Driving Under the (Cellular) Influence*

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Abstract

The link between driver cell phone use and crash risk has become an area of active research. Most existing studies associate cell phone use with substantial crash risk and even compare the danger of such use to illicit levels of alcohol. We investigate the causal link between cellular usage and crash rates by exploiting a natural experiment—the discontinuity in marginal pricing at 9pm on weekdays when recent cell phone plans transition from "peak" to "off-peak" pricing. We first document a jump in call volume of about 20 to 30% at pricing thresholds for two samples of callers. We then document the corresponding change in the fatal and non-fatal crash rate. Using the years prior to the introduction of two-tier pricing as a control, as well as weekends as a second control, we find no evidence for a relative rise in crashes after 9pm on weekdays from 2002-2005. The upper bound of our estimates rules out increases in all crashes larger than 1.0% and increases larger than 1.3% for fatal crashes–lower than the increase implied by most existing studies. We confirm our results with three additional empirical approaches: (i) we compare regional trends in cell phone ownership and crashes, (ii) we estimate the impact of existing legislation banning driver cell phone use, and (iii) we examine differences in urban and rural crash rates. None of these analyses suggest a link between cellular use and vehicle crashes. We discuss possible explanations and present a behavioral model to reconcile this counterintuitive finding with existing research.

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1 Introduction

Does talking on a cell phone while driving increase your risk of a crash? The popular belief is that it does—a recent Gallup poll found that 70% of Americans believe that cell phone use by drivers causes crashes (Gallup 2003). This belief is echoed by recent research. Over the last few years, more than 125 published studies have examined the impact of driver cell phone use on vehicular crashes.[1] The most widely cited of these have found strong links between cellular usage and crash risk. Experimental and epidemiological studies have even compared the relative crash risk of phone use while driving to that produced by illicit levels of alcohol (Redelmeier and Tibshirani 1997; Strayer and Drews and Crouch 2006).

If alcohol, however, is responsible for 40% of fatal and 7% of all crashes each year, as reported by the National Highway Traffic Safety Administration, then Figure 1 illustrates a puzzle. Cell phone ownership (i.e. cellular subscribers / population) has grown sharply since 1990, average use per subscriber has risen from 140 to 740 minutes a month since 1993, and surveys indicate that as many as 40% of drivers have at some point used their phones while driving—yet aggregate crash rates have fallen substantially over this period.[2]

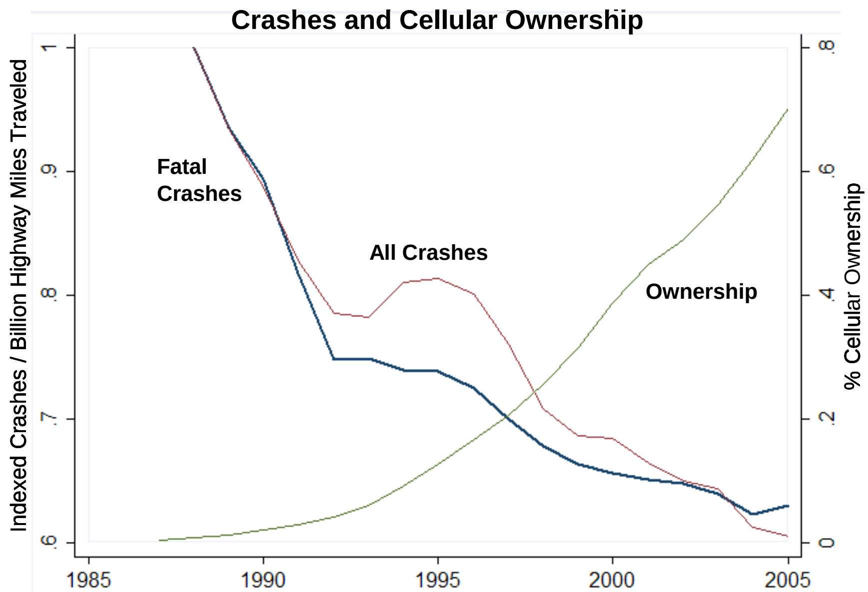

Figure 1, Cellular Ownership and Indexed Crashes/ Highway VMT for 1988 -2005

In the absence of an exogenous rise in cell phone calls by drivers, measuring the dan-

[1] As counted by McCartt et. al. 2006.

[2] The figure plots fatal and all crashes nationwide from 1988 to 2005 per billion highway miles traveled. The modest rise in crashes during the mid 1990s is attributable to relaxation of federal speeding regulations (NHTSA 2005).

ger of driving with a cell phone is impossible since one cannot disentangle innocuous from harmful use. Past researchers have employed a variety of strategies to estimate this relationship. These range from cross-sectional surveys of large groups of drivers, simulations in the lab, inspection of crash reports, observational studies using in-car cameras or confederate observers, longitudinal studies of small samples of drivers, as well as correlations of aggregate cell phone ownership and crash records. In a 1997 issue of the New England Journal of Medicine, Redelmeier and Tibshirani (hereafter "RT") published an influential study which concluded that cell phones increase the relative likelihood of a crash by a factor of 4.3 (Redelmeier and Tibshirani 1997). This implies a 33% increase in annual crashes.[3] While the existing research is valuable, due to difficulties associated with causal inferences for surveys and longitudinal studies, and questions regarding the external validity of lab evidence, the reliability of this research is unclear.

In this paper, we adopt a unique approach to estimate the causal link between cellular use and the crash rate. Specifically, we exploit a natural experiment which arises from a feature characterizing a large share of recent cellular phone plans—a discontinuity in the marginal price of a phone call at 9pm on weekdays. We first provide evidence that a discontinuous rise in prices drives a sharp increase in call volume for two samples of callers. We then test to see whether the rise in call volume leads to a corresponding rise in the fatal and non-fatal crash rate relative to two control periods. Our analysis suggests that current cell phone use *does not* result in a measurable increase in either the fatal or non-fatal crash rate.

Figures 2 and 3 illustrate our primary findings. Figure 2 presents the distribution of cellular call volume across weekday and weekend evenings for a sample of callers in 2005. The plot depicts a 24% relative rise in phone calls at the weekday 9pm threshold when providers systematically transition from "peak" to "off-peak" pricing.[4] Figure 3 plots the parallel distribution of fatal crashes from all states in 2005 for weekday and weekend evenings.[5] Crash rates do not appear to change across the 9pm threshold on weekdays relative to weekends during this period.[6] However, because weekend traffic may not serve as a reliable control for traffic on weekdays, our regression estimates use an earlier period which predates the introduction of two-tiered pricing plans (1990 - 1998) as a second control. Comparison with neither control suggests a measurable rise in relative crash rates at 9pm.

[3] We discuss the assumptions underlying this calculation later in the paper.

[4] We recognize that call volume is a function of both calls made as well as call duration. Later, we demonstrate that the call duration remains unchanged across the threshold implying that call volume rises in proportion to calls made. Additionally, while the 24% rise cited here is derived from 2005 data, we have a second dataset from 2000-2001 which indicates an even larger rise in calls made at the threshold. We discuss both pieces of evidence in greater detail below.

[5] Data for non-fatal crashes is not available for 2005.

[6] The periodicity evident in Figure 3 is due to significant heaping in the timing of accident reports. We discuss this reporting bias, as well as strategies through which to overcome it, in the analysis below.

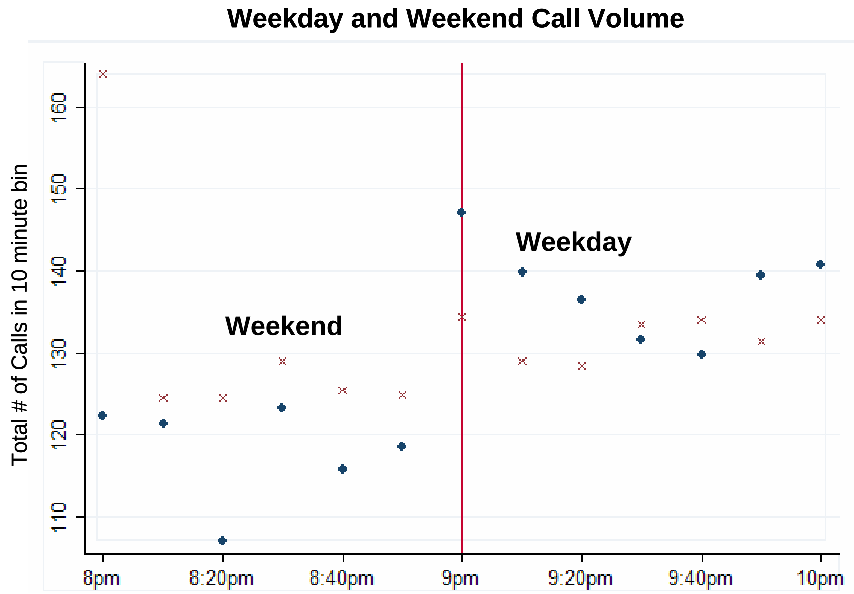

Figure 2, Outgoing Calls from 8pm – 10pm in 2005
(10 mn bins)

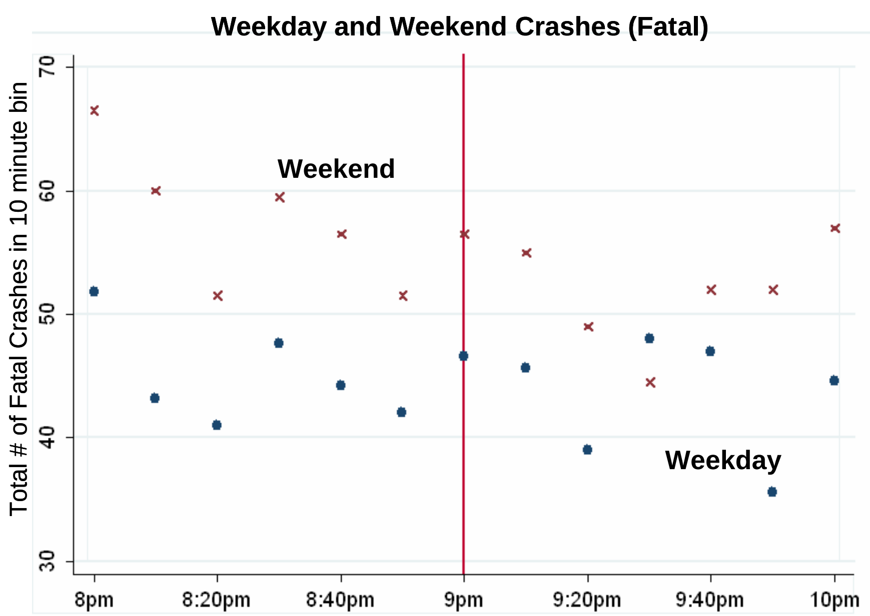

Figure 3, Fatal Crashes from 8pm – 10pm in 2005
(10 mn bins)

While we find no evidence for a relative rise in crashes at the 9pm threshold, we also calculate upper bounds above which we can comfortably rule out any effects. Our estimation allows us to reject, with a 95% confidence interval, any rise in fatal crashes larger than 1.3% and any rise for all crashes larger than 1.0% at 9pm. Given the RT estimate of relative crash risk, the size of the observed discontinuity in call volume, and a conservative assumption for evening driver cell phone use, one would expect to see a 1 to 8% rise in all crashes in the hour following the price threshold.[7]

We offer a number of explanations to reconcile our result with the existing research. One possibility is that drivers compensate for the dangers of cell phone use by driving more carefully. This argument is similar to one articulated by Peltzman in his consideration of the effects of seat belt use (1975). If driver cell phone use is dangerous, and awareness of such danger is as high as surveys indicate, then compensation through safer driving may explain why so many drivers remain on their phones. In the discussion section, we present a simple model of driver behavior under the assumption that cell phones are both beneficial as well as a substantive source of distraction. The model implies that compensatory behavior is a rational response to the dangers, and benefits, of cell phone use. We also cite experimental and field evidence consistent with the existence of compensatory driving in the face of attentional distraction. A second explanation is that the absence of an effect is due to the possibility that risk-loving drivers simply substitute one source of risk (speaking with others, listening to the radio) with another (cell phones).

The need to accurately gauge the detrimental influence of cell phones resonates beyond academic discourse. Every state has considered some form of legislation to restrict the usage of cell phones— or requiring the usage of hands-free devices—while driving for some or all groups of drivers. Twelve states already have such legislation on the books.[8] Given the strength of our research design, we believe that our paper adds to the discourse on the efficacy of policies restricting driver cell phone usage.

While the natural experiment represents our most credible research design, we explore three additional sources of variation in call volume. A first approach compares yearly variation in regional cellular ownership against yearly changes in crash rates. Our unit of analysis is an "economic area" (EA). Defined by the Bureau of Economic Analysis to denote regions of contiguous economic activity, EAs represent the most disaggregated geographic units for which ownership data is available. We exploit the non-linear, and heterogeneous pickup of cellular technology across these EAs in order to estimate any resulting increase in the crash rate. To our knowledge, this is the first paper to present region-year regressions of driver cell phone risk at the level of the EA.

A second approach recognizes that some states have recently enacted bans on hand-

[7] This estimated range assumes a 4 to 10% driver usage rate, and relies on estimates from the two samples of call volume data. The discussion presents a sensitivity analysis of the findings.

[8] Three states have banned hand-held cell phone use by all drivers, while an additional 9 states have enacted partial bans primarily targeting younger drivers. Other states have banned cell phone use by those driving school busses.

held cell phones. We measure the impact of this legislation on crash rates with a panel estimation as well. Finally, due to the absence of data on cell phone ownership at a level of disaggregation higher than an EA, a third approach utilizes the additional variation provided by differences in ownership across rural and urban areas over time. We first document a lag in the rate of growth of cell phone coverage in rural as compared to urban areas. We then compare the change in crashes in largely urban areas over time from those of more rural controls. None of these strategies suggest a positive relationship between cell phone use and crash rates.

