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Q&A FOR RADIO AND NEWSPAPER INTERVIEWS
Cell Phone Use and Crash Risk

Evidence for Positive Bias

Richard A. Young

Background: Recent epidemiologic studies have estimated little or no increased risk of automotive crashes related to cell phone conversations by the driver, whereas earlier case-crossover studies estimated the relative risk as close to 4. Did earlier studies introduce a positive bias in relative risk estimates by overestimating driving exposure in control windows?

Methods: Driving exposures in a “control” window and a corresponding “case” window on the subsequent day were tabulated across 100 days for 439 GPS-instrumented vehicles in the Puget Sound area during 2005–2006.

Results: For control windows containing at least some driving, driving exposure was about one-fourth that of case windows. Adjusting for this imbalance reduces relative risk estimates in the earlier case-crossover studies from 4 to 1.

Conclusion: Earlier case-crossover studies likely overestimated the relative risk for cell phone conversations while driving by implicitly assuming that driving during a control window was full-time when it may have been only part-time.

(Epidemiology 2012;23: 116–118)

Relative risk (RR) estimates in recent epidemiologic studies of call-crash association are near one, whereas those of earlier studies are near 4. Because RR estimates calculated from different study designs should be very close for small risks in the absence of biases, this large discrepancy implies some source of bias. This report examines possible bias arising from a confounding third variable (driving) that could affect both cell phone use and crash frequency and that may have been only partially matched (driving) that could affect both cell phone use and crash frequency and that may have been only partially matched.

These studies used a case-crossover design to compare cell phone use at various time intervals before a crash (the case window) with cell phone use during the same period on a previous day (the control window). The term “use” refers here solely to conversation time, the only phone-related task information in billing records. Crash occurrence ensures that a person was driving during the case window, but the person may not have been driving during all (or a part) of the control window. Nondriving during a control window confounds RR estimates because it eliminates the possibility of a crash and can reduce the probability of a call. Reduced call probability when not driving cannot be rejected based on existing data because epidemiologic studies, to date, have good accuracy for driving times from GPS data or calling times from cellular billing records, but not both in the same study. Nondriving during a control but not a case window would make it seem (erroneously) that cell phone usage was less during control periods, making the denominator smaller and leading to the (false) conclusion that calling while driving has an RR above 1.

Earlier studies attempted to reduce confounding by nondriving in control windows by asking subjects to recall their driving at that time, retaining only subjects or control windows with recalled driving. Redelmeier and Tibshirani interviewed 100 people who were not in the original study, and only 65% recalled driving during a “selected period.” They multiplied their crude RR (and confidence range) of 6.54 (95% confidence interval [CI] = 4.5–9.9) by this 65% “driving consistency” factor, yielding an adjusted RR of 4.3 (3–6.5). Similarly, McEvoy et al found that drivers who crashed driving in only 36% of control windows and analyzed only those control windows to yield an RR estimate of 4.1 (2.2–7.7).

However, neither study controlled for those who may have been driving for only part of the control window. Part-time driving in a control window biases RR estimates upwards, just as complete nondriving does. The present study accounts for the full range of driving in control windows and adjusts the earlier RR estimates accordingly.

METHOD

Day-to-day driving consistency was calculated from 100 days of GPS data collected from 439 vehicles during 2005–2006 in Puget Sound, Washington. The data had been previously deidentified and made publicly available. To ensure that all vehicles were matched on day of week, the
analysis for each vehicle was started on the first Monday for which it had GPS data.

Driving consistency was calculated for each vehicle for a “day-pair.” The number of driving minutes during the day 2 window that overlapped with driving minutes during the corresponding day 1 window was tabulated. (Day 2 is analogous to the “case” or crash day, and day 1 is analogous to the “control” day in the case-crossover studies.6,7) An array was created for each day containing 1440 bins, each representing a 1-minute period from 3:00 AM to 2:59 AM the next day. A bin was assigned a value of “1” if driving occurred during that minute and “0” otherwise. Each bin for day 2 was compared with its corresponding bin for day 1. The number of bins with overlapping 1s, divided by the total driving minutes during day 2, defines the driving consistency for a given vehicle,

\[
\text{driving consistency} = 100 \times \frac{\sum_{i=1}^{1440} \text{day}_1 (i) \times \text{day}_2 (i)}{\sum_{i=1}^{1440} \text{day}_2 (i)}
\]

where \(\text{day}_1 (i)\) and \(\text{day}_2 (i)\) are the bin entries for the first and second days, respectively.

Driving consistency was then tabulated for every vehicle with GPS data in the day-pair. Because Redelmeier and Tibshirani6 eliminated subjects for whom no driving occurred at the same clock time during days 1 and 2 (ie, drivers for whom driving consistency was 0), the current analysis retained only vehicles with driving-consistency values exceeding 0. The mean of this value over all vehicles for a given day-pair is the mean part-time driving consistency. This calculation was repeated for the 100 consecutive day-pairs. The grand mean part-time driving consistency is the average of the mean part-time driving consistency across the 100 day-pairs. It represents the probability that a vehicle is driven during a minute on a “control” day conditional on it having been driven during the corresponding minute on the next day. Its 95% confidence limits were calculated across the 100 day-pairs. Using Redelmeier and Tibshirani’s6 method for removing driving bias with out-of-sample data, the RR estimates and confidence intervals6,7 were adjusted by multiplying with the grand mean part-time driving consistency.

The uncertainty of the consistency estimate expands the adjusted RR uncertainty range because of propagation of uncertainty.11,12 Hence, the confidence limits for the adjusted RR estimate were further adjusted by the percentage of uncertainty in the consistency estimate.

Note that driving consistency generalizes to control windows of any duration at any time of day. Assume that a person was driving for \(k\) minutes on day 2 and a crash occurs at clock time \(t\). The expected minutes that the person would have driven in a \(k\)-minute control window on day 1 (preceding the corresponding clock time \(t\)) would be \(k \times p\). For example, with a mean driving consistency of 15%, and a 10-minute case window with known driving, the expected driving during a 10-minute control window would be 1.5 minutes.

Controls

Control analyses (see eAppendix, http://links.lww.com/EDE/A535) accounted for missing GPS data, the order of the 2 paired days, driving time per day, and weekday versus weekend driving days.

RESULTS

The Figure illustrates the distribution of average driving consistency values observed across the 100 paired days. An average of 81.9 vehicles (36% of the total vehicles in the Figure) had 0% consistency (left-most bar). This finding is consistent with the 35% recall-based estimate reported by
Redelmeier and Tibshirani, and is about half the 64% estimate in the McEvoy et al study.7

Note that all vehicles in the Figure with a mean driving consistency above 0 would have been treated as having 100% driving consistency in the earlier studies.5,7 However, as illustrated in the Figure, almost all these vehicles had only part-time driving (ie, consistency values >0% but <100%). Among vehicles with 70% consistency, the grand mean part-time driving consistency across the 100 day-pairs was 26.4% (25.2%-27.6%), with a ±5% uncertainty range.

