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The Impact of Driver Cell Phone Use on Accidents

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Executive Summary

Cell phone use is increasing worldwide, leading to a concern that cell phone use while driving increases accidents. Several countries, three 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 be overstated by about one-third. Second, once we correct for endogeneity, there is no significant effect of hands-free or hand-held cell phone use on accidents.

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The Impact of Driver Cell Phone Use on Accidents*

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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, three 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.

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KEYWORDS: cellular telephones and driving, safety regulation, selection effects

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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 182 million, and revenue has climbed from under $1 million to $105 billion per year. 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 exceeds the number of traditional, fixed line phones both worldwide and 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. California, 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 legislation 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 indi-

[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 2004 (FCC, 2005). Subscriber data for Europe is from Q4 2004 (see http://www.3g.co.uk/PR/June2005/1651.htm), from Forrester Research. Data on the number of lines 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 (2005).

[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.

[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.

vidual-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.

Our econometric models assume that 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 (such as age, sex, or income) that the econometrician observes, which raises the question of selection bias for any estimation sample.

Our hypothesis is that the same amount of usage increases some drivers' risk more than others'. If the driver's unobserved type influences the relationship between usage and accident risk, then usage risk is heterogeneous across drivers. 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. 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.

We find support for the hypothesis. The impact of cell phone use on accidents varies across the sample, 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%.

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. Since we consider a total ban on usage, our results also call into question partial bans (on hand-held usage only) such as the ones passed in California, Connecticut, New York, New Jersey, and Washington, D.C.

The plan of the paper is as follows. The next section introduces 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 IIV, 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 and concave, and that an interior solution $(a^*,y^*) > 0$ exists. Finally, assume that v exhibits constant absolute risk aversion, parameterized by r.[6]

[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)

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 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 $da^*/dr > 0$ and $dy^*/dr > 0$.[8] Q.E.D.

The implication of the proposition for empirical work is that even when controlling for all observed characteristics, if drivers vary in their attitudes toward

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family of preference scaling functions, for example, which allows both constant and decreasing absolute risk aversion.

[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.

risk and their risky driving behavior, both unobserved, then the naïve observed correlation between cell phone use and accidents overstates the true causal risk. With panel data such as we have, we avoid this problem by including an individual-specific effect to capture the driver's unobserved choice of *a*. Furthermore, since in general the causal effect of cell phone use on accidents is likely to depend on *a* (i.e., $\partial^2 p/\partial a \partial y \neq 0$), in our empirical model we allow the causal effect to be correlated with the individual-specific effect and to vary among individuals.

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 referred to as 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 analysis. 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).

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 dis-

[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).

cussions 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 who had accidents, the representativeness of the sample is open to question, particularly if our hypothesis discussed above is true. If usage risk varies across drivers, 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.

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

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

[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

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). 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.[14] 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-

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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.

[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.

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.

The retrospective survey data are subject to error if subjects do not accurately recall how much they used a phone while driving in the past. Regarding the amount of usage, however, respondents had only to assess their average usage during a calendar year. The quarterly recall of when a subject had a phone might be more subject to error. However, the majority of respondents (71%) whose possession of a phone during the sample period varied had a simple pattern: they did not have a phone in the early part of the sample, and did at the end. One plausible explanation is that individuals began to use a cell phone for the first time during the sample period.[19] We do not believe recalling which quarter one first started using a cell phone is that difficult if it was within the last 16 months. Accident recall may be more difficult for respondents, but again they only had to place it into the correct three month period. It is important to note, however, that the survey did not require the respondent to check their records of cell phone bills or accident reports. Therefore, in the estimations below, we test the sensitivity of the estimates to varying the recall length of the sample. We do not find that our conclusions change if we use longer or shorter panel lengths. Nevertheless, if there is mismeasurement in the cell phone usage variable due to respondents' faulty recall, then the estimated connection between usage and accidents may appear weaker than it actually is.

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

[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 these and other variables is in Table 1.