Our primary estimation is subject to two major caveats. First, it is possible that the responsiveness of drivers to the change in prices at 9pm differs from the broader population for which we have data. In the discussion, we calculate that for the upper bounds of our estimation to rule out the RT result, under reasonable assumptions of baseline driver cell phone use, the ratio of the change in call volume for drivers to that of the broader population at 9pm must be approximately .1 to .5. We argue that this is a reasonable assumption. Second, the analysis yields only an average local treatment effect of cell phone use across all types of drivers and driving conditions at 9pm on weekdays. Additional research is required to explore whether certain types of drivers or certain driving conditions elicit a heightened crash risk.

Beyond contributing to the literature on the danger of cellular use, our paper is in the spirit of studies which use natural experiments to assess the effect of driver behavior on crash risk (Levitt and Porter 2001a; Levitt and Porter 2001b). This study also relates to the literature that examines the theory of compensating behavior with respect to driving risk factors (Peltzman 1975; Cohen and Einav 2003), as well as the literature speaking to differences between experimental and field evidence (DellaVigna and Dahl 2007; Levitt and List 2007).

The remainder of this paper proceeds as follows. Section II describes the background of research on the link between cell phones and crashes. Section III describes the data while Section IV outlines the main empirical approach and presents the results of the analysis. The next section details the results of three alternative empirical strategies. In Section VI, we report the sensitivity of our findings to changes in various assumptions, and discuss possible explanations through which to reconcile our estimates with the existing research. The final section offers conclusions, and discusses drawbacks of the study as well as possible directions for future research.

2 Background

The sharp rise in cell phone ownership over the last several years has been paced by a rise in research examining the effect of such ownership on vehicular crashes. Ignoring the substantial literature on the cognitive and neural underpinnings of limited attention and multi-tasking, analyses of crash risk due to cellular use generally fall into one of five major methodological categories: (i) Experimental studies that focus on subject behavior in simulated, or highly controlled, driving conditions, (ii) naturalistic studies of drivers on the actual road, (iii) studies which inspect police annotations of crash records, (iv) correlational analyses of aggregate crash records and cell phone ownership, and (v) longitudinal analyses of individual level phone and crash records. Beyond estimating the impact of phone use on crashes, other researchers have measured the frequency of such use by drivers. Several excellent surveys of these literatures exist (Hahn and Tetlock and Burnett 2000; Lissy et. al. 2000; Hahn and Dudley 2002; Hahn and Prieger 2006; McCartt et. al. 2006).

In the standard experimental paradigm, a researcher assesses subject driving performance in a simulator across a variety of metrics (e.g. crash frequency, driving speed, reaction time for braking, following distance, obedience of traffic signals, time to crash etc.) under varying forms of distraction. These studies generally conclude that instructing subjects to use cell phones impairs driving by a factor of 3-4 as compared to their unencumbered counterparts (Strayer and Drews and Johnston 2003). Authors of this research have even compared the effects of cellular use to moderate levels of intoxication (Strayer and Drews and Crouch 2006). Many of these studies have found heterogeneous treatment effects, with, for instance, older drivers being more susceptible to impairment than middle-aged drivers, and mixed evidence for the susceptibility of younger drivers (McCartt et. al. 2006). Importantly, these studies find no differences between hand-held and hands-free devices.

Simulations are able to precisely assess the relative levels of impairment across various distractions, as well as to illuminate the specific capacities that are likely to be impaired. A shortcoming of such studies, however, is their questionable external validity. It is unclear whether cell phone use in simulations is analogous to use in environments where driver well-being, or survival, is at stake. Finally, experimental studies tend to produce estimates of relative, but not absolute, crash risk.

A second set of naturalistic studies employs visual and audio recording devices to monitor driver behavior in authentic road conditions. In an example of one such study, "The 100-Car Naturalistic Study," researchers equipped 100 vehicles with five cameras and sensors and tracked 241 primary and secondary drivers for over 1 year (NHTSA 2006). After amassing nearly 43,000 hours of driving data, the authors found that 78% of the 69 crashes and 65% of the 761 "near-crashes" committed by drivers in their sample was due to some form of driver inattention. They calculated that dialing a cell phone increased the approximate risk of a crash by a factor of 3, while listening or speaking with a cellular device made drivers 1.3 times more likely to have a crash. The majority of near-crashes were also associated with cellular use.

Much like experimental studies, naturalistic approaches pinpoint the specific causes of driver impairment and characterize their relative danger. It is unclear, however, given that drivers may be aware of being monitored, whether such studies improve upon the external validity of studies conducted in the lab. Further, because of the high costs of these studies, the sample sizes are often too small, and unrepresentative, to meaningfully infer crash risk (Lissy et. al. 2000).

A number of studies exploit the existence of police annotations of crash reports to estimate the effect of cell phone use on crashes.[9] Studies examining police reports attribute approximately one percent of crashes to phone use (Lissy et. al. 2000). However, attempts to infer the causal effects of cell phone use from crash reports suffer from source unreliability (NHTSA 1997), and reporting bias due to a recent increase in the salience of cell phone use as a possible crash cause (McCartt 2006). Most importantly, one cannot infer causality from correlations between police reports and crashes since the growth in cell phone ownership amongst drivers should mechanically increase the observed fraction of police reports which cite such use during a crash. For example, a rise in cell phone ownership from 50% to 75% would produce an increase in the proportion of crash reports citing cell phone use due both to an increase in impaired driving, as well as an increase in innocuous phone use. Disentangling these effects is not possible.

A fourth strategy, which generates absolute estimates of crash risk, is the comparison of aggregate trends in cell phone ownership with trends in crash rates. Researchers have examined correlations between crashes and phone ownership at the state, national and local levels (Lissy et. al. 2000). Studies in this class generally find no statistically significant link between cellular use and crashes (Lissy et. al. 2000). Given the strong secular trends in overall crashes (See Figure 1), trend analyses which aspire to identify the possibly modest effect of cellular use are not considered persuasive (Min and Redelmeier 1998). An additional complication with the aggregate approach is that often there is not very much variation in ownership to exploit, and sufficiently disaggregated data on ownership is difficult to obtain.

A final class of studies tracks phone use and driving behavior for a small number of drivers (Violanti and Marshall 1996; Redelmeier and Tibshirani 1997; Violanti 1998; Dreyer

[9] Three states – Oklahoma, Minnesota and Tennessee– explicitly include distraction via use of cell phones as a standardized query on police reports (Lissy 2000). In other states or localities, case-reports or police narratives may offer explanations of crash causes (Goodman 1999; others, see McCartt 2006).

and Loughlin and Rothman 1999; Hahn and Prieger 2006). The most widely cited of these is the analysis by RT (1997). In their influential paper, the authors inspect crash records and detailed phone bills for 699 Toronto drivers recently involved in a minor car crash. To control for heterogeneity in driver quality, the paper relies on a technique commonly employed in epidemiological research— the "case cross-over method"— to study the health effects of transient exposure to a risk factor. For each driver, the authors compare exposure to cell phone use immediately prior to the crash, with exposure during a crash free control period one day before the crash occurred. By examining the relative use of cell phones during the two periods, the authors control for driver specific variation in crash likelihood. Using a conditional logit regression, the paper infers that cell phone use increases the relative likelihood of a crash by a factor of 4.3 (with a 95 percent confidence interval of 3.0 to 6.0). The study fails to find significant differences in increased crash risk across age or gender.

While the paper is considered perhaps the most convincing example of this, or any class, of studies, Hahn and Prieger point out that the study relies on a very unrepresentative sample of drivers recently involved in a crash (2006). If drivers with greater risk of crashing while using cell phones are overrepresented in the RT sample, the relative risk estimate would be an upper bound for the broader population of drivers. An additional concern with the RT study is that while the methodology does control for fixed driver characteristics, it does not control for time varying unobservables. For instance, boredom or stress may cause both cell phone use and poor driving. If so, then observed correlation of cellular usage and crashes could reflect underlying boredom or anxiety. Finally, much like naturalistic or experimental studies, the analysis produces estimates of relative risk which are not easy to translate into aggregate estimates of crash impact.

A more recent study replicated the RT analysis for drivers in Perth, Western Australia (McEvoy et. al. 2005). The authors found that hand-held devices increased crash risk by a factor of 4.9 (with a 95 percent confidence interval of 1.6 to 15.5). Consistent with experimental findings, the researchers also found no significant difference between handheld and hands-free devices.[10]

Table 1 summarizes estimates of relative and absolute risk emerging from each of the described methodological classes. Meaningfully translating across relative and absolute risk, however, critically relies on the accuracy of assumptions regarding the frequency of driver cell phone use.

A number of studies have attempted to estimate the frequency of such use. These include surveys of driver usage, as well as observational studies with experimenters or cam-

[10] Analagous studies have not been conducted in the United States due to the absence of billing records from domestic cell phone providers.

Table 1

EFFECT OF CELLULAR USE ON CRASH RISK: COMPARISON BY METHODOLOGY

	RELATIVE RISK	EXTRAPOLATED ABSOLUTE RISK
Present Analysis (9pm Discontinuity)	X	4.7% increase in all crashes (upper bound)
Experimental Studies	3 to 4 times impairment (Strayer 2003; Strayer 2006)	20 to 30% increase in all crashes
Naturalistic Studies	1.3 times collision risk (NHTSA 2006)	3% increase in all crashes
Police Annotations	X	1% increase in all crashes (Lissy et. al. 2000)
Aggregate Crash Trends	X	0% increase in all crashes (Min and Redelmeier, 1998)
Individual Crash Records	4.3 times collision risk (Redelmeier and Tibshirani 1997)	33% increase in all crashes

eras stationed at intersections. An example of the latter, the 2005 National Occupant Protection Use Survey (NOPUS) observed some 43,000 stopped vehicles at 1,200 probabilistically sampled intersections and stop signs in July of 2005 and found that 6% of drivers used handheld cell phones at any point during the day (i.e. 8am to 6pm). The authors estimate, using existing survey data, that an additional 4% of drivers were on hands-free phones (NHTSA 2006).[11]

Earlier NOPUS reports indicate that the rate of handheld use has been increasing over the last several years from 5% in 2004, 4% in 2002, and 3% in 2000 (Glassbrenner 2005). NOPUS also hints at considerable heterogeneity in cellular use across driver age and location—but not gender—with handheld cell phone use of drivers from 16-24 years approaching as high as ten percent (Glassbrenner 2005).

The only study of which we are aware that explicitly considers differential usage across the day and night involves an assessment of 40,000 vehicle photographs taken on the high-speed NJ Turnpike in 2001 (Johnson et. al. 2004). The authors find no significant difference between driver cellular usage during the late evening (i.e. from 8pm to 12am) and the afternoon (i.e. from 12pm to 4pm).

Given these estimates of driver cell phone use, Table 1 provides extrapolations of the

[11] NOPUS also reports the incidence of observed "head-set" use which, in 2005, was .7%. The NOPUS estimate of total hands-free usage combines observed head-set usage with driver survey results (Stutts et al., 2003; Boyle and Vanderwolf, 2005).

absolute crash risk implied by studies of relative crash risk. These extrapolations assume (1) the 10% NOPUS rate of (handheld and hands-free) cell phone usage and (2) randomization in usage across driver type. Assuming for example, that cell phone use occurs during 10% of total driving time, then, ignoring selection, a 4.3 fold increase in the relative likelihood of a crash translates to an expected 33% increase in total crashes. Accordingly, estimates of the effect of cell phone use on the change in total crashes range from 0 to 33%.[12]

The first row of the table reports the upper bound for the aggregate absolute crash risk from the present analysis. The 4.7% figure assumes that a 1% rise in cellular call volume is equivalent to a 1% rise in cellular ownership, and is calculated by multiplying the 1.0% upper bound of the estimated crash increase at 9pm by the ratio of current ownership (75%) to a conservative estimate of the rise in call volume at 9pm (16%).

3 Summary of Data

This analysis relies on a wide array of data. These sources are summarized in Table A1 of the Appendix. Each empirical approach depends on data on crash records, as well as data on changes in cell phone ownership. The Fatality Analysis Reporting System (FARS) provides data for the universe of fatal crash records from 1987 to 2005 for each of the 50 states. FARS captures any vehicle crash resulting in a death within 30 days of the collision. The State Data System (SDS) provides data for the universe of all crashes (fatal and non-fatal) for selected years from 1990 to 2004 for California, Florida, Illinois, Maryland, Missouri, New Mexico, and Pennsylvania. Of these states, Maryland and New Mexico have the most extended data coverage (i.e. from 1990 to 2004). The SDS and FARS databases are administered by the National Highway Transportation Safety Administration (NHTSA) which collects records from participating state agencies. A total of eighteen states participate in the SDS, but only seven states release data which is both recent and covers the universe of fatal and non-fatal crashes.

Figure 1 depicts the rate of nationwide fatal and all crashes, indexed to highway traffic volume, for each year from 1988 to 2005. Data on all crashes in this plot is based on a national sample, the General Estimates Survey (GES), conducted by the NHTSA. The plot indicates a decrease in crashes over the last fifteen years, with a slight rise in the mid-1990s. Much of the drop in crash rates over this period is attributable to the increasing prevalence and usage of safety devices as well as a decline in driver alcohol use (NHTSA 2005). The mild rise in the mid-1990s can be at least partially attributed to relaxation in nationwide

[12] These calculations do not take into account possible heterogeneity of cell phone use across drivers. If only risk loving drivers use cell phones, for example, and the use of cell phones is merely a substitute of one form of distraction for another, then our extrapolations may represent an upper bound of the predicted effect range.

speeding regulations (NHTSA 2005). Recently, there have been about 40,000 fatal crashes, and approximately 6 million total crashes reported each year nationwide.

Much of the analysis for the alternative empirical approaches is at the level of the Economic Area (EA). EAs were originally defined by the Bureau of Economic Analysis (BEA), and are currently used by the Federal Communications Commission (FCC) to denote regions of contiguous economic activity. Each of the 172 EAs consists of one or more economic nodes—a metropolitan or micropolitan statistical area that serves as a regional economic center. Examples of EAs include "Minneapolis-St.Paul", "Washington-Baltimore", as well as the largest "New York-Northern New Jersey-Long Island." The BEA uniquely mapped counties to an Economic Area in 2000. We use these mappings to construct EA level crash and population data. Table A2 in the Appendix provides EA level summary statistics on cell phone ownership, population, and crash rates. To our knowledge, our study is the first to estimate the correlation between nationwide crashes and ownership at a unit of analysis as disaggregated as the EA.

