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1. Introduction

Cell phone use is increasing worldwide.[1] Strong demand, declining prices, and improvements in service quality have led to substantial increases in usage. In January of 1985, there were fewer than 100,000 subscribers in the U.S. Today there are over 159 million.[2] During that period revenues climbed from under $1 million in 1985 to $88 billion in 2003.[3] Roughly 65% of the U.S. population owns a cell phone and that number can be expected to grow as rates continue to decline and services, such as email and Internet access, increase (Gallup Organization, 2003). In fact, the number of cellular phones is estimated to exceed the number of traditional, fixed line phones worldwide, and accounts for about 45% of total phone lines in the U.S.[4]

The increase in cell phone demand has led to concern that cell phone use while driving increases accidents. Risk associated with calling while driving has been widely discussed in the media, and has been investigated by governmental agencies (NHTSA, 1997). Previous studies estimate that cell phone use in vehicles may cause anywhere from 10 to 1,000 fatalities per year in the U.S. and a great many more non-fatal accidents.[5] The regulation of cell phones while driving has become a significant policy issue. The states of New York and New Jersey, dozens of municipal governments in the U.S., and many countries worldwide have banned the use of hand-held cell phones while driving. Many other bans are being considered (Lissy *et al.*, 2000; Hahn and Dudley, 2002); most proposed legislation would ban the use of hand-held cell phones while driving, while allowing the use of phones with hands-free devices.[6]

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[1] The term “cell phone” is used in this paper for any type of mobile radiotelephone.

[2] Data from December 2003, from the Cellular Telecommunications and Internet Association Web site, at http://www.wow-com.com/industry/stats/surveys.

[3] Revenues are in nominal dollars and include the total service revenues from providers of cellular, PCS and ESMR services. See *id*.

[4] International Telecommunications Union, “Key Global Telecom Indicators for the World Telecommunication Service Sector, available at <http://www.itu.int/ITU-D/ict/statistics/at_glance/KeyTelecom99.html>; FCC (2003).

[5] This range represents about 0.02% to 2% of traffic fatalities in the U.S. See Redelmeier and Weinstein (1999), which estimates 730 annual fatalities a year caused by cell phones. Hahn, Tetlock, and Burnett (2000) calculate a range of 10 to 1,000 deaths, with a best estimate of 300 fatalities per year.

[6] “Hands-free” refers to a phone that has a headset, is built into the car, or otherwise does not require the user to hold it during operation.

Economically rational policymaking should weigh the costs and the benefits of a ban. A small literature estimates the costs and benefits of cell phone use while driving (Redelmeier and Weinstein, 1999; Hahn, Tetlock, and Burnett, 2000; Cohen and Graham, 2003). A key deficiency in this literature is that not much is known about the relationship between cell phone use while driving and accident levels. Previous statistical work estimates risk of use as a multiple of an individual's unknown baseline accident rate rather than absolute risk of use (Redelmeier and Tibshirani, 1997a; Violanti, 1998). No existing paper uses data and methods that allow for a direct computation of the effect of a cell phone ban on the number of accidents. Consequently, the cost-benefit analysis literature has relied on out-of-sample assumptions about average minutes of use while driving and average accident rates to estimate accidents from usage. If individuals who use cell phones have different baseline accident rates than those who do not, however, using average rates to calculate the reduction in accidents from a ban can be inaccurate. We estimate accident rates and the impacts of various amounts of cell phone usage for each driver, and use individual-level data on minutes of phone use to directly estimate the effect of a cell phone ban on the number of accidents.

The purpose of this paper is to develop a new approach for estimating the relationship between cell phone use while driving and accidents. We explore data from a new survey of over 7,000 individuals that provides information on cell phone use and vehicle accidents. This research differs from previous work in two significant ways. First, our econometric methodology is designed to detect and correct for selection bias of two types. Specifically, we hypothesize that drivers who use cell phones while driving may be more likely to get into accidents than drivers who do not, even when they are not using the phone (Hypothesis 1). If so, cell phone users are a selected group of riskier drivers. We also hypothesize that the causal impact of usage on accidents may be heterogeneous across drivers, in the sense that the same amount of usage increases some drivers' risk more than others' (Hypothesis 2). In this case, a sample of drivers who all had accidents, such as Redelmeier and Tibshirani (1997a) and Violanti (1998) use, will be composed disproportionately of individuals with large usage effects.

Our second expansion upon previous studies is that our work is based on a more comprehensive sample of drivers. The sample is larger than other studies using individual-level data, and contains both drivers who use a cell phone and drivers who do not. Under Hypothesis 1, a sample containing both users and non-users is required to reveal selection effects and determine the causal impact of cell phone usage on accidents. Furthermore, our sample contains drivers who had accidents and drivers who did not. Under Hypothesis 2, restricting the sample to drivers who had accidents may lead to incorrectly high estimates of the causal impact of usage on accidents.

To expand upon these two hypotheses, consider the stylized representation of determinants of accident risk in Figures 1 and 2. The determinants of collision risk begin with the type of driver on the left. Drivers' types range from very careless drivers to extremely safe drivers. The inherent type of the driver is not completely captured by any set of characteristics (age, sex, income, etc.) that the econometrician could observe. In Figure 1, which depicts Hypothesis 1, this unobserved type affects the amount of cell phone usage while driving and whether the driver uses a hands-free device. Usage is also determined by external factors influencing demand for calling while driving, such as income and price of usage. The most natural story, which is supported by our analysis, is that more careless people are more likely to use the phone while driving, and less likely to use hands-free devices. Collision risk is determined by cell phone usage while driving, external factors such as weather, and the driver's type. A simple observed correlation between cell phone usage and collisions therefore confounds the direct causal effect from usage with the effect of the unobserved type. If riskier drivers are more likely to use cell phones, then simple estimates of the impact on accident rates from cell phone usage may be biased upward due to the common factor of the unobserved type influencing both usage and accidents.

Hypothesis 2, that usage risk is heterogeneous, is depicted in Figure 2. Here the usage impact is assumed to vary across individuals due to unobservable factors. The wide arrow from usage to collision risk represents the effect of the driver's unobserved type on the relationship between usage and accident risk. A natural expectation is that more careless drivers are those for whom cell phone usage increases accident risk the most. This would be true if, for example, inherently careless people use a cell phone in a more careless fashion, such as allowing themselves to become engrossed in conversation.

We find support for both hypotheses. Selection effects due to the endogeneity of cell phone usage appear to be present. The evidence for endogeneity is strongest for the decision to use a hands-free device: individuals who are more likely to use hands-free devices are more careful drivers anyway. Once we correct for the endogeneity of hands-free usage, our models find accident risk from hands-free usage to be the same as from handheld usage, which calls into question bans on hand-held usage such as the ones passed in New York and New Jersey. We also find support for Hypothesis 2, that usage impacts are heterogeneous in the population even after controlling for observable driver characteristics, particularly for female drivers. This second result implies that previous studies of cell phone usage and accident risk are subject to selection bias; we calculate that the causal impact of usage on risk for the population may be 27% lower than previous estimates.

Finally, we explore the impact of a ban on all kinds of cell phone use while driving. We cannot reject the hypothesis that a ban would have no effect on the number of accidents, even if compliance with the ban were 100%. Our estimates of the reduction in accidents from a ban on cell phone use while driving are both lower and less certain than some previous studies indicate.

The plan of the paper is as follows. The next section reviews the literature on the effect of cell phone use on driving. In section III we describe our survey data. We report the results of our statistical work in section IV, consider the effects of a ban in section V, and conclude in section VI.

2. Literature Review

There are four strands to the literature on the effects of cell phone use on driving. Several studies attempt to find a statistical association between cell phone use and accidents using individual-level data (Violanti and Marshall,1996; Redelmeier and Tibshirani, 1997a; Violanti, 1998; Dreyer, Loughlin, and Rothman, 1999). The other strands are simulator or on-road controlled experimental studies, analysis of automobile crash data from police reports, and analysis of aggregate crash and cell phone statistics.[7] Hahn and Dudley (2002) review and critique this literature, and find that while each approach has its shortcomings, there is widespread agreement that using a cell phone while driving increases the risk of an accident. Most germane to our study, and the most influential among policy makers, is the case-crossover study by Redelmeier and Tibshirani (1997a) (hereafter, RT). Case-crossover methods (Maclure, 1991; Marshall and Jackson, 1993) are used in the medical literature to study the determinants of rare events—accidents, in RT's case. RT collect a sample of Toronto-area drivers who own cell

[7] See Lissy *et al.* (2000) for citations.

phones and had recent minor traffic accidents . They examine cell phone records to determine if the driver was using the phone at the time of the crash and during a reference period at the same time the previous day. The case-crossover method relies on the observation that if cell phone usage increases accident risk, then the driver is more likely to be on the phone at the time of the crash than during the earlier reference period. By comparing the individual's behavior across time, each person serves as his own control. RT's case-crossover methodology yields fixed-effects estimates that approximate the relative risk of phone usage on accidents.[8] RT conclude that a driver is 4.3 times as likely to have a collision while using a phone as when not using a phone, with a 95% confidence interval of (3.0, 6.5).

Although there are a few other epidemiological studies on cell phones and accidents (Tibshirani and Redelmeier, 1997; Violanti, 1998), RT's results are widely quoted in the media and continue to be the most highly cited in policy discussions about banning phone usage while driving. RT were careful not to assert causality,[9] but others have used RT's results to perform cost-benefit analyses of hypothetical cell phone bans, thereby ascribing a causal interpretation to RT's results (Redelmeier and Weinstein, 1999; Cohen and Graham, 2003). The case-crossover methodology is not without weaknesses, however (Redelmeier and Tibshirani, 1997b; Hahn and Dudley, 2002). While it avoids bias due to bad controls (in the sense that an individual is the best control for himself), it does not avoid bias due to selection of the cases. In particular, since the method uses only cell phone users, all of whom had accidents, the representativeness of the sample is open to question, if either of our hypotheses discussed above are true. If the sample is not representative, then extrapolating RT's results to the population is incorrect. We explore how representative the drivers who had accidents in our data are compared to our full sample, and find that their accident rates increase much more from cell phone usage than do the rest of our sample.

As discussed in the introduction, a further weakness of existing cost-benefit analyses is that the epidemiological studies upon which they are based (Violanti and Marshall, 1996; Redelmeier and Tibshirani, 1997a; Violanti, 1998) estimate *relative risk*, the risk multiple on baseline crash risk from cell phone usage. Unlike our study, they do not estimate individual-

[8] While it is not clear from RT that case-crossover analysis is maximum likelihood, the connection is made explicit in Tibshirani and Redelmeier (1997).
[9] For example, RT note that emotional stress may lead to both increased cell phone use and decreased driving ability, leading to spurious correlation.

specific baseline accident rates and cannot directly estimate the effect of a cell phone ban without using out-of-sample information.

3. Description of the Survey Data

Survey Design

We commissioned a survey to gather individual-level data on cell phone usage and driving patterns. The survey was administered over the Internet in January and early February 2003.[10] Internet-based surveying has advantages over telephone surveying, particularly for sensitive questions (Chang and Krosnick, 2003). Although Internet survey samples are not random, because participants self-select into the panels, survey research indicates that Internet surveys are better at eliciting socially undesirable answers (such as admitting cell phone use while driving) from respondents than are telephone surveys.[11] Our largest usable sample consists of 7,327 individuals.[12] We explore the degree to which our final survey panel is representative of the general public below.

The survey design is retrospective: we ask individuals to provide data on driving accidents and cell phone usage over calendar years 2001 and 2002. From the survey responses we create a panel data set with quarterly observations on individuals. Of the up to eight quarters of data collected per individual, we use the four quarters from October 2001 to September 2002 in most of our estimations. Data in these quarters are available for 7,268 individuals, yielding 26,572 observations (an average of 3.7 quarters per individual).[13] This subset avoids using the

[10] The survey administrator, NFO Worldgroup, recruits individuals on their web site by offering "reward points" that can be redeemed for cash and prizes, sweepstakes, and the claim that "Your opinions shape new products!" The survey project management was done by Allison-Fisher International (AFI), which protects the confidentiality of respondents. AFI does not release personally identifying information of respondents, and household location is known only at the five digit ZIP code level.

[11] See Chang and Krosnick (2003), who also cite many other studies showing that eliminating interaction with an interviewer increases willingness to report behavior that is not "respectable". In addition, Chang and Krosnick (2003) also find that Internet survey participants' responses contained fewer errors than their telephone counterparts, and offered two explanations for these differences in addition to the "social compliance" phenomenon noted above. First, unlike telephone surveys, Internet surveys have no time pressure because they are self-paced. Second, limited short-term memory leads telephone respondents to disproportionately choose the last response offered. The only two other studies we found that directly compare survey modes (Best *et al.*, 2001; Berrens *et al.*, 2003) found that the Internet mode produced data of comparable quality to the telephone mode.

[12] Our survey was sent to 48,110 households, of which 20,287 responded (a 42% response rate). The final sample size is smaller due to screening and survey non-completion.

[13] A quarter is missing for an individual if they did not drive a 1999 or newer model year vehicle that quarter.

earliest quarters, for which recall bias may be worst, and the last quarter, for which overcounting of accidents may be present.[14]

Given the potentially sensitive nature of questions concerning phone use while driving, we designed the survey with an eye toward eliciting candid responses. The respondents answered whether they had an accident in the past two years at the beginning of the survey in a way that gave them no reason to believe the survey was about cell phones or accidents.[15] Questions about cell phone usage while driving were asked before collecting specific information about accidents for those who had them. To increase the likelihood of truthful reporting, we did not give those who said they had an accident an option to reverse their answer after answering the cell phone questions.

