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Abstract

Cell phone use is increasing worldwide, leading to a concern that cell phone use while driving increases accidents. Several countries, two states and Washington, D.C. have banned the use of hand-held cell phones while driving. In this paper, we develop a new approach for estimating the relationship between cell phone use while driving and accidents. Our approach is the first to allow for the direct estimation of the impact of a cell phone ban while driving. It is based on new survey data from over 7,000 individuals.

This paper differs from previous research in two significant ways: first, we use a larger sample of individual-level data; and second, we test for selection effects, such as whether drivers who use cell phones are inherently less safe drivers, even when not on the phone.

The paper has two key findings. First, the impact of cell phone use on accidents varies across the population. This result implies that previous estimates of the impact of cell phone use on risk for the population, based on accident-only samples, may therefore be overstated by 36%. Second, once we correct for endogeneity, there is no significant effect of hands-free or hand-held cell phone use on accidents.

The Impact of Driver Cell Phone Use on Accidents

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I) Introduction

Cell phone use is increasing.[1] Since 1985, the number of subscribers in the United States has grown from 100,000 to over 159 million, and revenue has climbed from under $1 million to $88 billion. 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 Europe, cell phone penetration has reached about 80%. 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.[2]

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 United States and a great many more non-fatal accidents.[3] The regulation of cell phones while driving has become a significant policy issue. Connecticut, New York, New Jersey, Washington, D.C., dozens of municipal governments in the U.S., much of Europe, and many other 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 legisla-

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

[2] Subscriber and revenue data for the U.S. are from December 2003, from the Cellular Telecommunications and Internet Association web site, at http://www.wow-com.com/industry/stats/surveys. Subscriber data for Europe is from Q4 2004, from Forrester Research (see http://www.3g.co.uk/PR/June2005/1651.htm). Number of lines data are from 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 and FCC (2003).

[3] 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.

tion would ban the use of hand-held cell phones while driving, while allowing the use of phones with hands-free devices.[4]

Policy makers should compare the costs and benefits of a ban. The primary purpose of this paper is to measure the potential benefits of a ban by 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 all previous work in two significant ways: it is the first study designed to account for the non-experimental nature of accident data; and it uses a more comprehensive data sample than previous studies. The sample is larger than other studies using individual-level data. Moreover, it contains drivers who had accidents and drivers who did not, and drivers who use a cell phone and drivers who do not.

We detect and correct for selection bias of two types. Our first hypothesis is that the causal impact of usage on accidents is heterogeneous across drivers, so that the same amount of usage increases some drivers' risk more than others'. 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. Under this hypothesis, restricting the sample to drivers who had accidents may lead to incorrectly high estimates of the causal impact of usage on accidents. Our second hypothesis is that drivers who use cell phones while driving are more likely to get into accidents than drivers who do not, even when they are not using the phone. If so, cell phone users are a selected group of riskier drivers. We formalize this hypothesis with a model of choice under uncertainty in the next section. To test Hypothesis 2 and determine the causal impact of cell phone usage on accidents, we require a sample containing both users and non-users.

[4] "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.

To expand upon these two hypotheses, consider the stylized representation of determinants of accident risk in Figures 1 and 2. Collision risk is determined by cell phone usage while driving, external factors such as weather, and the driver's type. Usage is determined by external factors influencing demand for calling while driving, such as income and price of usage. 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 observes. In Figure 1, which depicts Hypothesis 1, the unobserved type affects the relationship between usage and accident risk, causing usage risk to be heterogeneous. Here the usage impact is assumed to vary across individuals due to the unobservable factors, represented by the wide arrow from usage to collision 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.

Figure 2 depicts Hypothesis 2, in which the driver's type affects the amount of cell phone usage while driving and whether the driver uses a hands-free device. More careless people may be more likely to use the phone while driving, and less likely to use hands-free devices. 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.

We find support for both hypotheses. The impact of cell phone use on accidents varies across the population, even after controlling for observable driver characteristics, particularly for female drivers. This result implies that previous estimates of the impact of cell phone use on risk for the population, based on accident-only samples, may therefore be overstated by 36%. Selection effects due to the endogeneity of cell phone usage also appear to be present. Once we correct for endogeneity, our models find accident risk from cell phone and hands-free usage to be insignificant, which calls into question bans on hand-held usage such as the ones passed in Connecticut, New York, New Jersey, and Washington, D.C.

We also explore the impact of a ban on cell phone use while driving. 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, in addition to the selection bias problem discussed above, 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. 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 formalizes our hypotheses with a theoretical model of driving and cell phone use. Section III reviews the literature on the effect of cell phone use on driving. In section IV we describe our survey data. We report the results of our statistical work in section V, and conclude in section VI.

II) A Model of Driving and Cell Phone Use

To motivate our empirical models concerning accidents and cell phone use, let $y \geq 0$ be a driver's amount of cell phone use while driving, and $a \geq 0$ be a choice variable related to safety, such as speed, recklessness, or inattention.[5] The probability of an accident is p, a strictly increasing function of y and a (assume for simplicity that there is no chance of multiple accidents in the relevant time period). The driver is risk averse and has a concave preference scaling function v. The monetary benefits of calling and speeding are increasing, concave functions $b(y)$ and $d(a)$, respectively. The benefit function $d(a)$ represents the monetary equivalent of benefits gained from arriving quicker at the desired destination, the thrill of reckless driving, or the reduced effort cost of paying attention behind the wheel. If the driver's initial wealth is w and the cost of an accident is $c > 0$, then the driver chooses (a^*,y^*) to maximize the expected utility function U:

$$U(a,y) = p(a,y)v\big(w + b(y) + d(a) - c\big) + \big[1 - p(a,y)\big]v\big(w + b(y) + d(a)\big)$$

The first term is the driver's utility when there is an accident, weighted by the probability of occurrence, and the second term is for the no-accident state. Assume that U is twice differentiable, concave, and that an interior solution $(a^*,y^*) > 0$ exists. Finally, assume that v exhibits constant absolute risk aversion, parameterized by r.[6]

In empirical applications, the risk aversion of the driver is not observed. We want to compare the causal effect of cell phone use on accidents with the correlation between use and accidents observed in equilibrium from a sample of drivers differing in their risk aversion. To highlight the essential difference, assume that we have a sample of drivers identical in all respects except in their risk aversion r. Thus, in equilibrium observed differences in p, a, or y are

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[5] To keep the analysis simple, assume that drivers do not differ in miles driven, so that y does not confound risk from phone use with risk from additional miles traveled

[6] CARA utility lends a convenient interpretation to r but is not essential for the proposition which follows. A weaker condition that suffices is $\partial^2 v / \partial w \partial r < 0$ for any concave v that exhibits increasing risk aversion in r. This condition is satisfied by the hyperbolic absolute risk aversion (HARA) family of preference scaling functions, for example, which allows both constant and decreasing absolute risk aversion.

driven entirely by differences in r. We want to compare the causal effect of increasing phone use on accidents, $\partial p / \partial y$, with the observed difference in accidents among individuals with differing phone use in the sample:

$$\frac{dp}{dy} = \frac{\partial p}{\partial y} + \frac{\partial p}{\partial a}\frac{da^*}{dr}\frac{dr}{dy^*} = \frac{\partial p}{\partial y} + \frac{\partial p}{\partial a}\frac{da^*}{dr} \bigg/ \frac{dy^*}{dr}$$

The first term on the right hand side of the last equality is the causal effect of cell phone use. The second term is the indirect effect through a^*. When changes in y^* come only from differences in phone use across individuals in the cross-section, differences in risk aversion are the cause, and if risk aversion changes then a^* changes, too.