Measures of cell phone ownership require data on the number of subscribers as well as the population in a region. Data on cell phone subscribers for each EA from 2001 to 2005 was collected from the FCC (2006). Historical population data was downloaded from the Bureau of Labor Statistics website. Figure A1 in the Appendix depicts trends in cell phone ownership nationwide as well as the growth in the average usage of each phone per user (FCC 2006). Overall, both ownership and usage increase exponentially over this period. By 2005, 2 of every 3 residents in a typical state own a cell phone despite only 1 of 3 owning a cell phone just six years earlier.

The central empirical strategy in this paper is based on the claim that discontinuities in cell phone pricing prompt sharp increases in cell phone call volume. To illustrate this first stage relationship between call volume and call pricing, this analysis relies on two large sets of calls. First, complete logs of cell phone activity for approximately 65 students and faculty over the course of 2005 was obtained from the Reality Mining Project (RMP) at the MIT Media Lab (Eagle and Pentland 2006). As part of a broader study examining the evolution of social networks and the transmission of information, RMP researchers embedded surveillance technology in the cellular phones of each subject in their sample. Approximately 80,000 outgoing calls were logged over the course of the surveillance period. Electronic logs ensure that the timing of calls are accurately documented to the second.

However, because data comprised entirely of MIT students and faculty is unrepresentative, we appeal to a second dataset of phone calls of over 560,000 calls made by 9,907 cell phone users from U.S. households in 2000 and 2001 (TNS 2001).[13] The second dataset was

[13] While TNS Telecom continued to harvest cellular phone bills after 2001, we were unable to acquire this data for a more recent period.

harvested from cellular phone bills voluntarily submitted from households that had been randomly selected to participate in an earlier survey of telecommunications behavior and attitudes administered by a private research firm. While this data is likely to be representative, it is hourly data, and is from a period characterized by non-uniform cell phone plans with switching thresholds ranging from 6 to 10pm, or no threshold at all. The data usefully provides peak and off-peak designations for each call, and allows for the analysis of individual call patterns. Plan switching times can only be inferred for a small portion of the data, however.

Data on historical cellular pricing plans was obtained through screen-shots of cell phone provider websites taken monthly from 2002 to 2005 by Econ One Research. The Econ One Wireless Survey details the availability of pricing plans by provider, the schedule of marginal prices per call, as well as the time threshold at which tiered pricing plans switch from peak to off-peak pricing.[14] While the survey targets New York City, we assume that the pricing details of national calling plans available to New York subscribers are similar to those available to other users nationwide. Market shares for each provider were collected from S&P Industry Analysis Reports (S&P 2002-2006).

The first alternative empirical approach is a comparison of aggregate cellular ownership and crash rates. This analysis includes a robustness check which controls for state-level traffic data. Data on annual highway traffic volume for all states from 1989 to 2005 was obtained from the Federal Highway Traffic Administration. The agency collected traffic data from counting stations positioned on roadways across the country. Total traffic volume on U.S. highways grew from 162 billion miles in January of 1990 to 222 billion miles in January 2005.

A second alternative approach in this paper involves the analysis of legislation banning driver use of cell phones. Descriptions of state legislation is gathered from the American Automative Association as well as the National Conference of State Legislatures (NCSL 2005; AAA 2007). Finally, a third alternative approach in this paper exploits the differential cellular coverage in rural as compared to urban regions. Classifications of the urban-rural character of counties is collected from the U.S. Department of Agriculture.

4 Empirical Analysis of 9pm Price Discontinuity

This section outlines the estimation strategy and identifying assumptions, and presents the empirical findings for the primary analysis. First, we provide evidence for the sensitivity of call volume to systematic and transparent discontinuities in the marginal prices of cellular

[14] The survey is available at *www.econone.com*.

calls. We then estimate the effect that increased usage has on the frequency of fatal and non-fatal crashes. Specifically, we document that, since 2002, most cellular users subscribe to plans which feature near zero marginal costs for a phone call after 9pm on weekdays. We then provide evidence for a jump in weekday, but not weekend, call volume immediately after 9pm. Finally, we check for a rise in crashes corresponding to this documented rise in weekday call volume. We compare the difference in the crash rate before and after 9pm on weekdays since 2002, to the same difference on weekends as well as the years prior to the introduction of pre-paid plans in 1998.

4.1 Estimation Strategy and Identifying Assumptions

Let $Crash_{r,p,wk,h,w}$ refer to the number of reported crashes in region r, in either "post" or "pre" period p, in hour h, minute window w either on weekdays or weekends as signalled by wk. "Post" refers to the period characterized by high cell phone ownership and high plan conformity around a specific threshold, while "pre" refers to the period prior to the era of two-tiered monthly pricing plans. In this framework, reported crashes are jointly determined by the traffic level denoted by $Traffic_{r,p,wk,h,w}$, bias in the reporting of crashes denoted by $RepBias_{r,p,wk,h,w}$, and the covariate of interest, cell phone use, which is denoted by $Cell_{r,p,wk,h,w}$. We also include a vector of additional covariates, $X_{r,p,wk,h}$, which we believe may influence the rate of vehicular crashes. These factors include speeding regulations, weather conditions, and the availability and adoption of safety technology:

$$\text{(1)} \quad Crash_{r,p,wk,h,w} = \alpha + \theta_1 Traffic_{r,p,wk,h,w} + \theta_2 RepBias_{r,p,wk,h,w} + \theta_3 X_{r,p,wk,h} + \lambda Cell_{r,p,wk,h,w} + \varepsilon_{r,p,wk,h,w}$$

Unbiased estimation of λ, the causal effect of cell phone use on vehicular crashes, is problematic since cell phone use is not randomized across drivers. Specifically, it is possible that drivers who use cell phones have a greater affinity for risk, and that the risk affinity (R) of drivers on the road produces a higher likelihood of entering into a crash: $E(\varepsilon \mid R) \neq 0$. Since $Cell_{r,p,wk,h,w}$ may also be a function of the risk affinity of drivers, attempts to estimate λ through OLS will be biased. One strategy through which to circumvent this bias is to assume that the distribution of unobserved driver risk is the same immediately before and after the 9pm pricing threshold:

$$\text{(2)} \qquad \lim_{\Delta \to 0^+} E(\varepsilon | R_{9pm+\Delta}) = \lim_{\Delta \to 0^+} E(\varepsilon | R_{9pm-\Delta})$$

If we define a control function $g(R) = E(\varepsilon_{r,p,wk,h,w} \mid R)$ which is continuous through the 9pm threshold, we can rewrite equation (2) as:

$$\text{(3)} \quad Crash_{r,p,wk,h,w} = \alpha + \theta_1 Traffic_{r,p,wk,h,w} + \theta_2 RepBias_{r,p,wk,h,w} \\ + \theta_3 X_{r,p,wk,h} + \lambda Cell_{r,p,wk,h,w} + g(R) + v_{r,p,wk,h,w}$$

where the error term $v = \varepsilon - E(\varepsilon|R)$ is now independent of $Cell_{r,p,wk,h,w}$. Given our assumption of a continuous risk function at the pricing threshold, any break that we see at that point in crashes should be attributable to the change in the remaining covariates—traffic patterns, reporting bias, the controls included in X as well as cell phone use. We formalize this RD at the threshold then, by calculating a first difference, $D_{r,1,1,h}$, which represents the change in crashes during some time window immediately before the threshold from some window immediately after the threshold. Intially, we restrict focus to weekdays during the post period. Assuming that speeding regulations, weather, and safety technology and compliance are unchanged locally around the threshold, $X_{r,1,1,h}$ drops out of the first difference:

$$\text{(4)} \quad D_{r,1,1,h} = Crash_{r,1,1,h,w} - Crash_{r,1,1,h,w'} = \theta_1' \Delta Traffic_{r,1,1,h} \\ + \theta_3' \Delta RepBias_{r,1,1,h} + \lambda' \Delta Cell_{r,1,1,h} + v_{r,1,1,h}'$$

Intuitively, our RD model assumes that traffic patterns and reporting bias may vary across the threshold. The flexibility that this assumption adds to the estimation will be explored more fully below. However, in the face of covariates which vary across the threshold, we can calculate a second difference, $DD_{r,1,h}$, by comparing the difference (D) in crashes around the time threshold during the post period from a pre period prior to the threshold era:

$$\text{(5)} \qquad DD_{r,1,h} = D_{r,1,1,h} - D_{r,0,1,h} = \lambda''(\Delta Cell_{r,1,1,h} - \Delta Cell_{r,0,1,h}) + v_{r,1,h}''$$

If we assume that the difference in traffic as well as the difference in the reporting bias around the threshold on weekdays in the pre compared to post periods does not systematically differ, then the double difference in crash rates is simply a function of the residual post-pre threshold difference in cell phone use.

Finally, to allay the concern that the differences in reporting bias across the threshold may systematically vary across the pre and post periods, as a placebo test we can analogously calculate a second double difference, for weekend periods. We discuss details of the pricing discontinuity and document the subsequent change in cell phone call volume below.

4.2 Change in Call Volume at Price Discontinuity

Pricing Plans. In recent years, contracts for cell phones have been characterized by a flat monthly fee which entitles subscribers to a specified number of minutes depending on the time of use. Any use in excess of this allotment is subject to relatively high marginal fees. For instance, a "900 Nation" plan offered by Cingular in 2006 allows 900 minutes of peak usage from 6am to 9pm each weekday, unlimited use for off-peak periods after 9pm and before 6am on weekdays, and unlimited use all day on weekends.[15] Marginal fees for excess usage commonly range from \$.35 to \$.45 per minute.

Table 2 documents the evening thresholds for which major providers distinguished between peak and off-peak usage for national calling plans offered to New York subscribers from 2002 to 2005.[16] In the absence of plan level market shares and turnover rates, calculating the share of users tied to a particular threshold is not possible. The table does, however, report the estimated share of new users associated with each threshold in a given year.

We estimate threshold specific shares by calculating the unweighted proportion of provider plans associated with each threshold, and then weighting these figures by the estimated local market share of each provider reported in the table's final column. While we expect plans within a provider to vary in popularity, for the most part, our naive, unweighted estimation only confounds those few instances for which a provider has plans that do not share a common threshold. Local market shares are extrapolated from national figures published in the S&P Industry Guide since the local shares for New York providers are not available (S&P 2002-2006).

There is reason to feel confident that national plans in New York City may be representative of broader offerings in other markets. Although not all providers service all regions, national calling plans offered by major providers are typically identical for subscribers regardless of local origin. Therefore, New York City plans are likely to be approximately representative of plans nationwide.

[15] Actual plans often specify some large, but finite, limit for non-peak usage. Cingular, for example, had usage limits even for non-peak periods that were marketed as allowing for "unlimited" usage. These limits are typically 5,000 or 10,000 minutes.

[16] The table displays only those plans which were listed on the websites of each provider based on monthly snapshots taken by Econ One Research. National calling plans, which tend not to distinguish between local and non-local calls, are most likely to feature the described pricing structure.

Table 2

PRICING PLAN THRESHOLDS FOR NYC CALLING PLANS, 2002 - 2005

	SWITCHING THRESHOLD			
	7PM	8PM	9PM	MARKET SHARE
2002				
Sprint	0	0	22	0.17
AT&T	0	26	10	0.23
Verizon	0	0	42	0.35
Cingular	0	0	14	0.25
Total Share	0.00	0.17	0.83	
2003				
Sprint	0	0	42	0.15
AT&T	0	0	18	0.20
Verizon	0	0	43	0.33
Cingular	30	0	30	0.21
T-mobile	0	0	8	0.11
Total Share	0.11	0.00	0.89	
2004				
Sprint	82	0	68	0.16
AT&T	6	0	10	0.16
Verizon	0	0	58	0.29
Cingular	6	0	9	0.18
T-mobile	0	0	9	0.11
Nextel	0	0	3	0.10
Total Share	0.22	0.00	0.78	
2005				
Sprint	46	0	64	0.16
Verizon	0	0	28	0.29
Cingular	0	0	12	0.32
T-mobile	0	0	12	0.12
Nextel	0	0	7	0.11
Total Share	0.07	0.00	0.93	

Notes: The table displays the number of pricing plans associated with each switching threshold by provider. The data is from monthly snapshots of provider websites targeting subscribers in New York City and originally reported in the Wireless Survey administered by Econ One Research. The estimated total market shares are generated by multiplying the unweighted fraction of plans associated with each time threshold by the estimated market share reported in the last column.

Table 2 depicts strong consistency in available pricing plan options across providers for the years from 2002 to 2005. By 2002, most providers had abandoned the 8pm threshold, which had been popular in earlier years, in favor of a 9pm threshold. As a promotional incentive, providers in 2003 began offering plans with earlier switching times of 7pm. However, we estimate that at least 75% of new subscribers in each year since 2002 enrolled in

9pm plans. Assuming a 1 to 2 year typical contract duration, and in light of the dramatic rise in cellular ownership since 2001, Table 2 suggests that, in recent years, most active cellular users faced a 9pm threshold.

Cellular Call Volume. Does the existence of a sharp change in marginal pricing lead to a corresponding change in the actual volume of calls? There is suggestive evidence that cell phone subscribers are price sensitive. In a Pew Research Center survey of 1503 people in 2006, 44% of cell phone users reported delaying their calls until they did not count against their allotment of peak minutes.[17] In another survey of 30,000 cell phone users, those who exceeded their allotment were subject to "overage" fees which, on average, amounted to 50 to 60% of their usual bill.[18] These surveys suggest that the weekday evening price threshold is likely to be salient for many users.

We explicitly test for the correspondence between the change in call price and usage at the plan threshold by using two rare datasets of actual calls.[19] A first data set was acquired from a research group at the MIT Media Lab which embedded surveillance technology in cellular phones in order to track subject movements, interactions, and cellular communication over the course of 1-2 years (Eagle and Pentland 2006). We examine the full distribution of outgoing cell phone calls for 65 subjects—comprised of both students and faculty—over the course of 2005.[20] This amounts to more than 80,000 call records.