The variable for intensity of cell phone usage is taken from the question "how many minutes of use did you typically talk on the phone while driving", where the categories are none, 0-15 minutes per week, 2-20 minutes per day, 20-60 minutes per day, or more than one hour per day.[16] This question is asked separately for each year, but the usage variable can also vary quarter to quarter if the driver began or stopped using a cell phone during the year.[17] Much of the variation in this variable is across individuals, however: the "between" standard deviation for the 0-15 minutes per week indicator variable is 2.8 times the "within" standard deviation, for example. The other usage variable of interest is whether the driver uses a hands-free device.

Other variables collected in the survey include the vehicle driven each quarter, driving patterns, annual miles driven, duration of typical commute, and whether most driving is rural vs. urban and freeway vs. surface street. We use these to control for other factors that can affect accident rates. For each accident reported in the two year period, we collect the quarter of occurrence and characteristics of the accident (property damage in excess of $500, injury accident, etc.). We also have demographic information for the drivers and their households,

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[14] Respondents were asked if they had any accidents "in the last two years". Given that the survey was administered in January and early February 2003, a person with an accident in January 2003 would have answered "yes" but later in the survey would have been asked to place the accident in one of the quarters of 2001 and 2002. Q4 2002 would have been the closest option.

[15] We asked the respondents if they had had 12 unrelated "life experiences" (including "get into an automobile accident in which you were the driver," "get married," and "purchase or upgrade a home computer") in the past two years.

[16] We also asked about the typical number of calls made or received; this variable is highly correlated with the minutes of use variable ($\rho = 0.84$).

[17] Because we know each quarter that the driver had a cell phone, usage while driving in quarters the driver did not have a phone is set to "none". The frequency of observation of the variables is in Table 1.

including most variables one would find in Census data. We also collected additional data from other sources, such as vehicle characteristics and quarter-specific local meteorological variables (counts of days with rainfall, snowfall, and temperatures below freezing, and average hours of light in the quarter) based on the ZIP code of the household. We use these additional variables to control for differences in vehicle safety and for driving conditions that varied over time or location.[18]

Representativeness of the Survey Sample

In this section we explore how representative our sample is of the general population, in terms of demographics, cell phone usage, and vehicular accidents. Summary statistics for the four quarter estimation sample are presented in Table 1. Given that our survey respondents pass through several levels of screening to make it into the estimation sample (e.g., they all drive late-model vehicles and are Internet users), we explore the representativeness of our sample through several means. First, note that about 68% of adults in the U.S. used the Internet at the time our survey was administered.[19] In Table 2 we compare the demographic characteristics of our estimation sample with the general population, the Internet-using population, and the survey respondent sample before screening on vehicle driven or survey completion. Our sample is representative of the age and regional distribution of the population. However, Internet users, and our sample even more so, tend to be from higher population areas and have higher incomes than average. Finally, our sample contains a disproportionate number of females: two-thirds of the respondents in our sample are female.[20] A subsample of responses from a gender-balanced panel is available,[21] which we explore below, but our main estimation strategy is to use the full unbalanced sample and to control for gender by interacting it with the main variables of interest or using single-gender samples. We also calculated survey weights (see Appendix) for use in the counterfactual exercises in Section VI.

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[18] Details on the weather variables and geocoding are in Appendix B.7. (Appendix B is available upon request).

[19] Three polls conducted in the first quarter of 2003 report Internet usage at 67% (Pew Research Center (2003a) or 68% (Council for Excellence in Government, 2003; CBS News, 2003) of adults in the U.S.

[20] Due to an error by the survey administrator, the survey offer was sent to a panel that was balanced with respect to general Internet users' demographics along many dimensions, but not on gender. The panel was balanced on age, Census division, household income and size, and market size.

[21] The survey administrator combined a subset of the first survey panel with an additional panel of male respondents that were contacted in a second survey round to create an *a priori* gender-balanced panel, from which 1,491 men and 1,750 women responded.

There are no official statistics on cell phone usage while driving. We instead compare our survey results with other recent surveys on cell phone usage (Table 3). Of our respondents, 84% have a cell phone and 73% use a cell phone while driving at least occasionally. When the survey weights are used to adjust these figures, our estimates of cell phone ownership and use while driving are 78% and 64%, respectively. Our estimates of phone use while driving are on the high end of the range found in other surveys in Table 3, which is 30% to 59%. Table 3 also reports the few external estimates of hands-free device usage that we found and compares them with our figures. We find that 28% of drivers and 44% of those who use a cell phone while driving use a hands-free device of some sort at least sometimes with their phone while driving. These figures are also higher than the external estimates. Our estimates of phone use while driving may be higher than other estimates because our question was very broad: a driver is categorized as a cell phone user if they answer anything other than "never" to the usage while driving question. Some of the other surveys lumped "rarely or never" responses together as non-users. Furthermore, given the evidence mentioned above that Internet surveys can elicit more candid answers than telephone surveys, our estimates may be higher than the others because respondents feel uncomfortable admitting usage while driving to a live questioner over the telephone.

The accident rates in our sample (5.4% per year; 6.3% per year using survey weights) are comparable to those of the general driving public in the U.S.; there is thus no evidence of underreporting of accidents.[22] The accident rates differ significantly according to whether the driver has a cell phone and whether he or she uses it while driving (see Table 4).[23] In our data, those who use the phone while driving have the highest accident rate (5.9% raw, 7.1% weighted). An intriguing finding is that those who have a cell phone but do *not* use it while driving have a lower accident rate (3.7%) in the raw data than those who do not have a cell phone at all (4.4%). This provides some evidence against dishonest reporting of phone usage while driving. If respondents who reported having an accident falsely claimed they did not use a cell phone while driving later in the survey, then we would expect the accident rate for drivers who claim not to use their phone to be higher than average, not lower.

[22] The most comprehensive collision data are from the National Highway Traffic Safety Administration (NHTSA), which calculates the collision rate for drivers in non-fatal accidents to have been 5.7% per year in 2002. NHTSA data are meant to be comprehensive (NHTSA, 2004, table 63), but because some accidents are not reported are an undercount.

Table 4 also shows that drivers who use the phone more while driving have higher accident rates (except for the highest category of use). Accident rates also differ by amount of hands-free device usage (accident rates are lower if hands-free devices are always used instead of just sometimes used) and gender (men have more accidents). These accident rates do not control for other factors. For example, drivers who use hands-free devices have higher accident rates than those who do not, but this is probably because the latter group drives less. Without controlling for miles traveled (and other factors) we cannot isolate the impact of hands-free device usage. The model-based estimations in the next section are designed to control for other factors and to test the hypotheses of selection effects and heterogeneous impacts of cell phone use.

4. <u>Estimations</u>

The Model

The estimations we perform are based on various special cases and extensions of an econometric model for panel data on accidents, cell phone usage, and vehicle safety characteristics. Let $i = 1, \ldots, N$ index individuals and $t = 1, \ldots, T$ index periods. Denote the number of collisions in period t for individual i as y_{1it} , the amount of cell phone usage as y_{2it} , and a safety characteristic of the individual's primary vehicle as y_{3it} . We model y_{1it} as a count variable, y_{2it} as a vector of binary indicator variables representing an ordered discrete variable, and y_{3it} as either a vector of indicator variables or a scalar continuous variable, depending on the specification. Conditional on covariates (x_{it}, y_{2it}, y_{3it}) and a random effect v_{it}, the number of accidents, y_{1it}, follows the Poisson distribution with mean

$$\mathrm{E}(y_{1it}|x_{it}, y_{2it}, y_{3it}, v_{it}) = s \exp(\beta' x_{it} + \gamma' y_{2it} + \delta' y_{3it}) v_{it} \tag{1}$$

$$v_{it} = \exp(\alpha_i + \varepsilon_{it}) \tag{2}$$

[23] Pearson's chi-square equality-of-proportions test has a two-sided p-value of 0.012.

where s is 0.25, the period length in years,[24] x_{it} is a vector of exogenous variables, and v_{it} is an unobserved multiplicative effect composed of an individual-specific effect α_i and an i.i.d. shock ε_{it}. The mixing term v_{it} induces heterogeneity into the mean accident rate even for individuals who are observably similar. We assume α_i is independent of ε_{it} but (unlike in typical random effect models) may be correlated with y_{2it} and y_{3it}; in other words, cell phone usage and vehicle safety may be endogenous. Depending on the specification, y_{2it} represents either average cell phone usage minutes while driving (none, low usage [0-15 minutes per week], medium usage [2-20 minutes per day], high usage [20-60 minutes per day], or very high usage [more than one hour per day]), or usage of a hands-free device while driving (never, sometimes, all the time). Thus, γ, the coefficient on the cell phone usage variable, is of primary interest. The vehicle variable y_{3it} is a characteristic affecting safety, such as the category of the vehicle (minivan, SUV, luxury car, etc.) or a continuous measure such as vehicle weight. Below, we also consider a random coefficient version of (1) in which the cell phone coefficient vector γ varies across individuals.

Given the multiplicative specification in (1), coefficients are easiest to interpret when exponentiated, which yields the "incident rate ratio" (IRR) for the variable. For example, if the driver is female, she has $\exp(\beta_{Female})$ times as many expected accidents as does a male driver. Thus, variables that are correlated with higher accident rates have IRR's greater than one.

Reduced Form Estimations

Our first estimation is of a reduced form (RF) model. The RF model is Poisson regression performed on the pooled data, which is equivalent to maximum likelihood estimation (MLE) of (1) assuming that y_{1it} in (1) follows a Poisson distribution and that $v_{it} = 1$ (*i.e.*, that there is no individual-specific effect α_i or heterogeneity term ε_{it} in the mean accident rate). As is typical with pooled estimators of this sort, if either α_i or ε_{it} is present, or if there is correlation of any other kind among an individual's observations, then RF still yields consistent estimates of

[24] It is common in vehicle accident studies to perform all analysis on the accident rate per vehicle mile traveled (VMT). In terms of equation (1), this would mean replacing time with VMT as our measure of risk exposure. Using VMT as the exposure measure is equivalent to including log VMT as an explanatory variable in equation (1) and restricting the coefficient to one. Given that individuals may not be able to accurately report their VMT, we instead include it (measured for the quarter as reported annual VMT divided by four) as an explanatory variable but leave its coefficient unrestricted. The *1995 Nationwide Personal Transportation Survey* performed by the Federal Highway Administration found that the correlation between self-reported and odometer-measured vehicular mileage was 0.11. Our own exploration of the NPTS data (see Appendix B.6) confirms that drivers make systematic errors when self-reporting mileage.

the coefficients in model (1)-(2) (as long as y_{2it} or y_{3it} are not endogenous) but is no longer MLE.[25] The RF model does not yield consistent estimates if y_{2it} or y_{3it} is correlated with the individual-specific effect α_i. In this section, therefore, we assume that cell phone usage and vehicle choice are exogenous—an assumption we explore and reject in the next section. Despite the suspect assumption of exogeneity, the RF estimations in this section reveal correlations in the data and provide a useful baseline for more general models that correct for endogeneity.

In the first specification, RF1 in Table 5, we include only the cell phone usage and hands-free variables (along with a full set of quarter and state dummy variables included in all regressions). The cell phone usage dummy variables are coded (here and in all subsequent models) so that the coefficient of a usage category represents the incremental risk over having a cell phone but not using it while driving. Thus if cell phone usage is not correlated with accident rates, the IRRs for all the usage categories would be 1.0. The estimated usage IRRs are in fact all greater than one. The effects are significant, and the associated increase in accident risk is 1.5 times to 2.8 times, rising with the amount of usage.[26] The IRR for not having a phone at all is (insignificantly) higher than one, reflecting the same pattern shown in Table 4. The average risk multiplier in the sample, conditional on cell phone usage (weighted by fraction of drivers in each phone and hands-free device usage category), is 1.7. This risk multiplier cannot be compared directly to RT's risk multiple of 4.3; we defer comparing the magnitude of our results with RT's until section V.[27] The IRR for always using a hands-free device is 0.73, implying use is associated with 27% reduction in accident risk.

Given the gender imbalance in our main survey sample, we are interested in exploring differences in the cell phone effects between men and women. In Table 6, we present the results from four estimations that allow the cell phone effects to differ by gender. The first, RF2, is the same as RF1 except for the gender-specific cell phone coefficients. Although the weighted average IRR for cell phone users, 1.6, is about the same as in RF1, splitting the IRRs by gender

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[25] In this case the RF estimation is pseudo maximum likelihood (see section 3.2.3 of Cameron and Trivedi (1998)). We report standard errors robust to the presence of ε_{it} and α_i.

[26] The coefficient for CellMinsVHi is slightly lower than that for CellMinsHi, but the difference is not statistically significant (p-value = 0.27).

[27] Our IRRs are incomparable to RT's figure for two reasons. First, RT examine minor accidents only (*i.e.*, property damage). Second, our risk multiplier implies that an individual who uses a cell phone while driving has an average of 1.7 times as many accidents during a quarter as he would if he did not use his phone while driving; in RT's case the risk multiplier implies that the *instantaneous* accident risk for the individual is 4.3 times as high when using a cell phone as when not.

reveals that the women's cell phone effects in RF2 are significantly higher than the men's.[28] The men's effects are generally statistically insignificant, perhaps due to the relatively smaller number of men in the sample. RT also found that cell phone usage by women appears to be riskier than usage by men.[29] Similarly, the only hands-free device IRR that is significant is for the women.