Multiplying the final RR estimates by 26.4% to adjust for part-time driving consistency (and expanding the uncertainty range to account for the uncertainty in the 26.4% estimate) yielded a final adjusted RR of 1.1 (0.75–1.8) for Redelmeier and Tibshirani and 1.1 (0.55–2.1) for McEvoy et al.7

DISCUSSION

Objective GPS methods confirm that the subjective recall methods in the earlier studies correctly estimated the percentage of drivers who did not drive at all during a control window, and properly removed those subjects or control windows from their analyses. However, neither study corrected for part-time driving during control windows, which is difficult or impossible using recall-based methods because of insufficient accuracy.

Assuming full-time driving in a control window when only part-time driving occurred inflates the RR estimates, even after discarding subjects with no driving in a control window, because the denominator can still have less cell phone usage than the numerator solely because of lower driving exposure. (The period represented by the numerator has a high probability of driving because it is defined as the time leading up to the crash.)

The current study used objective GPS data to estimate the mean part-time driving consistency in control windows at 26.4%. Using Redelmeier and Tibshirani’s method for out-of-sample data to adjust the RR estimates for part-time driving consistency yielded RR estimates near 1 for the earlier case-crossover studies (with a confidence range from slightly below to slightly above 1), resolving the discrepancy with more recent studies.1-5

However, the current study did not use the same drivers as in the case-crossover analyses and so loses the benefit of controlling for driver differences. Indeed, the current GPS data are from a different sample, different countries, and different points in time relative to the earlier studies.6,7 Although a comparable analysis of Chicago GPS data had similar results,2 studies are not definitive proof that the high RR estimates in the earlier studies are entirely attributable to driving exposure overestimates in control windows.

Despite this limitation, the present study establishes a plausible hypothesis that earlier case-crossover studies overestimated the RR for cell phone conversations while driving because they did not adjust for part-time driving exposure in control windows. This part-time driving hypothesis likely accounts for much of the discrepancy between recent and earlier RR estimates for conversation while driving.

ACKNOWLEDGMENTS

I thank Ken Rothman, Linda Angell, Richard Deering, Rich Hanowski, Barbara Wendling, and especially Joshua T. Cohen for comments on earlier drafts. I thank Sean Seaman for computational assistance with the GPS data, and Steve Tengler for artwork.

REFERENCES

Erratum: Cell phone use and crash risk: Evidence for positive bias

Reference


On page 118, left column, line 8, the sentence should say “with >0% consistency,” not “with 70% consistency.”
SUPPLEMENTAL DIGITAL CONTENT: CONTROL ANALYSES

This document contains online supplemental material for the Brief Report, “Cell Phone Use and Crash Risk: Evidence for Positive Bias” in the Journal Epidemiology.

The supplemental material consists of four control analyses:

1. **Missing GPS Data.** Days in the GPS dataset had “blank” or missing data for some vehicles, and it was not possible to determine whether that meant no driving during that day or whether the GPS unit had failed for some reason for that day. To cover both possibilities, all analyses were carried out with two methods: dropping missing days from the analysis, or counting the missing days as having no driving. This control analysis showed no effect of missing GPS data on the part-time driving amounts.

2. **Reverse Day Order.** The analyses were also done in reverse day order, with day 1 of each pair acting as the test day, and day 2 as the control day. The control analysis showed virtually no effect from reversing the day order.

3. **Driving Time per Day.** The mean driving time per day was calculated across all days and compared with other instrumented vehicle studies. The driving times in this study were comparable with other studies.

4. **Weekday vs. Weekend.** Analyses were averaged across the individual days of the week, to control for the effect for example weekdays vs. weekends because of the different amounts of driving time on those days. Some slight effects were observed on consistency if one of the two paired days was a weekend, but the main results were robust to day of week.

In sum, the results and conclusions in the brief report were robust to all the controls indicated.

1. **Controls for Missing GPS Data**

   Some vehicles had missing GPS data for one or more days, randomly interspersed. The investigators of the GPS data collection study for Puget Sound (personal communication) said they were not able to determine whether the missing data for that day meant: (1) there was no vehicle travel; or (2) the GPS unit or storage procedures had failed for some reason. Therefore, the data were analyzed in two different ways to account for both possibilities.

   For each histogram for each paired day, the main analysis (as presented in the published report) used only data where there was GPS data for both of the paired days.

   This control analysis treated all the missing data as zeros (assumes no travel on that day), and redid all the analyses.
What was noteworthy is that the only thing that was different in the 100 individual paired-day histograms, and therefore in the grand mean histogram (Fig. 1 in the main report), was the size of the left-most histogram bar for 0% consistency. In the grand mean control histogram, the left-most bar became 113.3 (49.4% of the vehicles in the histogram) instead of 81.9 (36% of vehicles in the histogram) when the missing data were treated as 0 rather than dropped from the analysis. This value of about 50% is in between the 35% non-driving in control windows estimate of Redelmeier and Tibshirani, and the 64% estimate in McEvoy et al.

All other histogram bars (from 1-100% driving consistency) were identical, which is logical because whether a day’s travel was 0 or missing could only affect the left-most bar (0% consistency). Therefore, the “grand mean part-time driving consistency” metric in the main report holds regardless of whether missing GPS data is discarded or treated as zeros.

Counting days with missing GPS data as zeros did produce slightly lower mean driving times per day as shown in Section 3 below, but again this could not affect the main metric (the mean part-time driving consistency) because that metric is based only vehicles with at least some driving in a control window.

2. Controls for Day Order

To demonstrate robustness of the metric, the entire analysis was replicated but with reversed day order. That is, Day 1 was now treated as the surrogate for the “crash” day, and Day 2 as a surrogate for the “control” day in the case-crossover studies. The crossproducts in the numerator of Eq. 1 remain the same; the only difference is in the divisor, which is Day 2 in the main study, but Day 1 here. With the reverse day order, the mean part-time driving consistency across all vehicles with 1-100% consistency in the final mean histogram (average and standard deviation across 100 histograms) was 26.3% (95% C.I. 25.0-27.7), a negligible difference from the regular day order in the main study of 26.4% (95% C.I. 25.2%-27.6%).

3. Mean Driving Time per Day

The mean driving time for the main study across all 100 control days for all vehicles with GPS data on both days was 73.4 min (95% CI 72.0 to 74.8). For the 100 test days the mean driving time was 73.1 min (95% CI 71.9 to 74.4), a negligible difference.