Figure 2 depicts the distribution of calls made by subjects in the sample over 10 minute intervals from 8pm to 10pm across weekdays and weekends in 2005, while Figure A2 in the appendix depicts call volumes across hourly intervals over a longer portion of the day. A vertical line in each plot marks the 9pm threshold at which time the marginal price of calls on weekdays—but not weekends—drops sharply. The latter figure illustrates a steady rise in call volume through the weekday afternoon and early evening, a modest drop at around six o'clock, followed by a rise through the late evening. Call volumes are considerably less variable on the weekends. This pattern of high evening and low afternoon weekday calling seems consistent with a typical subject's likely schedule (e.g. the start and end of classes etc.).

Collectively the figures indicate a sharp increase in the number of calls made immediately after 9pm on weekdays but not weekends. The increase in calls is on the order of 15-25% and seems to persist at least until midnight. Table 3 explicitly enumerates the change in call

[17] Survey published in an Internet Project Data Memo entitled "Cell Phone Use" from April 2006.

[18] This is according to an analysis of 30,000 cell phone users conducted by Telephia as part of their *Customer Value Metrics Service* in 2006.

[19] Data on call volume is difficult to acquire. Providers generally view such data as propriety, and the few third party firms which maintain private databases of billing statements either do not release individual call records, or make it available only at prohibitively high prices.

[20] Not all of the subjects remain in the sample for the course of the calendar year. Many of the subjects exited the sample or temporarily left the area over the summer. Consequently much of the call volume is concentrated in non–summer months.

volumes in windows of varying lengths around each hour from 7pm to 10pm on weekdays relative to the same change on weekends. Standard errors are reported parenthetically for the 9pm threshold. The table confirms the pattern evident in the figures—call volume increases by 16% from 9pm to 10pm on weekdays, and is unchanged over this period on weekends. Proximal weekday hours do not experience similarly pronounced changes in call volume.

Table 3

CHANGES IN HOURLY CALL VOLUME (MIT), 7PM - 10PM, 2005

	7PM (1)	8PM (2)	9PM (3)	10PM (4)
WEEKDAY				
10 minute bins	10%	-3%	24% (9%)***	1%
20 minute bins	4%	7%	22% (9%)***	2%
30 minute bins	2%	3%	18% (9%)**	2%
60 minute bins	8%	12%	16% (12%)	-1%
WEEKEND				
10 minute bins	0%	18%	8% (8%)	2%
20 minute bins	6%	17%	5% (5%)	5%
30 minute bins	4%	8%	3% (4%)	1%
60 minute bins	5%	5%	0% (2%)	1%

Notes: Each cell reports the change in call volume across the time threshold for the respective time window. Standard errors are reported in parentheses. For the 10, 20 and 30 minute bins, the standard errors are computed from observations in the 8 pm to 10 pm time band. For the 60 minute bins, the standard errors are computed using the 7 pm to 11 pm band.

* significant at 10%; ** significant at 5%; *** significant at 1%

It is important to note, however, that this sample of callers is unlikely to include many drivers. Most students and faculty at MIT live near campus, and the campus itself is situated in close proximity to public transportation. Moreover, the subject pool may not be representative of the larger population across a variety of additional dimensions. To address these concerns, we appeal to a second dataset of over 560,000 calls made by

9,907 cell phone users from households across the country in each quarter of 2000 and 2001 (TNS 2001). The data was extracted from cellular phone bills voluntarily submitted from households randomly selected as part of a broader survey of telecommunications behavior and attitudes.[21] The data includes calling plans with switching thresholds that range from 6pm to 10pm, as well as plans with no (inferable) switching threshold. When possible, we infer the time of a plan's switching threshold by exploiting the peak and off-peak labels assigned to individual calls.

Unfortunately, a large majority of the 9907 callers in the data have plans whose threshold either does not exist or cannot be inferred. We therefore retain a subsample of callers that satisfy each of the following conditions: (i) Callers are in the sample for at least 30 consecutive days (ii) Callers log a minimum of at least 30 calls (incoming and outgoing) (iii) Callers have no calls that are ambiguously tagged (i.e. each call is tagged as either "peak" or "off-peak" rather than "unclear") (iv) Callers have a mix of peak and off-peak calls which allows us to infer the switching hour of the caller's plan.[22] Of the remaining 407 callers in this subsample, most have plans with switching thresholds at either 7pm (139), 8pm (166) or 9pm (102). These individuals make a total of 18,107 calls.

Importantly, we cannot be sure whether this sample of callers reflects the population of drivers who use cell phones. It is possible that driver sensitivity to a price threshold is less than that of non-drivers. However, the rise in cell phone usage amongst drivers at the threshold would have to be modest for the substantive results of the analysis to change. We discuss this issue at length, and provide a sensitivity analysis, later in the paper.

Figure 5 illustrates the sensitivity of callers in the 7pm, 8pm and 9pm plans to their particular plan thresholds on weekdays. The figure depicts a relative rise of about 15% for callers on 7pm plans at 7pm relative to other callers, 25% for callers on 8pm plans at 8pm, and about 30% for callers on 9pm plans at 9pm. The rise in call volume for each plan at that plan's respective threshold hour is in contrast to the general decline in calls associated with all plans at non-threshold hours.[23]

[21] The "ReQuest Consumer Survey" is a quarterly survey of about 30,000 households on consumer behavior and attitudes related to telecommunications. Households were offered a small payment in exchange for copies of one month's worth of cellular, cable, TV and internet bills. In the fourth quarter of 2001, households were offered \$5 and participation in a "special cash prize raffle" for their bills.

[22] We impute the switching hour by computing the change in the average peak/off-peak rating for each evening hour. Peak calls are tagged with the value "1" while off-peak calls are tagged with the value "2". In principle, if a caller has a 7 pm switching threshold, then the average peak/off-peak rating should jump cleanly from 1 to 2 at 7 pm on weekdays. However, due to the presence of holidays or calls made in excess of the allowed quota for that month, we do not always observe unit jumps in the rating. In the absence of clean rating jumps, we tag the evening hour with the largest jump in average peak/off-peak rating as the switching hour for each caller.

[23] The rationale for the length and call duration requirement is to ensure sufficient power for a fixed effects estimation, as well as to minimize any potential miscategorization of switching time thresholds. The basic results and figures are robust to less strict selection criteria.

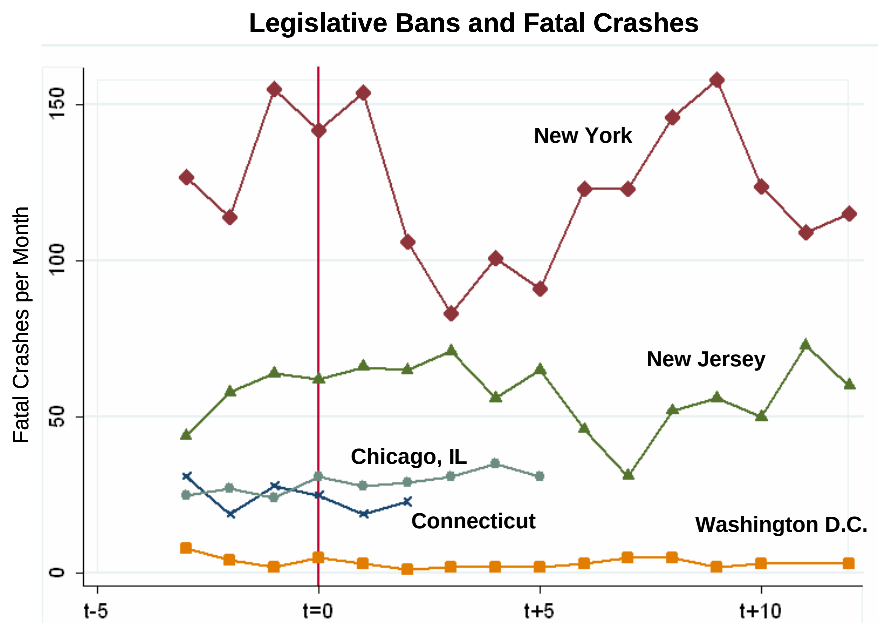

Figure 7, Monthly Fatal Crashes Before and After Legislative Bans

A panel regression at the level of the individual caller, sizes and confirms this sensitivity of callers to their respective plan thresholds:

$$Calls_{h,s,i} = \alpha + \gamma Switch_s + \theta AfterSwitch_{h,s,i} + \eta_h + \eta_i + \varepsilon_{h,s,i} \tag{6}$$

where $Calls_{h,s,i}$ refers to the total calls of caller i, in hour h, under a calling plan which transitions to off-peak pricing at hour s. $Switch_s$ refers to the transition hour, while $AfterSwitch_{h,s,i}$ denotes hours after (but not inclusive of) the switching threshold. Fixed effects are included to control for hour specific variation, as well as for each individual caller. The Poisson regression is estimated for all weekday outgoing and incoming calls made from 5pm to 12am for those callers included in the sample.

The coefficient estimates of Table 4 indicate that the difference in the logs of expected hourly calls before and after the switching threshold is .286. This translates to an estimated increase in call volume of 33% at the switching threshold. Incoming calls sustain a smaller, less precisely estimated increase in call volume. To address the concern that the rise in calls at the switching threshold may be counterbalanced by a fall in duration, the final column of the table shows no evidence for a significant fall in duration at the threshold.

Table 4

CHANGE IN CALL VOLUME AT PLAN THRESHOLD, 2000-2001

DEPENDENT VARIABLE: HOURLY WEEKDAY CALLS & DURATION			
	POISSON REGRESSION		OLS
	Outgoing Calls (1)	Incoming (2)	Duration (3)
Switching Threshold	0.286***	0.092	-0.095
	(0.076)	(0.093)	(0.429)
After Switching Threshold	0.073	-0.044	-0.102
	(0.125)	(0.165)	(0.551)
Individual Fixed Effects	X	X	X
Hour Fixed Effects	X	X	X
N	N = 3256	N = 2736	N = 2270
R^2			0.01

Notes: The dependent variable for the first two columns is the number of hourly phone calls made on weekdays from 2000 - 2001 for callers included in the TNS sample from 5pm to 12am. The first column presents results of a poisson regression for all outgoing calls, while the second column estimates the model for incoming calls. The switching threshold is a dummy variable which indicates the hour when a caller transitions from peak to off-peak pricing. The after switching threshold is a dummy variable which denotes those hours following (but not inclusive of) the switching hour. The final column presents OLS regression results for the link between call duration and switching time thresholds. The dependent variable here is measured in minutes. Standard errors are robust and clustered by the individual caller.

* significant at 10%; ** significant at 5%; *** significant at 1%

4.3 Change in Crash Rate at Price Discontinuity

Do crash rates respond to the increased cellular usage induced by a change in prices? We answer this question for both fatal and all crashes by comparing driver behavior at the 9pm threshold on recent weekdays with such behavior on weekends as well as an additional control period preceding the one of interest.

Reporting Bias. A well recognized drawback of using a crash database based on self-reports is the presence of substantive periodic heaping. The trajectory of a fatal crash record helps to illuminate the origin of this bias in FARS. Once a fatality linked to a vehicular crash occurs, it is documented by one, or several, state governmental bodies, and is then translated onto standardized paperwork and inputted into the FARS database by a trained analyst at a federally sponsored state agency. Consequently, crash statistics can emerge from one of several possible records such as police crash reports, death certificates, or hospital notation. Any bias which is likely to occur then, may vary in severity across states as well as over time. Figure 6 illustrates the extent and nature of the heaping that occurs over the course of a representative hour in 2005. A close examination of the crash records indicate that over 8% of crashes are reported to have occurred exactly on the hour. Nearly 27% of crashes are reported to have occurred either on the hour, half hour, or quarter hour, and 61% of crashes are reported to have occurred in a minute ending in either 0 or 5.

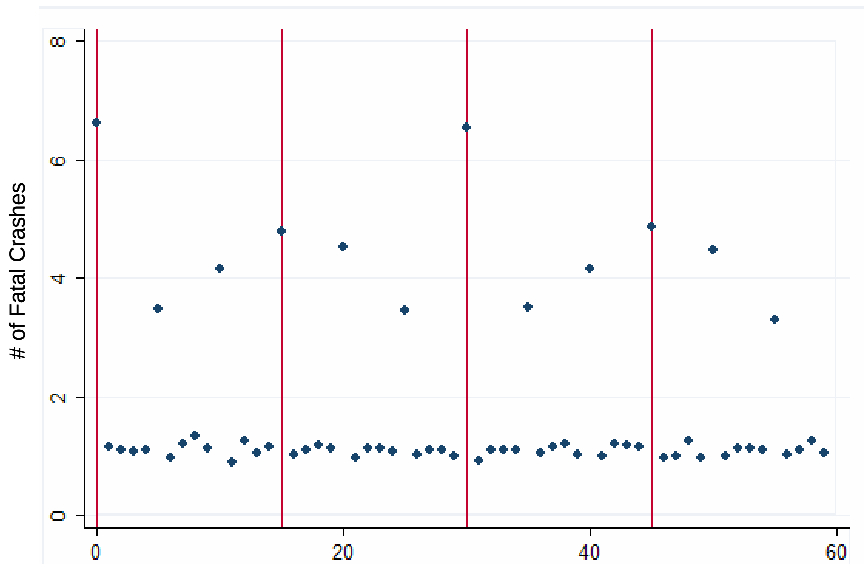

Figure 5, Average FARS Crashes by Minute in 2005

The periodic heaping in crash reports complicates a standard RD design. In principle, one should be able to describe the change in crashes induced by a fall in prices at the threshold by fitting lines, or higher order polynomials, on either side of 9pm on weekdays in recent years. The challenge, however, is to disentangle the on-hour spike in reported crashes from authentic changes in actual crashes at the threshold. One might be tempted, in a regression framework, to control flexibly for the reporting bias by including dummy indicators for minutes ending in 0 or 5. However, such a strategy yields imprecise estimates of the change in crashes at 9pm. The reason for this imprecision is that the on-hour spike in reported crashes is an order of magnitude larger than any plausible change in the crash count induced by higher cellular use. Intuitively, although the spike is estimated with relative precision, the residual uncertainty in the size of the spike induces large standard errors in the estimate of the change in the crash count. One strategy through which to deal with such complication is to smooth the count data by choosing a unit of analysis which aggregates crashes into larger minute bins (e.g. intervals of 15 or 30 minutes). However, the simple RD, with smoothed count data, still produces imprecise estimates of the change in crashes at the threshold. As a result, we rely on a double difference approach in addition to smoothing in order to adjust for the observed heaping. Assuming no systematic change of biases across time, as the model above outlines, the double difference across the pre and post period should mitigate the impact of any reporting bias. We first present a graphical illustration of this approach followed by a formal regression analysis.