There are additional factors that may influence accident risk. If these factors are not accounted for, the cell phone usage coefficients may be biased. For example, a driver may feel invulnerable when driving a large vehicle and be more likely to engage in distracting behaviors like using the phone. If large vehicles have higher accident rates than other cars, then not controlling for vehicle choice could result in spuriously high cell phone usage coefficients. We include several covariates such as weather and driving variables in specification RF3. Because the vehicle safety variable, y_3 (a vector of indicators for vehicle type: SUV, minivan, etc.), is not available for 5% of the sample we include it in a separate estimation, RF4. In RF3, the magnitudes of the cell phone effects are smaller than in RF2 and only the two highest usage categories for the women remain significant at the 5% level. The weighted average IRR for cell phone users is 1.1, lower than before, which indicates that some of the correlation between usage and accidents found in RF1 and RF2 is due to omitted variables such as miles driven. The "always use hands-free" variable is still significantly correlated with lower accident risk for women. Many of the additional covariates also have significant effects. Women and married drivers have lower accident risk. Age has a U-shaped effect, with the minimum accident risk occurring around age 55.[30] Full time employment, higher annual mileage, and longer commuting time are all correlated with increased accident risk. More daylight hours and driving mainly on rural roads are correlated with decreased accident risk. The plausibility of these results lends credence to the survey data. The weather variables generally show no significant effects, perhaps because they reflect average conditions in the quarter rather than precisely at the time of the accident. In estimation RF3, the addition of the vehicular controls increases the cell phone

[28] A Wald test of the cell phone and hands-free effects rejects the null hypothesis of equal coefficients between the sexes in estimations RF2 and RF3 at the 5% level. The same test for RF4 has a p-value of 0.052.

[29] Redelmeier and Tibshirani (1997) estimated a multiple on accident risk from using a cell phone while driving of 4.1 for men and 4.8 for women. As previously discussed, the magnitudes of their figures are not directly comparable to ours.

[30] Lower accident rates for women and a U-shaped age pattern are also evident in official accident statistics (NHTSA, 2004).

effects a small amount for the women. The coefficients for the other covariates are generally similar to those in RF2.

We also estimate models with a host of alternative samples of the data, other dependent and explanatory variables, and weighted estimations.[31] The main alternative sample for estimation is the gender-balanced sample (RF5 in Table 6).[32] The cell phone effects rise for the women, but because the IRR for females is lower than before, the net impact of the women's usage IRRs is roughly the same magnitude as in RF3 and RF4. Other samples include using all quarters of data and dropping various outliers. None of these alternatives leads to starkly different results (the results of these and other alternative estimations are included in Appendix B.13).[33] We also experimented with weighted estimations using the survey weights we constructed. Under the maintained assumptions of the pooled Poisson model (in particular, correct specification of the conditional mean, constant cell phone effects, and exogeneity of covariates), weighting is not needed for consistency of the estimates. However, when coefficients actually vary across individuals, weighting the data can bring the estimates more in line with the average coefficient values in the population. The cell phone coefficients display the same general pattern as in RF3, but are smaller in magnitude with larger standard errors.

The following three points summarize the results from the RF estimations. First, the significance and plausible direction of the effects for many of the covariates give us confidence in the veracity of our survey data. Second, in our sample more phone usage while driving is associated with higher accident risk for women. Third, use of hands-free devices is correlated with lower accident risk, at least for women. If the association is causal, the growing movement to ban usage while driving unless a hands-free device is used may be justified. However, this result depends on the exogeneity of hands-free usage, a suspect assumption that we reject in the

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[31] We perform these alternative estimations with the RF model instead of the MLE models in the next section because the RF model can be estimated much quicker than the MLE models.

[32] Refer to the discussion in section III on the gender imbalance in the main sample.

[33] Other subsamples included dropping any individual with implausibly high mileage, dropping the 79 individuals that required resurveying due to a survey programming error, and dropping the two individuals who had right censoring in the number of accidents reported for a quarter (if the individual had more than three accidents, we asked for the quarter of the latest three only). Alternative dependent variables we try for y_1 include various subcategories of accidents: accidents resulting in property damage only, injury accidents, accidents requiring hospitalization, accidents requiring medical treatment, and accidents resulting in someone taken away by ambulance. The cell phone coefficients are not significant in these models, due to the lack of precision caused by the small number of accidents in the sample in any subcategory. Alternative explanatory variables include race, income, and vehicle characteristics such as four wheel drive, traction control, and antilock brakes, and usage/age interactions. No variable that we add is significant or substantially changes the cell phone effects from those

following two subsections. The estimated effects on accidents of cell phone usage are generally robust to alternative specifications and estimation subsamples. Therefore, in the following estimations we restrict attention to the main specification (RF4) and sample.

The RF models are not robust to endogeneity of cell phone use and vehicle safety choice, and we do not treat the results here as having significance for policy. We now turn to models that allow us to investigate our two hypotheses discussed in the introduction. Given that there are statistically significant differences in the cell phone effects between men and women in our sample, we allow these coefficients to differ in subsequent estimations.

Multiple-Equation Models for Heterogeneity and Endogeneity

This section contains our preferred estimations, in which we explicitly model the endogeneity of the use of cell phones and hands-free devices while driving in a parametric multiple-equation system. We also consider a model that estimates whether the cell phone effects are heterogeneous across individuals, even after controlling for observables such as gender. Our three equation model adds equations for cell phone usage and car weight to accident equation (1) (repeated here as (3)), to allow usage and car choice to be endogenous:

$$y_{1it}|x_{1it}, y_{2it}, y_{3it}, v_t \sim Poisson\ (\text{mean} = s\ \exp(\beta_1'x_{1it} + \gamma' y_{2it} + \delta y_{3it})v_i\ ) \qquad (3)$$

$$y^*_{2it} = \beta_2' x_{2it} + u_{2it} \qquad (4)$$

$$y_{3it} = \beta_3' x_{3it} + u_{3it} \qquad (5)$$

We use the notation from above. Expression (3) is the equation for the quarterly accident counts. Equation (4) is for cell phone usage. We explore two definitions of y_2 in this section: minutes of use while driving and usage of a hands-free device. Because usage levels are discrete, we impose the ordered probit observation rule: instead of observing the latent, normally-distributed y^*_2 in (4), we observe y_2, which takes one of K discrete values. Each value of y_2 represents a different class of cell phone usage while driving. In one set of estimations, the classes are the five minutes–of-usage categories; thus K = 5. With this definition, equation (4) is present only

reported in Table 6.

for those individuals who have a cell phone. In the other set of estimations, the cell phone usage classes for y_2 are the amount of hands-free device usage while driving: never, sometimes, and all the time. Here K = 3, and (4) is present only for those individuals who both have a cell phone and use it while driving.[34] For $k = 0,1,\ldots,K$-1, the observation rule is

$$y_{2it} = k \text{ if } \kappa_k < y^*_{2it} \leq \kappa_{k+1} \tag{6}$$

By convention, $\kappa_0 = -\infty$, $\kappa_0 = 0$, and $\kappa_K = \infty$.

The third equation, (5), is for log car weight, where y_3 is a fully observed normal random variable. We use log vehicle weight for y_3 as a single characteristic to control for vehicle safety choice instead of using the vehicle categories as in RF3 and RF6 for four reasons. First, it is infeasible to include the whole set of vehicle indicators used above; each indicator would add another equation to the system. Second, car weight has a significant coefficient in the accident equation (if the vehicle category indicators are not present) in RF estimations. Third, there is evidence that heavier cars are safer for their occupants in a crash than are lighter cars, so that endogenous safety choices may be embodied in car weight.[35] Finally, car weight has been found in external data sets to be highly correlated with (and thus to control for) other vehicle safety variables such as antilock brakes and four wheel drive.[36]

The errors in (3)-(5) are specified as:

$$v_i = \exp(\alpha_{1i}) \tag{7}$$

$$u_{2it} = \alpha_{2i} + \varepsilon_{2it} \tag{8}$$

$$u_{3it} = \alpha_{3i} + \varepsilon_{3it}$$

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[34] In other words, we assume that there is no selection bias caused by the choice to have a phone or not, and that selectivity problems arise with choice of hands-free usage only when the individual already uses a phone while driving.

[35] A recent federal study concludes that the heavier the vehicle, the lower the risk of a fatality to any occupant in a crash, for all but the heaviest vehicles (Kahane, 2003). These results were widely reported in the press. Summarizing other studies on vehicle weight and crash safety, the Los Angeles Times (February 18, 2003, part 3, p.1) concluded that despite conflicting evidence on heavy vehicles and *overall* fatalities, "[n]o expert contends that, all other things being equal, heavier vehicles aren't safer for their passengers than are light ones." The association between vehicle weight and crash safety has been known for decades; Crandall and Graham (1989) cite many such studies, dating back to 1977. Recent studies indicate that heavier vehicles may crash more, negating their greater safety given a crash (Gayer, 2004). However, for vehicle weight to be a good proxy for vehicle safety choice, it is only required that car buyers *believe* that heavier cars are safer.

where the α are correlated across equations but the ε are not. The random effects u_{it} are composed of individual-specific components α_i and idiosyncratic shocks ε_{it} as described above for model (1)-(2). Because there is no evidence of heterogeneity in the mean accident rates after controlling for α_{1i} and covariates, we do not include an ε_{1it} in (7) (*i.e.*, we set ε_{it} = 0 in (2)).[37] The vector (ε_{2it} , ε_{3it}) is normally distributed with zero mean and covariance matrix

$$\Sigma_\varepsilon = \begin{bmatrix} 1 & 0 \\ 0 & \tau^2 \end{bmatrix}$$

and $E(\varepsilon_{itk}\varepsilon_{jsl}) = 0$ if $i \neq j$, $t \neq s$, or $k \neq l$.[38] The individual-specific random effect $\alpha_i = (\alpha_{1i}, \alpha_{2i}, \alpha_{3i})$ is normally distributed with mean $(-\sigma_1^2/2, 0, 0)$ and covariance matrix

$$\Sigma_\alpha = \begin{bmatrix} \sigma_1^2 & \rho_{12}\sigma_1\sigma_2 & \rho_{13}\sigma_1\sigma_3 \\ \rho_{12}\sigma_1\sigma_2 & \sigma_2^2 & \rho_{23}\sigma_2\sigma_3 \\ \rho_{13}\sigma_1\sigma_3 & \rho_{23}\sigma_2\sigma_3 & \sigma_3^2 \end{bmatrix}$$

and is assumed to be independent of x.[39] The α are independent of ε for all individuals and periods. With this specification, y_2 is endogenous in (3) if $\rho_{12} \neq 0$ and y_3 is endogenous if $\rho_{13} \neq 0$. In addition to the coefficients of interest (β_1, γ, δ), the model requires estimation of nuisance parameters (β_2, β_3, σ_1^2, σ_2^2, σ_3^2, ρ_{12}, ρ_{13}, ρ_{23}, τ, κ).[40] We estimate the model by MLE. Given the parametric assumptions it is possible to find a closed-form expression for the density of all quarters of an individual's observations on (y_{1it},y_{2it},y_{3it}) conditional on v_i, denoted $f_i(y_i|v_i)$. The likelihood for MLE is then

[36] See, *e.g.*, Kahane (2003), pp. 65 and 126.

[37] Formally, we test and fail to reject that $y_{1it}|x_{1it}$, y_{2it} , y_{3it} is equidisperse relative to the variance implied by the model with v_i specified as in (7). We use tests inspired by the overdispersion tests for simpler models from Cameron and Trivedi (1998), sec. 3.4. If there is no overdispersion in y_{1it} after including individual-specific random effects, then an additional heterogeneity term ε_{1it} is not needed. Furthermore, if ε_{1it} is added to the model, the estimate of its variance is nearly zero. See Appendix B.10 for details of the tests.

[38] The variance of ε_2 is fixed for identification in the ordered probit equation.

[39] The mean of α_1 is adjusted so that $E[\exp(\alpha_{1i})|x_{1it},y_{2it},y_{3it}] = 1$.

[40] When y_2 takes the definition of hands-free device usage, there is one minor modification to the above. In this case y_2 does not vary over time for an individual, so α_2 and ε_2 are redundant and ε_2 is dropped from the model.

$$\ln L = \sum_{i=1}^{N} \log \int_0^\infty f_i\left(y_i \mid v_i\right) dF(v) \tag{9}$$

where $F(v)$ is the lognormal density of v. The integral is evaluated numerically and MLE proceeds as usual; see Appendix for the likelihood function and details.[41] We have not found this model developed elsewhere in the literature, but we use standard techniques to solve for the likelihood of multiple equation models for mixed continuous and discrete variables.

The covariates for the accident equation (3) are the same as in estimation RF3. We use two sets of covariates for x_2 in (4), the cell phone usage equation. The "small set" contains several variables also included in x_1 (age, VMT, commute length, drive mostly on freeways, employment status, gender, and marital status), and some that are not. These latter "instruments" are variables that potentially affect prices, quality, and competition in the mobile phone service market.[42] When competition is stronger, mobile phone service providers may offer lower prices, higher service quality, and may be more likely to offer hands-free devices with subscription, all of which may be correlated with minutes of use and hands-free device usage. These mobile phone market variables are the number of subscribers per capita in the state (in levels and squares), the size of the market (MSA) the individual lives in, the cellular antenna site density within 25 miles of the household,[43] and two industry cost shifters: the average wage in the cellular industry,[44] and the average electricity price in the state. The small set also includes two variables related to the willingness of the individual to adopt modern communications technology: indicators for whether the household has a VCR and cable TV service.

The second, larger set of covariates for x_2 includes the small set plus additional demographic variables such as income that may influence phone and hands-free device usage.[45] None of these variables appear in x_1, and their exclusion may be harder to defend than for the

[41] This estimation problem is also a candidate for simulated maximum likelihood. However, given that expectation need be taken over a univariate random variable only, numerical integration of the likelihood via Gauss-Hermite quadrature is tractable and yields more precise estimates than simulation.