These mean driving times (calculated across a hundred days of GPS data) are slightly higher than the two days of GPS data analyzed in a Chicago dataset of 240 vehicles using similar methods in another study, that had mean driving times of 68.1 min for day 1 and 70.8 min for day 2. However, the Chicago study driving time averages included 10 vehicles with known days of zero driving from driver interviews that were conducted in that study.
The mean driving times in the current study are slightly lower than the mean daily driving time of 84.5 minutes estimated in a study of the association between personal calls made using the OnStar hands-free calling system, and crashes severe enough to deploy an airbag.\textsuperscript{4} The average distance estimated from the electronically reported odometer readings of 585,719 OnStar-equipped vehicles from May 2005 to May 2006 was 44.1 miles/day. This was divided by an average speed of 31.3 mph estimated from a nationwide sample of 171 instrumented GM vehicles during naturalistic driving from 1983–2009 to yield the estimated driving time of 84.5 minutes per driver, per day, about 11 minutes longer (16\%) than the GPS recorded driving times of about 73 min found in the present study. The slightly lower driving time per day found with the GPS data of about 73 min per day was well within the limits of the sensitivity analysis of the relative risk vs. driving time as shown in Fig. 3 of that study.\textsuperscript{2} The relative risk in that study would have been slightly lowered to 0.53 compared to the reported point estimate of 0.62 (CI 0.37 to 1.05) for hands-free conversation while driving if the more robust direct GPS estimate of driving time per day in the current study had been used.

The effect of driving time per day is analyzed further in the controls for day of the week in the next section.

4. Controls for Day of Week

There was a slight variation in the average driving time for weekdays vs. weekends. This driving time variation manifested itself in a slight variation in the driving consistency metric for weekdays vs. weekends.

Fig. 2 shows the variation in average driving time per day of the week for control days (solid line) and case days (dotted line) with known GPS data for the case and control days.
FIGURE 2. The horizontal axes are the day of the week for the control day. The vertical axes are the average driving minutes per day across all the vehicles. The solid lines are the driving minutes for the control day and the dotted lines for the case day, for days with known GPS data.

Fig. 2 shows that the average driving times for weekend days tended to be lower than weekdays, as is plausible. The weekend control days are the last two points on the solid line (marked Sat and Sun). The shortest driving time occurred on a Sunday for the control days (60.9 min) and the longest on a Friday (81.1 min).

For the case days (dotted line), the weekend days occurred when the control day was a Friday or a Saturday. The shortest driving time for the case days occurred on a Sunday (63.2 min, with a Saturday control day), and the longest driving time on a Friday (78.7 min, with a Thursday control day).

Fig. 3 gives the variation in average driving time per day of the week when missing GPS data treated as zeros (no driving). The average driving times are smaller than in Fig. 2 where missing GPS data were excluded, because days with no GPS data are counted in Fig. 3 as zero driving time for that day (see Section 1 above). The pattern of variation of the mean driving team across days of the week for control and case days is identical to Fig. 2.

Because there was no way of determining whether missing GPS data has a true zero driving time for that day, or the GPS unit had failed for some reason (see Section 1), the actual driving minutes per day are likely somewhere in between the values shown in Figs. 2 and 3.
FIGURE 3. Same legend as Fig. 2 except that the driving minutes when all GPS data are missing for a day, is given a value of 0 minutes of driving on that day.

Fig. 4 shows the effect of the day of the week on the mean driving consistency. (Because these data are the means across the consistency amounts from 1% to 100% and do not include the vehicles with 0% consistency, the graph is the same whether the days with missing GPS data are dropped from the analysis or treated as zeros as per Section 1 above).

In Fig. 4, for the “Fri” control day, the case day is on a Saturday; for the “Sat” control day, the case day is on Sunday; and for the “Sun” control day, the case day is on Monday, etc. Clearly, when at least one of the days in a day-pair is on a weekend the three right-most points in Fig. 4), the mean part-time driving consistency decreases. This result suggests a positive correlation between driving consistency and the duration of driving: the more driving in a day-pair (whether on a case or control day), the greater the driving consistency. This result is intuitive if one considers that as the minutes in a day fill up with driving, it is more likely that some of those minutes will overlap with driving on another day.

Note that this variation of the mean part-time consistency with the day of the week does not affect the main results or conclusions of this report. A crash can happen on any day of the week at any time. By simply taking the average driving exposures across all days of the week, the data as analyzed correctly reflect the actual amount of driving exposure that a large cohort of drivers would face, and the amount of day-to-day consistency that they would exhibit given that amount of driving exposure.
This association between driving time and consistency is illustrated in another way in Fig. 5, which plots the amount of consistency as predicted by the amount of driving on the control day (the graph is similar if the horizontal axis is made the case day instead of the control day). The symbols are assigned according to the particular paired days as shown in the legend. The day-pairs comprised of two weekdays are shown as is a weekend day.

A regression line between all the points is shown, with a correlation of 0.314, which is statistically significant \((n = 100, p = 0.001)\). It indicates that vehicles with heavier travel on the control day tend to have a slightly higher percent consistency, as shown by the regression line.

At first glance, this correlation might be suspected of being an artifact, attributable to the difference between the weekend and weekday travel patterns rather than their durations (note that the regression line through all the 100 points almost perfectly divides the weekday vs. weekend symbols). When the data are stratified into weekday vs. weekend groups however, the correlations within each subgroup re still statistically significant. Using only data points with all weekday driving (the filled black circles), the correlation between minutes of driving on the control day and the mean part-time driving consistency is 0.319 \((n = 48, p = 0.015)\). For data points with at least one weekend in the pair (open symbols), the correlation is 0.463 \((n = 42, p = 0.002)\). Hence, the overall
variation of the consistency amounts with minutes of driving is a result of the amounts of driving and not just different temporal patterns of driving on weekdays vs. weekends.

Because these data are the means across only the part-time consistency amounts from 1% to 100%, they do not include vehicles with 0% consistency (no overlap of control day driving with the case day driving). Hence, the graph is identical regardless of whether the days with missing GPS data are dropped from the analysis or treated as zeros, so this effect is robust with respect to missing GPS data.

In short, Fig. 5 shows that there is a tendency for lower driving amounts to be associated with lower consistency. Note also that less day-to-day driving consistency lowers the relative risk estimate, due to adjustments for driving consistency bias, as shown in the main report. These two findings together logically lead to the hypothesis that relative risk after adjustment for driving consistency will tend to be lower for those drivers who have relatively smaller amounts of driving time per day (or smaller distances driven). A finding of a lower relative risk for crashes associated with cell phone use for those with
lower amounts of driving distance, has been reported by Laberge-Nadeau et al. in a logistic-normal regression model based on data from 1998-2000 in Quebec, Canada, consisting of 36,078 driver questionnaires, cell phone billing records, and reported crashes.

References


LETTER 1

Cell Phone Use and Crash Risk

To the Editor:

In a recent report,1 Young argues that prior studies indicating harmful effects of cell phone use are confounded by driving time, and “corrects” his estimates to claim that no association exists. However, driving time does not confound the association—it is a requirement for the occurrence of the outcome, just as person-time with a uterus is necessary for uterine cancer.