Fatal Crash Analysis. We turn first to the distribution of fatal crashes around the pricing plan threshold. We test for a link between cellular use and crashes by comparing the difference in crash rates before and after 9pm on weekdays during the post period from 2002 to 2005 to a pre period before 1998 when monthly fixed price plans were first introduced. As a robustness check, we examine this same double difference for weekends when the pricing thresholds are not in effect.

Figure 6 illustrates the raw differences in crash rates across each of the control periods. The histograms depict the average number of yearly crashes nationwide for increasingly larger windows, ranging from 2 to 15 minutes, on both sides of 9pm for both weekdays and weekends in the pre and post periods. We exclude crashes reported as having occurred exactly at 9pm itself to avoid on-hour bias in reporting. Treating each year as an independent draw allows for the calculation of standard errors which are displayed in parentheses. Figure 6 indicates that the quadrant in which the pricing discontinuity actually occurs (i.e. the post-weekday quadrant) features the largest relative fall in crashes. None of the changes in crashes across any of the quadrants, however, is significant given the calculated standard errors.

Figure 3 depicts the distribution of total fatal crashes summed across 20 minute intervals for weekdays compared to weekends from 8 to 10pm in 2005. The vertical line again marks the onset of the pricing plan threshold. In contrast to the depiction of call volumes in Figure 2, the plot of crash frequencies does not reveal any discernible break in weekday crashes around the threshold. Figure A3 in the Appendix compares the weekday trend in crashes in the pre versus the post period for a slightly longer time window. Again, there is no clear visual evidence for a rise in fatal crashes at the threshold.

We formally estimate the relative change in crashes around 9pm with the following regression analogue to Figure 6:

$$\begin{aligned}(7) \quad Crash_{y,m,d,b} = \alpha + \beta(Post \, * \, After \ 9pm)_{y,b} + \gamma_1 After \ 9pm_b \\ + \gamma_2 Post_y + \eta_y + \eta_m + \eta_d + \varepsilon_{y,m,d,b}\end{aligned}$$

where $Crash_{y,m,d,b}$ denotes the fatal crashes in year y, month m, date d, and minute bin b

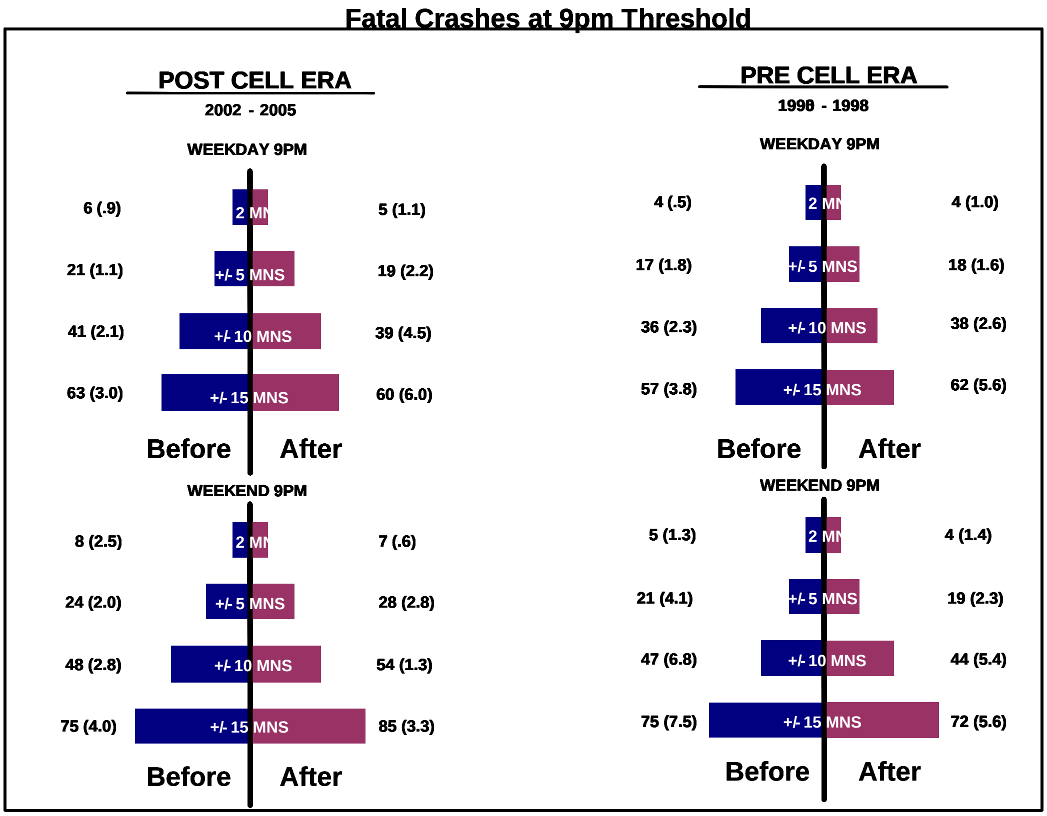

Figure 6, Average Fatal Crashes Before and After 9pm for Pre & Post Era

aggregated across all states. $Post_y$ indicates whether the crash occurred during the period of both high ownership and threshold conformity from 2002 to 2005, and $After\ 9pm_b$ is a dummy variable indicating whether the crash occurred after 9pm. The interaction term $(Post * After\ 9pm)_{y,b}$ is the explanatory variable of interest.

Intuitively, the experiment simulated by this regression is a comparison of the difference in crashes around the threshold in the post period from 2002 to 2005 with the same difference in a control period from 1995 to 1998 for varying, symmetric estimation windows around 9pm. We estimate a baseline regression from 8 to 9:59pm. This ensures that the post-threshold window corresponds to that estimated in the first stage regression of call volume. The model is additionally estimated for a narrower window from 8:30 to 9:29pm. The narrower estimation window around 9pm is less likely to be confounded by unobservable changes in pre and post trends before or after the threshold, but is more sensitive to the considerable reporting biases documented above. As such, standard errors actually increase for the tighter estimation periods. The size of the minute bin b ensures that there is a single, equal-sized bin before and after the threshold strengthening the correspondence to Figure 6. The model is estimated separately for weekdays and then weekends as a robustness check. The model includes fixed effects to control for year, month and day of the week specific variation. The regression is estimated with a Poisson distribution.[24]

The first three columns of Table 5 provide results for fatal crashes. The first two of these columns report negative but insignificant point estimates for the interaction term of interest for the baseline and more narrow estimation window. The results indicate no evidence for a positive relative increase in crashes after the threshold in the post period relative to the pre period. The favored specification of Column 1 implies an upper bound, using a 95% confidence interval, of 1.3%.[25] As a robustness check, Column 3 estimates the double difference for fatal crashes on weekends rather than weekdays. The higher point estimate for the weekend provides evidence that a systematic and unobserved change in the driving environment across the pre and post periods is not masking the estimation of a positive weekday differential. Overall, the results provide no evidence for a positive relative change in fatal crash rates.

All Crash Analysis. We turn next to the pattern of all crashes around the pricing plan threshold. A benefit of expanding focus to all crashes is that non-fatal crashes are about 100 times more frequent than their fatal counterparts. A drawback is that, unlike the FARS dataset, the SDS data is limited to five states with continuous availability from 1996 to 2003 and only two states with continuous data from 1990 to 2004.[26] Figure A4 in the appendix depicts the trend in crashes summed across 20 minute intervals for weekdays in the pre and post periods for those states for which data is available. Once again, no relative break is evident at the threshold.

We again formally test for driver response to the 9pm price discontinuity by estimating Equation 7 for all crashes. The model is estimated with a Poisson regression for the years from 1996 to 1998 and 2002 to 2003 for California, Illinois, Maryland, Missouri and New Mexico. The sample period is restricted due to data availability for these states. Estimations for a longer sample period, but with a smaller number of states, is provided in Table 6 as a robustness check.

The last three Columns of Table 5 provide results of the all crash regression. The estimates are more precise than the fatal crash counterparts, but the coefficient for the

[24] The estimation choice is dictated by the highly non-normal shape of the crash count distribution. Many of the year-weekday-minute bin cells contain 0 fatal crashes. A Poisson distribution represents one possible distributional choice for our count data. Our results are also robust to estimations based on alternative distributional assumptions (e.g. the linear probability model, and negative binomial regression).

[25] Note that for coefficients near zero, the interpretation of a Poisson regression is similar to that of a percent change. For example, we can calculate the upper bound of the interaction variable as $e^{(-.0479+1.96*.031)} \approx .013$.

[26] For Florida, data is available only until 2002, while for California, Illinois, Missouri and Pennsylvania, data is available through 2003. Data is available until 2004 for New Mexico and Maryland. Illinois reports the time of crash only beginning in 1996, and Pennsylvania did not make crash records available to the SDS for 2002. The variability in data availability is understandable given that the SDS must ultimately rely on each state to provide its own crash records.

Table 5

RELATIVE PRE-POST CHANGE IN CRASH RATE AT 9PM THRESHOLD

	DEPENDENT VARIABLE - CRASHES PER MINUTE BIN					
	FATAL CRASHES			ALL CRASHES		
	WEEKDAY		WEEKEND	WEEKDAY		WEEKEND
	8:00 - 9:59 60 mn bin (1)	8:30 - 9:29 30 mn bin (2)	8:00 - 9:59 60 mn bin (3)	8:00 - 9:59 60 mn bin (4)	8:30 - 9:29 30 mn bin (5)	8:00 - 9:59 60 mn bin (6)
Post x After 9pm	-0.0479 (0.031)	-0.0474 (0.043)	0.0685 (0.045)	-0.016 (0.013)	-0.0003 (0.018)	0.0312 (0.023)
After 9pm	0.0535* (0.024)	0.0654* (0.033)	-0.0972** (0.034)	-0.0380** (0.011)	0.0488** (0.016)	-0.0627** (0.019)
Post	0.0624 (0.033)	0.0658 (0.047)	0.0386 (0.047)	0.3312** (0.018)	0.3174** (0.026)	0.3173** (0.034)
Yr, DOW, Month Fixed Effects	x	x	x	x	x	x
N	N = 3656	N = 3656	N = 1458	N = 2612	N = 2612	N = 1040

Notes: The table presents poisson regression results for the pre and post difference in aggregated crashes after 9pm on a particular date. The pre-period for fatal crashes extends from 1995 to 1998, while the post-period extends from 2002 to 2005. Fatal crashes are aggregated across all 50 states. For all crashes, the pre-period extends from 1996 to 1998 (since data is not available for 1995 for Illinois), and the post period extends from 2002 to 2003. The all crash data includes California, Illinois, Maryland, Missouri and New Mexico in order to ensure a balanced panel. The first two columns present the estimation results for fatal crashes on weekdays from 8pm to 9:59pm while the third column presents the baseline specification but for weekends. The final three columns present analogous estimates for all crashes. Fixed effects control for day of week, month and year specific variation in crash rates. The coefficient for the After 9pm dummy variable is coded as 1 for any crash occuring at 9 pm or after in the estimation period. All estimations use robust standard errors and are clustered by date.

* significant at 10%; ** significant at 5%; *** significant at 1%

interaction term of interest is similarly non-positive and insignificant. The favored specification for the 8 to 9:59pm estimation window implies an upper bound of the estimated effect of 1.0%. The double-difference estimated for the weekend, reported in the final column, once again offers no evidence that an unobserved change in the driving environment across the pre and post periods is confounding the result.

Robustness Checks. Table 6 reports the results of a series of robustness checks. The first panel replicates the above analysis for all crashes but for Maryland and New Mexico over an extended sample period stretching through 2004. The results for these states confirm a non-positive relative change in crashes around the threshold with modestly positive upper bounds and a non-negative relative change in crashes on the weekend.

The second panel of Table 6 estimates the baseline specification of Equation 7 for both fatal and all crashes, but with different strategies for allocating crashes to the before and after 9pm minute bins. The variation in calculating the minute bins is meant to deal with the possibility that the double difference approach does not adequately correct for the reporting bias in the above estimation. Accordingly, the panel first shifts the minute bin so that fatal crashes reported from 8:01 to 9:00 are treated as having occurred prior to the threshold while crashes reported from 9:01 to 10:00 are treated as having occurred after the threshold. The second column of the panel estimates the baseline specification but after *eliminating* fatal crashes reported at 9:00 in both the pre and post periods. Both columns produce non-positive estimates of the change in crashes which are in contrast to the positive change estimated for weekends under the bin realignment.[27]

The final three columns of the second panel repeat the exercise for all crashes. Again, coefficient estimates are mildly negative, and the weekend offers no evidence for a possible confound.

In summary, the 9pm pricing analysis provides no evidence for a relative increase in crashes at the threshold. In fact, across some specifications, there is suggestive evidence for a *negative* change in crashes. The upper bound of the estimated relative change is 1.3% for fatal crashes and 1.0% for all crashes. These upper bounds compare to the 4% (8%) increase that one would expect to see at the threshold given the RT estimate of a 4.3 fold increase in relative crash risk, a baseline driver cell phone usage of 10%, and the 16% (33%) discontinuity in call volume implied by the MIT (TNS) data. We comment on the implications of these estimates further in the discussion below.

While the pricing discontinuity offers a relatively clean research design, a drawback of this natural experiment is that it produces only a local average treatment effect of the increase in crash rates around weekdays at 9pm. The observed estimates represent the average influence of cell phones for all types of drivers and driving conditions around the evening threshold. It could be, for instance, that certain types of drivers—for example, young male drivers—are actually endangered by cellular use, while other drivers benefit from such use. We explore this possibility further in the discussion.[28] The applicability of this effect to other times of the day is also debatable. Partially to address this limitation, we outline an additional set of analyses in the next section.

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[27] The estimation of the coefficient for weekend crashes after the elimination of on-hour crashes is similarly positive and insignificant.