[42] Unlike linear systems of equations, there are no exclusion restrictions for x_1 in (3); the Poisson parametric assumption alone identifies the coefficients in (3). Thus x_2 and x_3 need not contain any variables not found in x_1, even when y_2 and y_3 are endogenous in (3). Due to the tenuous nature of identification solely through functional form, we do not rely on this to identify the system but instead use the instruments discussed here.

[43] This variable was constructed by taking the number of cellular antenna sites within 25 miles of the household's location (as proxied by their five digit ZIP code centroid), and dividing by the population of all Census tracts that overlap with that circle. The antenna data are from the FCC's cellular tower registration database.

[44] The county average is used when available; the state average is used when not. Data are from the BLS.

[45] The variables new to the large set are racial and ethnicity dummies, employment status, whether she lives in a condominium, whether she is a female head of household, her education level, whether she took a major vacation,

excluded variables in the small set. For example, although there are no statistically significant impacts of income on accident rates once other variables are included, it may be that income belongs in the accident equation, if (for example) wealthier drivers buy cars that are safer in ways for which we do not adequately control.

For x_3 in (5), the car weight equation, we use age, marital status, commute length, and two variables not included in x_1: gas price in levels and squares.[46] In addition to this small set of covariates, we also use a larger set of covariates for x_3.[47]

To test for the endogeneity of cell phone use and car weight in the accident equation, we first define y_2 to be cell phone usage minutes while driving. Based on estimations for various samples (men and women separately and together) and using both the small and large sets of instruments, we cannot reject the hypothesis that there is no endogeneity in the accident equation. The endogeneity parameters ρ_{12} and ρ_{13} are estimated to be negligible in magnitude and statistically insignificant. This is in contrast to alternative IV estimations we discuss below, in which there is evidence that usage in endogenous. The estimated cell phone effects differ little from the corresponding RF estimations and we do not report the results here.

However, when we switch the role of the second equation and let it represent usage of a hands-free device, we find evidence of endogeneity. In this model, hands-free devices are treated as endogenous in the accident equation, and cell usage minutes (given the results just described) are taken to be exogenous. Use of a hands-free device may be endogenous if, for example, drivers that are inherently more careless are also less likely to use a headset while speaking on the phone. Estimation results are presented in Table 7.[48] For each instrument set results are presented for the combined samples (estimations ML1-2) and the single gender sample (ML3-6).

Of most interest from the estimations are three results. First, the correlation between the accident equation and the hands-free equation, ρ_{12}, is large and negative in every specification we tried, regardless of the instrument set or sample used. A finding of negative correlation between α_1 and α_2 implies that unobserved factors that make an individual more likely to use a

got married, and purchased or upgraded a computer in the last two years, and the household size.

[46] After controlling for miles traveled, the price of gas should not affect the accident rate.

[47] The variables new to the large set are racial and ethnicity dummies, employment status, home ownership, and the household size. They are significant in OLS estimations with log car weight as the dependent variable.

[48] Coefficients for the cell phone and car weight equations are not reported in Table 7, but generally had plausible signs. See Table B.13.5 in Appendix B.13.

hands-free device also make the individual a safer driver, independent of any causal effect of cell phone usage mode. Results regarding the statistical significance of the negative correlation vary across specifications, but generally we reject the hypothesis that use of hands-free devices is exogenous.[49]

The second fact of interest is that there is no evidence of significant reductions in accidents from the use of hands-free devices, as opposed to the large effects found in the reduced form specifications in which ρ_{12} is constrained to be zero. Finding that hands-free devices have no significant impact on accidents is in accord with many other field and laboratory studies (e.g., RT; Haigney and Taylor, 1999; Crawford *et al.*, 2001; Strayer and Johnston, 2001; and Strayer, Drews, and Johnston, 2003).[50] In fact, the IRRs for the hands-free variables are all greater than one. None of these IRRs is statistically significant, and we therefore do not want to overinterpret this result. However, it may be that some aspects of hands-free device usage lead to greater driver inattention.[51]

Third, we find that when hands-free usage is treated as endogenous, the effects of minutes of cell phone usage while driving are smaller than in the RF models. For example, in specifications ML1 and ML2 (the estimations including both genders), the IRRs for the CellMins variables are lower for each variable than in RF1. The same is true when comparing the single gender estimations to the gender-specific coefficients in RF2-RF5. This is true despite the fact that minutes of usage are treated as exogenous in both the RF and the ML models.

Of less importance for our main investigation in this paper, but interesting in its own right, is that the correlation between the accident equation and the vehicle safety equation is generally estimated to be positive, implying that drivers choosing heavier cars have a higher baseline accident rate to begin with.

[49] For the combined gender sample, *t* tests of $\rho_{12} = 0$ have low *p*-values (below 0.01) in ML2 but not in ML1. The LR statistics testing the full models vs. their restricted counterpart lacking heterogeneity and correlation (see Appendix A.4 for details) have *p*-values less than 0.001 for all models. For the male sample, *t* tests of $\rho_{12} = 0$ have low *p*-values (below 0.001) and the LR statistics have *p*-values less than 0.001. For the female sample, the *t* tests do not have small *p*-values but the LR statistics do.

[50] In addition, Hahn and Dudley (2002) review the numerous studies comparing hands-free to handheld phones and conclude that while the literature is not unanimous, the general finding is that the risk posed by dialing is small compared to the risks associated with conversation, and that conversation risks are unaffected by phone type.

[51] For example, a consumer review of several hands-free devices found that fumbling with putting on a headset when answering a call and the poor audio quality of some hands-free phones may be more distracting than using a handset (Susan Stellin, "Hands-Free Calling Options for the Road," *New York Times*, July 26, 2001, p.G9).

We turn now to our second hypothesis, which is that identical amounts of cell phone use affect accident risk differently across people, even after controlling for observables. To keep the model simple, we treat cell phone use and vehicle choice as exogenous and drop equations (4) and (5). Using the same notation as above, the accident equation is modified to be:

$$y_{1it}|x_{1it}, y_{2it}, y_{3it}, v_i, \eta_i \sim Poisson(\text{mean} = s\ \exp(\beta_1' x_{1it} + \tilde{\gamma}_i' y_{2it} + \delta y_{3it}) v_i) \tag{10}$$

where $\tilde{\gamma}_i$ is a random coefficient, possibly correlated with the individual-specific random effect v_i :

$$\tilde{\gamma}_i = \bar{\gamma} + \eta_i \tag{11}$$

$$v_i = \exp(\alpha_i)$$

In (11), $\bar{\gamma}$ is the mean coefficient vector and η_i is a scalar that represents driver i's departure from the average cell phone coefficients. Because η_i is scalar, the randomness in the usage effects is symmetric across usage classes. For example, if a driver has η_i = log(1.1) then his usage IRR for all categories of cell phone minutes is 10% higher than the average IRR, exp($\bar{\gamma}$). This assumption is made for convenience, to keep the dimension of the numerical integration of the likelihood manageable, and because it parallels the way the multiplicative random effect v_i enters the model. The (α_i, η_i) are assumed to be independent across individuals and normally distributed with covariance matrix

$$\Sigma = \begin{bmatrix} \sigma^2 & \rho\sigma\omega \\ \rho\sigma\omega & \omega^2 \end{bmatrix} \tag{12}$$

The mean accident rate in (10) can be rewritten as

$$\lambda_{it} = s\ \exp(\beta_1' x_{1it} + \bar{\gamma}' y_{2it} + \delta y_{3it}) \zeta_{it} \tag{13}$$

where the random terms have been collected into a heteroskedastic, unit mean, composite error $\zeta_{it} = \exp(\alpha_i + \eta_i d_{it})$, where d_{it} is an indicator that usage is not in the excluded category.[52] The

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We assume that E(α_i) = $-\sigma^2/2$ and E(η_i) = $-\omega^2/2 - \rho\sigma\omega$ to ensure that E(ζ) = 1 and that the constant in β_1 is density of all quarters of an individual's observations on y_1 conditional on α_i and η_i is available in closed form; evaluating the likelihood for MLE requires numerical integration as above. To our knowledge, ours is the first application of a random coefficient panel Poisson model in the literature. The likelihood is presented in the Appendix.

The results of MLE for this model for the combined-gender sample (labeled RC1) and the women-only sample (RC2) are presented in Table 8.[53] In both samples, the likelihood is maximized with $\sigma^2 = 0$ (and thus ρ is neither interesting nor identified). In RC1, there is no convincing evidence of heterogeneity in the cell phone effects; neither a *t* test nor an LR test rejects the hypothesis that $\omega = 0$ (*i.e.*, that there is no randomness in the usage coefficients).[54] The lack of significance may be due to the smaller number of observations in the four-quarter subsample; when all quarters are used (results not reported),[55] $\hat{\sigma}^2 > 0$ and the LR test does reject that $\sigma^2 = \omega = 0$. There is more evidence of heterogeneity in the usage effects in RC2. For the women, $\hat{\omega}$ is significant, whether tested by a *t*- or LR test.

The means of the cell phone usage coefficients, $\bar{\gamma}$, are not far from the analogous reduced form estimations above. However, the standard deviation of the random coefficients is quite large: $\hat{\omega}$ = 0.49 for the combined sample and 0.71 for the women. This would give the IRR for CellMinsLow, for example, a 95% confidence interval of (0.45, 3.07) from RC1 and (0.35, 5.58) from RC2. Note that these wide intervals are not due to estimation error but the intrinsic variability of the random coefficient. Thus, there appears to be wide variation across individuals in the impact of identical amounts of phone use on accidents.

If indeed the contribution of cell phone use to accident risk is so heterogeneous even after controlling for observables, it suggests that methods using only a sample of drivers who had accidents will overestimate the average cell phone effects in the population. Within each usage class, drivers with the highest realized values of the phone usage coefficients $\tilde{\gamma}$ are most likely

identified. Since the conditional variance of ζ is $\sigma^2 + 2\rho\omega\sigma d + \omega^2 d^2$, there is an identification problem when y_2 consists of a set of zero-one indicator variables for the usage categories. In that case $d^2 = d_2$ and only σ^2 and $(2\rho\omega\sigma + \omega^2)$ are identified. Given that the MLE of σ^2 turns out to be zero, however this additional complication is moot.

[53] Results for the men-only sample are not reported; both the heterogeneity in the baseline accident rate (σ^2) and the s.d. of the random coefficient (ω) were negligible. MLE requires two-dimensional Gauss-Hermite quadrature to integrate ε and u out of the likelihood (see Appendix A.3 for details).

[54] The LR statistic has a non-standard distribution because ω is on the boundary of the parameter space under the null hypothesis (Self and Liang, 1987).

[55] See Table

to have accidents. The expected value of η (and thus $\tilde{\gamma}$) given that the driver had an accident can be calculated using Bayes' rule. For the combined gender estimation, the cell phone usage IRR is 5.6% higher on average within each usage category conditional on having an accident than the population mean IRR; for the women-only estimation, the cell phone effects are 13.6% higher conditional on having an accident. Thus, a case-crossover estimation would overestimate the true average cell phone effects in the population, and by *more* than the above amounts. This is because RT estimate an instantaneous risk multiple from phone usage, and our IRRs, on the other hand, reflect changes in total risk, averaged over time when the phone is in use and when it is not. To be precise, in our model the percentage change in expected accidents in a time period from cell phone use is $IRR - 1$. The same in terms of RT's relative risk (RR) is $f(RR - 1)$, where f is the fraction of driving time spent on the phone.[56] Equating these leads to the conversion formula

$$RR = \frac{IRR - 1}{f} + 1 \tag{14}$$

We use equation (14) with Cohen and Graham's "central" estimate of f of 2% and the average IRR from our RC models to analyze how much RT's estimates are overstated. The results, in Table 9, show that RT's relative risk estimate of 4.3 is overstated by 36.3%. Similarly, RT's estimate of 4.8 for women is overstated by 36.0%.

Alternative Estimations

In this final estimation section we briefly mention alternative estimations we tried: instrumental variables (IV) and fixed effects models. Each is an alternative method to the multiple-equation models for endogeneity to obtain consistent estimates of the cell phone usage effects. These methods do not, however, incorporate random coefficients. In the IV model we treat the cell phone and hands-free device usage variables and car weight as endogenous and instrument for them with the variables described for the multiple-equation models.[57] For the sake of brevity we mention here only the main results from the estimations; details on the method are in the

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[56] This expression is equation (2) in Cohen and Graham (2003).

[57] To assess the strength of the instruments we used two tests found in the weak instrument literature: Stock and Staiger's (1997) F test (variable by variable), and Stock and Yogo's (2002) test for multiple endogenous regressors (see Appendix B.9). The results from the F test show that weak instruments may be of concern, particularly with the small set. Stock and Yogo's (2002) test rejects the null hypothesis of weak instruments for the large set but not the small set. These tests are only meant to be suggestive, since are not designed for models with multiplicative means

Appendix. Linear IV methods, which assume additive means and errors, are not appropriate for our specification for the accident equation (1), which has a multiplicative mean and error. We instead follow Windmeijer and Santos Silva (1997) and use GMM based on moment conditions appropriate for (1). The generalized method of moments (GMM) estimator we use is consistent even if accidents do not have the Poisson distribution or (y_2, y_3) are endogenous, as long as the specification of the mean is correct and the instruments are valid. Following our treatment in the RF model we pool the data.