In both the density-sampled case-control and case-crossover designs, the exposure distribution in the controls is meant to estimate the exposure distribution in the person-time at risk for the outcome. In general, one samples directly from at-risk person-time. However, it is also possible to obtain valid estimates of the exposure distribution in...
the person-time at risk by sampling from nonrisk periods if the exposure distribution is unrelated to outcome risk.2

In a case-crossover study of cell phone use and collisions, cell phone use in the period immediately before the collision (hazard period) is compared with use during driving time earlier in the past (control period); cell phone use when the participant is not driving is irrelevant because one is not at risk for a collision. Furthermore, control persons/person-time must be selected independent of exposure opportunity; adjusting for the likelihood of cell phone use during the hazard or control period induces a bias by incorrectly controlling for exposure opportunity.3,4

Young1 reasons that if one underestimates driving time in the control period, one underestimates exposure (cell phone) time, resulting in an underestimate of the relative risk.

However, this assumption implies that one talks on a cell phone only while driving and not during other times of the day. Although true with OnStar (OnStar, Detroit, MI) (a car-specific communications device), that is not necessarily true for cell phone use more generally; it is equally (if not more) likely that cell phone use is higher while not driving. In this case, exposure during nondriving control times would be an overestimate of exposure during the actual time at risk, and would lead to an underestimate of the relative risk.

Instead of appropriately restricting control periods to driving time, Young multiplies his results by a “correction” factor based on the proportion of cases that did not drive during the control period; applying this estimate to all cases induces a downward bias by attempting to increase comparability between case and control periods with respect to “exposure opportunity”—a recognized fallacy.4

After commenting on this faulty logic previously,3,4 we are disappointed that researchers continue to propagate this erroneous method.

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To the Editor:

We read with interest the article by Young,1 in which he surmises that the imbalance he found in his study between case and control windows of driving exposure might suggest a positive bias in a case–crossover study, examining the association between cell phone use and crash risk conducted by us previously.2 However, his assumption that we examined phone conversation only is not supported by our article. The phone companies provided us with the precise time when a call was placed (dialing task) or received (answering task), the length of time on the phone (conversing task), and the time at which the phone call was completed (hanging up task). We also had data on phone messaging, both sent and received (tasks of writing or accessing messages). Unfortunately, due to part in part to uncertainty in determining time of collision precisely, we could not tease out what aspect of phone use might lead to crashes. In that regard, naturalistic designs are a superior methodology. Young cites several papers featuring this design that found no increased risk of crash due to conversation per se.3–5 However, these same studies did find an increased risk of crash or near-crash due to cell phone tasks, such as texting, dialing, and reaching. In this respect, the findings are consistent with ours, and the odds ratios obtained by these authors were within or above our 95% confidence interval of 2.2–7.7.

The maximum case and control period we considered was 10 minutes. For those driving <10 minutes at the time of crash (63% of case–cross-over participants), only the actual minutes driven before the crash were taken into account, both for the case and control windows. Our crashes were predominantly weekday crashes during peak hours, when driving habits are more likely to be routine. Cases confirmed they were driving during the relevant control periods at the time of hospital interview, thus reducing recall bias. Taking these factors into account, the likelihood of misclassifying time driven during the control period is low. This is in contrast to Young’s study, which examined a 100-day, 24-hour pattern of driving. It would have been interesting for Young to assess driver recall in his sample to test concordance of self-report with the naturalistic data over a short time frame.

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The author responds:

The central point in the letter from Mittleman, Maclure, and Mostofsky is valid: if portable cell phone use were uncorrelated with driving, then control periods should not be corrected. However, portable cell phone use is correlated with driving and not in the direction that they speculate. The problem is that if phone use is, for example, 4 times as prevalent in general when people drive as when they do not drive, the relative risk (RR) will likewise be biased upward by a factor of 4. This bias can be corrected by dividing the estimated RR by the ratio of phone use while driving to that while not driving.

Video recordings in a 2009–2010 “naturalistic” study of 108 drivers indicate the proportion of time drivers conversed on any type of portable cell phone while their car was not in “park” was 0.067. Multiplying this proportion by mean daily driving time of 68.9 minutes yields 4.6 daily conversation minutes while driving. For 2009–2010, subscribers averaged 12.03 daily calls, with a mean conversation duration of 1.17 minutes (1.67 minutes billed, minus 0.5 minutes to correct for the billing practice of rounding up to the nearest minute). Daily conversation minutes (driving and nondriving) per subscriber are then 14.1 (12.03 times 1.17). Daily conversation minutes while not driving are 9.5 (14.1–4.6). Daily total nondriving minutes are 1371.1 (1 day minus 68.9 driving minutes), and therefore, the conversation rate while not driving is 0.0069 (9.5/1371.1). Hence, the conversation rate is 9.7 times greater while driving than not driving (0.067/0.0069), a substantial potential upward bias in the absence of correction.

Redelmeier and Tibshirani reported that 170 subjects used a phone during the 10-minute case period immediately preceding a crash, compared with 37 who used a phone during the same period on the prior control day. This call imbalance yields a simple and direct crude RR estimate of 4.6 (170/37). However, this entire call imbalance can arise solely from a driving imbalance between case and control periods. A case period with known driving has an estimated conversation proportion of 0.067 as noted. Control periods likely had small driving amounts (the weighted average of all data in the Figure of my paper indicates a driving rate of 15% during an arbitrary control period). Hence, control periods (combining driving and nondriving segments) have an estimated conversation proportion of 0.016 [(15% × 0.067) + (85% × 0.0069)]. The correction ratio is then the conversation proportion for the case period divided by that for the control period, or 4.2 (0.067/0.016). Applying this correction ratio to the crude RR yields an adjusted RR of 1.1 (4.6/4.2), independently confirming the previous conclusion.

I agree with Mittleman et al that having calling and driving time data for the control periods would be ideal. That information is unavailable for the 2 studies in question, but 3 “naturalistic” case-control studies (with exact and objective calling and driving times based on video recordings in vehicles) confirm that the RR is near 1 for cell phone conversation while driving.

The letter from McEvoy, Stevenson, and Woodward questions my earlier claim that their study addressed only conversation tasks. Tasks involving either manual or voice dialing do not occur during a call’s recorded billing period because the call time stamp (which specifies when the outbound call began) occurs only after a button is pressed to place the call. Similarly, text messages are time stamped only after the send key is pressed (ie, after the text is entered). Hence, the study by McEvoy et al could not have recorded crashes occurring while such tasks were in progress. Regardless, the prevalence of texting was minimal in the general population in Australia and (the United States) in their 2002–2004 study years.