[19] Recall that mobile telephony in the sample period was not as ubiquitous as it is today.

ter) 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 are not a random sample from the population *(*i.e., they are Internet users and were willing to complete the survey), we explore how representative our sample is through several means. First, note that about 68% of adults in the U.S. used the Internet at the time our survey was administered.[20] 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. Finally, our sample contains a disproportionate number of females: two-thirds of the respondents in our sample are female.[21] 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.

Given that we control for demographics and that survey weights are available, a remaining concern is that differences between our sample and the population in observed characteristics indicate that there are also differences in unobserved factors influencing risk from phone usage. If so, then our results could not be extrapolated to the population. This potential criticism could also be leveled at RT, who do not attempt to balance their sample toward the population. RT did not find that relative risk from usage varied significantly with observed demographic attributes. However, our critique of RT is not based on the demographic

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[20] 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.

[21] 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.

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	44.93	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 free-ways	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	18.04	24.73	0.000	90.00	b
Hours of daylight, average	26,572	Q	12.11	1.671	9.217	14.86	c
Income (household income)	26,572	O	84.53	52.72	5.279	349.7	Survey
Children in household	26,572	O	0.471	0.499	0.000	1.000	Survey

Continued next page

Table 1: Summary Statistics of the Data (continued)

Variable	Obs.	Freq.	Mean	Std. Dev.	Min	Max	Source
Continued from previous page							
Luxury Car (vehicle type indicator)	25,251	Q	0.082	0.274	0.000	1.000	d
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

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 is asked annually but variable is calculated in conjunction with the quarterly cell phone use variable.

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

*Significant at the 1% level.
†Calculated from gender-specific online access rates from Pew Research Center (2003b) from March 2003 and the gender ratio from the CPS in column one.
Figures for Online Households are from NFO Worldgroup (unpublished). Figures for our estimation sample are for the pooled four-quarter data set. *CPS* is the Current Population Survey, conducted by the U.S. Bureau of the Census for the U.S. Bureau of Labor Statistics.

composition of their sample, but rather on the fact that they select on having an accident, an observable characteristic that is likely to be correlated with the magnitude of the risk from usage.

There are no official statistics on cell phone usage while driving. We instead compare our survey results with other 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 (after weighting) 28% of drivers and 44% of those who use a cell phone while driving use a hands-free device of some sort at least sometimes with their phone while driving. These figures are also higher than the external estimates. Our estimates of phone use while driving may be higher than other estimates because our question was very broad: a driver is categorized as a cell phone user if they answer anything other than “never” to the usage while driving question. Some of the other surveys lumped “rarely or never” responses together as non-users. Furthermore, given the evidence mentioned above that Internet surveys can elicit more candid answers than telephone surveys, our estimates may be higher than the others because respondents feel uncomfortable admitting usage while driving to a live questioner over the telephone.

The accident rates in our sample–an average of 5.39% of drivers per year and a weighted average of 6.34% using survey weights–are roughly comparable to those of the general driving public in the United States. The latter figure is most appropriate for comparison to the population. The most comprehensive official data are from the National Highway Traffic Safety Administration (NHTSA), which calculates the collision rate in 2002 for drivers in non-fatal accidents to have been 5.05% per year for the population age 21 years or older.[22] NHTSA data are meant to be comprehensive, and rely on the fact that most states require drivers involved in an accident resulting in property damage in excess of $500, or in any bodily injury, to report to the state department of motor vehicles or to the police (which forward the data to NHTSA). Nevertheless, some accidents reported in our survey may not have been reported to NHTSA. If the true accident rate in the population were more than 1.29 percentage points higher than the official rate–or, to put it another way, if the true accident rate is more than 26% higher than the reported rate–then the accident rate in our survey is lower than that for the population.

[22] Calculated from data from NHTSA (2004), table 63. Our sample contains a few 18-20 year olds (fewer than 0.9% of the sample) and so is not strictly comparable to the NHTSA population.