[28] It is worthwhile to note that the estimated behavioral response at 9pm is based on changes in cellular usage rather than changes in cell phone ownership. This complicates attempts to extrapolate the estimates to calculate an aggregate effect. This concern can be allayed with a simple assumption equating increased usage with increased ownership.

Table 6

CHANGE IN CRASH RATE AT 9PM THRESHOLD - ROBUSTNESS CHECK

	DEPENDENT VARIABLE - CRASHES PER MINUTE BIN		
	ALL CRASHES (Maryland and New Mexico)		
	Weekday		Weekend
	8:00 - 9:59 60 mn bin	8:30 - 9:29 30 mn bin	8:00 - 9:59 60 mn bin
Post x After 9pm	-0.024 (0.016)	-0.011 (0.021)	0.049* (0.025)

	FATAL CRASHES			ALL CRASHES (5 States)		
	Weekday		Weekend	Weekday		Weekend
	8:01- 10:00 Start Bin :01 (1)	8:01- 9:59 No :00 (2)	8:01- 10:00 Start Bin :01 (3)	8:01- 10:00 Start Bin :01 (4)	8:01- 9:59 No :00 (5)	8:01- 10:00 Start Bin :01 (6)
Post x After 9pm	-0.022 (0.013)	-0.059** (0.030)	0.071 (0.044)	-0.062* (0.031)	-0.019 (0.014)	0.025 (0.023)

Notes: The table presents the Post x After 9pm coefficient for the Poisson estimation of Equation 7 across a number of alternative specifications. The first panel estimates the model for all crashes for the two states (Maryland and New Mexico) for which data over an extended period is available (i.e. pre-period of 1990-1998, and post-period of 2002-2004). The second panel provides Post X After 9pm coefficients for Equation 7 for fatal crashes and all crashes (across all five states) after varying the strategy by which reported crashes are allocated to minute bins. Column 1 organizes bins as starting from 8:01 and 9:01 rather than 8:00 and 9:00. The second column omits accidents falling exactly at 9:00 while the third column reports the estimation with the alternative bin structure for weekends. The first and third columns use 60 minute bins, while the second column uses 59 minute bins. The final three columns provide analogous results for fatal crashes. All standard errors are robust and clustered at the date level.

* significant at 10%; ** significant at 5%; *** significant at 1%

5 Empirical Analyses of Crashes, Ownership and Legislation

A series of additional empirical approaches confirm our basic results. In the first approach, we compare aggregate national trends in crashes and cellular ownership at the EA level. Next, using a EA-month panel, we examine whether legislative bans on driver cell phone use in a number of states reduced the fatal crash rate. Finally, we exploit implied differences in cellular ownership in predominantly urban versus rural counties within an EA as an even more precise test of the link between ownership and crashes.

5.1 Panel Estimation of Crashes and Cellular Ownership

A basic test of whether cell phone use causes crashes is to compare the change in cell phone ownership with the change in the rate of crashes over time. Figure 1 jointly depicts the trend in cellular ownership with the trends in traffic adjusted fatal and all crashes. If anything, the figure hints at a negative correlation between the two series. Such a negative correlation would be even more pronounced if the change in cell phone usage per month, depicted in Figure A1, was considered as well.

However, given the heterogeneous rise in cell phone ownership across regions, we can exploit variation across regions as well as years to more accurately pin down the relationship between ownership and crashes. Indeed, EAs are associated with considerable variation in ownership. Ownership rates ranged from 19 to 57% across EAs in 2001. By 2005, the range in ownership widened from 41 to 95%. Accordingly, we estimate the following model with an OLS regression:

$$\ln(Crash)_{r,y} = \alpha + \gamma Cell\ Own_{r,y} + \theta \ln(Traffic)_{r,y} + \eta_{1r} + \eta_{2y} + \varepsilon_{r,y}$$

where $\ln(Crash_{r,y})$ denotes the log of crashes for region r and year y, while $Cell\ Own_{r,y}$ refers to cell phone ownership in percent terms for region r and year y. The model also includes fixed effects to control for region and year specific variation as well as more flexible controls for region specific linear and quadratic time trends. As a robustness check, we include one specification with an additional covariate, $\ln(Traffic)_{r,y}$, to control for highway traffic volume across region and year. All estimations are conducted at the EA level, with the exception of the robustness specification which is estimated at the state level.

Table 7 presents the results of the estimation for both fatal and all crash data. The first two columns provide estimations for the universe of fatal crashes for all 172 EAs from 1987 to 2005 for all states. Since cellular ownership is only observed at the EA level from 2001 to 2005, we code it as missing from 1993 to 2000, and assume it be zero prior to 1993. This strategy allows us to effectively construct a control period with zero ownership and contrast it with the period for which ownership is both positive and known.

The first column reports the estimated percent change in the fatal crash rate given a 1% increase in cell phone ownership in a representative EA after controlling for EA and year fixed effects. To control for the possibility that omitted trends within a state over time that cause crashes are correlated with cellular ownership, the next column includes more flexible controls which allow for EA specific time trends. Columns 3 and 4 repeat the exercise for all crashes from 1990 to 2004 for the six states and state-years for which data is available. None of the estimated coefficients in these specifications suggest a positive link between ownership and fatal crashes.

The final column of the table provides an important robustness check of the results by controlling for changes in traffic volume across regions and time. Since traffic volume is only coded at the state level, this regression is limited to fatal accidents at the state, rather than the EA, level.[29] While the estimation is highly imprecise—which may be partially attributable to the highly aggregated unit of analysis—it suggests that changes in traffic volume do not seem to alter the earlier results.

Table 7

TRENDS IN CELLULAR OWNERSHIP AND CRASHES ACROSS REGION-YEAR

	DEPENDENT VARIABLE - LN(CRASHES PER 100,000 POP)				
	Fatal Crashes		All Crashes		Fatal Crashes
	Economic Area		Economic Area		State
	(1)	(2)	(3)	(4)	(5)
Cell Phone Ownership	-0.0010 (0.0013)	-0.0004 (0.0002)	-0.0018 (0.0024)	-0.0032 (0.0031)	-0.1274 (0.3948)
ln(Traffic Volume)					0.3978 (0.2803)
Region Fixed Effects	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X
Region FE x Year		X		X	X
Region FE x Year^2		X		X	X
N	N = 1361	N = 1361	N = 315	N = 315	N = 540
R^2	0.86	0.93	0.96	0.99	1.00

Notes: The dependent variable of this OLS regression is the natural log of the number of crashes, per capita, in a given year for a particular region from 1990 to 2005 for fatal crashes, and from 1990 to 2004 for all crashes. Ownership prior to 1993 is assumed to be 0, and ownership from 1993 to 2000 is coded as missing. For the first two columns, crashes are confined to fatal crashes, while the next two columns report all crash data. Column 5 reports a robustness check for fatal crashes at the state level after controlling for state-year traffic volume. The explanatory variable of interest is the rate of cell phone ownership (i.e. cell phone subscribers / population) for the corresponding year and region. All estimations use robust standard errors and are clustered by EA or state.

* significant at 10%; ** significant at 5%; *** significant at 1%

We can calculate upper bounds for the link between ownership and all crashes from our favored specification in column 4. The estimation allows us to reject any effect size larger than .0029 (-.0032 + 1.96*.0031). This suggests that the upper bound of the effect on the all crash rate for a 1% point increase in cellular ownership is .29% given a 95% confidence

[29] Traffic is coded at the state level. Regressions are confined to fatal accidents since the limited number of states in the SDS dataset precludes including all crashes in the estimation. As opposed to EA level penetration which is available only since 2001, state level ownership data is available since 1999.

interval. With current ownership at 75%, a simple linear extrapolation then suggests that the introduction of cellular technology has caused no more than a 22% increase in all crashes compared to the counterfactual scenario in which cell phones were never introduced. An analogous calculation, using the regression result for fatal crashes in column 2, suggests that the introduction of cell phones did not cause any increase in fatal crashes as compared to the counterfactual.[30]

There are plausible explanations for why our estimations do not yield significant results. One, of course, is the absence of any genuine correlation between crashes and cellular ownership. A second possibility is the existence of unobserved, time-varying determinants of crashes which are correlated with the growth in cell phone ownership. While the inclusion of controls for region and year fixed effects, and region specific time trends is meant to guard against this possibility, the likelihood of bias is more pronounced in the absence of EA level ownership data before 2001. A final possibility is that our test lacks power to detect the size of the true effect.

Though the EA represents the most disaggregated level for which subscription data is widely available, our analysis ignores the potential variation of cell phone ownership within a given EA. If systematic historical differences in ownership across rural and urban areas exist, one strategy through which to exploit this variation is to infer county specific cellular ownership from the rural-urban character of each county. We attempt to do this later in the paper.

5.2 Analysis of Legislative Bans on Cell Phones

In a second approach, we estimate the influence of legislative bans which restrict cellular use by drivers. Three states have banned hand-held phones (almost) without exception.[31] New York was the first in November of 2001, followed by New Jersey in July 2004, and then Connecticut in October 2005. Beyond these states, a number of municipalities have also enacted complete bans. The largest of these municipalities are Washington D.C. which enacted a complete ban in July 2004, and Chicago, Illinois whose ban took effect in July of 2005. Nine additional states have legislated partial bans on cellular use, but these bans typically target a modest fraction of drivers (Table A3 in the Appendix enumerates the states and large municipalities with complete or partial bans).[32]

[30] These upper bounds neglect the dramatic rise in cell phone usage per subscriber in recent years, as well as the increase in usage of cell phones specifically by drivers. The FCC reports that cell phone use per subscriber has risen from 140 to 740 minutes per month from 1993 to 2005. If one were to weight yearly ownership by usage by subscriber, then our estimates of the upper bounds of the effect would be even lower.

[31] One common exception is the use of cell phones for emergency calls.

[32] The table excludes numerous states which ban cellular use by drivers of school busses. A list of municipalities with bans can be found in the "Phones and Highway Safety: 2005 Legislative Update" published by the National Conference of State Legislatures. It is available at:

Figure 7 reports the raw monthly counts of fatal crashes for the months preceding and following the enactment of each complete ban for the five relevant regions. The series for Connecticut and Chicago are truncated due to the relatively recent imposition of their respective bans. With the possible exception of Connecticut, the figure indicates no apparent drop in crashes for any of the regions during the month immediately following the ban $(t+1)$ as compared to the month immediately preceding the ban $(t-1)$. Longer horizons reveal no significant decline in crashes with the exception, at first glance, of New York. However, we attribute the drop in crashes in New York at least partially to the attacks on September 11th, 2001, as opposed to the imposition of the legislative restrictions. In fact, the New York legislation, while nominally enacted in November of 2001, was not enforced with binding fines until March of 2002 which corresponds to $(t+4)$ in the figure.

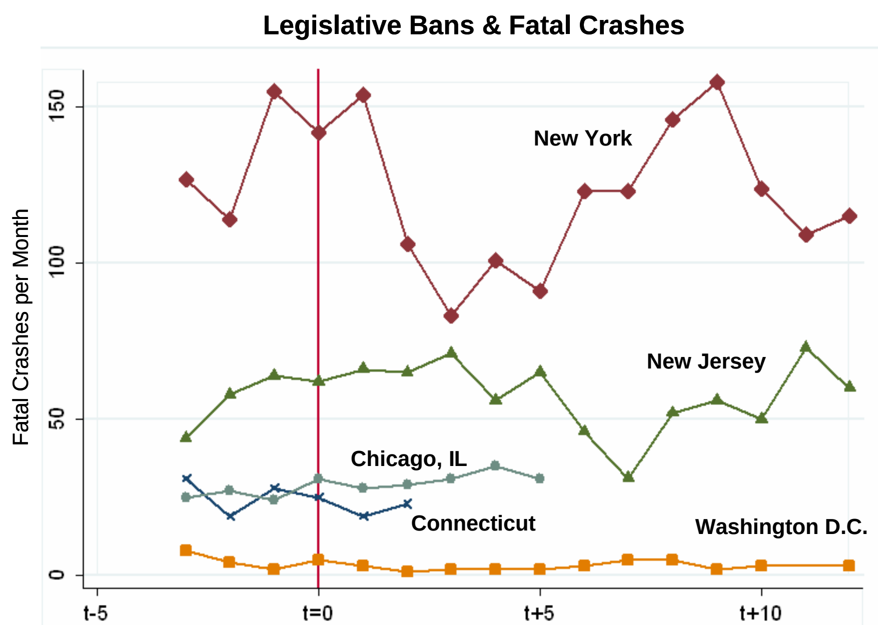

Figure 7, Monthly Fatal Crashes Before and After Legislative Bans

In order to control for possible confounds in crash patterns during this period, we estimate the following Poisson regression at the EA level for fatal crashes from 2000 to 2005:

$$Crash_{r,m,y} = \alpha + \lambda Ban_{r,m,y} + \eta_r + \eta_m + \eta_y + \varepsilon_{r,m,y}$$

where $Ban_{r,m,y}$ is a dummy variable which indicates that a complete ban was in effect for any part of a given EA r, in month m, and year y. Month, year, and EA fixed effects were included along with linear time trends by EA to flexibly control for time and region specific variation in crashes. The estimated coefficient of interest, $\widehat{\lambda}$ (1.38%, $p = .93$), confirms the

$www.ncsl.org/programs/transportation/cellphoneupdate05.htm\#stateCell$)

general theme underlying Figure 7—legislative bans on cellular use do not seem to reduce fatal crash counts.

5.3 Analysis of Urban – Rural Variation in Coverage

One drawback of the region-year analysis is that cellular ownership and crashes are compared at a high level of aggregation. This aggregation introduces imprecision in the estimated correlation between the trends. As an alternative, more precise, estimation strategy, we exploit systematic differences in the spread of cellular coverage in urban as compared to rural areas. This additional precision does not overturn the prior finding of no link between ownership and crashes.

Urban-Rural Coverage Gap. Policy makers have long recognized the existence of an urban-rural gap in telecommunications infrastructure. The FCC first explicitly addressed urban-rural differences in cellular service provision in its annual report in 2002 (FCC 2002). The organization assessed competitive differences between urban and rural markets using a variety of classification schemes through which they distinguished urban from rural areas.[33] The FCC concluded that rural consumers had far less choice in providers—and inferior coverage—than their urban counterparts. The media has characterized rural areas as having less provider choice, more dead zones and worse service quality.[34] Such factors cause—or perhaps reflect—the lower ownership levels that are likely to be found in rural as opposed to urban populations. At least in the early years of cell phone technology, the marginal urban consumer has been more profitable to serve than her rural counterpart.