McEvoy et al shortened control periods to match the recalled driving time before the crash, but this method does not address the major bias in these studies, which is part-time driving during individual control periods. Subjects in the case-crossover studies were asked to recall whether they drove during control periods but not how many minutes they drove. It would have been impossible to provide an accurate answer even if asked and not simply because of recall bias. The acknowledged “uncertainty in determining time of collision” means the control periods are essentially randomly distributed relative to the true collision time. Moreover, contrary to speculation by McEvoy et al, driving habits are far from “routine” even during weekdays. An adjustment accounting for the probability of driving during a period on 1 day contingent on known driving during that same period the next day therefore seems appropriate.

In lieu of exact and objective information in the case-crossover studies, I suggest it is appropriate to adjust RRs to account for likely differences in phone use rates during the case and control periods arising from driving bias. Although the adjustment introduces some uncertainty, attempting this adjustment is preferable to ignoring a likely source of substantial upward bias.
especially in the case of a pandemic.1 Clearly beneficial to society as a whole, this issue.

To the Editor:

We welcome the research by Klick et al2 on the role of mobile phones in motor vehicle collisions. We have a few questions regarding their methodology and data analysis.

CORRECTED ODDS RATIOS

Published odds ratios 0.89 (0.78–1.01) 0.0651
0.82 (0.66–1.02) 0.1107
0.92 (0.80–1.07) 0.0745

ERASURE OF THE SAMPLING FRAMES.

Bias due to incomplete study-base cov-
pensity scores to adjust for selection
frames to demonstrate the use of pro-
linked the case and control sampling
quently overlooked. In this study, we
plete sampling-frame coverage is fre-
advantage of the propensity score meth-
propensity score estimation. The ad-
require correct model specification for
quire this assumption and, in addition,
methods implemented here also re-
similar for cases and potential con-
sion in the control sampling frame are
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0.86 (0.74–0.99) 0.0732
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LETTER 3

More on Cell Phone Use and Crash Risk

To the Editor:

Young1 identifies and attempts to quantify a potential source of bias in estimates by McEvoy et al2 and Redelmeier and Tibshirani3 that drivers’ cell phone use is associated with a 4-fold increase in crash risk. Both epidemiologic studies used case-crossover designs comparing drivers’ cell phone use during a time interval before a crash (hazard window) with use during an earlier comparable time interval (control window).

2. Austin PC. The relative ability of different propensity score methods to balance measured covariates between treated and untreated sub-
According to Young,1 if subjects had been driving during only part of the control window, the estimated crash risk would be inflated. To estimate this bias, Young derived a “driving consistency index” of 26.4% based on the average overlap in minute-to-minute driving on consecutive days, using Global Positioning System data from 439 household vehicles. He then multiplied the relative risk ratios by 26.4%, yielding no evident increase in crash risk associated with cell phone use in either study.

It is correct that estimates would be biased if subjects had not driven during the entire control window. However, the epidemiologic studies2,3 took steps to account for this. McEvoy et al2 removed participants who reported no driving in the 10-minute control window, and they reduced hazard and control windows if participants drove fewer than 10 minutes before crashing. Both studies conducted sensitivity analyses with various control windows, and varied the latency between hazard and control windows. The findings were robust.

Young’s1 index objectively measures day-to-day driving consistency for his sample. However, its relevance to the epidemiologic studies2,3 is doubtful, and there is no basis for believing his adjusted estimates of crash risk from cell phone use are accurate. Young’s index does not reflect driving consistency during the periods of driving in the epidemiologic studies; his assumption that the consistency of driving is uniform across all hours of the day and days of the week seems unlikely. He estimates driving consistency on 2 adjacent days; this does not reflect driving consistency in all the matching procedures in the epidemiologic studies and cannot account for increased crash risk in each. Young’s GPS data recorded the use of household vehicles, and so his estimates reflect the driving consistency of one or more drivers’ use of a household vehicle on consecutive days. Finally, Young’s adjustment assumes cell phone use occurs only during driving. If cell phone use is more prevalent when people are not driving, this would increase the frequency of cell phone use in control windows relative to hazard windows and bias crash risk estimates downward, rather than upward.

See http://www.iihs.org/research/topics/pdf/r1169.pdf for a more detailed response to Young.1

ACKNOWLEDGMENTS
We thank our colleagues who contributed to this review by providing information and helpful feedback, including David Zuby, Chuck Farmer, and Adrian Lund.

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REFERENCES

The author replies: Kidd and McCartt1 agree that relative risk (RR) estimates are biased when participants drive during only part of a control window (ie, part-time driving bias2,3). However, the adjustments they identify1 do not address this bias. Adjusting the crude RR by excluding participants not driving during an entire control window1 still leaves participants who did not drive during 74% of their control window periods (26% consistency2), a remaining 4-fold upward RR bias.2 Reducing window durations for participants driving fewer than 10 minutes before crashing1 addresses part-time driving only during case windows, not control windows. The table shows part-time driving consistencies for case windows restricted to weekday daytime hours1 for various control-day latencies.1

The Toronto study “restricted” part-time consistencies across its multiple comparison days2 ranged from 31% to 37%—slight increases from the 26% unrestricted part-time consistency.2 The often-cited Toronto adjusted RR of 4.3 is based on the “−1” control day, which has a restricted part-time driving consistency of 34%, a 3-fold bias. The Australian study5 used control-day latencies of −1, −3, and −7, with restricted consistency values of 29%, 23%, and 31%, respectively—for a mean of 28%, a 4-fold bias.

An alternate correction method can directly adjust a simplified crude Toronto RR of 4.59.3 First, calculate a combined driving consistency across all control windows—those with total non-driving as well as part-time driving. This value for the −1 control day is 20% after again restricting case windows to the Toronto study daytime hours on weekdays. Second, note that observational and survey data indicate 11% of US drivers used a cell phone of any type during 11 daylight hours in 2008.6 Third, substitute these 3 values (20%, 11%, and 11 hours) in the alternate correction method calculations.3 This method estimates a weekday daytime cellular conversation rate of 3.08% during a control window, after combining driving and nondriving segments. The combined correction ratio is then 3.57 (11%/3.08%), which adjusts the crude

### TABLE.

GPS-determined2 Part-time Driving Consistency (%) for Case Windows Restricted to Weekday Daytime Hours for Control Windows 1 to 7 d Prior

<table>
<thead>
<tr>
<th>Weekday Case Times</th>
<th>Control-Day Latency Before Case Day</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>−7</td>
</tr>
<tr>
<td>Toronto study4 10 AM to 6 PM</td>
<td>37</td>
</tr>
<tr>
<td>Australian study5 8 AM to 9 PM</td>
<td>31</td>
</tr>
</tbody>
</table>

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Toronto RR to 1.29 (4.59/3.57), identical to the crash/near-crash odds ratio for handheld cellular conversation in a real-world “naturalistic” case-control study with accurate video-recorded calling, driving, and crash times.7

I agree that case-crossover findings appear “robust” given RR values near 4 for all control windows, demographic strata, and phone types.4,5 But this apparent robustness simply reflects the impact of a consistent uncontrolled part-time driving bias2,3 in all conditions.