To show that the observed effect exaggerates the causal effect, we prove the following proposition:

Proposition: if $\frac{\partial^2 U}{\partial y \partial a} \geq 0$, then $\frac{da^*}{dr} > 0$ and $\frac{dy^*}{dr} > 0$, and therefore $\frac{dp}{dy} > \frac{\partial p}{\partial y}$.

Proof: under the assumptions of the model, it can be shown that $\partial^2 U / \partial y \partial r > 0$ and $\partial^2 U / \partial a \partial r > 0$. Thus, with the assumption in the proposition,[7] U is supermodular in (a,y,r) and it follows from the monotone comparative statics literature (e.g., Milgrom and Shannon (1994)) that $\frac{da^*}{dr} > 0$ and $\frac{dy^*}{dr} > 0$.[8] Q.E.D.

The implication of the proposition for empirical work is Hypothesis 2 stated above: even when controlling for all observed characteristics, if drivers vary in their attitudes toward risk and their risky driving behavior, both unobserved, then cell phone use is endogenous. The naïve observed correlation between cell phone use and accidents overstates the true causal risk.

[7] The assumption that utility exhibits increasing differences in y and a is not guaranteed by the other assumptions on the primitives of the model, but can be assured by bounding the curvature of v.

[8] Technically speaking, the usual monotone comparative statics result gives weak inequalities. In our model the assumptions guarantee strict inequalities, however.

III) 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.[9] 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 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.[10] 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).

[9] See Lissy *et al.* (2000) for citations.
[10] While it is not clear from RT that case-crossover analysis is maximum likelihood, the connection is made explicit in Tibshirani and Redelmeier (1997).

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

[11] For example, RT note that emotional stress may lead to both increased cell phone use and decreased driving ability, leading to spurious correlation.

IV) Description of the Survey Data

A) Survey Design

We commissioned a commercial survey administrator 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. Internet-based surveying has advantages over telephone surveying, particularly for sensitive questions (Chang and Krosnick, 2003). Although Internet survey samples are not random, since 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.[12] Our largest usable sample consists of 7,327 individuals.[13] 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).[14] A quarter is missing for an individual if they did not drive a 1999 or newer model year vehicle that quarter. We restricted attention to drivers of newer vehicles to reduce the differences in safety features among vehicles.

[12] 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.

[13] 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.

[14] In particular, every vehicle driven in our sample is equipped with front air bags by federal law.

This subset avoids using the earliest quarters, for which recall bias may be worst, and the last quarter, for which overcounting of accidents may be present.[15] We explore the representativeness of our sample in the next section.

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.[16] 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, 1-15 minutes per week, 2-20 minutes per day, 20-60 minutes per day, or more than one hour per day.[17] 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.[18] 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.

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[15] 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.

[16] 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.

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

[18] 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.

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, including most variables one would find in U.S. Census data. We also collected additional data from other sources, such as vehicle characteristics, variables related to local traffic congestion (local population density and commuting times) 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.

B) Representativeness of the Survey Sample

In this section we explore how representative of the general U.S. population are the demographics, cell phone usage, and vehicular accidents in our sample. 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 are Internet users and were willing to complete the survey), 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. Thus we control for population density and household income in the estimations.

[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.

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, 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 exercise in Section V.

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.

[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.

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 United States; there is thus no evidence of underreporting of accidents.[21] 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).[22] 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.

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 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.

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[21] 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.

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

V) Estimations

A) The Model

The estimations we perform are based on 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. The variable of interest is y_{2it}, modeled as a vector of binary indicator variables for average cell phone usage minutes while driving (none, 1-15 minutes per week, 2-20 minutes per day, 20-60 minutes per day, or more than one hour per day) and usage of a hands-free device while driving (never, sometimes, all the time). Depending on the specification, y_{3it} is either a vector of indicator variables for the category of the vehicle (minivan, SUV, luxury car, etc.) or a scalar continuous variable, vehicle weight. 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

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

$$v_i = \exp(\alpha_i) \tag{2}$$

$$u_{it} = \exp(\varepsilon_{it}) \tag{3}$$

where s is 0.25, the period length in years, x_{it} is a vector of exogenous variables, v_i and u_{it} are unobserved multiplicative effects composed of an individual-specific effect α_i and an i.i.d. shock ε_{it}, respectively.[23] The multiplicative formulation treats unobservables α_i and ε_{it} symmetrically with observables y_2 and y_3. The coefficient on the cell phone usage variable, γ, is of primary in-

[23] 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.

terest. 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_2 and y_3; in other words, cell phone usage and vehicle safety may be endogenous. 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.

B) Poisson Estimations

Our first estimation 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 Poisson regression still yields consistent estimates of the coefficients in model (1)-(3) (as long as y_2 or y_3 are not endogenous) but is no longer MLE.[24] The Poisson model does not yield consistent estimates if y_2 or y_3 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 the following sections. Despite the incorrect assumption of exogeneity, the Poisson estimations in this section reveal correlations in the data and provide a useful baseline for more general models that correct for endogeneity.

[24] In this case Poisson regression 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.

In the first specification, P1 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 so that the coefficient of a usage category represents the incremental risk over not having a cell phone. Thus if cell phone ownership and 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 associated increase in accident risk is 1.2 times to 2.3 times, rising with the amount of usage.[25] The IRR for the 2-20 minutes/day category is significant at the 10% level and the IRR for the 20-60 minutes/day category is significant at the 5% level. The IRR for having a phone but not using it while driving is (insignificantly) less 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 1.4. 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 the next section.[26] 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, P2, is the same as P1 except for the gender-specific cell phone coefficients. Although the weighted average IRR for cell phone users, 1.3, is about the same as in P1, splitting the IRRs by gender reveals

[25] The coefficient for highest use is slightly lower than that for the next highest use, but the difference is not statistically significant.

[26] 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 1.4 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.

that the women's cell phone effects are significantly higher than the men's.[27] The men's effects are statistically insignificant, while the higher usage categories for the women are significant. RT also found that cell phone usage by women appears to be riskier than usage by men.[28] The only hands-free device IRR that is significant is also that 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 P3. 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, P4. In P3, the magnitudes of the cell phone effects are smaller than in P2 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 1.06, lower than before, which indicates that some of the correlation between usage and accidents found in P1 and P2 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.