The intuition underlying the analysis in this section is that trends in the urban-rural gap in cellular ownership should be at least partly mirrored by trends in urban-rural differentials in crash rates if cell phone usage impacts driving safety. Unfortunately, precise measures of the urban-rural gap are difficult to locate. The challenge is that ownership data is not separately available for rural and urban areas. However, we are able to confirm the suggestion of lagging rural ownership using an indirect approach.

Since the most disaggregated subscription data is available at the level of the EA, a first step in assessing urban-rural ownership is to identify the urban-rural character of each EA. Counties in the US are often classified along a urban-rural continuum depending on the size of the urban population and proximity to a metro area.[35] Appendix Table A4

[33] These classification schemes included "Cellular Market Areas," population density, and "Economic Area Nodal" versus "Economic Area Non-Nodal" counties. The FCC reports state, however, that "the FCC does not have a statutory definition of what constitutes a rural area."

[34] A news article in the *USA Today* highlights the concerns of rural consumers and the factors determining choice of cell tower location in rural markets (Davidson, USA Today, December 20th, 2005).

[35] This coding scheme was originated in 1975 by David Brown, Fred Hines, and John Zimmer for a report entitled "Social and Economic Characteristics of the Population in Metro and Nonmetro Counties: 1970." It was updated after the both the 1980 and 1990 census. The current coding is from 2003 and is similar

enumerates the nine categories of counties on the urban-rural continuum and also displays the distribution of counties and population across these categories in 2000.

From these county level classifications, we generate two measures of EA urban-rural character. The first is the population-weighted average of the urban rural continuum codes of the counties in each EA. The second is the distribution of the EA population across the nine county types. Figure 8 presents an EA level scatter plot of cellular ownership and the urban character of an EA (as defined by the share of included counties with urban-rural continuum codes 1 to 5). The plot is drawn for 2001 which is the first year EA level ownership data is available. The figure illustrates that more urban EAs tend to have higher levels of cellular ownership.

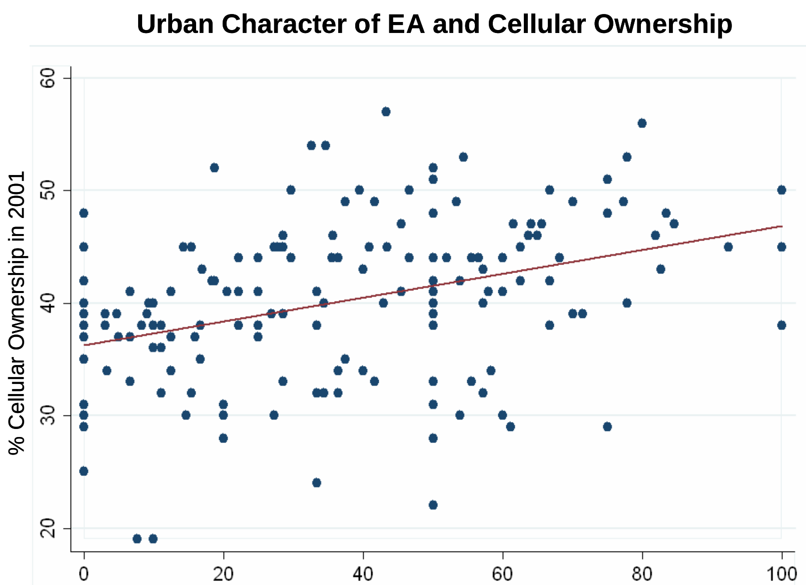

Figure 8, % Share of Urban Population by EA
(U/R Codes < 6)

To test this relationship more formally, Table A5 in the Appendix presents the results of EA level regressions of the change in ownership first from 1992 to 2001 and then from 2001 to 2005, on the two measures of urban-rural character.[36] The results confirm the graphical intuition that cellular ownership was lagging in more rural EAs as of 2001. On average, cell phone ownership was 1.9% lower for every one point increase in the EA average urban-rural continuum code in the period prior to 2001. This translates into a difference of about 14% in cell phone ownership between the most urbanized and the most rural of

in spirit to the earlier approach. However, the 2000 census outlined major changes to definitions of metro, urban and rural areas and, as a consequence, the current coding is not comparable to that of the earlier period.

[36] This change is equivalent to the level of penetration in 2001 given the assumed 0% penetration in 1992.

EAs in 2001.[37] The analogous estimate for the change in ownership from 2001 to 2005 is negative but insignificant. If rural areas narrowed the ownership divide during that period, one would expect positive coefficients on rural markers. If anything, the results of the regressions suggest that the most urban counties made further gains in ownership relative to their counterparts from 2001 to 2005.

Vehicular Crashes and Urban-Rural Character. Next we turn to trends in the urban-rural differential in fatal and non-fatal vehicular crashes during the period of high cellular ownership. Since rural ownership lags urban ownership within an EA, we expect ownership levels to be decreasing in the urban-rural continuum codes. Consequently, within an EA, more urban counties should have a higher level of cellular ownership than suggested by their EA average, while the more rural counties should have ownership levels lower than what would be predicted by their EA average. We aggregate counties into two groups to simplify the analysis. We label counties with urban-rural continuum codes 1 to 5 as urban and those with codes 6 to 9 as rural.[38] We further classify fatal crashes as having occurred in either urban or rural counties using the same taxonomy.

To test the relationship between fatal crashes and cell phone ownership as inferred by county type, we regress the log of fatal crash rates at the county level on EA level ownership as well as an interaction of ownership and a dummy variable D_t which indicates county type t:

$$\text{(8)}\quad \ln(Crash)_{r,y,t} = \alpha + \gamma Cell\ Own_{r,y} + \sum_{t\in\{urb,rur\}} \gamma_t [Cell\ Own_{r,y} * D_t] + \eta_r + \eta_y + \eta_t + \varepsilon_{r,y,t}$$

A strategy identical to that outlined earlier was used to create a control group with zero ownership (prior to 1993) and a treatment group with known ownership (from 2001 - 2005). Fixed effects and EA specific linear and quadratic time trends are used to control for time-varying confounds within an EA. If cellular ownership influences fatal crashes, one would expect the relative link between ownership and crashes in urban counties to be stronger than in rural counties relative to the EA average.

The first three columns of Table 8 provide OLS estimation results for fatal crashes. The first column points to a large difference in the number of crashes across urban and rural counties. On average, fatal crashes are 45% higher in rural counties. This is not surprising. While rural area crashes are far less common than in more populous urban and

[37] The regression also suggests that penetration in 2001 is strongly correlated with the population share of the major metropolitan counties—that is, a 1% increase in the metro population share of an EA is associated with a 0.14% increase in cell phone ownership.

[38] We performed the analysis at a more disaggregate level, using the 1 to 9 urban rural continuum code, and found similar results.

suburban areas, such crashes are more likely to be fatal since they generally involve higher speeds, fewer safety restraints, and relative delays in the arrival of medical care.[39]

Table 8

TRENDS IN CRASHES AND OWNERSHIP BY URBAN-RURAL CHARACTER

	DEPENDENT VARIABLE - LN(CRASHES PER 100,000 POF			
		Fatal Crashes		All Crashes
	(1)	(2)	(3)	(4)
Rural County	0.4556*** (0.0231)	0.4061*** (0.0231)	0.4047*** (0.0231)	-0.0510 (0.0330)
Cell Phone Ownership		0.0016 (0.0016)	0.0021 (0.0019)	-0.0032** (0.0015)
Rural x Ownership		0.0014*** (0.0003)	0.0015*** (0.0003)	0.0005 (0.0004)
EA Fixed Effects	X	X	X	X
Year Fixed Effects	X	X	X	X
EA Fixed Effects x Year		X	X	X
EA Fixed Effects x $Year^2$			X	X
N	N = 23226	N = 23226	N = 23226	N = 2611
R^2	0.29	0.30	0.30	0.58

Notes: The dependent variable for the first three columns is the natural log of the number of fatal crashes, per capita, for a given year for a particular EA. The dependent variable for the final column is the natural log of the number of all crashes, per capita, for a given year for a particular EA for region-years for which data is available. Counties with urban-rural continuum codes 6 and above are designated as rural. All errors are robust and clustered at the EA level.

* significant at 10%; ** significant at 5%; *** significant at 1%

The second and third columns reaffirm the absence of a link between ownership and the fatal crash rate for urban counties found in the earlier EA-level panel regressions. Moreover, there is evidence that rural counties are associated with a *stronger* link between cellular ownership and crashes than urban counterparts. This result is at odds with the evidence of ownership and urban character if driver cell phone use does cause crashes. These estimates are robust to the inclusion of flexible controls.

Column 4 estimates the same model but for all crashes using the SDS data for available state-years from 1990 to 2004. The results indicate, albeit with some imprecision, that the frequency of crashes is lower in rural as opposed to urban areas. Once again the

[39] There is the intriguing possibility that the spread of cell phones may actually help reduce crash fatalities especially in rural areas if crash victims or passing motorists are able to summon medical assistance more promptly.

interaction term offers no evidence for the dampened influence of ownership on crashes in rural counties. There is even evidence that the cellular ownership is *negatively* related to the crash rate in urban areas. This is consistent with the existence of omitted factors, associated with crash reduction (e.g. safety technology or regulatory changes), that may be correlated with the rise in ownership. On the whole, Table 8 offers no evidence that crash rates increased more rapidly in urban as compared to rural areas.

6 Discussion

The present analysis suggests that cell phone use by drivers is not associated with higher crash rates. This finding is contrary to numerous other studies as well as the popular consensus. However, whether the upper bounds which emerge from this analysis are able to confidently reject existing research depends critically on a number of assumptions. Table 9 compares the findings of the natural experiment with the expected crash increases at 9pm implied by RT under varying assumptions of baseline usage by drivers and call volume increase.

One key assumption relates to the baseline level of cell phone usage during nighttime driving. The lone nighttime assessment of cell phone usage is the 1.5% estimate of drivers on the NJ Turnpike (Johnson et. al. 2004). Published in 2004, the study relies on data collected between March and July of 2001 and focuses explicitly on handheld usage by drivers on high speed roadways. As such, the estimates are from a period with minimal cellular ownership and for a group of drivers who are not broadly representative.[40] While it is reasonable to assume that nighttime usage may be lower than the daytime NOPUS figure of 10%—although interestingly the NJ Turnpike study found no significant difference between afternoon and nighttime usage—it is also likely that combined usage (handheld and hands-free) during the late evening hours in 2005 is well above 1.5%.[41]

A second critical assumption concerns the rise in cellular call volume at 9pm. Two data sets of callers provides evidence for a 16 to 33% rise in cellular call volume. However, the correctness of comparing the price sensitivity of broader cell phone use to such use by drivers is unclear. While our research design only assumes a non-positive rise in call volume at the switching threshold for cellular users behind the wheel, the size of the upper bounds depends on this relative sensitivity. The parentheticals in Table 9 indicate the minimal share of the overall rise in call volume that must characterize driver cell phone use in order

[40] Note that the NOPUS estimates of cellular usage doubled from 2000 (3%) to 2005 (6%), and was 4% in 2002.

[41] The study did find lower usage during the 12am to 8am period, but did not find significantly lower usage from 8pm to 12am. Usage during this latter period was estimated as .85 of baseline usage from 12pm to 4pm, but this difference was not statistically significant.

for the calculated upper bound for all crashes (1.0%) to rule out the implied RT estimate. For example, if driver cell phone usage is 4%, and the overall rise in call volume for cellular users is 33%, then the rise in call volume for drivers must be at least 9% (i.e. .26 x .33) in order for the upper bound of 1.0 to reject the implied RT estimate. Assuming at least 4% nighttime usage, the rise in driver calls must be about .1 to .5 as large as the broader rise in calls at the threshold in order to rule out RT.

We argue that this range of calibrations is reasonable. It is unclear, for example, whether drivers, ex-ante, should be considered more or less sensitive to the change in prices at the threshold than the broader population. One might argue that driver phone use is more urgent, and thus more price insensitive. On the other hand, it has also been demonstrated that such calls are made by younger drivers and such drivers may be more price sensitive than a typical caller. Whether drivers are more or less aware of the current time than other cellular users is also unknown. It does seem reasonable to assume that at least half of driver cell phone calls are received as opposed to made. If drivers exhibit price insensitivity while making calls, assuming that a driver does not ignore calls received, one might still expect the price sensitivity of drivers to be at least half of that of the broader population.[42]

Under reasonable assumptions, the findings of this paper are more consistent with the trends of Figure 1 than that of the estimates produced by RT. In fact, for all but implausibly low ranges of baseline cell phone use, the upper bounds we calculate for all crashes fall below the RT estimates. Despite the lower precision in estimates of fatal crashes, the upper bound of 1.3% for the fatal crash rate also falls below plausible RT estimates.

What might explain the departure of our results from RT? As mentioned, the RT study suffers from two principle drawbacks. The first is that it relies on an unrepresentative sample of those involved in a recent crash. Selection implies that the RT result is at best an upper bound for the population of drivers as a whole. Additionally, there is the possibility that the RT result is driven by a confound such as driver anxiety which prompts both cellular use as well as higher crash risk. Finally, it is also possible that the findings of RT, generated in 1997, may no longer apply to the recent, more seasoned, cell phone driver. Later studies, however, have closely replicated the RT results (McEvoy et. al. 2005). We turn next to the mechanisms which might explain the absence of a correlation between crashes and cellular use.

[42] This argument neglects that a fraction of incoming calls are also likely to be made by landlines. Moreover, some incoming calls may be made by callers in different time zones who effectively have different switching thresholds.