In Table 10, we first present the GMM estimates when all the variables in (1) are taken to be exogenous (GMM1). The estimated IRRs for the x_1 covariates are similar to those in RF3 and RF4 above and we omit them from the table. The cell phone effects in GMM1 are similar to those in RF4, the analogous parametric specification, although dropping the Poisson assumption leads to some loss of precision of the estimates. When the phone and hands-free usage and vehicle weight variables are treated as endogenous (estimations GMM2, which uses the small set of instruments, and GMM3, which uses the large set of instruments, in Table 10), all statistical significance of the impacts of the cell phone minutes of usage variables goes away.[58] Furthermore, the magnitude of the female cell phone effects—the ones that appear large in the RF, ML, and RC models—fall in GMM2 and GMM3 to modest levels (with the exception of the highest use category in GMM2). As in the ML models, the large reduction in accidents due to the use of hands-free devices by women implied in the RF models disappears—the hands-free IRRs are not significant and the direction of the effect even switches. These IV results confirm the findings from the multiple-equation models that selection is present and that correcting for the endogeneity of cell phone and hands-free use removes all certainty about the impact of usage on accidents (in the sense of statistical significance).[59] These IV results also show that our conclusions are not dependent on the particular distributional assumptions chosen for the ML models.

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and errors. Nevertheless, we note that our instruments are not ideal and proceed with that caution in mind.

[58] To aid convergence in these models, the top two usage categories (which have relatively few drivers in them) were combined for each gender.

[59] To test whether the cell phone and car weight variables are indeed endogenous we performed Hausman tests. Each model is tested against a pooled Poisson MLE. The Hausman test rejects the null hypothesis that (y_2, y_3) are exogenous if all coefficients in the model are tested; we fail to reject exogeneity if only the suspect coefficients—those for y_2 and y_3–are tested. The failure to reject exogeneity in the latter case is due to the large standards errors on the coefficients of the instrumented variables.

As a final alternative estimation we explored a fixed effects (FE) model, the closest model to the case-crossover method that is estimable with our data.[60] FE models (Hausman, Hall, and Griliches, 1984) for count data are often attractive because they are robust to the presence of heterogeneity and endogeneity due to α_i and ε_{it} in (1), and do not require the parametric assumptions of our multiple-equation models. The disadvantage of the FE model that renders it unsuitable for our application is that (like the case-crossover model) estimates are based solely on drivers who had at least one accident. In our sample this amounts to throwing away about 90% of the data in a potentially non-random manner. Given the evidence from the previous section that the cell phone coefficients vary in the sample, FE estimates would suffer from the same upward bias we demonstrated for RT's estimates. Indeed, the average IRRs for cell phone use from FE models estimated with our data (see Appendix B.13) range from 2.6 to 4.2. These IRRs are much higher than the analogous figures from the other estimations above, which is entirely consistent with selection into the accident sample created by the RC model. There is no significant impact from usage of hands-free devices in these FE estimations.

5. <u>The Effect of Banning Cell Phone Use While Driving on Accidents</u>

As discussed in the literature review, several studies have combined RT's results with assumptions on the number of cell phone users, average phone use while driving, and miles driven to calculate the reduction in accidents from a hypothetical ban on cell phone usage while driving. Redelmeier and Weinstein (1999) calculate that a ban would result in 2% fewer collisions. Cohen and Graham (2003) calculate that a ban would result in 2-21% fewer accidents, with a central estimate of 6%.[61] We have argued above that RT's estimates are not representative of the population; if so, extrapolating them for purposes of cost-benefit analyses will overstate the number of accidents prevented by a cell phone ban. To compare the magnitude of our findings with these studies we perform similar calculations using our data. We use the survey weights to make all figures nationally representative. Because we have individual-level frequency of cell phone use, and can calculate individual-level accident risk, we perform a finely

[60] We cannot replicate RT's case-crossover analysis exactly because we do not have closely spaced point-in-time observations on cell phone usage.
[61] There are other estimates of the impact of a ban on accidents, based on police accident reports (Hahn, Tetlock, and Burnett (2000), NHTSA (1997)). These estimates are lower than those based on RT, and range from 0.003% to

tuned analysis, unlike previous analyses that based calculations on national averages and out-of-sample assumptions about accident rates and cell phone usage.

As mentioned in the discussion of Table 3, the fraction of drivers using cell phones while driving is open to question. We report figures in Table 11 based on three sets of survey weights that span the range of estimates from Table 3: a "high estimate" assuming 64% of drivers use cell phones while driving (the figure from our survey), a central estimate of 50%, and a low estimate of 30%. We assume an unrealistic 100% compliance with a ban, so that the mean accident rate for a driver after the ban is given by equation (1) with all phone usage and hands-free device indicator variables set to zero.[62] Given that compliance with an actual ban would not be perfect, our estimates are upper bounds on accident reductions.

In Table 11 we report reductions in accidents based on the ML and RC estimations. Given the significant gender differences found in our data, we use estimates that account for the gender disparity in cell phone effects. The ML estimations (rows one and two) imply accident reductions in the 1.9-4.1% range, somewhat lower than Cohen and Graham's (2003) central estimate. The random coefficient model consistent with hypothesis two (row three) yields the lowest estimated reductions: 0.9-1.9%. Note that, in contrast to previous analyses, in *all* cases the 95% confidence intervals are wide enough to include the possibility that there is no effect of a ban at all. Because the cell phone effects were insignificant in the GMM estimations, those estimations would also fail to exclude zero effect of a ban on accidents. Given that none of our models rejects the possibility that a ban on cell phone usage while driving has no effect at all on accidents, and given that the sample RT use may overstate the impacts of cell phone use, we believe that the evidence that a ban would prevent accidents is not as clear as Redelmeier and Weinstein (1999) or Cohen and Graham (2003) indicate.

6. Conclusion

Our new approach for estimating the relationship between cell phone use while driving and accidents is the first to test for selection effects and the first that allows direct estimation of

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0.03%

[62] For the mean accident calculations, v_i in (1) is replaced with its expected value (unity) in the RF and RC models. For the ML models, v_i is replaced with its expected value for the individual given (y_{2it},y_{3it}). Mean accident rates are calculated using actual covariate values for each driver and are the average over the sample.

the impact of a cell phone ban while driving. We have three key findings. First, there is evidence of selection effects. Our analysis suggests that individuals who are more likely to use hands-free devices are more careful drivers even without them. Once we correct for the endogeneity of hands-free usage, our models predict no statistically significant reduction in accidents from bans on hand-held usage, such as the bans enacted in New Jersey and New York. Second, we find that the impact of cell phone use on accidents varies across the population. In particular, even after controlling for observed driver characteristics, our random coefficient models show there is additional variation in the cell phone impacts on accidents, particularly for female drivers. Previous studies, which study accident cases only, thus suffer from selection bias, and we calculate that previous estimates of the impact of cell phone usage on risk for the population may be overstated by 36%. Finally, we explore the impact of a ban on cell phone use while driving. We cannot reject the hypothesis that a ban would have no effect on the number of accidents. Our estimates of the reduction in accidents from a ban on cell phone use while driving are both lower and less certain than some previous studies indicate.

Our study has several policy implications. First, policy makers should factor into their decisions that we find no significant impact of a cell phone ban or a hands-free requirement on accidents. Furthermore, because we find there is more uncertainty than previously suggested in the relationship between cell phone use while driving and accidents, cost-benefit analyses of proposed bans should reflect this uncertainty. We expect that including the uncertainty in the relationship between cell phone use and accidents will make the decision to regulate more difficult. Finally, however, we note that our results do not imply that nothing should be done to regulate drivers while using cell phones. Rather, our study provides additional evidence that policy makers should consider before regulating.

A natural question following from our study is how to get more precise estimates of the impact of cell phone use while driving on accidents. We see a few promising avenues, but no panaceas. One is to do larger surveys of the type done here, recognizing that such surveys have clear limitations. A second is to consider real-world policy changes and look for "natural experiments". For example, there are many jurisdictions that have implemented policy changes requiring hands-free devices. These policies could be evaluated using standard statistical methods. There are several problems that would need to be addressed in such empirical studies, however. For example, when compliance with a ban is low, then failure to find a lower accident rate after a ban may be due to a low compliance rate, a lack of causality between cell phone usage and accidents, or both.[63] Disentangling these two explanations would be complicated by the fact that the effects of a hand-held ban are likely to be small.[64] Furthermore, it may be difficult to find individual-level data for such studies, and the selection effects and varying impacts of cell phone use found in our study imply that aggregated data may mask important parts of the story.

Because cell phone use while driving is likely to increase unless it is constrained by regulation, it poses interesting challenges for researchers as well as policy makers. This paper has shown that analyzing cell phone use while driving is more complicated than some earlier studies would suggest. In essence, we have shown that selection effects and heterogeneity among drivers are likely to be important, and should not be ignored in a policy setting. Exactly how important is less clear. What is clear is that more work will be needed on various aspects of this problem to develop policies that actually reduce accidents at a reasonable social cost.

[63] Compliance with the ban on hand-held cell phone usage in New York State appears to be low, for example. As of March 2003 (two years after the ban), McCartt and Geary (2004) find that handheld cell phone usage while driving was back up to pre-ban levels.

[64] As noted earlier, however, there is little research supporting the view that existing hands-free technology will reduce accidents.

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Appendix

This appendix contains brief additional information on the data and estimations. Additional supplementary material and greater detail can be found in Appendix B.

Survey Weights

Survey weights for our data were constructed to make each cross section representative of the general population in the mainland U.S. The weights sum to the correct marginal distributions for the number of households in each state, and the same for the household type (married couple, single male, etc.), size, and income; size of MSA the household is in; and individual age/gender, race, ethnicity, and education in the mainland U.S.

Likelihood of the Multiple Equation System

Here we present the likelihood for the model defined in equations (3)-(9), a three equation random effects system for count data with endogenous ordered and continuous variables.

The notation in the main text does not reflect the differing frequency of observation in the data. The accident counts for the first equation and the car weights in the third equation are observed each quarter. The cell phone usage variables y_2 are observed yearly and the time subscript for u_{it2} and ε_{it2} is for years. Collect the random effects in (4)-(5) into column vectors $u_{i2} = (u_{i12}, u_{i22})'$ and $u_{i3} = (u_{i13}, \ldots, u_{i83})'$ and let $u_{1i} = \alpha_{1i}$. Here the likelihoods are derived for all eight quarters of data; the modification for the four quarter subset or for missing quarters is straightforward. Define

$u_i = [u_{i1}, u'_{i2}, u'_{i3}]'$. Then var (u_i) is

$$\text{var}\,[u_i] = \begin{bmatrix} \sigma_1^2 & \rho_{12}\sigma_1\iota_2 & \rho_{13}\sigma_1\sigma_3\iota_8' \\ \rho_{12}\sigma_1\iota_2 & I_2 + \sigma_2^2\iota_2\iota_2' & \iota_2\iota_8'\rho_{23}\sigma_2\sigma_3 \\ \rho_{13}\sigma_1\sigma_3\iota_8 & \iota_8\iota_2'\rho_{23}\sigma_2\sigma_3 & \tau^2 I_8 + \sigma_3^2\iota_8\iota_8' \end{bmatrix}$$

$$\equiv \begin{bmatrix} \Sigma_{u11} & \Sigma_{u12} & \Sigma_{u13} \\ \Sigma_{u21} & \Sigma_{u22} & \Sigma_{u23} \\ \Sigma_{u31} & \Sigma_{u32} & \Sigma_{u33} \end{bmatrix}$$

where ι_k is a k-row column vector of ones and I_k is a k-rank identity matrix.

The observed data for an individual is $y_{i1} = (y_{i11}, \ldots, y_{i81})'$, $y_{i2} = (y_{i12}, y_{i22})'$, $y_{i3} = (y_{i13}, \ldots, y_{i83})'$. To simplify notation, drop the i subscripts from here on. The joint density of the data conditional on u_1, $f(y_1, y_2, y_3 \mid u_1)$, is

$$f(y_1, y_2, y_3 \mid u_1) = f(y_2, y_3 \mid u_1) f(y_1 \mid y_2, y_3, u_1)$$

where

$$f(y_2, y_3 \mid u_1) = f(y_3 \mid u_1) \int f(y_2^* \mid u_1, y_3) dy_2^* \tag{A.1}$$

$$f(y_3 \mid u_1) = \phi_8(\beta_3' x_3 + \frac{\rho_{13}\sigma_3}{\sigma_1}\alpha_1 \iota_8, \tau^2 I_8 + (1-\rho_{13}^2)\sigma_3^2 \iota_8 \iota_8') \tag{A.2}$$

$$f(y_2^* \mid u_1, y_3) = \phi_2(AB^{-1}C, \sum u_{22} - AB^{-1}A') \tag{A.3}$$

$$A \equiv \begin{bmatrix} \Sigma_{u21} & \Sigma_{u23} \end{bmatrix}$$

$$B \equiv \begin{bmatrix} \Sigma_{u11} & \Sigma_{u13} \\ \Sigma_{u31} & \Sigma_{u33} \end{bmatrix}$$

$$C \equiv \begin{bmatrix} u_1 & y_3 - \beta_3' x_3 \end{bmatrix}'$$

$$f(y_1 \mid y_2, y_3, u_1) = \prod_{t=1}^{8} \frac{\exp(-s\lambda_t)(s\lambda_t)^{y_{1t}}}{y_{1t}!}$$

$$\lambda_t = v_t \exp(\beta_1' x_{t1} + \gamma' y_{t2} + \delta y_{t3})$$

All densities are to be read as conditional on the x covariates. The limits of the rectangular integration region in (A.1) are the appropriate κ's for the value of y_{t2} for year 1 and year 2, based on (6). $\phi_p(\mu, \Sigma)$ in (A.2) and (A.3) is the p.d.f. of a p–variate normal *r.v.* with mean vector μ and covariance matrix Σ. If the individual does not have a cell phone in any period in a year, there is no selection equation for minutes of usage and the integral pertaining to that year in (A.1) drops out.