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REFERENCES
LETTER 4

Cell Phone Use and Crash Risk

To the Editor:

Young1 attempted to correct for a potential bias in early epidemiological studies of cell phone use and crash risk. Concerns with his analyses have been raised, including an incorrect assumption that people talk on phones only while driving. In response, Young2,3 conducted two reanalyses that substantially revise his original correction method by (1) refining the estimates of driving inconsistency and (2) adding a new correction ratio to account for the prevalence of phone use during periods of driving and nondriving. Based on these reanalyses, Young concludes there is no increase in crash risk with phone use (adjusted risk ratio = 1.292 and 1.13). However, his reanalyses are based on data that are not comparable with the epidemiological study samples, which greatly affect the validity of his corrected estimates.

The new correction ratio of Young2,3 is based on data from several studies of US drivers and wireless subscribers. These studies provide reasonable estimates of cell phone use in the United States, but not necessarily the epidemiological study populations. Young's data on US phone use are much more recent (2009–2010) than the epidemiological study data (1994–1995 and 2002–2004), even though cell phone use has changed over time. Moreover, handheld cell phone use was prohibited during the more recent epidemiological study, but not in the earlier Canadian study or in most US states. Evidence shows that handheld cell phone bans reduce use during driving.

Even small inaccuracies in Young's estimates can greatly affect conclusions drawn from the correction ratio. For example, Young estimates the average duration of cell phone conversations at 1.17 minutes using 2009–2010 US data,4 but the same data show that the average cell phone conversation during the more recent epidemiology study period was 2.95 minutes. When the latter value is used in Young's correction ratio equation without other changes, the adjusted crash risk ratio increases from 1.29 to 2.5. Compounding possible problems with the precision of Young's estimates is that some are inexplicably inconsistent in his two reanalyses. He uses different estimates of the prevalence of phone use while driving (11% and 6.7%), driving consistency (20% and 15%), and total hours in the day when a phone could be used (11 and 24 hours).

In conclusion, Young is correct that estimates of crash risk associated with phone use from early epidemiological studies may not have accounted sufficiently for driving inconsistency. His original correction to these estimates was flawed, and his revised corrections exacerbate rather than remove these flaws.

ACKNOWLEDGMENTS

We thank our colleague David Zuby who contributed with helpful feedback.

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REFERENCES

The author responds:

Dr. Kidd\(^1\) agrees that previous epidemiological studies on the risk ratio of cell phone conversations while driving were biased because they did not take into account the proportion of time spent not driving during a control period (ie, part-time driving).\(^2\) Dr. Kidd’s first objection\(^1\) to the adjustment method is that the 2005–6 Seattle Global Positioning System (GPS) data\(^3\) are a different time and place than the epidemiological studies. An analysis\(^3\) of 2007–8 Chicago GPS data yielded similar results to Seattle,\(^2\) supporting the robustness of such data.

His second objection\(^1\) is that the two prevalence estimates\(^4,5\) of phone conversation while driving are inconsistent. This is a misreading. The prevalence of 6.7%\(^4\) is for a 24-hour prevalence period to match the 24-hour period in the GPS driving consistency analysis in my original article.\(^2\) The prevalence of 11%\(^5\) is for a daytime prevalence period in my subsequent analysis\(^5\) to match the daytime hours in the epidemiological studies.

Kidd’s third objection\(^1\) is that small differences (eg, in average call duration) might produce important changes in the adjusted risk ratio. Such variations make it preferable to use an adjustment method that is valid for any average call duration. The Table accomplishes this by calculating a rate ratio (RR) from the Toronto study\(^6\) raw caller counts and window durations.

The crude RR is 4.59, which incorrectly assumes that subjects were in their cars during an entire 10-minute control window (1700 total person-minutes).

However, subjects were likely in their cars during only 20% of a prior-day control window.\(^5\) To avoid this part-time driving bias, control-window in-car person-minutes were adjusted to 340 (Table). Let \(\rho\) be the ratio of out-of-car to in-car control-window caller rates (\(\rho\) does not depend on call duration, window duration, or in-car time). The number of callers in the in-car control window is readily solved as 37/(4\(\rho + 1\)). For portable phones, \(\rho\) is estimated as 0.1,\(^4\) yielding 26.4 in-car control callers and adjusted RR 1.29.\(^5\) For embedded phones, \(\rho\) is 0, yielding 37 in-car control callers and adjusted RR 0.92, within the 95% confidence interval of the OnStar embedded phone with RR 0.62 (0.37–1.05).\(^7\)

Future studies should report the number of people conversing on cell phones before crashes and in control periods only while driving. It is clear that previous epidemiological estimates of cell phone conversation producing a relative risk about 4 times greater than without cell phone conversation are wrong.

TABLE.  Crude and Adjusted RR Calculations for the Toronto Study\(^6\)

<table>
<thead>
<tr>
<th></th>
<th>Crude</th>
<th>Adjusted</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Case</td>
<td>Control</td>
</tr>
<tr>
<td>Number of callers</td>
<td>170</td>
<td>37</td>
</tr>
<tr>
<td>Person-minutes(^a)</td>
<td>1,700</td>
<td>1,700</td>
</tr>
<tr>
<td>Rate (callers per min)</td>
<td>0.1</td>
<td>0.0218</td>
</tr>
<tr>
<td>RR (95% CI)</td>
<td>4.59 (3.22–6.56)</td>
<td>1.29 (0.85–1.94)</td>
</tr>
</tbody>
</table>

\(^{a}\)10-minute case and control windows.

ACKNOWLEDGMENTS

I thank Joshua Cohen, Charlene Hallett, Linda Angell, Katja Kircher, Greg Fitch, and Michael Posner for helpful comments and Sean Seaman for programming assistance in analyzing the GPS datasets.

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REFERENCES

Study by Wayne State University researcher shows early studies on cell phone conversations likely overestimated crash risk

DETROIT — A Wayne State University study published in the January 2012 issue of the journal *Epidemiology* points out that two influential early studies of cell phone use and crash risk may have overestimated the relative risk of conversation on cell phones while driving.

In this new study, Dr. Richard Young, Professor of Research in Wayne State University’s Department of Psychiatry and Behavioral Neurosciences in the School of Medicine, examined possible bias in a 1997 Canadian study and a 2005 Australian study. These earlier studies used cell phone billing records of people who had been in a crash and compared their cell phone use just before the crash to the same time period the day (or week) before – the control window.

Young said the issue with these studies is that people may not have been driving during the entire control window period, as assumed by the earlier study investigators.

“Earlier case-crossover studies likely overestimated the relative risk for cell phone conversations while driving by implicitly assuming that driving during a control window was full time when it may have been only part time,” said Young. “This false assumption makes it seem like cell phone conversation is a bigger crash risk than it really is.”

