire 24-hour day, but McEvoy et al. and Redelmeier and Tibshirani recruited participants only during selected hours of the day and on weekdays (McEvoy et al.: Monday-Friday, 8 am-9 pm; Redelmeier and Tibshirani: Monday-Friday, 10 am-6 pm). Young's driving consistency estimate assumes that the probability of driving is uniform across all hours of the day and days of the week, but driving on consecutive days may be more consistent during some time periods than others. For instance, driving may be more consistent during weekday rush hour time periods that are structured around the workday compared with weekday nights when people run occasional errands. McEvoy et al. and Redelmeier and Tibshirani's recruitment periods did not include late night hours, so driving consistency during these time periods may have been higher (or lower) than the overall driving consistency that Young calculated for an entire day.

Second, driving consistency might vary not only as a function of time of day but also by the matched day pairs. Young⁵ estimates driving consistency on two adjacent days. As discussed above, McEvoy et al.¹ and Redelmeier and Tibshirani² varied the latency between the hazard and control windows. Young's driving consistency estimate applies to those cases where the hazard window and control window were separated by 24 hours, but not necessarily those separated by multiple days or one week. Driving time on Monday may be different from driving time on Sunday (less driving consistency), but likely would be similar to driving time on the

Monday 1 week prior (greater driving consistency). Young's estimate does not accurately reflect the driving consistency in all of the matching procedures conducted in the epidemiological studies, and thus cannot account for the significant increases in crash risk observed in each.

Third, several characteristics of Young's⁵ data may confound his estimate. The GPS data from the Traffic Choices study recorded the day-to-day use of a household vehicle and not individual drivers. Some households in the data set may have multiple drivers who shared the same vehicle. Hence, Young's estimate of driving consistency does not reflect the day-to-day driving consistency of a single driver, but the consistency of one or more drivers' use of a household vehicle on consecutive days.

Fourth, Young's⁵ driving consistency estimate also could be confounded by the research aims of the Traffic Choices study. The Traffic Choices study measured changes in travel behavior in response to variable or congestion-based tolling. An on-board "tolling device" was installed in every vehicle and tracked travel behavior and travel expenses. The tolling device could have altered day-to-day driving routines. There was an initial 6-month period of baseline driving before the tolling intervention was implemented (M. Kitchen, personal communication, December 20, 2011), but it is not clear if Young sampled his data from this baseline period.

In summary, there are several important factors that make Young's⁵ estimate of driving consistency of doubtful relevance to the epidemiological studies. Certainly, these issues raise serious doubts about the strict mathematical correction procedure applied by Young.

Young's Adjustment Makes an Unrealistic or At Least Unknowable Assumption about Cellphone Use

The case-crossover design employed by McEvoy et al.¹ and Redelmeier and Tibshirani² assumed that the crash risk attributable to driving exposure was similar during the hazard and

control windows. This assumption allowed them to measure the increase in crash risk associated with cellphone use while driving. Young⁵ is correct that if people did not drive during the entire control window, then the crash risk attributable to driving exposure would be lower in the control window than the hazard window and bias crash risk estimates upward.

However, Young's⁵ adjustment, as noted earlier, assumes cellphone use occurs only during driving. He states that "Nondriving during a control window ... can reduce the probability of a call" and that "Nondriving during a control but not a case window would make it seem (erroneously) that cell phone usage was less during control periods" (p. 1). However, this may not be the case as it is possible for cellphone use to occur during non-driving portions of the control windows.

Cellphones may be used less frequently when people drive because they may engage in self-regulatory behaviors to reduce risk (e.g., Schömig et al.⁷) or may be deterred by legislative bans (e.g., McCartt et al.⁸) and high visibility enforcement campaigns (e.g., Cosgrove et al.⁹). If cellphone use is more prevalent when people are not driving than when they are driving, then cellphone use could have been more frequent in control windows that included periods of non-driving. Contrary to Young's⁵ assumption, this would increase the frequency of cellphone use observed in the control windows relative to the hazard windows and bias crash risk estimates downward.

At this time, there is no available research on the frequency of cellphone use when driving relative to use at other times, so it is unreasonable to speculate how this potential bias would influence crash risk estimates.

CONCLUSION

Epidemiological studies and naturalistic studies have arrived at different conclusions about the risk of safety-critical events, near-crashes, and crashes associated with using a cellphone while driving. However, McEvoy et al.¹ and Redelmeier and Tibshirani's² studies are the only research estimating crash risk using an adequate sample of crashes involving verified cellphone use. This is not to say that these two studies provided an unbiased estimate of crash risk. McEvoy et al. and Redelmeier and Tibshirani's describe several sources of potential biases in their studies. Farmer et al.¹⁰ discuss several assumptions in case-crossover designs that, if violated, could yield biased estimates of risk. Young⁵ identifies an additional potential source of bias. However, it is unlikely that Young's⁵ estimate of driving consistency applies to the studies of McEvoy et al. and Redelmeier and Tibshirani. Whether it applies or not, Young's adjustment to the estimates of crash risk in McEvoy et al. and Redelmeier and Tibshirani makes an unsubstantiated assumption about the relationship between cellphone use, driving exposure, and crash risk. Consequently, there is no basis for believing his adjusted estimates of the crash risk from cellphone use are accurate.

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