The likelihood for the data is then found as (9), where the integral there can be written

$$\int_{-\infty}^{\infty} f(y_1, y_2, y_3 \mid u_1) \frac{1}{\sigma_1} \phi(\frac{u_1 + \sigma_1^2/2}{\sigma_1}) du_1$$

This integral is evaluated for each i by Gauss-Hermite quadrature with 16 evaluation points. MLE is performed using the BFGS variant of the DFP algorithm with numerical derivatives.

When y_2 represents hands-free device usage, minor modifications are required. First, the hands-free usage question is asked once for all quarters, so a period-specific error in (8) is redundant with α_{i2} and ε_{it2} is dropped. Furthermore, with a single observation per individual on y_2, the integral in (A.1) becomes unidimensional and σ_2 is no longer identified and is fixed to unity. Finally, if the individual does not use a cell phone while driving in any period, there is no selection equation for hands-free device usage and the integral in (A.1) drops out.

Likelihood of the Random Coefficient Model

Here we present the likelihood for the model defined in equations (10)-(13), a random coefficient model for count data with random effects. The density of the observed data yi1 is Poisson mixed over (*vi*, *ηi*). Thus the log likelihood for MLE is

$$\ln L = \sum_{i=1}^{N} \ln \int_{-\infty}^{\infty} \int_{-\infty}^{\infty} \prod_{t=1}^{8} \frac{\exp(-s\lambda_{it})(s\lambda_{it})^{y_{1it}}}{y_{1it}!} \phi_2(\mu, \Sigma) d\alpha d\eta \qquad \text{(A.4)}$$

where λ_{it} is the Poisson conditional mean from (13),

$$\mu = (-\sigma^2/2,\ -\omega^2/2 - \rho\,\sigma\,\omega)' \qquad \text{(A.5)}$$

and Σ is as in (12). See the footnote following (13) on identification. This likelihood is evaluated with bivariate 32-point Gauss-Hermite quadrature and MLE is performed as described for the previous model.

LR Tests of the Parametric Models

The likelihood ratio tests of the parametric models mentioned in the text are non-standard because they involve parameters on the boundary of the parameter space and because some of the nuisance parameters appear only under the alternative hypothesis. The null hypothesis for the tests for the ML models is H_0: $\sigma_1 = \sigma_3 = 0$ vs. H_A: $\sigma_1 > 0$, $\sigma_3 > 0$, $\rho \equiv (\rho_{12}, \rho_{13}, \rho_{23}) \in (-1,1)^3$. Under the null, σ_1 and σ_3 are on the boundary of the parameter space and ρ is a nuisance parameter that appears only under the alternative. Test statistics with parameters appearing only under the alternative hypothesis have complicated distributions in general (Andrews, 2001), whereas parameter-on-the-boundary (PB) problems with all parameters appearing both under the null and the alternative hypotheses generally lead to simpler distributions. Using techniques from King and Shively (1993), we therefore transform this test through reparameterization into a simpler PB problem so that the test statistic is a mixture of chi-squares. Appendix B.11 contains details.

GMM Estimation

Here we follow Windmeijer and Santos Silva (1997). Equation (1) implicitly defines a multiplicative model

$$y_{1i} = s \exp(\beta x_i + \gamma' y_{2i} + \delta y_{3i})\, \xi_i \tag{A.6}$$

where $\xi_i = v_i\, \zeta_i$, ζ_i is a multiplicative error satisfying $E(\zeta_i | x_i, y_{2i}, y_{3i}, v_i) = 1$, and time subscripts are suppressed. If instruments z_i satisfy $E(v_i|z_i) = 1$, then $E(\xi_i - 1|z_i) = 0$. Solving for ξ_i from (A.6) then leads to the conditional moment condition

$$E\left(\frac{y_{1i}}{s\exp\left(\beta' x_i + \gamma' y_{2i} + \delta y_{3i}\right)} - 1 \middle| z_i \right) = 0 \tag{A.7}$$

Our GMM procedure relies on (A.7), using sample analogs of unconditional moments of the form

$$E\left[\left(\frac{y_{1i}}{s\exp\left(\beta' x_i + \gamma' y_{2i} + \delta y_{3i}\right)} - 1\right)h\left(z_i\right)\right] = 0$$

with z_i equal to the instrument sets discussed in the text (plus the predicted values of the binary endogenous variables from the first stage probit regressions) and h chosen to be optimal instruments. See Appendix B.8 for further detail.

Additional References in Appendix

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Table 1: Summary Statistics of the Data

Variable	*Obs*	*Freq.*	*Mean*	*Std. Dev.*	*Min*	*Max*	*Source*
Accidents in quarter	26,572	Q	0.013	0.117	0.000	2.000	Survey
Cell phone minutes of use while driving:							
NoPhone (no cell phone)	26,572	Q	0.162	0.369	0.000	1.000	Survey
CellMinsLow (1-15 mins/wk)	26,572	C	0.474	0.499	0.000	1.000	Survey
CellMinsMed (2-20 mins/day)	26,572	C	0.152	0.359	0.000	1.000	Survey
CellMinsHi (20-60 mins/day)	26,572	C	0.066	0.248	0.000	1.000	Survey
CellMinsVHi (> 1 hour/day)	26,572	C	0.024	0.153	0.000	1.000	Survey
NoPhoneM (no cell phone, male)	26,572	Q	0.058	0.233	0.000	1.000	Survey
NoPhoneF(no cell phone, female)	26,572	Q	0.105	0.306	0.000	1.000	Survey
CMinsLowM (1-15 mins/wk, male)	26,572	C	0.140	0.347	0.000	1.000	Survey
CMinsLowF (1-15 mins/wk, female)	26,572	C	0.335	0.472	0.000	1.000	Survey
CMinsMedM (2-20 mins/day, male)	26,572	C	0.056	0.231	0.000	1.000	Survey
CMinsMedF (2-20 mins/day, female)	26,572	C	0.095	0.294	0.000	1.000	Survey
CMinsHiM (20-60 mins/day, male)	26,572	C	0.027	0.161	0.000	1.000	Survey
CMinsHiF (20-60 mins/day, female)	26,572	C	0.039	0.194	0.000	1.000	Survey
CMinsVHiM (> 1 hour/day, male)	26,572	C	0.012	0.107	0.000	1.000	Survey
CMinsVHiF (> 1 hour/day, female)	26,572	C	0.012	0.110	0.000	1.000	Survey
Use of hands free device while driving:							
HFreeSome (sometimes use)	26,572	H	0.151	0.358	0.000	1.000	Survey
HFreeAlwys (always use)	26,572	H	0.145	0.352	0.000	1.000	Survey
HFreeSomeM (sometimes use, male)	26,572	H	0.056	0.229	0.000	1.000	Survey
HFreeSomeF (sometimes use, female)	26,572	H	0.095	0.294	0.000	1.000	Survey
HFreeAlwysM (always use, male)	26,572	H	0.053	0.225	0.000	1.000	Survey
HFreeAlwysF (always use, female)	26,572	H	0.092	0.289	0.000	1.000	Survey
Variables appearing in accident equation (not all used in all specifications):							
Age	26,572	O	44.931	13.30	18.00	98.00	Survey
CarWgtLn (log vehicle weight)	25,251	Q	1.253	0.212	0.703	2.000	a
CommuteLn (log time of commute)	26,572	Y	2.865	1.110	0.000	5.704	Survey
Female	26,572	O	0.670	0.470	0.000	1.000	Survey
FreezeTemp (# days below freezing)	26,572	Q	18.037	24.73	0.000	90.00	b
HrsOfLight (ave. hours of daylight)	26,572	Q	12.108	1.671	9.217	14.86	c
KidsInHH (children in household)	26,572	O	0.471	0.499	0.000	1.000	Survey
Luxury Car (vehicle type indicator)	25,251	Q	0.082	0.274	0.000	1.000	d
Married	26,572	O	0.725	0.446	0.000	1.000	Survey
MilesLn (quarterly mileage driven)	26,572	Y	1.041	0.931	-8.294	3.359	Survey
Minivan (vehicle type indicator)	25,251	Q	0.005	0.068	0.000	1.000	d
Pickup Truck (vehicle type indicator)	25,251	Q	0.104	0.305	0.000	1.000	d
Precip (# days with precipitation)	26,572	Q	5.525	3.996	0.000	30.00	b
Quarter_5 (quarter indicator for 1Q02)	26,572	Q	0.243	0.429	0.000	1.000	Survey
Quarter_6 (quarter indicator for 2Q02)	26,572	Q	0.256	0.437	0.000	1.000	Survey
Quarter_7 (quarter indicator for 3Q02)	26,572	Q	0.268	0.443	0.000	1.000	Survey
RuralFrwy (drive on rural freeways)	26,572	Y	0.187	0.390	0.000	1.000	Survey
RuralSrfc (drive on rural surface streets)	26,572	Y	0.064	0.245	0.000	1.000	Survey
Snow (# snow days)	26,572	Q	2.701	9.121	0.000	90.00	b
Sporty Car (vehicle type indicator)	25,251	Q	0.038	0.191	0.000	1.000	d
SUV (vehicle type indicator)	25,251	Q	0.247	0.431	0.000	1.000	d

UrbanSrfc (drive on city surface streets)	26,572	Y	0.322	0.467	0.000	1.000	Survey
Van (vehicle type indicator)	25,251	Q	0.114	0.318	0.000	1.000	d
WorkFullTime (employment status)	26,572	O	0.589	0.492	0.000	1.000	Survey
Additional variables appearing in cell phone usage or vehicle weight equations (not all used in all specifications):							
Antenna sites per capita	26,572	O	0.043	0.046	0.000	0.591	e
Asian	26,572	O	0.021	0.145	0.000	1.000	Survey
Black	26,572	O	0.039	0.193	0.000	1.000	Survey
Cable TV (subscribe to cable)	26,455	O	0.705	0.456	0.000	1.000	Survey
Carriers (wireless carriers in state)	26,486	S	11.624	3.171	4.000	19.00	f
Cellular industry wages (in state)	26,140	O	55.278	16.43	8.173	144.7	g
Drive mostly on freeway	26,572	Y	0.614	0.487	0.000	1.000	Survey
Electricity price (in state)	26,572	Y	8.103	2.397	5.000	13.30	h
Employment: no answer	26,327	O	0.039	0.194	0.000	1.000	Survey
Employment: not employed	26,327	O	0.123	0.329	0.000	1.000	Survey
Employment: part time	26,327	O	0.102	0.303	0.000	1.000	Survey
Employment: retired	26,327	O	0.141	0.348	0.000	1.000	Survey
Female head of household	26,572	O	0.211	0.408	0.000	1.000	Survey
Gasoline price (in city or state)	26,572	Q	1.345	0.155	1.041	1.680	i
Income (household income)	26,572	O	84.534	52.72	5.279	349.7	Survey
Live with parents	26,572	O	0.024	0.153	0.000	1.000	Survey
Market size (size of MSA)	26,572	O	3.117	1.084	1.000	4.000	Survey
Recent new computer (within 2 years)	26,572	O	0.524	0.499	0.000	1.000	Survey
Recent vacation travel (within 2 years)	26,572	O	0.250	0.433	0.000	1.000	Survey
Recently married (within 2 years)	26,572	O	0.056	0.229	0.000	1.000	Survey
Subscribers (per capita wireless subs.)	26,509	S	0.438	0.048	0.277	0.728	f
VCR (have a VCR in household)	26,455	O	0.928	0.258	0.000	1.000	Survey

Table notes: Statistics are for the 4Q2001-3Q2002 subset of periods used for most of the estimations.

Frequency codes:

C Quarterly at most; question asked annually but is linked to the quarterly cell phone use variable.
H Quarterly at most; question asked once but is linked to the quarterly cell phone use variable.
O Observed once per individual.
S Semi-annual observation.
Y Annual observation.

Source codes:

[a] Survey (for vehicle); *Ward's Automotive Yearbook* and *Automotive News Market Data Book* (weight).
[b] National Climatic Data Center, Database TD3220 – Monthly Surface Data for U.S. cooperative weather stations.
[c] Calculated based on latitude of household's ZIP code.
[d] Survey (for vehicle) and NFO Interactive (for classification)
[e] Federal Communications Commission's Universal Licensing System.
[f] *Local Telephone Competition: Status as of December 31, 2002*, Industry Analysis and Technology Division, Wireline Competition Bureau, Federal Communications Commission, June 2003, and similar earlier semi-annual reports.
[g] Bureau of Labor Statistics, Covered Employment and Wages.
[h] *Electric Power Monthly*, Energy Information Administration, Department of Energy.
[I] *Petroleum Marketing Monthly*, Energy Information Administration, Department of Energy. Table 31, Motor Gasoline Prices by Grade, Sales Type, PAD District, and State and *Historical Trends in Motor Gasoline Taxes, 1918-2002*, American Petroleum Institute.

Table 2: Comparison of Survey Sample with General Population (percentages)

	General Population (age 18+) March 2003 CPS	Online Households January 2003	Our Survey Respondents (completes & incompletes) February 2003	Estimation Sample (4Q 2001 – 3Q 2002) February 2003	Difference between Our Survey and General Population
Census Region					
Midwest	23.0	23.1	22.9	23.9	0.9
Northeast	19.1	18.7	19.7	19.2	0.1
South	36.0	35.2	32.7	35.5	-0.5
West	21.8	22.9	24.8	21.4	-0.4
Market Size					
Under 100K	21.9	17.5	15.2	13.7	-8.2*
100K – 499K	17.5	14.2	13.6	12.5	-5.0*
500K+	60.5	68.4	71.2	73.8	13.3*
Household Income					
Under $20K	22.6	15.3	8.6	3.8	-18.8*
$20K - $34.9K	18.9	19.0	14.0	8.6	-10.3*
$35K - $54.9K	19.5	19.9	18.0	15.1	-4.4*
$55K - $84.9K	19.1	22.1	27.6	30.0	10.9*
$85K+	19.7	23.7	31.8	42.5	22.8*
Age					
Mean (18+)	45.2	46.0	45.6	44.9	-0.3
Median (18+)	44.0	44.0	45.0	44.0	0.0
Gender					
Female	51.1	49.5†	66.0	67.0	15.9*
Male	48.9	50.5†	34.0	33.0	-15.9*

*Significant at the 1% level.
†Calculated from gender-specific online access rates from Pew Research Center (2003b) from March 2003 and the gender ratio from the CPS in column one.
Figures for Online Households are from NFO Worldgroup (unpublished). Figures for our estimation sample are for the pooled four-quarter data set.