In Young’s new study, his research team used Global Positioning Satellite data to track day-to-day driving of more than 400 drivers during a 100-day period. He then divided the days into pairs, with the first day representing the “control” day and the second day representing the “crash” day in the earlier studies. Overall, the team found little driving consistency in any given clock time period between the two days -- driving time on the control day was only about one-fourth of the driving time on the crash day, during any specific clock time period.
“This underestimation of the amount of driving in the control windows by nearly four times could reduce cell phone conversation time in that control period,” Young said. “It makes it appear that there is less cell phone conversation in control periods than in the time just before a crash, making the relative risk estimate appear greater than it really is.”

Young found that when the cell phone conversation time in the control window was adjusted for the amount of driving the amount of cell phone usage in the control window was about the same as in the minutes before a crash. He concluded that the crash risk for cell phone conversation while driving is one-fourth of what was claimed in previous studies, or near that of normal baseline driving.

Young added that many well-controlled studies with real driving show that the primary increase in crash risk from portable electronic devices comes from tasks that require drivers to look at the device or operate it with their hands, such as with texting while driving. Five other recent real-world studies concur with his conclusion that the crash risk from cellular conversations is not greater than that of driving with no conversation.

“Tasks that take a driver’s eyes off the road or hands off the steering wheel are what increase crash risk,” said Young. “Texting, emailing, manual dialing and so forth – not conversation – are what increase the risk of crashes while driving.”

The National Transportation Safety Board has recommended that all 50 states and the District of Columbia ban the non-emergency use of portable electronic devices for all drivers. Young said this recommendation goes beyond the data from newer studies, including his, because it would ban cell phone conversations while driving.

“Recent real-world studies show that cell phone conversations do not increase crash risk beyond that of normal driving – it is the visual-manual tasks that take the eyes off the road and the hands off the wheel that are the real risk,” said Young.

###

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economic growth and improve the quality of life in the city of Detroit, state of Michigan and throughout the world. For more information about research at Wayne State University, visit http://www.research.wayne.edu.
Q &A for Prof. Richard Young
(Regarding his recent study on cell phone conversations and driving, in the context of the recent NTSB recommendation to ban all portable device usage while driving)

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Briefly

Q. What did you find in your recent study of cell phone conversations and driving?

A. My recent study shows that previous real-world studies indicating a four times increase in crash risk from cell phone conversations contain a scientific bias. These studies assumed that more driving took place in control periods than actually occurred. When the bias is removed, the relative crash risk of cell phone conversations while driving is no different from that of normal driving without cell phone conversation. (See p. 4 for further explanation.)

Q. What do your findings indicate with respect to the recent National Transportation Safety Board recommendation that all 50 states and the District of Columbia “Ban the non-emergency use of portable electronic devices (other than those designed to support the driving task) for all drivers”?

A. My study and other current scientific evidence with real-world data indicates that imposing a ban on cell phone conversations will not reduce crashes. Other recent scientific studies with real drivers in real-world driving indicate that hands-free cell phone conversations, voice-activated tasks, or tasks that require only a single button press – such as speed dialing – likewise do not increase crash risk, and a ban of those tasks will not reduce crashes. These real-world studies indicate that as long as a task allows a driver to keep eyes on the road and hands on the wheel, that drivers in the real world can perform that task without increasing their crash risk beyond that of normal driving. A ban on those operations while driving would not improve driving safety or reduce crashes.

Q. Do these more current real-world studies indicate that anything can be done while driving and you do not have to worry about the crash risk?

A. No. These same real-world driving studies find that crash risk is increased by tasks such as texting (23 times), manual dialing (2.5 times), or any other visual-manual operations of portable electronic devices that take the eyes off the road or the hands off the wheel. These studies indicate that a ban on those operations while driving would improve driving safety and reduce crashes.
Q. So what are you saying briefly?

A. The best real-world scientific evidence to date indicates that a ban on texting while driving and other visual-manual operations that take your eyes off the road or hands off the wheel will reduce crashes, but a ban on tasks such as hands-free cellular conversations that allow you to keep your eyes on the road and hands on the wheel will not reduce crashes.

**NTSB Background**

Q. What is the NTSB?

A. The NTSB is an independent federal agency charged with determining the probable cause of transportation accidents, promoting transportation safety, and assisting victims of transportation accidents and their families. It has about 400 employees and mainly conducts crash investigations of large accidents involving airplanes, trains, buses and commercial vehicles (trucks). The NTSB has previously investigated airline and train incidents involving distraction. It has performed a limited number of investigations of car crashes as well.

Q. What is the latest vehicle crash investigation by the NTSB?

A. The NTSB released a synopsis on December 13 (http://www.ntsb.gov/news/events/2011/gray_summit_mo/index.html) of a crash investigation report they will publish soon. The crash was multi-vehicle crash with two fatalities in Gray Summit, Utah, on Aug. 5, 2010, involving two school buses, a tractor-trailer and a pickup truck. The pickup driver was fatigued at the time of the accident due to cumulative sleep debt and acute sleep loss, which could have resulted in impaired cognitive processing or other performance decrements. The pickup truck driver also reportedly sent 11 text messages in the 11 minutes before the first crash, in which his truck crashed into the rear of a slowing or stopped tractor-trailer. One school bus then rear-ended the pickup truck, and the second school bus rear-ended the first bus.

Q. What has the NTSB recently recommended about portable electronic devices?

A. The National Transportation Safety Board recommended on December 13 (among other things) that all 50 states and the District of Columbia “Ban the non-emergency use of portable electronic devices (other than those designed to support the driving task) for all drivers.” This is their first major recommendation concerning driver distraction in passenger vehicles.

**Recent Scientific Evidence**

Q. Is the NTSB recommendation consistent with current real-world scientific data?
A. The NTSB report unfortunately has far gone beyond its own data and current real-world scientific evidence in recommending a near-total ban that would include all cellular conversations, whether hands-free or hand-held. Their call for a total ban is not consistent with current scientific studies concerning the real-world risk of cellular conversations and driving.

Q. What scientific evidence is there that is not supportive of the NTSB call for a total ban?

A. The current scientific evidence shows that there are several different types of portable device usage that need to be separately considered. For example, texting and talking do not have the same level of risk. Yet the NTSB lumps these together in public statements by its chairman, Deborah Hersman, as if they carry the same level of risk.

Scientific findings have shown that the general category of “device use” is made up of sub-types of use – and these sub-types differ greatly in how much risk they pose. Some types pose much more risk, others small amounts of risk, and others no increase in risk, compared to baseline driving:

Visual-manual tasks. Studies with real driving on real roads with real drivers in their own vehicles show that the primary increase in crash risk from portable electronic devices comes from those devices and tasks that require drivers to interact through a visual-manual interface, that is, tasks that take a driver’s eyes off the road or hands off the wheel. These are tasks like reaching for the phone, dialing the phone and texting on the phone – anything that takes the eyes off the road, or the hands off the wheel. Texting, for example, poses 23 times the risk as normal driving. These visual-manual tasks with portable devices are those that should be banned while driving.