Some of the additional covariates also have significant effects. Married drivers have lower accident risk. Age has a U-shaped effect, with the minimum accident risk occurring around age 55. Similar age patterns are also evident in official accident statistics (NHTSA, 2004). Full time employment and longer personal commuting time are correlated with increased accident risk. More daylight hours and driving mainly on rural roads are correlated with de-

[27] A Wald test of the cell phone and hands-free effects rejects the null hypothesis of equal coefficients between the sexes at the 5% level.

[28] 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.

creased accident risk. Other variables have insignificant yet plausible effects: men have more accidents than women. Higher income, annual mileage, local population density, and average local commuting time, are all correlated with higher 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 P3, the addition of the vehicular controls increases the cell phone effects a small amount for the women. The coefficients for the other covariates are generally similar to those in P2. In estimation P4, which includes controls for the vehicle type, the cell phone usage IRRs are similar to those in P3.

We also estimate models with a host of alternative samples of the data, other dependent and explanatory variables, and weighted estimations. The main alternative sample for estimation is a gender-balanced sample (P5 in Table 6).[29] The biggest change in P5 is for the IRR for women who have but do not use a cell phone while driving, which falls to 0.36. If this result were causal, which we do not believe, it would imply that the presence of the cell phone even when not used reduces the accident rate by two thirds. The analogous IRR for the men also falls to a lesser extent. We take this as evidence that whatever selection effects are present in the data, using a gender-balanced sample does not solve the problem, and indeed seems to exacerbate it.

Other samples include using all quarters of data and dropping various outliers. None of these alternatives leads to starkly different results.[30] We also experimented with weighted esti-

[29] 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.

[30] The results of these and other alternative estimations are included in Appendix B.13 of Hahn and Prieger (2004). 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

mations using the survey weights we constructed. Under the maintained assumptions of the pooled Poisson model 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 P3, but are smaller in magnitude with larger standard errors.

The following three points summarize the results from the Poisson 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. The estimated effects on accidents of cell phone usage are generally robust to alternative specifications and estimation subsamples. 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 following two subsections.

The Poisson 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.

C) A Model for Heterogeneity

This section and the next contains our preferred estimations, in which we explore our hypotheses of heterogeneity and endogeneity. Given the difficulty of estimating a model incorporating both hypotheses, we explore them one at a time (although our second model, for endogeneity, is ro-

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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 reported in Table 6.

bust to the presence of heterogeneity). First, we estimate whether the cell phone effects are heterogeneous across individuals, even after controlling for observables such as gender. We find substantial heterogeneity, and show that RT's relative risk estimate from cell phone use is likely to be greatly overstated as a result.

For our first hypothesis, that identical amounts of cell phone use affect accident risk differently across people, we modify the accident equation to be Poisson with mean

$$\mathrm{E}(y_{1it}|x_{it}, y_{2it}, y_{3it}, v_i, \eta_i) = s\,\exp(\beta' x_{it} + \tilde{\gamma}_i' y_{2it} + \delta y_{3it}) v_i \qquad (4)$$

where $\tilde{\gamma}_i$ is a random coefficient for minutes of use, possibly correlated with the individual-specific random effect v_i (defined in (2)):

$$\tilde{\gamma}_i = \bar{\gamma} + \eta_i \qquad (5)$$

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 $\eta_i = \log(1.1)$ 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. Because there is no evidence of heterogeneity in the mean accident rates after introducing α_i and covariates, we do not include u_{it} in (4).[31] The (α_i, η_i) are assumed to be independent across individuals, uncorrelated with the regressors, and normally distributed with covariance matrix

[31] Formally, we test and fail to reject that $y_{1it}|x_{it}, y_{2it}, y_{3it}$ is equidisperse relative to the variance implied by the model with v_i specified as in (5). 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_{it} is not needed. Furthermore, if ε_{it} is added to the model, the estimate of its variance is nearly zero. See Appendix B of Hahn and Prieger (2004) for details of the tests.

$$\Sigma = \begin{bmatrix} \sigma^2 & \rho\sigma\omega \\ \rho\sigma\omega & \omega^2 \end{bmatrix} \tag{6}$$

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

$$\lambda_{it} = s\ \exp(\beta' x_{it} + \bar{\gamma}' y_{2it} + \delta y_{3it})\zeta_{it} \tag{7}$$

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.[32] The 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 two-dimensional Gauss-Hermite quadrature to integrate α and η out of the likelihood (see Appendix for likelihood and details). To our knowledge, ours is the first application of a random coefficient panel Poisson model in the literature.

The results of MLE for this model for the combined-gender sample (labeled RC1) and the women-only sample (RC2) are presented in Table 7.[33] In both samples, the likelihood is maximized with $\sigma^2 = 0$. 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).[34] 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), $\hat{\sigma}^2 > 0$ 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.

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[32] We assume that $E(\alpha_i) = -\sigma^2/2$ and $E(\eta_i) = -\omega^2/2 - \rho\sigma\omega$ to ensure that $E(\zeta) = 1$ and that the constant in β is 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 $d^2 = d$ 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.

[33] 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 and the cell phone coefficients are similar to those in estimation P3.

[34] 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).

The means of the cell phone usage coefficients, $\bar{\gamma}$, are not far from the analogous Poisson 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 using a cell phone 1-15 minutes per week, 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 (such as RT's case-crossover analysis or panel fixed effects methods) 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 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.[35] Equating these leads to the conversion formula

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

$$RR = \frac{IRR - 1}{f} + 1 \tag{8}$$

We use equation (8) with Cohen and Graham's "central" estimate of f of 2% and the average IRR from our random coefficient models to analyze how much RT's estimates may be overstated. The results, in Table 8, imply 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%. Looking at the results another way, the figures imply that risk from cell phone use may be 27% lower than RT's estimate. Note that the reduction in the accident effect relies solely on hypothesis 1, and does not correct for the potential endogeneity of usage. The evidence in the next section indicates that correcting for endogenous cell phone use greatly reduces the magnitude of the cell phone effects.

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%.[36] If RT's estimates are not representative of the population, usingthem for purposes of cost-benefit analyses will overstate the number of accidents prevented by a cell phone ban. To compare 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 tuned analysis, unlike previous analyses that based calculations on national averages and out-of-sample assumptions about accident rates and cell phone usage.

[36] 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 0.03%

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 9 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 (4) with all phone usage and hands-free device indicator variables set to zero.[37] Given that compliance with an actual ban would not be perfect, our estimates are upper bounds on accident reductions.

In Table 9 we report reductions in accidents based on the random coefficient estimations. The estimated reductions are 0.9-1.9%. All of these are lower than Cohen and Graham's (2003) central estimate of 6%. Note that, in contrast to previous analyses, the standard errors are large enough to include the possibility that there is no effect of a ban at all. Given that, in addition, 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.