Table 3: Estimates of the Proportion of Drivers Using Cell Phones and Hands-Free Devices while Driving

		% of drivers who use a cell phone while driving, out of…		% of drivers who use HF device while driving, out of…		
Study or Poll	**Time Period**	**All Drivers**	**Drivers who Have a Cell Phone**	**All Drivers**	**Drivers who Have a Cell Phone**	**Source**
Authors' survey, raw average.	*Oct 2001—Sept 2002*	*73*	*86*	*30*	*41*	*Authors' survey.*
Authors' survey, weighted average.	*Oct 2001—Sept 2002*	*64*	*82*	*28*	*44*	*Authors' survey.*
Gallup Poll	Nov 2003	40	62	23	NA	Gallup Organization (2003).
Quinnipiac	Oct 2002	51	78	NA	NA	Quinnipiac University (2003).
UNC HSRC 2002	June—July 2002	59	NA	NA	28	Stutts et al. (2002).
NHTSA 2002	Feb 2002—Apr 2002	31	52	NA	NA	Royal (2003).
AAA/UNC HSRC 2003	Nov 2000—Nov 2001	30	NA	NA	NA	Stutts et al. (2003).
Highway and Auto Safety	July 2001	30	43	NA	NA	Advocates for Highway and Auto Safety (2001).
Gallup Poll	June—July 2001	43	79	NA	NA	Gallup Organization (2001).
Gallup Poll	June—July 2001	49	89	NA	NA	Gallup Organization (2001).
SurveyUSA	June 2001	33	NA	NA	NA	SurveyUSA (2001).
NHTSA 2000	Nov 2000—Jan 2001	39	73	NA	NA	Boyle and Vanderwolf (2001).

Table notes:
In the authors' survey, figures for cell phone use are the percentage of the 7,327 respondents who chose an answer other than "none" to "During [the time period in question], how many minutes did you typically talk on your cell phone while driving?" Details concerning wording of the other survey questions and sample sizes are in Appendix B.14.

Table 4: Overview of Accidents and Cell Phone Use

Category	*N*	Percent of sample	Yearly Accident Rate x 100 (raw)	Equality of Proportions Test (*p*-value)	Yearly Accident Rate x 100 (weighted)
Cell Phone Usage				0.012	
Do not have cell phone	4,313	16.2	4.4		5.0
Have cell phone, do not use while driving	3,238	12.2	3.7		5.1
Use cell phone while driving	19,021	71.6	5.9		7.1
Cell Phone Minutes of Use				0.006	
Less than 15 minutes/week	12,604	47.4	5.3		6.6
2-20 minutes/day	4,028	15.2	6.3		6.8
20-60 minutes/day	1,755	6.6	9.6		10.9
More than 1 hour/day	634	2.4	6.3		3.9
Hands-Free Device Usage While Driving				0.078	
Never use hands-free device*	11,152	42.0	5.8		5.5
Sometimes use hands-free device*	4,012	15.1	7.3		10.2
Always use hands-free device*	3,857	14.5	4.9		7.1
Gender				0.083	
Men	8,773	33.0	6.1		7.6
Women	17,799	67.0	5.0		5.2
Entire Sample	26,572	100.0	5.4		6.3

*Driver also uses cell phone while driving.

Table notes: data source is the authors' survey, four quarter subsample. The accident rates are per driver (not per vehicle miles traveled). The counts in column one are quarterly observations on 7,395 drivers. The equality of proportions test is Pearson's chi-square two-sided test of the null hypothesis that all rates are equal within each category. The last column uses the survey weights described in the text.

Table 5: Accidents: Reduced Form (RF) Estimation with Combined-Gender Cell Phone Effects

	RF1	
accinqtr	IRR	*P*-value
NoPhone	1.209	0.419
CellMinsLow	1.472*	0.061
CellMinsMed	1.770**	0.016
CellMinsHi	2.792***	0.000
CellMinsVHi	1.895*	0.087
HFreeSome	1.138	0.394
HFreeAlwys	0.733*	0.069
Log likelihood	-1867.48	
χ^2 statistic (dof)	72.0 (49)	0.018
N	26,572	

*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Notes: Dependent variable is the quarterly traffic accident count for an individual. All specifications include quarter and state fixed effects. Sample covers Q4 2001—Q3 2002. IRR is incident risk ratio, $\exp(\hat{\beta})$. *P*-values are for the hypothesis test that the estimated coefficient is zero (equivalently, that the estimated IRR is 1.0) and are calculated from standard errors robust to heteroskedasticity and clustering on individuals.

Table 6: Accidents: Reduced Form (RF) Estimations with Gender-Specific Cell Phone Effects

	RF2		RF3		RF4		RF5	
accinqtr	IRR	*P*-value	IRR	*P*-value	IRR	*P*-value	IRR	*P*-value
NoPhoneM	0.932	0.839	0.850	0.635	0.813	0.543	1.186	0.715
CMinsLowM	1.058	0.853	0.924	0.796	0.852	0.603	0.716	0.455
CMinsMedM	0.838	0.627	0.605	0.175	0.591	0.167	0.500	0.200
CMinsHiM	1.148	0.736	0.816	0.620	0.788	0.575	0.659	0.445
CMinsVHiM	0.190	0.120	0.143*	0.070	0.168*	0.097	0.211	0.179
NoPhoneF	1.419	0.279	1.212	0.554	1.319	0.417	2.778	0.114
CMinsLowF	1.807**	0.039	1.402	0.238	1.556	0.145	2.916*	0.083
CMinsMedF	2.693***	0.002	1.713*	0.089	1.761*	0.093	4.573**	0.017
CMinsHiF	4.639***	0.000	2.689***	0.004	3.008***	0.002	5.447**	0.013
CMinsVHiF	5.271***	0.000	2.951***	0.009	3.365***	0.008	3.148	0.198
HFreeSomeM	1.506*	0.096	1.307	0.268	1.281	0.329	1.907*	0.054
HFreeAlwysM	1.202	0.473	1.180	0.512	1.098	0.732	1.732	0.129
HFreeSomeF	0.973	0.886	0.883	0.508	0.910	0.622	1.099	0.772
HFreeAlwysF	0.520***	0.006	0.506***	0.003	0.507***	0.004	0.390**	0.023
Female	0.498*	0.058	0.593	0.164	0.524*	0.099	0.299*	0.083
Married			0.685***	0.003	0.693***	0.005	0.678**	0.044
KidsInHH			1.130	0.328	1.165	0.245	1.004	0.983
Age			0.900***	0.000	0.905***	0.000	0.898***	0.000
AgeSq			1.001***	0.000	1.001***	0.001	1.001***	0.002
WorkFullTime			1.433***	0.006	1.496***	0.003	1.226	0.283
MilesLn			1.108	0.162	1.120	0.146	1.104	0.209
CommuteLn			1.153**	0.014	1.161**	0.012	1.202**	0.042
RuralFrwy			0.742*	0.063	0.777	0.122	0.895	0.633
UrbanSrfc			1.122	0.356	1.123	0.365	1.094	0.645
RuralSrfc			0.503**	0.045	0.540*	0.075	0.309	0.105
Precip			0.995	0.755	0.993	0.671	0.969	0.283
Snow			0.985	0.171	0.976**	0.042	0.983	0.345
FreezeTemp			0.991	0.129	0.995	0.347	0.994	0.432
HrsOfLight			0.593**	0.013	0.579**	0.013	0.597	0.103
Pickup					0.660*	0.078		
Minivan					0.642	0.636		
SUV					0.821	0.171		
Luxury					0.758	0.231		
Sporty					0.731	0.254		
Van					0.945	0.772		
Average cell phone IRR	1.567		1.122		1.163		1.943	
χ^2 statistic (dof)	95.6 (57)	0.001	224.3 (71)	0.000	224.8 (77)	0.000	14035 (71)	0.000
Log likelihood	-1854.93		-1806.54		-1705.15		-725.49	
N	26,572		26,572		25,251		11,618	

*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Notes: Dependent variable is the quarterly traffic accident count for an individual. All specifications include quarter and state fixed effects. Sample covers Q4 2001—Q3 2002. *P*-values based on standard errors robust to heteroskedasticity and clustering on individuals. *Average cell phone IRR* is the average IRR from the cell phone and hands free device variables, weighted by the number of drivers in each phone/hands-free device category. RF5 uses the gender-balanced sample; see text for details. See notes to Table 5 on IRR.

Table 7: Accidents, Hands-free Device Usage, and Vehicle Safety: Three Equation MLE
Panel a: Combined sample (both genders)

Coefficient and Variable		ML1 Small set of instruments IRR	ML1 *P*-value	ML2 Large set of instruments IRR	ML2 *P*-value
β_1	NoPhone	1.082	0.766	1.078	0.777
β_1	CellMinsLow	0.983	0.953	0.969	0.908
β_1	CellMinsMed	0.875	0.709	0.860	0.636
β_1	CellMinsHi	1.268	0.557	1.236	0.559
β_1	CellMinsVHi	0.751	0.632	0.718	0.546
γ_1	HFreeSome	1.886	0.263	2.025	0.111
γ_2	HFreeAlwys	1.923	0.464	2.174	0.249
δ	CarWgtLn	0.092	0.201	0.286	0.267
	Other controls as in RF3	yes		yes	
	Average cell phone usage IRR	1.377		1.436	
		parameter		*parameter*	
σ_1^2		0.695*	0.078	0.633**	0.046
ρ_{12}		-0.709	0.191	-0.880***	0.007
ρ_{13}		0.515	0.246	0.179	0.626
	LR statistic	5.2E04	0.000	5.2E04	0.000
	Log likelihood	24,109.0		24,289.3	
	# individuals	6,877		6,877	
	# observations	24,897		24,897	

Table 7: Accidents, Hands-free Device Usage, and Vehicle Safety: Three Equation MLE
Panel b: Single gender sample

		Small set of instruments				Large set of instruments			
		ML3 Men		ML4 Women		ML5 Men		ML6 Women	
Coefficient and Variable		IRR	*P*-value	IRR	*P*-value				
β_1	NoPhone	0.813	1.350	1.350	0.418	0.812	0.627	1.360	0.405
β_1	CellMinsLow	0.658	1.356	1.356	0.452	0.675	0.352	1.407	0.381
β_1	CellMinsMed	0.405	1.505	1.505	0.389	0.419	0.106	1.581	0.308
β_1	CellMinsHi	0.505	2.356	2.356	0.103	0.531	0.313	2.527*	0.061
β_1	CellMinsVHi	0.102*	2.008	2.008	0.398	0.108*	0.061	2.210	0.316
γ_1	HFreeSome	1.949	1.670	1.670	0.522	1.900	0.210	1.456	0.573
γ_2	HFreeAlwys	2.583	1.246	1.246	0.862	2.455	0.216	1.001	0.999
δ	CarWgtLn	0.044	0.278	0.278	0.622	0.061	0.169	0.753	0.833
	Other controls as in RF3	yes		yes		yes		yes	
	Average cell phone usage IRR	1.315				1.278			
		parameter		*parameter*		*parameter*		*parameter*	
σ_1^2		0.759	0.255†	0.666*	0.071†	0.684	0.173†	0.630*	0.061†
ρ_{12}		-0.544***	0.002	-0.658	0.408	-0.587***	0.000	-0.540	0.471
ρ_{13}		0.774***	0.000	0.140	0.857	0.766***	0.000	-0.192	0.650
	LR statistic	1.68E04	0.000	3.61E04	0.000	1.66E04	0.000	3.54E04	0.000
	Log likelihood	7,516.5		16,597.8		7,557.6		16,752.0	
	# individuals	2,256		4,612		2,256		4,612	
	# observations	8,144		16,720		8,144		16,720	

†One sided p value.

*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

Table notes: LR statistic is the likelihood ratio statistic for test H_0: $\sigma_1^2 = \sigma_3^2 = 0$ vs. H_A: $(\sigma_1^2,\sigma_3^2) > 0$, $(\rho_{12},\rho_{13},\rho_{23}) \in (-1,1)^3$. It has a non-standard distribution; see Appendix A.4 for details. Estimated but not reported: the rest of β_1 (for the other controls included as in RF3 [including time dummies but with region dummies replacing state dummies]) and (β_2,δ, κ). Likelihood is calculated via Gauss-Hermite quadrature (see Appendix A.2), with 16 evaluation points. The standard errors account for the panel structure of the data. *Average cell phone usage IRR* is the average IRR from the cell phone and hands free device variables, weighted by the number of drivers in each phone/hands-free device category. See notes to Table 5 on IRR.