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

		% of drivers who use a cell phone while driving, out of…		% of drivers who use HF device while driving, out of…		
Study or Poll	**Time Period**	**All Drivers**	**Drivers who Have a Cell Phone**	**All Drivers**	**Drivers who Have a Cell Phone**	**Source**
Authors' survey, raw average	Oct 2001-Sept 2002	73	86	30	41	Authors' survey.
Authors' survey, weighted average	Oct 2001-Sept 2002	64	82	28	44	Authors' survey.
Gallup Poll	Nov 2003-	40	62	23	NA	Gallup Org. (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 -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 & Auto Safety (2001)
Gallup Poll	June-July 2001	43	79	NA	NA	Gallup Org. (2001).
Gallup Poll	June-July 2001	49	89	NA	NA	Gallup Org. (2001).
SurveyUSA	June 2001	33	NA	NA	NA	SurveyUSA (2001).
NHTSA 2000	Nov 2000-Jan 2001	39	73	NA	NA	Boyle and Van-derwolf (2001).

Table notes: NA means "not available." 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 Hahn and Prieger (2004), Appendix B.14.

The accident rates in the survey differ significantly according to whether the driver has a cell phone and whether he or she uses it while driving (see Table 4).[23] In our data, those who use the phone while driving have the highest accident rate (5.9% raw, 7.1% weighted). Those who have a cell phone but claim they do not use it while driving have the lowest accident rate (3.7% in the raw data), and the accident rate of those who do not have a cell phone at all falls in the middle (4.4%). The comparison of these latter two groups provides some evidence against dishonest reporting of phone usage while driving. If respondents who initially 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 closer to those who use a phone while driving than to those who do not have a phone.

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.

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}),

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

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.

an individual-specific effect α_i, and an i.i.d. random effect ε_{it}, the number of accidents, y_{1it}, follows the Poisson distribution with mean

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

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

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

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 individual-specific and idiosyncratic effects, respectively.[24] 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 interest. The composite term $v_i u_{it}$ induces heterogeneity into the mean accident rate even for individuals who are observably similar. We assume α_i is independent of ε_{it}. In this paper, we also treat α_i and ε_{it} as uncorrelated with y_2 and y_3, as in typical random effect models.[25] 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} follows a Poisson distribution and that $v_{it} = 1$ (*i.e.*, that there is no individual-specific

[24] It is common in vehicle accident studies to perform all analysis on the accident rate per vehicle mile traveled (VMT). In terms of equation (1), this would mean replacing time with VMT as our measure of risk exposure. Using VMT as the exposure measure is equivalent to including log VMT as an explanatory variable in equation (1) and restricting the coefficient to one. Given that individuals may not be able to accurately report their VMT, we instead include it (measured for the quarter as reported annual VMT divided by four) as an explanatory variable but leave its coefficient unrestricted.

[25] In Hahn and Prieger (2004) we explicitly test the assumption that cell phone usage and vehicle safety are endogenous. While there is some evidence that they are, the final conclusion of the paper is the same even so: there is no statistically significant effect of a cell phone ban on accidents.

effect α_i or heterogeneity term ε_{it} in the mean accident rate).[26] The Poisson model does not allow the effects of cell phone usage γ to vary individuals—an assumption we explore and reject in the following section. Despite the incorrect assumption of homogenous cell phone effects, the Poisson estimations in this section reveal correlations in the data and provide a useful baseline for a more general model that allows for heterogeneity.

The estimation results for various specifications and samples are presented in Tables 5 and 6. The cell phone usage coefficients represent the incremental risk over not having a cell phone. Thus if cell phone usage is not correlated with accident rates, the IRR's for all the usage categories would be 1.0.[27] The following three points summarize the results from the Poisson estimations. First, more phone usage while driving is associated with higher accident risk for women in our sample. RT also found that cell phone usage by women appears to be riskier than usage by men. The men's effects, which are split out from the women's in Table 6, are statistically insignificant, while the higher usage categories for the women are generally significant.[28] The increase in accident risk for women also rises with the amount of usage. Second, use of hands-free devices is correlated with lower accident risk, at least for women. The IRR for women who always use a hands-free device is generally around 0.5, implying a halving of accident risk. Third, the significance and plausible direction of the effects for many of the covariates give us confidence in the veracity of our survey data.