D) A Model for Endogeneity

We turn now to our second hypothesis, which is that the use of cell phones and hands-free devices while driving is endogenous, and show that after controlling for endogeneity, cell phones do not appear to increase accidents and hands-free devices do not appear to reduce accidents. The endogeneity is due to the unobserved type of the driver, which incorporates attitudes toward risk and the individual's degree of carelessness. The unobserved type is taken to be constant over our relatively short time span and fully captured by the individual effect α_i from (2). The instruments we use for these variables are inspired by Hausman and Taylor's (1981) estima-

[37] For the mean accident calculations, v_i in (1) is replaced with its expected value (unity) in the RC model. Mean accident rates are calculated using actual covariate values for each driver and are the average over the sample.

tor for linear models in which endogeneity is due to correlation of a time-varying regressor, here cell phone usage, with the individual-specific error α_i but not the error peculiar to an individual and a time period, ε_{it}. In such cases, instruments may be found from "inside" the equation: the deviations from an individual's mean of the endogenous regressor over time will be uncorrelated with the individual-specific error. Given that the deviations from the time-means are valid instruments only if cell phone usage is not systematically related to ε_{it} , we test this assumption.

We also treat vehicle safety choice as endogenous. We use the logarithm of vehicle weight as a single characteristic to control for vehicle safety choice. We use weight instead of the vehicle categories used in estimation P4 for four reasons. First, the number of good instruments available for vehicle choice is less than the number of vehicle categories. Second, car weight has a significant coefficient in the accident equation in Poisson estimations when the vehicle category indicators are not present. 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.[38] 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.[39] We instruments for car weight with local gasoline prices and weather variables.

Linear IV methods, which assume additive means and errors, are not appropriate for the multiplicative mean and error of accident equation (1). We instead define appropriate moment conditions for (1) and use the nonlinear instrumental variables (NLIV) estimator (Amemiya,

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[38] 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.

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

1974). To recast the Poisson model (1)-(3) in the NLIV framework, note that (1) implicitly defines a multiplicative model

$$y_{1it} = s \exp(\beta' x_{it} + \gamma' y_{2it} + \delta y_{3it})\, \xi_{it} \tag{9}$$

where $\xi_{it} = v_i u_{it} e_{it}$, e_{it} is a multiplicative error satisfying $E(e_{it}|x_{it}, y_{2it}, y_{3it}, v_i, u_{it}) = 1$ by definition, and other notation follows (1)-(3) (Windmeijer and Santos Silva, 1997). The endogeneity of y_2 (binary indicator variables for the cell phone and hands-free device usage categories) and y_3 (log car weight) implies that $E(\xi_{it}|x_{it}, y_{2it}, y_{3it}) \neq 1$. As discussed above, we restrict the correlation between y_2 and ξ_{it} to come only through v_i, the individual-specific error. In particular, we assume that $E(u_{it}|x_i, y_{2it}) = 1$. We place no such restriction on y_3. The model does not explicitly include random coefficients, but if they are present as in (4)-(5) then (under slightly stronger assumptions than are made above) our estimate of the cell phone effect γ is still consistent for the average effect $\bar{\gamma}$ (see Appendix A.3). If instruments z_{it} can be found such that $E(v_i u_{it}|z_{it}) = 1$, then $E(\xi_{it} - 1|z_{it}) = 0$ (see the appendix). Solving for ξ_{it} from (9) leads to the conditional moment condition

$$E\left(\frac{y_{1it}}{s \exp\left(\beta' x_{it} + \gamma' y_{2it} + \delta y_{3it}\right)} - 1 \middle| z_{it} \right) = 0 . \tag{10}$$

The NLIV procedure minimizes the objective function $(\xi - 1)'\mathbf{Z}'(\mathbf{Z}'\mathbf{Z})^{-1}\mathbf{Z}(\xi - 1)$, in the usual matrix notation, where ξ is replaced with functions of the data as in (10). Following our treatment in the previous sections, we pool the data and adjust standard errors for clustering on individuals.[40] The NLIV estimator is consistent even if accidents do not follow a Poisson stochastic process or (y_2,y_3) are endogenous, as long as the specification of the mean is correct and the instruments are valid.

[40] Moment condition (10) is valid even if observations across time for an individual are not independent, and estimates from pooling the data are consistent.

The cell phone usage categories for y_2 are collapsed from those used in the Poisson estimations. Usage is collapsed into two categories: 1-15 minutes per week and higher amounts of usage. The purpose of collapsing the categories is to increase the precision of the instrumental variables estimations, because convergence was difficult to obtain with more finely cut categories. We treat the decision to own a cell phone as exogenous.

Following Hausman and Taylor (1981), the instruments include all exogenous variables and the deviations from an individual's mean over time of all time-varying variables (except car weight), including y_2. For example, the deviation from average cell phone use while driving for an individual, $y_{2it} - \bar{y}_{2i}$, is a valid instrument for y_{2it} under the maintained assumptions (which we test). However, the deviations from average use are not as good instruments for the men as for the women, because there is less deviation from average use for the men: the "within" standard deviation for cell phone usage for men is only about two-thirds what it is for the women. This led to difficulty obtaining convergence of the estimator for the combined sample. Therefore we estimate the NLIV model for the women-only sample. Given that the previous estimations suggested that impacts of cell phone use on accidents are not significant for the men, we do not view this as a drawback. Following Windmeijer and Santos Silva (1997), the predicted values of the binary endogenous variables from first stage probit regressions are also included as instruments.

For car weight, the additional instruments are local gasoline price in levels and squares and two weather variables, the number of days with snowfall and total snowfall depth. The gas price is for the MSA, where available, or for the state. After controlling for miles traveled, the price of gas should not affect the accident rate. Households in areas with more snowfall may be more likely to purchase heavier vehicles such as SUVs and other vehicles with four-wheel drive and traction control, both of which add to vehicle weight. As with the weather variables included in x, snowfall is measured at the weather station closest to the driver's household. To ensure the snowfall instruments are properly excluded from the accident equation (9), we follow Gayer (2004), who also uses snow-related variables as instruments, and use snow measurements at a time other than the current period *t*. Here, we use measurements for the non-current year of our sample (i.e., for quarters in 2001 we use snowfall from 2002, and vice versa). Given that we already control for current weather in the accident equation, out-of-period snowfall measurements should not violate equation (10).[41]

Table 10 contains the estimation results for the NLIV model, labeled IV, as well as for the analogous Poisson model, labeled P6. The cell phone and hands-free coefficients (here, for women) are of greatest interest, given the results of the Poisson estimations. Estimation P6 shows a pattern similar to the previous Poisson estimations: women who use the phone more than 15 minutes per week while driving have significantly more accidents (IRR = 1.65), and women who use a hands-free device all the time have significantly fewer accidents (IRR = 0.52). Once the endogeneity of usage is controlled for in the IV estimation , the picture changes markedly. There is evidence that cell phone usage and car weight are indeed endogenous: Hausman tests reject the null hypothesis of exogeneity at any reasonable significance level (see Table 10).[42] Importantly, the magnitude of the cell phone usage coefficients drops in the IV estimation. The IRR for women who use the phone more than 15 minutes per week while driving falls from 1.65 to 1.19. Neither cell phone usage coefficient is significant once we control for endogeneity. Regarding hands-free device usage, neither coefficient is significant, and the IRR for the "always use" category rises from 0.52 to 0.73. Finding that hands-free devices lead to no significant reduction in accidents is in accord with many other field and laboratory studies (e.g., RT; Haigney