Table 8: Accidents: Random Coefficient (RC) for Cell Phone Usage

Variable		RC1 Men and Women Combined IRR	RC1 *P*-value	RC2 Women Only IRR	RC2 *P*-value
β_1	NoPhone	1.055	0.833	1.342	0.403
$\bar{\gamma}_1$	CellMinsLow	1.175	0.501	1.598	0.153
$\bar{\gamma}_2$	CellMinsMed	1.123	0.672	1.868*	0.084
$\bar{\gamma}_3$	CellMinsHi	1.803*	0.053	3.136***	0.004
$\bar{\gamma}_4$	CellMinsVHi	1.150	0.758	3.001**	0.049
β_1	HFreeSome	1.051	0.753	0.975	0.897
β_1	HFreeAlwys	0.686*	0.056	0.499**	0.012
δ	Log Car Weight	0.462***	0.007	0.431**	0.026
	Other controls as in RF3	yes		yes	
	Average cell phone usage IRR	1.160			
		parameter		*parameter*	
σ^2		0.000	(fixed)†	0.000	(fixed)†
ω		0.489	0.194	0.710***	0.005
ρ		0.000	(fixed)	0.000	(fixed)
LR statistic		0.616	0.216	2.099	0.074
Log likelihood		-1670.8		-1069.4	
# individuals		6,809		4,609	
# observations		24,645		16,699	

*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

†Likelihood is maximized at boundary with $\sigma^2 = 0$.

Table Notes: Estimated but not reported: The other elements of β_1 (for the other controls included as in RF3 [including time dummies but with region dummies replacing state dummies]). Likelihood is calculated via Gauss-Hermite quadrature, with 32 evaluation points. *LR* statistic is the likelihood ratio statistic for test H_0: $\tau = 0$ vs. H_A: $\tau > 0$. It has a non-standard distribution; see Appendix A.4 for details. See notes to Table 5 on IRR. The standard errors account for the panel structure of the data. *Average cell phone usage IRR* is the average IRR from the cell phone and hands free device variables, weighted by the number of drivers in each phone/hands-free device category.

Table 9: Implications of the Random Coefficient Model for RT's Estimates of Relative Risk

	Model RC1 (both genders)	**Model RC2 (women only)**
Average estimated true IRR in sample	1.2	1.6
Overstatement of IRR if use accident-only sample	5.6%	13.6%
Assumed fraction of driving time spent on the phone (*f*)	1.9%	1.9%
RT's estimate of relative risk (*RR*)	4.3	4.8
Implied overstatement of *RR* if use accident-only sample	36.3%	36.0%
Implied corrected *RR*	3.2	3.5

Table notes: Row one calculated as the weighted average of the IRRs for each cell phone/hands free device usage cell, using the estimated coefficients from the model given in the column heading. Row two is the expected overstatement of IRR if the sample is restricted to drivers who had accidents; see Appendix B.12 for details. Row three *f* is from Cohen and Graham (2003). Row four *RR* is from Redelmeier and Tibshirani (1997). Row five is calculated using equation (9) in the text. Row six is calculated as (row four)/(1 + row five). See notes to Table 5 on IRR.

Table 10: Accidents: GMM Estimations with Gender-Specific Cell Phone Effects

	Exogeneity Assumed		Endogeneity Assumed			
	GMM1 No additional instruments		GMM2 Small set of instruments		GMM3 Large Set of Instruments	
accinqtr	IRR	*P*-value	IRR	*P*-value	IRR	*P*-value
NoPhoneM	0.567	0.162	0.870	0.794	0.686	0.549
CMinsLowM	0.975	0.946	1.242	0.862	1.648	0.653
CMinsMedM	0.637	0.305	0.211	0.193	0.159*	0.079
CMinsHiM	0.760	0.606	0.436	0.624	0.301	0.341
CMinsVHiM	0.300	0.265	0.436†	0.624	0.301†	0.341
NoPhoneF	1.454	0.334	1.341	0.578	1.384	0.527
CMinsLowF	1.762	0.112	0.784	0.686	1.177	0.798
CMinsMedF	1.832	0.119	1.185	0.890	0.903	0.880
CMinsHiF	2.918**	0.011	6.434	0.837	0.711	0.728
CMinsVHiF	3.528**	0.042	6.434†	0.837	0.711†	0.728
HFreeSomeM	0.922	0.786	6.232	0.408	2.808	0.311
HFreeAlwysM	1.571	0.228	6.232†	0.408	2.808†	0.311
HFreeSomeF	0.935	0.768	4.344	0.386	3.490	0.143
HFreeAlwysF	0.655	0.203	4.344†	0.386	3.490†	0.143
Female	0.455*	0.084	0.722	0.651	0.597	0.526
CarWeightLn	0.335***	0.002	1.260	0.920	1.241	0.866
Other controls as in RF3	yes		yes		yes	
Hausman test statistic 1 (dof)			1015.8 (21)	0.000	72.1 (19)	0.000
Hausman test statistic 2 (dof)			5.8 (11)	0.889	8.4 (11)	0.679
GMM Criterion	1.55E-30		2.05E-30		3.13E-30	
N	25,251		24,717		24,502	

*, **, and *** denote significance at the 10%, 5%, and 1% level, respectively.

†In GMM2-3, coefficients for the two highest minutes of usage categories for each sex are constrained to be equal.

Notes: Dependent variable is the quarterly traffic accident count. All models use optimal instruments and are just identified. For GMM1, exogeneity is assumed and no additional instruments are used. For GMM2-3, CMinsLow-VHi (male and female), the hands-free variables, and CarWeightLn are treated as endogenous. For GMM2, the small set of instruments is used (see text) to form optimal instruments; for GMM3, the large set of instrument is used. Hausman test statistics are with reference to pooled Poisson MLE. Hausman test statistic 1 tests all coefficients; Hausman test statistic 2 tests only the coefficients for the variables treated as endogenous. All specifications include all the variables from RF2 in addition to the ones listed in the table. Sample covers Q4 2001—Q3 2002. *P*-values based on robust standard errors. *Average cell phone usage IRR* is the average IRR from the cell phone and hands free device variables, weighted by the number of drivers in each phone/hands-free device category. See notes to Table 5 on IRR.

Table 11: Reduction in Accidents from a Ban on Cell Phone Use While Driving

Estimation:	High Estimate Point Estimate	High Estimate 95% Conf. Int.	Central Estimate Point Estimate	Central Estimate 95% Conf. Int.	Low Estimate Point Estimate	Low Estimate 95% Conf. Int.
ML3 & ML4 (gender-specific cell phone effects using small set of instruments for hands-free usage)	4.1%	(-29.5%,37.7%)	3.2%	(-22.2%,30.4%)	1.9%	(-11.7%,19.8%)
ML5 & ML6 (gender-specific cell phone effects using large set of instruments for hands-free usage)	4.5%	(-28.6%,37.5%)	3.5%	(-21.4%,30.3%)	2.1%	(-11.0%,20.0%)
RC2 & RC3 (gender-specific, random cell phone effects)	1.9%	(-30.5%,34.3%)	1.5%	(-23.5%,27.2%)	0.9%	(-13.3%,17.1%)
Assumptions:						
Percentage of drivers using cell phone while driving:	63.9%		50.0%		30.0%	
Source of cell phone use percentage:	our survey		range from Table 3		range from Table 3	

Table notes: Confidence intervals are asymptotic approximations calculated from the variance of the underlying estimations via the delta method. Figures are calculated from individual-level mean accident rates using equation (1) in the text. Compliance is assumed to be 100%, so that the mean accident rate for a driver after the ban is given by (1) with all phone usage and hands-free device indicator variables set to zero.

Figure 1: Factors Affecting Collision Risk (Model 1)

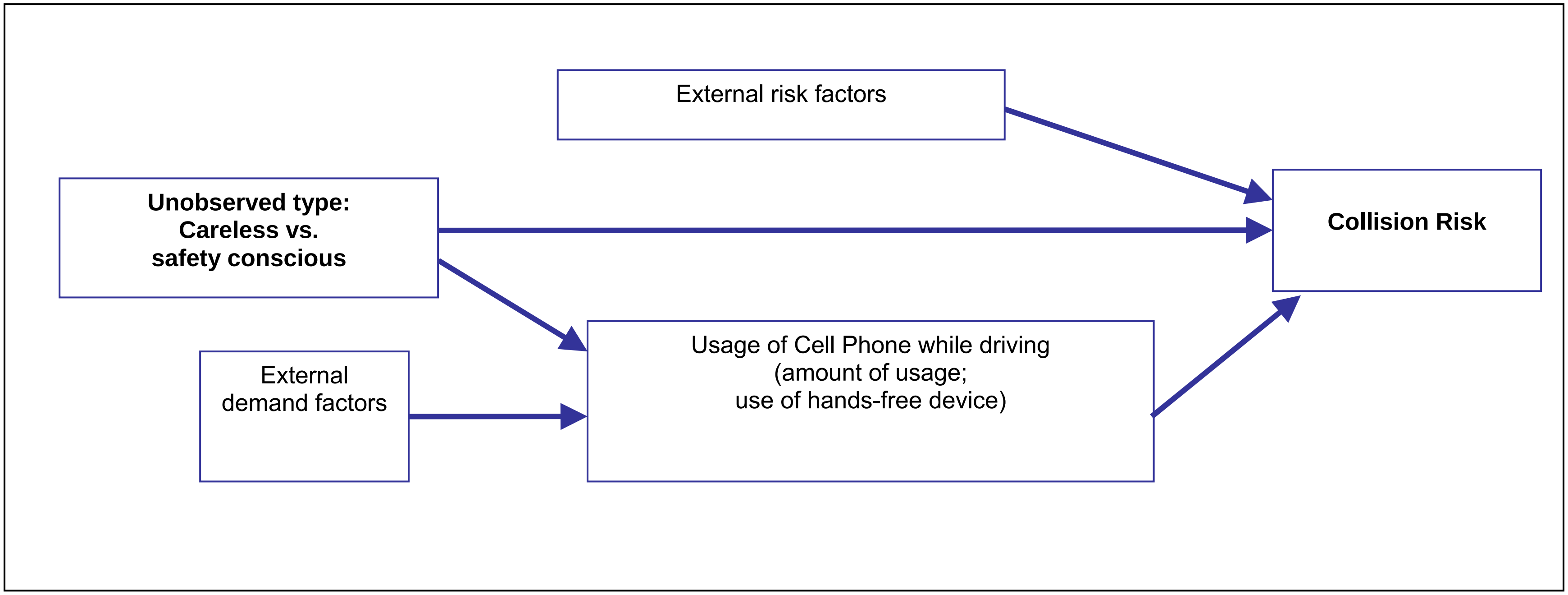
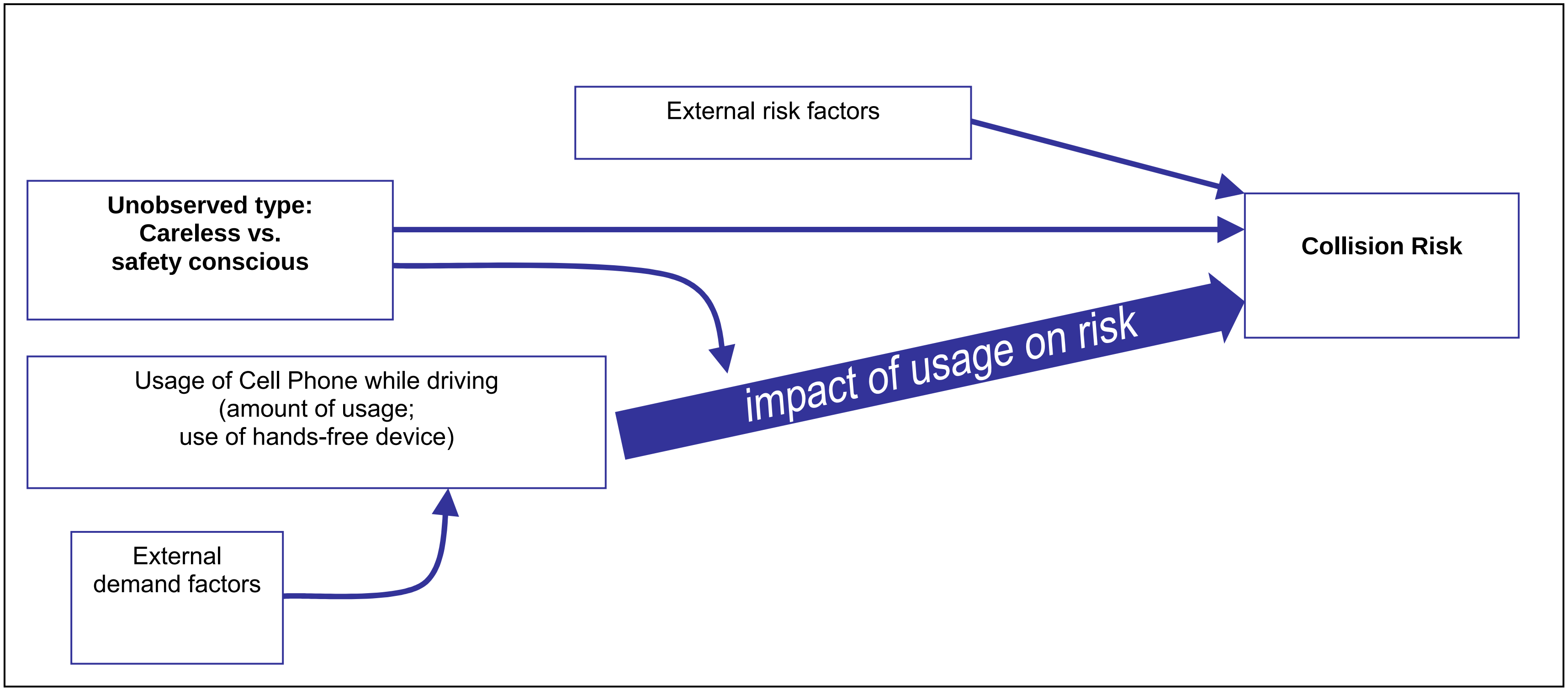


Figure 2: Factors Affecting Collision Risk (Model 2)



Bonus Figure: The Aftermath

"I was distracted for a moment. Go on."