The estimated effects on accidents of cell phone usage are generally robust to alternative specifications and estimation subsamples. Other than phone usage, there are additional factors that may influence accident risk, and we include covariates such as demographics, weather, and driving variables in specifications P3-P5.[29] The lower average IRR[30] for cell phone users in these estimations indicates that some of the correlation between usage and accidents found in P1 and P2 is due to omitted variables such as miles driven.

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[26] If either α_i or ε_{it} is present (or correlation of any kind among an individual's observations) then Poisson regression yields consistent but inefficient estimates (see section 3.2.3 of Cameron and Trivedi (1998)). We report standard errors robust to the presence of ε_{it} and α_i.

[27] These risk multipliers cannot be compared directly to RT's risk multiple of 4.3 for two reasons. RT examine minor accidents only (*i.e.*, property damage). Also, our risk multipliers are for quarterly accidents given an average level of phone usage; 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.

[28] 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.

[29] 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 only in a separate estimation (P4).

[30] The average risk multiplier reported near the bottom of the tables is calculated conditional on cell phone usage and weighted by the fraction of drivers in each phone and hands-free device usage category.

Table 5: Accidents: Poisson Estimation with Combined-Gender Cell Phone Effects

	Estimation 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.

Some of the covariates also have significant and plausible effects. Married drivers have lower accident risk, a common finding in the accident literature (Whitlock et al. (2004), and references therein). Whitlock et al. (2004) note that if the link between marital status and risk is causal, it might reflect a generally greater willingness by single people to take risks while driving (a tendency documented for some risk factors for vehicle related fatality, including drunk driving and not using a seatbelt).[31] Age has a U-shaped effect, with the minimum accident risk occurring around age 55. A similar age pattern is also evident in official accident statistics (NHTSA, 2004). Longer personal commuting time and full time employment are correlated with increased accident risk. The latter is in accord with the increase in work-related roadway crashes in recent years (NIOSH, 2003). Even controlling for miles driven, full time employment may increase ac-

[31] See Morelock et al. (1985), West et al. (1996), and Hersch (1996).

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

	Estimation P2		Estimation P3		Estimation P4		Estimation 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		

Continued next page

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

	Estimation P2		Estimation P3		Estimation P4		Estimation P5	
	IRR	*P*-value	IRR	*P*-value	IRR	*P*-value	IRR	*P*-value
Continued from previous page								
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.

cident risk by increasing fatigue. Driver fatigue is a leading contributor to roadway crashes among workers as well as the general population (NIOSH, 2003). More daylight hours and driving mainly on rural roads are correlated with decreased 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.[32] When children are present in the household, the accident risk is higher, although not significantly so.[33] The plausibility of these results lends credence to the survey data.

We also estimated other models with a host of alternative samples of the data, other dependent and explanatory variables, and weighted estimations.[34] The main alternative sample for estimation is a gender-balanced sample (P5 in Table 6).[35] 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. We take this as evidence that

[32] 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.
[33] It is likely that there are competing effects stemming from this variable. Children in the household will be present in the car at times, and may distract the driver. On the other hand, drivers who are also parents may be less willing to take risks then childless drivers.
[34] The results of many other alternative estimations, which led to similar results, are included in Hahn and Prieger (2004).
[35] 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.

whatever selection effects are present in the data, using a gender-balanced sample does not solve the problem, and indeed seems to exacerbate it. We also experimented with weighted estimations using the survey weights we constructed.[36] The cell phone coefficients display the same general pattern as in P3, but are smaller in magnitude with larger standard errors. The same is true when we use longer or shorter sample periods to look for evidence of recall bias. Finally, at the suggestion of a referee we explored interacting the cell phone usage variables with miles driven and with commute time. In neither case was specification P3 rejected in favor of the expanded version with the interaction terms.[37]

If the association these estimations uncover between phone usage and accidents is causal, the growing movement to allow usage while driving only if 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; therefore, 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 contains our preferred estimations, in which we explore our hypothesis of heterogeneity. 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.