[41] If the two other-year snow variables are included in estimation P3, they are not statistically significant, corroborating evidence that they satisfy the exclusion restriction.
[42] The Hausman tests were conducted in comparison to pooled Poisson MLE (estimation P6). Alternative Hausman tests, comparing the NLIV estimates to estimates from a random effects Poisson MLE, also convincingly reject the exogeneity of the cell phone, hands free device, and vehicle weight variables.

and Taylor, 1999; Crawford *et al.*, 2001; Strayer and Johnston, 2001; and Strayer, Drews, and Johnston, 2003).[43] Together, the results for cell phone and hands free device usage indicate that a large part of the apparent connection for women between usage and accidents (if not all) in the Poisson and random coefficient estimations is due to endogeneity. Of less importance for our main investigation in this paper, but interesting in its own right, is that sign of the car weight variable switches to positive in the IV estimation. This finding is in accord with a recent study indicating that heavier vehicles crash more than lighter vehicles (Gayer, 2004).

IV estimation can lead to misleading inference if the instruments are invalid or weak. We test the validity of the instruments with tests of the overidentifying restrictions in the models. We report four overidentification tests in Table 10: an F statistic assuming homoskedastic errors, and three tests robust to clustering on individuals: Hansen's J statistic, a C statistic to test the cell phone usage and hands free "deviations from individual mean" instruments, and another C statistic to test the gasoline price and weather instruments for car weight (see Hayashi (2000) for details of these tests). None of these tests reject the null hypothesis that the instruments are valid at conventional significance levels. Although formal tests for weak instruments are available for linear IV, these do not apply to our model with multiplicative mean and errors. As noted in Cameron and Trivedi (2005), however, formal tests are unnecessary to some extent because weak instruments are easily detected if standard errors are much larger when IV is used. The standard errors of the IV estimates are generally about twice the size (or less) of the corresponding Poisson standard errors, which is better than the performance of IV in many published stud-

[43] 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.

ies.[44] Even if the standard errors in the IV estimation were as small as those in estimation P6, the cell phone and hands free device usage coefficients would still be statistically insignificant.

E) Alternative Estimations

In this final estimation section we briefly mention alternative estimations we tried: multiple-equation parametric models for endogeneity, linear IV models, and fixed effects models. Each is an alternative to the IV models for endogeneity presented here for hypothesis two. These methods do not, however, incorporate random coefficients. Specific results are presented in Hahn and Prieger (2004); here we discuss the approaches and the general results.

The system estimations include the accident equation (Poisson), a cell phone usage equation (ordered probit) and a vehicle weight equation (normal). The system includes individual-specific random effects with correlation between equations. The coefficients and correlation parameters are estimated by MLE based on the likelihood of the mixed (continuous and discrete) data. Unlike the IV estimations, with this model we cannot reject the hypothesis that cell phone usage while driving is exogenous, due to large standard errors. When the second equation is for the usage of hands-free devices, however, we do find selection effects. In particular, we find negative correlation between the accident and the hands-free device equations, implying that unobserved factors making an individual more likely to use a hands-free device also make the individual a safer driver, independent of any causal effect of cell phone usage mode. This finding is in accord with our NLIV results, but is stronger in that it reveals the sign of the correlation and thus the nature of the selection. There is also no evidence of significant reductions in accidents from the use of hands-free devices. Finally, we also find that the effects of minutes of cell phone

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[44] For example, in Levine and Zimmerman (2005), IV standard errors are about five times their OLS counterparts. In Cohen and Dehejia (2004), the same multiple is four. In neither case are weak instruments discussed. These studies were selected at random by finding the most recent articles in *The Journal of Public Economics* and *The Journal of Law and Economics*, respectively, that used IV estimation. Cameron and Trivedi (2005) use a multiple of 10 as an example of weak instruments.

usage while driving are smaller than in the Poisson models, despite the fact that minutes of usage are treated as exogenous.

The linear IV model we performed is a standard Hausman-Taylor (1982) estimation, which differs from the NLIV estimations presented above by assuming an additive specification of the conditional mean and the errors. The linear IV results are consistent with the NLIV model—when the phone, hands-free usage, and vehicle weight variables are treated as endogenous all statistical significance of the impacts of the cell phone and hands-free usage variables goes away—but the estimates are more imprecise than the NLIV estimates. These results confirm the findings from estimation IV 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).

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.[46] 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)-(3), and require neither instruments nor the parametric assumptions of the 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 random coefficient model 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 IRRs for cell phone use from FE models are much higher than the analogous figures from the Poisson and random coefficient models, which is consistent with selection into the accident sam-

[46] We cannot replicate RT's case-crossover analysis exactly because we do not have closely spaced point-in-time observations on cell phone usage.

ple created by the random coefficient model. There is no significant impact from usage of hands-free devices in these FE estimations.

VI) Conclusion

Our new approach for estimating the relationship between cell phone use while driving and accidents is the first to test for driver heterogeneity and selection effects and the first that allows direct estimation of the impact of a cell phone ban while driving. We have two key findings. First, 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. This result implies that previous estimates of the impact of cell phone use on risk for the population, based on accident-only samples, may therefore be overstated by 36%. Second, 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 Connecticut, New York, New Jersey, and Washington, D.C. 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, for example, differences in differences estimators. 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.[47] Disentangling these two explanations would be complicated by the fact that the effects of a hand-held ban are likely to be small.[48] 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 impor-

[47] 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.
[48] As noted earlier, however, there is little research supporting the view that existing hands-free technology will reduce accidents.

tant 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.

Appendix

This appendix contains brief additional information on the data and estimations. Additional supplementary material and greater detail can be found in the working paper (Hahn and Prieger, 2004; note to referees: available online at http://www.aei-brookings.com/publications/abstract.php?pid=806) and its appendices. Appendix B referred to in the text is from the working paper.

A.1 Survey Weights

Survey weights for our data were constructed to make each cross section representative of the general population in the mainland United States 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 United States.

A.2 Likelihood of the Random Coefficient Model

Here we present the likelihood for the model defined in equations (3)-(7), a random coefficient model for panel count data with random effects. The density of the observed data y_1 is Poisson mixed over (v_i, η_i). Thus the log likelihood for MLE is

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

where λ_{it} is the Poisson conditional mean from (7),

$$\mu = \left(\sigma^2/2, -\omega^2/2 - \rho\sigma\omega\right)'$$

and Σ is as in (6). See the footnote following (7) on identification. This likelihood is evaluated with bivariate 32-point Gauss-Hermite quadrature. MLE is performed using the BFGS variant of the DFP algorithm with numerical derivatives.