Auditory-vocal-cognitive tasks. In these same studies and others, it is found that auditory-vocal-cognitive activities, which involve listening, thinking and talking – allow the eyes to remain forward on the road and the hands on the wheel. Real-world scientific data collected with real vehicles shows that these purely auditory-vocal-cognitive tasks – which include hands-free cellular conversations or passenger conversations -- pose levels of risk that are not elevated above normal driving, and should not be banned while driving.

Q. Why is the NTSB recommendation for a total ban not based on scientific evidence?

A. The NTSB investigates individual cases – they do not do scientific studies. While individual cases are truly tragic, and move us to think deeply about issues, they are not the same as scientific studies. Scientific studies identify robust patterns across many, many cases and objectively quantify risk associated with many different factors (among other things).
Q. In what ways does the NTSB recommendation for a total ban go against current real-world scientific evidence?

A. The NTSB recommendation goes beyond the scientific evidence from six newer studies of real-world driving data (including my recent study). The NTSB recommendation: (a) treats the use of all devices as though they are the same and pose the same risk, and (b) proposes to ban activities -- such as conversation conducted while driving -- for which risk is not elevated.

These recent real-world studies show that conversation itself (excluding the acts of dialing a device to enable conversation, reaching for a device, texting or other visual-manual tasks) does not increase crash risk beyond that of normal driving. These recent studies of real-world crash risk associated with conversation are consistent with the conclusion that conversation itself does not elevate risk beyond normal driving.

The scientific findings on crash risk do not suggest that conversation or other auditory-vocal-cognitive tasks elevate risk beyond that of normal driving, and thus do not provide support for the complete ban recommended by the NTSB.

Q. What specifically does your recent study show?

A. My recent study involved a closer investigation of two older epidemiological studies from Toronto in 1997 and Australia in 2005, indicating a fourfold increase in risk. My recent study in the current issue of the journal Epidemiology shows that these studies contain a major bias. The bias was a confounding factor of part-time driving during comparison baseline periods. This bias gave rise to a fourfold overestimate of the risk of cell phone conversations while driving in those two older studies. The bias arose because at the time of the crash, people were obviously driving, but during the comparison period in those studies, the studies did not fully ensure that driving was occurring there too. Therefore, “apples were not compared to apples.” When the results of those studies were adjusted to remove the driving bias, the risk from cellular conversation is now found to be about the same as the crash risk during normal driving. This finding is consistent with five more recent real-world studies with real drivers in real vehicles.

Q. Could you provide more explanation of the “driving bias” in these early studies?

A. These studies had a false assumption made driving in the control window to which driving while conversing was compared. Hence, cell phone use may have been underestimated in the control window, overestimating the relative crash risk of cell phone conversations. During the control windows used for comparison, the early study researchers assumed that driving occurred during the entire time of the control window.

Q. How did your study adjust for the driving bias in these earlier studies that falsely elevated crash risk?
A. The objective Global Positioning System data analyzed in my study suggests that driving during the control windows in the earlier studies was done only part of the time (perhaps as little as one-quarter as much as they assumed). This means that in the older study computations, they divided the cell phone usage just before the crash by too small a number, causing their relative risk result to be about four times larger than it should have been.

Q. Could you give a simple example of what you are talking about with these effects on crash risk of different activities while driving?

A. Would you agree that 100% of crashes are associated with the driver breathing at the time of the crash? (You answer “Yes.”) Is it correct to conclude that breathing causes crashes? (You answer “No.”) That is correct; you cannot say that breathing causes crashes, just because 100% of crashes are associated with the driver breathing. That is because you must have to have a control period to compare to determine the true crash risk. During other times of driving when there are no crashes, 100% of the time people are also breathing. Therefore, the relative crash risk is 100% divided by 100%, or one. That is, there is no increase in crash risk from breathing while driving.

Now what if 100% of the time you were on a cell phone at the time of the crash? Is it correct to conclude that cell phone use causes crashes? (You answer, “no, I guess not for the reasons you gave above.) That is correct, for the same reasons as with breathing, you cannot conclude that cell phone use causes crashes, even if it people were on the cell phone before 100% of crashes. You have to compare this percentage, just as you did breathing, to the percentage of cell phone uses during a control period when people are driving but not crashing. If people are on a cell phone 100% of the time when driving, whether they crash or not, then the relative risk of cell phone use while driving is 100% divided by 100% or one. That is, there is no increase in crash risk from people conversing on a cell phone while driving.

What these early studies found is the cell phone use was about four times higher just before a crash than during a control period. However, these studies did not control for the amount of driving in the control period. The amount of driving in the control period, my study showed, was only about ¼ as much as these studies assumed. When people are not driving, they tend to use the cell phone less. Therefore, the reason the control period had a four times smaller cell phone use than the time before the crash, had to do with the driving in the control period.

Q. What happens when you adjust the results in these early studies to remove the driving bias?

A. When the relative risk is calculated properly (by correcting the false assumption that driving was full-time when a driver recalled driving in the control window on a previous day), the risk of crashing when conversing during driving is not distinguishable from the risk of crashing when “just driving.” That is, the risk of conversing on a cell phone while driving is not four times as risky as normal as the early reports estimated, but is close to
that of normal driving without conversation. This result is consistent with what other new studies also find, based on newer data with direct video recordings of drivers in vehicles and their crashes, or cellular data that gives exact times of calls for millions of drivers and compares that to the exact times of thousands of airbag-deployment crashes (http://www.ncbi.nlm.nih.gov/pubmed/19000076).

Q. What is the abstract for your study?

A. Background: Recent epidemiologic studies have estimated little or no increased risk of automotive crashes related to cell phone conversations by the driver, whereas earlier case-crossover studies estimated the relative risk as close to four. Did earlier studies introduce a positive bias in relative risk estimates by overestimating driving exposure in control windows?

Methods: Driving exposures in a “control” window and a corresponding “case” window on the subsequent day were tabulated across 100 days for 439 GPS-instrumented vehicles in the Puget Sound area during 2005–2006.

Results: For control windows containing at least some driving, driving exposure was about one-fourth that of case windows. Adjusting for this imbalance reduces relative risk estimates in the earlier case-crossover studies from four to one.

Conclusion: Earlier case-crossover studies likely overestimated the relative risk for cell phone conversations while driving by implicitly assuming that driving during a control window was full-time when it may have been only part-time.

Q, What is the link to the abstract for your study on the web?

A. http://journals.lww.com/epidem/Abstract/2012/01000/Cell_Phone_Use_and_Crash_Risk___Evidence_for.17.aspx

Q. What is the link for the Wayne State Press Release on your study?

http://research.wayne.edu/communications/news-release.php?id=272&y=&m=