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

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

[36] 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.

[37] None of the interaction terms were significant at the 5% level in either case, and neither were they jointly significant ($p = 0.20$ for the interactions with miles driven, and $p = 0.63$ for the interactions with commute time). An LR test fails to reject the simpler P3 specification without the interactions ($p = 0.59$ for the interactions with miles driven, and $p = 0.68$ for the interactions with commute time). The coefficients on the cell phone usage variables display the same pattern as in P3: none are significant for the men and the effect increases with usage for the women.

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)):

$$\tilde{\gamma}_i = \bar{\gamma} + \eta_i \tag{5}$$

In (5), $\bar{\gamma}$ 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).[38] The $(\alpha_i,\ \eta_i)$ are assumed to be independent across individuals, uncorrelated with the regressors, and normally distributed with covariance matrix

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

The mean accident rate in (4) 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.[39] 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

[38] 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.

[39] 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.

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.[40] 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).[41] 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.

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

[40] 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.

[41] 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).

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

Variable		Estimation RC1 Men and Women Combined IRR	Estimation RC1 *P*-value	Estimation RC2 Women Only IRR	Estimation 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.

in the population, and by more than the above amounts. This is because RT estimate an instantaneous risk multiple from phone usage, and our IRR's, 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.[42] Equating these leads to the conversion formula

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

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

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

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.

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%.[43] If RT's estimates are not representative of the population, using them 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.

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.[44] 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.

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[43] 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%

[44] 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.

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.

D) Alternative Estimations

In this final estimation section we briefly mention alternative estimations we tried: fixed effects models and models designed to correct for possible endogeneity in the usage of cell phones and hands-free devices while driving. These methods do not incorporate random coefficients. Specific results are presented in Hahn and Prieger (2004); here we discuss the approaches and the general results.

We explored a fixed effects (FE) model, the closest model to the case-crossover method that is estimable with our data.[45] 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 no instruments. 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. Given the evidence from the random coefficient model that the cell phone coefficients vary in the sample, discarding drivers with no accidents causes the FE estimates to suffer from the same upward bias we demonstrated for RT's estimates. Indeed, the IRR's for cell phone use from

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

FE models are much higher than the analogous figures from the Poisson and random coefficient models, which is consistent with selection into the accident sample created by the random coefficient model. There is no significant impact from usage of hands-free devices in these FE estimations.

In a second set of models, we attempted to test and correct for endogeneity of the use of cell phones and hands-free devices. We explored various alternatives (linear and non-linear instrumental variables models and fully parametric multiple-equation models), and in each case the coefficients on the variables of interest lacked precision. The result was the same, regardless of method: the coefficients for cell phone and hands-free device usage were not statistically significant, in most cases in part because the point estimates were smaller than the corresponding estimations that assumed exogeneity. As a result, from each model there is no statistically significant predicted effect of a cell phone ban on accidents. 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 and Taylor, 1999; Crawford *et al.*, 2001; Strayer and Johnston, 2001; and Strayer, Drews, and Johnston, 2003).[46] However, the validity of the estimates depends on the correctness of the parametric assumptions or the validity and strength of the instruments, which can be difficult to assess.

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 evidence 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 models predict no statistically significant reduction in accidents from bans on usage of cell phones while driving. 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.

[46] 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.

Our study has several policy implications. First, policy makers should factor into their decisions our finding of 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, many jurisdictions 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, if 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. Another area of potentially fruitful research is to monitor in real time how driving changes when using a cell phone. This can be done by installing cameras and sensors in vehicles (NHTSA, 2006).

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

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[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.

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

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