A.3 The Moment Condition for NLIV

Here we show that $E(\xi_i|z_i) = 1$, as claimed in the text. Drop subscripts and define $w = (x, y_2, y_3)$. We have $E(\xi|z) = E_{v,u,w|z}[E(\xi|v,u,w,z)|z]$ by iterated expectations. Then $E(\xi|v,u,w,z) = vuE(e|v,u,w,z) = vu$, and if $E(vu|z) = 1$, it follows that $E(\xi|z) = 1$. It follows that (10) is a valid moment condition for NLIV. If random coefficients are present, then the additional random term from the random coefficient is swept into the error term ξ_{it} as in (7). If the slightly stronger assumption that $E(vu\exp(\eta d)|z) = 1$ is satisfied, where the additional notation is from (7), then $E(\xi|z) = 1$ as above. The assumption requires that not only are deviations from an individual's mean of y_2 over time uncorrelated with the individual-specific error v, they are also uncorrelated with the individual-specific random coefficient. Given that we model both v and η as time-invariant, the additional assumption appears reasonable. If this assumption is not satisfied, it would (in principle) be detected by the specification tests reported in Table 10.

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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:							
No cell phone	26,572	Q	0.162	0.369	0.000	1.000	Survey
1-15 mins/wk	26,572	C	0.474	0.499	0.000	1.000	Survey
2-20 mins/day	26,572	C	0.152	0.359	0.000	1.000	Survey
20-60 mins/day	26,572	C	0.066	0.248	0.000	1.000	Survey
> 1 hour/day	26,572	C	0.024	0.153	0.000	1.000	Survey
No cell phone, male	26,572	Q	0.058	0.233	0.000	1.000	Survey
No cell phone, female	26,572	Q	0.105	0.306	0.000	1.000	Survey
1-15 mins/wk, male	26,572	C	0.140	0.347	0.000	1.000	Survey
1-15 mins/wk, female	26,572	C	0.335	0.472	0.000	1.000	Survey
2-20 mins/day, male	26,572	C	0.056	0.231	0.000	1.000	Survey
2-20 mins/day, female	26,572	C	0.095	0.294	0.000	1.000	Survey
20-60 mins/day, male	26,572	C	0.027	0.161	0.000	1.000	Survey
20-60 mins/day, female	26,572	C	0.039	0.194	0.000	1.000	Survey
> 1 hour/day, male	26,572	C	0.012	0.107	0.000	1.000	Survey
> 1 hour/day, female	26,572	C	0.012	0.110	0.000	1.000	Survey
Use of hands free device while driving:							
Sometimes use	26,572	H	0.151	0.358	0.000	1.000	Survey
Always use	26,572	H	0.145	0.352	0.000	1.000	Survey
Sometimes use, male	26,572	H	0.056	0.229	0.000	1.000	Survey
Sometimes use, female	26,572	H	0.095	0.294	0.000	1.000	Survey
Always use, male	26,572	H	0.053	0.225	0.000	1.000	Survey
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
Commute time in 3-digit ZIP area (log)	26,564	O	3.321	0.129	2.98	3.69	Census
Commute Time, log of driver's	26,572	Y	2.865	1.110	0.000	5.704	Survey
Drive mostly on city surface streets	26,572	Y	0.322	0.467	0.000	1.000	Survey
Drive mostly on rural freeways	26,572	Y	0.187	0.390	0.000	1.000	Survey
Drive mostly on rural surface streets	26,572	Y	0.064	0.245	0.000	1.000	Survey
Female	26,572	O	0.670	0.470	0.000	1.000	Survey
Freezing, # days below	26,572	Q	18.037	24.73	0.000	90.00	b
Hours of daylight, average	26,572	Q	12.108	1.671	9.217	14.86	c
Income (household income)	26,572	O	84.534	52.72	5.279	349.7	Survey
Kids 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
Miles (quarterly mileage driven, log)	26,572	Y	1.041	0.931	-8.294	3.359	Survey
Minivan (vehicle type indicator)	25,251	Q	0.114	0.318	0.000	1.000	d
Pickup Truck (vehicle type indicator)	25,251	Q	0.104	0.305	0.000	1.000	d
Pop. density within 25 mi. of household (log)	26,572	O	5.994	1.466	-1.09	9.38	Census
Precipitation days, # of	26,572	Q	5.525	3.996	0.000	30.00	b
Quarter indicator for 1Q2002	26,572	Q	0.243	0.429	0.000	1.000	Survey
Quarter indicator for 2Q2002	26,572	Q	0.256	0.437	0.000	1.000	Survey
Quarter indicator for 3Q2002	26,572	Q	0.268	0.443	0.000	1.000	Survey

Snow days, # of	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
Van (vehicle type indicator)	25,251	Q	0.005	0.068	0.000	1.000	d
Vehicle weight, log of driver's	25,251	Q	1.253	0.212	0.703	2.000	a
Work full time	26,572	O	0.589	0.492	0.000	1.000	Survey
Additional variables used as instruments:							
Gasoline price (in city or state)	26,572	Q	1.345	0.155	1.041	1.680	e
Snow days in the other year, # of	26,572	Y	19.803	29.77	0.000	220	b
Snowfall in the other year, inches of	26,572	Y	147.23	248.8	0.000	2962	b

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

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] *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 Respon-dents (com-pletes & in-completes)** 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 In-come					
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

Study or Poll	Time Period	% of drivers who use a cell phone while driving, out of… All Drivers	% of drivers who use a cell phone while driving, out of… Drivers who Have a Cell Phone	% of drivers who use HF device while driving, out of… All Drivers	% of drivers who use HF device while driving, out of… 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?" Weighted average is calculated using the survey weights. 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: Poisson Estimation with Combined-Gender Cell Phone Effects

	P1	
	IRR	*P*-value
Cell Phone Minutes of Use		
None	0.827	0.419
1-15 mins/week	1.217	0.262
2-20 mins/day	1.464*	0.073
20-60 mins/day	2.309***	0.000
> 1 hr/day	1.567	0.210
Hands-Free Device Usage		
Hands free sometimes	1.138	0.394
Hands free always	0.733	0.069
Average cell phone IRR	1.368	
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. Excluded cell phone dummy is "no phone". IRR is incident risk ratio, $\exp(\hat{\beta})$. *P*-values are for the hypothesis test that the estimated coefficient (log IRR) is zero and are calculated from 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 and hands-free device category.

Table 6: Accidents: Poisson Estimations with Gender-Specific Cell Phone Effects

	P2		P3		P4		P5	
	IRR	*P*-value	IRR	*P*-value	IRR	*P*-value	IRR	*P*-value
Men: have phone, no use	1.073	0.839	1.181	0.627	1.235	0.536	0.856	0.740
Men: 1-15 mins/week,	1.134	0.651	1.097	0.742	1.053	0.856	0.613	0.200
Men: 2-20 mins/day	0.899	0.757	0.715	0.340	0.725	0.379	0.426	0.081
Men: 20-60 mins/day	1.232	0.598	0.984	0.968	0.981	0.963	0.574	0.269
Men: > 1 hr/day	0.204	0.133	0.173	0.099	0.208	0.141	0.183	0.134
Women: have phone, no use	0.705	0.279	0.817	0.534	0.749	0.396	0.355	0.108
Women: 1-15 mins/week,	1.273	0.282	1.145	0.547	1.164	0.503	1.052	0.888
Women: 2-20 mins/day	1.898*	0.016	1.391	0.209	1.306	0.321	1.650	0.214
Women: 20-60 mins/day	3.269**	0.000	2.180**	0.008	2.224**	0.008	1.956	0.174
Women: > 1 hr/day	3.714**	0.001	2.442*	0.018	2.545*	0.021	1.165	0.840
Men: hands free some	1.506	0.096	1.265	0.331	1.246	0.383	1.894	0.057
Men: hands free always	1.202	0.473	1.156	0.567	1.078	0.783	1.731	0.130
Women: hands free some	0.973	0.886	0.869	0.458	0.896	0.570	1.084	0.802
Women: hands free always	0.520**	0.006	0.495**	0.003	0.499**	0.003	0.385*	0.021
Female	0.759	0.353	0.865	0.630	0.870	0.644	0.720	0.428
Married			0.695**	0.004	0.701**	0.007	0.684*	0.049
Kids in household			1.134	0.314	1.170	0.232	1.006	0.976
Age			0.899**	0.000	0.904**	0.000	0.897**	0.000
Age Squared			1.001**	0.000	1.001**	0.000	1.001**	0.001
Income (log)			0.976	0.770	1.005	0.953	0.952	0.686
Work Full Time			1.438**	0.008	1.492**	0.004	1.232	0.281
Miles driven (log)			1.119	0.134	1.131	0.123	1.114	0.178
Commute time (log)			1.147*	0.019	1.157*	0.015	1.198*	0.050
Rural freeways			0.792	0.169	0.831	0.285	0.924	0.744
Urban surface streets			1.136	0.308	1.137	0.318	1.098	0.633
Rural surface streets			0.550	0.083	0.591	0.131	0.322	0.123
Area pop. density (log)			1.095	0.112	1.096	0.125	1.052	0.524
Area commute time (log)			1.436	0.514	1.222	0.726	1.001	0.999
Precipitation days			0.995	0.765	0.993	0.682	0.970	0.290
Snow days			0.985	0.189	0.976*	0.046	0.983	0.345
Days below freezing			0.993	0.236	0.996	0.534	0.995	0.540
Hours of light daily			0.614*	0.021	0.600*	0.021	0.610	0.127
Pickup					0.680	0.103		
Minivan					0.942	0.761		
SUV					0.815	0.157		
Luxury					0.740	0.198		
Sporty					0.735	0.262		
Van					0.668	0.667		
Average cell phone IRR	1.303		1.062		1.048		0.902	
χ^2 statistic (dof)	95.6 (57)	0.001	228.3 (74)	0.000	227.9 (80)	0.000	14051 (74)	0.000
Log likelihood	-1804.93		-1804.37		-1703.40		-725.24	
N	26,572		26,564		25,243		11,614	

* and ** denote significance at the 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 and hands-free device category. P5 uses the gender-balanced sample; see text for details. See notes to Table 5 on IRR and *p*-values.

Table 7: Accidents: Random Coefficient (RC) Model for Cell Phone Usage

Variable		RC1 Men and Women Combined IRR	RC1 P-value	RC2 Women Only IRR	RC2 P-value
β_1	Have phone, no use	0.948	0.832	0.745	0.403
$\bar{\gamma}_1$	Use 1-15 mins/week	1.114	0.557	1.191	0.480
$\bar{\gamma}_2$	Use 2-20 mins/day	1.064	0.777	1.392	0.259
$\bar{\gamma}_3$	Use 20-60 mins/day	1.709*	0.034	2.337*	0.011
$\bar{\gamma}_4$	Use > 1 hr/day	1.090	0.839	2.236	0.119
β_1	HFreeSome	1.051	0.753	0.975	0.897
β_1	HFreeAlwys	0.686*	0.056	0.499**	0.012
δ	Log Vehicle Weight	0.462***	0.007	0.431**	0.026
	Other controls as in P3	yes		yes	
	Average cell phone usage IRR	1.100		1.177	
		parameter		*parameter*	
σ^2		0.000	(fixed)†	0.000	(fixed)†
ω		0.489	0.194	0.709***	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 P3 [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: $\omega = 0$ vs. H_A: $\omega > 0$. It has a non-standard distribution because ω is on the boundary of the parameter space under the null hypothesis (Self and Liang, 1987). See notes to Table 5 on IRR and p-values. 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. Results for the men-only sample (RC3) are not reported; both σ^2 and ω were negligible and the cell phone coefficients are similar to those in estimation P3.

Table 8: Implications of the Random Coefficient Model for RT's Estimates of Relative Risk

	Model RC1 (both genders)	**Model RC2 (women only)**
Average IRR from cell phone use, relative to having but not using a cell phone while driving	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%
Implied overstatement of *RR* if use accident-only sample	36.3%	36.0%
RT's estimate of relative risk (*RR*)	4.3	4.8
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. IRR is calculated relative to having a cell phone but not using it while driving (instead of relative to not having a phone, as in the other tables) to maintain comparability to Redelmeier and Tibshirani (1997), who use a sample of cell phone users. 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 is calculated using equation (9) in the text. Row five *RR* is from Redelmeier and Tibshirani (1997). Row six is calculated as (row five)/(1 + row four). See notes to Table 5 on IRR.

Table 9: Reduction in Accidents from a Ban on Cell Phone Use While Driving

	High Estimate	Central Estimate	Low Estimate
Point estimate	1.9%	1.5%	0.9%
Standard error	0.165	0.129	0.078
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: Calculations are based on estimations RC2 and RC3. Standard errors 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 using actual covariate values for each driver and are the average over the sample using the survey weights. 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.

Table 10: Accidents: Poisson and IV Estimations (Women Only)

	P6			IV		
	coef.	s.e.	IRR	coef.	s.e.	IRR
Women:						
Have phone, no use	-0.254	0.336	0.776	0.041	0.548	1.042
Use phone 1-15 mins/week	0.176	0.241	1.193	0.122	0.437	1.130
Use phone > 15 mins/week	0.499*	0.269	1.646	0.171	0.537	1.187
Hands-free device: some	-0.095	0.198	0.909	-0.455	0.343	0.635
Hands-free device: always	-0.644**	0.264	0.525	-0.316	0.473	0.729
Car weight (log)	-0.875**	0.367	0.417	1.998***	0.728	7.373
Married	-0.378**	0.162	0.685	-0.447	0.293	0.639
Kids in household	0.173	0.170	1.189	-0.021	0.365	0.980
Age	-1.181***	0.321	0.307	-1.663**	0.685	0.189
Age squared	0.118***	0.035	1.125	0.165**	0.075	1.179
Income (log)	0.083	0.145	1.086	-0.061	0.251	0.941
Work full time	0.415**	0.178	1.515	0.357	0.299	1.429
Miles driven (log)	0.145*	0.085	1.156	0.176	0.117	1.193
Commute time (log)	0.074	0.086	1.077	0.114	0.158	1.121
Rural freeways	-0.073	0.211	0.929	-0.021	0.397	0.979
Urban surface streets	-0.045	0.177	0.956	-0.167	0.322	0.846
Rural surface streets	-0.506	0.441	0.603	-0.954	0.605	0.385
Area pop. density (log)	0.125*	0.075	1.134	0.156	0.141	1.169
Area commute time (log)	-0.424	0.691	0.654	-0.695	1.313	0.499
Precipitation days	-0.004	0.019	0.996	-0.023	0.035	0.978
Snow days	-0.013	0.021	0.987	-0.064**	0.029	0.938
Days below freezing	-0.007	0.006	0.993	-0.001	0.010	0.999
Hours of light daily	-0.122**	0.059	0.885	-0.087	0.111	0.916
	distribution	statistic	*p*-val	distribution	statistic	*p*-val
χ^2 statistic (Wald test)	$\chi^2(24)$	1,661.0	0.000	$\chi^2(24)$	1069.5	0.000
OverID test statistic 1 (*F*)				$F(16,16936)$	1.05	0.598
OverID test statistic 2 (*J*)				$\chi^2(16)$	21.6	0.156
OverID test statistic 3 (C_1)				$\chi^2(3)$	2.44	0.487
OverID test statistic 4 (C_2)				$\chi^2(4)$	7.00	0.135
Exogeneity test statistic 1	$\chi^2(23)$	64.7	0.000			
Exogeneity test statistic 2	$\chi^2(5)$	29.2	0.000			
N		16,961			16,961	

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

Notes: Dependent variable is the quarterly traffic accident count for an individual. Omitted cell phone dummy is “no phone”. Sample covers Q4 2001—Q3 2002. Standard errors, *p*-values, and test statistics are robust to heteroskedasticity and clustering on individuals, except for *OverID test statistic 1*. The χ^2 *statistic (Wald test)* is for the joint significance of all coefficients. The *OverID test statistics* test the null hypothesis that the identifying instruments are uncorrelated with the error term in eqn. (3) and that they are correctly excluded from eqn. (3). Statistic F assumes homoskedastic errors. Statistic J is Hansen’s J statistic and is robust to heteroskedasticity and clustering on individuals. Statistic C_1 tests only the cell phone usage instruments and statistic C_2 tests only the instruments for car weight. The *Exogeneity test statistics* are for the Hausman test that the cell phone, hands-free, and vehicle weight variables are exogenous. Hausman test statistics are with reference to pooled MLE. Hausman statistic 1 tests all coefficients except the constant; statistic 2 tests only the coefficients for the variables treated as endogenous.

Figure 1: Factors Affecting Collision Risk (Hypothesis 1)

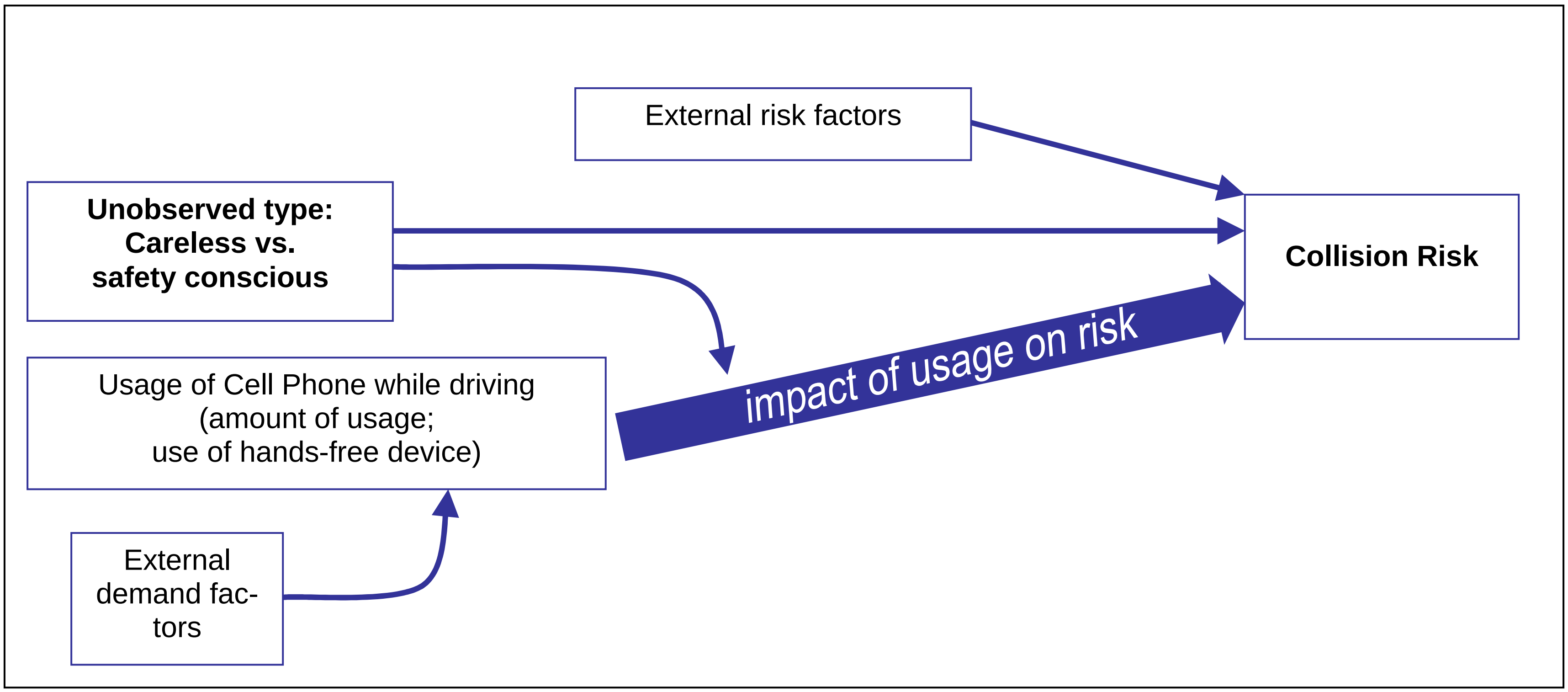

Figure 2: Factors Affecting Collision Risk (Hypothesis 2)