Table 9

COMPARISON OF 9PM % CRASH INCREASE IMPLIED BY RT AND PRESENT ANALYSIS

	RT STUDY				9PM PRICE DISCONTINUITY	
					Upper Bound of Estimate	
	Driving Time on Cell Phone				Fatal	All
9pm Call Volume Rise	1.5%	4%	6%	10%		
16% (MIT)	0.8	1.9 (.54)	2.6 (.38)	4.0 (.25)	1.3	1.0
33% (TNS)	1.6 (.64)	3.8 (.26)	5.5 (.18)	8.2 (.12)	1.3	1.0

Notes: This table presents the increase in aggregate crash risk due to driver cell phone use at 9pm implied by the RT study as compared to the present analysis. The table reports the risk increase implied by varying estimates of driver cell phone use, as well as estimates of the call volume increase from 9pm to 10pm indicated by the two first stage samples. The calculations use the RT relative crash risk estimate of 4.3. The parenthetical figures refer to the minimal fraction of the rise in call volume at 9pm which must be associated with the subpopulation of drivers with cell phones in order for the upper bound estimate for all crashes (1.0) to rule out the implied RT result.

Plausible Explanations for the Effect. If cell phones are a source of distraction, given limits to attentional capacity, how is it that such phones have zero, or perhaps even a mitigating, influence on crashes? There are a number of plausible explanations for why cell phone use may not raise crash frequency.

One explanation is that drivers who use cell phones compensate for the added distraction by modifying their driving behavior. This so called "Peltzman Effect" was popularized by Sam Peltzman who suggested that the benefits of seat-belt regulations might be offset by riskier driving (1975). While compensatory responses to the imposition of seat-belts may seem unlikely, it is plausible to imagine drivers who slow down, pull over, shift to uncongested lanes or roadways, or simply devote more attention to driving in response to making or receiving a cell phone call. If cell phones are as distracting as the experimental research suggests then given the frequency of their usage, and the potential costs of a crash, one should expect drivers to compensate.

Consider a simple model of driver utility under the influence of distracting, but beneficial, cell phone use:

$$U(a, s; m) = v(c) + w(s) - mc - p(s, c)L$$

Here s is the driving speed. Driver utility increases with higher speeds because drivers value their time and possibly enjoy the thrill of such driving. However, speeding is subject to diminishing marginal utility such that $w_s > 0$ and $w_{ss} < 0$. Drivers enjoy cell phone use, denoted by c, but the benefit of such use is also subject to diminishing marginal utility such that $v_c > 0$ and $v_{cc} < 0$. Additionally, m is the unit cost of cell phone use while the probability of an accident, p, is an increasing and convex function of speed and cell phone use such that $p_s > 0$, $p_c > 0$, $p_{ss} > 0$ and $p_{cc} > 0$. We also assume that $p_{cs} > 0$ to indicate that cellular use is increasingly dangerous at high speeds. Finally, L represents the loss from an accident and $L \gg m$.

For a given unit cost, m, a driver chooses (s^*, c^*) to maximize utility (see Appendix for derivation of first and second order conditions). The effect of a change in the cost of cellular usage, m, on the probability of an accident, $p(s^*, c^*)$ can be expressed as:

$$\frac{dp(s^*, c^*)}{dm} = p_s \frac{ds^*}{dm} + p_c \frac{dc^*}{dm}$$

A fall in the price of a cellular call, m, all else equal, will increase the probability of an accident by increasing cellular usage since $\frac{dc^*}{dm} < 0$. However, even if cellular use rises, the probability of a crash may remain unchanged, or even fall, so long as the driver compensates for the increased danger by driving more slowly (i.e. if $\frac{ds^*}{dm} > 0$).

We can show that such compensation arises under the stated assumptions and preferences by solving for $\frac{ds^*}{dm}$. Total differentiation of the FOC for (s^*, c^*) yields:

$$w_{ss} \frac{ds^*}{dm} - L(p_{ss} ds^* dm + p_{sc} \frac{dc^*}{dm}) = 0$$

$$v_{cc} \frac{dc^*}{dm} - L(p_{sc} \frac{ds^*}{dm} + p_{cc} \frac{dc^*}{dm}) = 0$$

This can be solved to show:

$$\frac{ds^*}{dm} = \frac{p_{sc} L}{(w_{ss} - p_{ss} L)(v_{cc} - p_{cc} L) - p_{sc}^2 L^2}$$

The numerator of the above equation is positive. The denominator can be expanded and rewritten as $w_{ss} v_{cc} - w_{ss} p_{cc} L - v_{cc} p_{ss} L + (p_{ss} p_{cc} L^2 - p_{sc}^2 L^2)$. Under the stated assumptions and preferences each term in this expression is positive. This ensures that $\frac{ds^*}{dm} > 0$. The relative magnitude of the respective terms determines whether partial, complete, or over-compensation occurs.

There is experimental and field evidence that drivers compensate when using cell phones. In driver simulations in the lab, subjects with impaired reaction times respond by increasing their following distance (Strayer and Drews and Johnston 2003; Strayer and Drews 2004;

Strayer and Drews and Crouch 2006) as well as by slowing down (Rakauskas et. al. 2004). The few studies which examine cell phone distraction in repeated trials find evidence for learning (Shinar et. al. 2005). There is additional field evidence consistent with compensation. The NOPUS estimate of cellular usage in 2001 of 3% is based on observation of stopped vehicles, whereas, during the same year, the NJ Turnpike study reports cellular usage for high speed vehicles is only 1.5%.

A second, related, explanation for our findings is that the drivers who use cell phones have an affinity for riskiness. In this scenario, risk loving drivers simply use cell phones as a substitute for other distractions (e.g. talking to a fellow passenger, or fiddling with their radios). Hahn and Prieger present a model for such behavior as well as survey evidence of drivers which suggests that driver heterogeneity in riskiness leads most research to significantly overestimate the impact of cell phone use on crashes (2006). Much like our study, they conclude that driver use of cell phones has close to a zero effect on crashes.

Finally, the effect of cellular use on crashes may be heterogeneous across drivers. While the local average treatment effect may be marginally negative or zero, there may be drivers for whom the use of cell phones is detrimental, as well as some drivers for whom cell phones are beneficial. Since our estimation does not distinguish between different driver types, our results could be masking the variation in the dangers of cell phone use that is evident in some experiments. For example, cell phones may actually improve selective driver outcomes by alleviating boredom. The NHTSA reports that 100,000 crashes, and 1500 fatal crashes each year are attributable to driver fatigue or sleepiness (NHTSA 2004), and "The 100-Car Naturalistic Study" concluded that 20% of crashes and 12% of near-crashes were linked to driver fatigue (NHTSA and Virginia Tech 2006). The dangers of fatigue may be particularly pronounced for drivers accustomed to driving long distances or long hours. To this point, the Federal Motor Carrier Safety Administration, in 2003, implicated fatigue as a factor in 13% of all fatal large-truck crashes.[43]

Implications for Welfare and Policy. Incontestably, cell phones provide economic value to drivers. Driver use of cell phones has been increasing over the years, and there is some evidence that such use continues even in spite of explicit regulations. The Harvard Center for Risk Analysis pegged the value of non-emergency cellular calls by drivers at $43 billion annually (Lissy et. al. 2000). Yet despite transparent benefits, a majority of Americans support bans of driver cell phone use and view such devices as a leading threat to public safety (Gallup 2003). Moreover, a large number of municipalities, states, and even Congress, have either considered or passed legislation restricting driver use of cell phones over the last several years.

[43] This statistic was reported as a part of the "Report to Congress on the Large Truck Crash Causation" (2003).

In light of the benefits of cellular devices, our results suggest that such bans on all cellular use may not be economically efficient. However, given that our results cannot rule out dangers for certain classes of drivers, partial bans may be worthwhile. Bans of cell phone use by teenagers in a number of states suggests that policy makers believe in such heterogeneity in risk (AAA 2007). More research is required to determine whether there exist subpopulations of drivers for whom the link to crashes may indeed be high.

Policies aimed at regulating driver cell phone use trade off benefits of usage against the risk to life, limb and property. As such, the estimates of our paper could be used to help calculate the statistical value of life implicit in such policies and could further inform cost-benefit analyses of the same (Johansson 2002; Kniesner and Viscusi 2003).

7 Conclusion

This paper exploits a natural experiment, induced by a discontinuity in pricing of popular cell phone plans, to estimate the influence of driver cell phone use on fatal and non-fatal crashes. We find no evidence for a link between such phone use and the crash rate. This result is at odds with much of the existing research. The most influential study on this topic (RT) suggests that cell phones result in a 4.3 fold increase in relative crash risk. This equates the danger of cellular use to that of illicit levels of alcohol. While the RT results imply an approximately 1 to 8% rise in all crashes across the pricing threshold, the upper bounds of our estimates allow us to rule out any rise in fatal crashes larger than 1.3% and any rise in all crashes larger than 1.0%. Even recognizing that the (unmeasured) change in driver cell phone use at the pricing threshold may fall below that of the broader population of callers, the upper bounds of our estimates are still likely to reject the RT result. To corroborate our findings, we pursue three additional empirical strategies. None of these provide evidence to support the relationship between phone use and crashes.

We note, however, that this research does not imply that cell phone use is innocuous. It simply implies that *current* cellular use by drivers does not appear to cause a rise in crashes. It is possible that drivers who use such devices compensate for the added distraction by driving more carefully. Alternatively, it could be that risk loving drivers may treat cell phones as a substitute for other, equally debilitating, distractions. Finally, because we measure a local average treatment effect, it could be that cell phones are dangerous for certain drivers or driving conditions, and may be beneficial for others.

In the least, we believe our findings should renew interest in empirical research examining the effects of cell phone use and reopen discussions on the costs and benefits of policy where such dialogue has quieted. One direction of future research, which may prove particularly important to policy makers, is to investigate whether the influence of cellular use differs across types of drivers and driving contexts. Our research design allows for such an analysis of driver heterogeneity if one exploits differences in price sensitivity across demographic groups as an additional source of treatment variation.

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9 Appendix – Tables and Figures

Table A1

SUMMARY OF DATA SOURCES

	DATA SOURCE	YEARS	DESCRIPTION
CRASH RECORDS			
Fatal Crashes	Fatality Analysis Reporting System (FARS)	1990 - 2005	Crash records for all fatal crashes for all 50 states
All Crashes	State Data System (SDS)	1990 - 2004	Crash records for all crashes for seven states
Traffic	Federal Highway Administration	1987 - 2005	Traffic volume by county by year
OWNERSHIP DATA			
Cellular Subscribers	Cellular Telephone Industry Association Survey	1999 - 2005	Cellular subscribers by state by year
	Federal Communications Commission	2001 - 2005	Cellular subscribers by Economic Area (EA)
Population	Bureau of Labor Statistics	1990 - 2005	Yearly population by county
EA - County Codes	The Bureau of Economic Analysis	2000	EA codes for each county
CALL VOLUME DATA			
	Reality Mining Project, MIT	2005	Logs tracking ~80,000 outgoing cellular calls for 65 students/faculty at MIT over the course of 2005
	TNS Telecom	2000 - 2001	Data from cellular phone bills for 9907 households
PRICING DATA			
Provider Pricing Plans	Econ One Research	2001 - 2005	Historical pricing plan details for all providers offering plans in NYC
Provider Market Shares	S&P Industry Reports	1999 - 2005	Market shares by provider by year
URBAN-RURAL GAP			
	United States Census	1990 - 2005	Population density by county
	United States Department of Agriculture	1990 - 2005	Urban/ Rural classifications by county

Table A2

SUMMARY STATISTICS

	BY ECONOMIC AREA (EA)				
	N	MEAN	MIN	MAX	MED
	(1)	(2)	(3)	(4)	(5)
POPULATION	in Millions				
1990	172	1.45 (2.60)	0.06	23.95	0.63
2005	172	1.72 (3.01)	0.06	26.38	0.76
PENETRATION	% share of Population				
2001	168	40.2 (7.2)	19	57	40.5
2005	169	68 (10.2)	41	95	67
FATAL CRASHES	rate per 100,000				
1990	172	19.4 (5.7)	9.4	40.0	18.7
2005	172	17.4 (6.3)	7.0	44.3	16.2
ALL CRASHES	rate per 100,000				
1990	55	2239 (921.2)	1013	4294	2105
2003	46	2170 (810.4)	793	3573	2370

Table A3

SUMMARY OF BANS ON HANDHELD CELL PHONES

REGION	DATE OF ENACTMENT	SCOPE OF BAN	PUNISHMENT
Connecticut	Oct 2005	Complete	$100 fine
New Jersey	July 2004	Complete	Secondarily enforced, fines from $100-250
New York	Nov 2001*	Complete	$100 fine
Washington D.C.	July 2004	Complete	$100 fine (first offense waivable)
Chicago, Illinois	July 2005	Complete	$50-100 fines
Colorado	--	Ban on permit drivers	Secondarily enforced, fine of $15
Delaware	--	Ban on permit drivers	Similar to reckless driving penalties
Illinois		Ban on permit drivers	Not Available
Maine	--	Ban on permit drivers	No penalty specified
Maryland	--	Ban on permit drivers	License may be suspended for up to 90 days
Minnesota	--	Ban on permit drivers	License may be restricted
Tennessee		Ban on permit drivers	$100 fine
Texas	--	Ban on permit drivers*	Not Available
Virginia		Ban on permit drivers	Fines up to $1000

Notes: Data was compiled from National Conference of State Legislatures reports, as well as various other news sources. States with bans on school bus drivers are not listed. "Complete" refers to ban on hand-held cell phone for all drivers. New York law was enacted in November 2001, but fines were not fully binding until March 2002. In New Jersey and Colorado, cell phone use is ticketed only in combination with some other violation. California has also passed a state-wide ban on handheld cellular usage, but the ban will not go into effect until July of 2008. The Illinois ban on teenage cellular use goes into effect in January of 2008. The Texas ban on permit drivers applies to drivers only for the first six months following the issuance of a permit. Date of enactment only reported for regions with complete bans

Table A4

COUNTY AND POPULATION DISTRIBUTION ACROSS THE URBAN-RURAL CONTINUUM IN 2000

CODE	DESCRIPTION	NUMBER	% POP
1	Counties in metro areas of 1 million in population or more	413	0.53
2	Counties in metro areas of 250,000 to 1 million in population	325	0.20
3	Counties in metro areas of fewer than 250,000 in population	351	0.10
4	Urban population of 20,000 or more, adjacent to a metro area	218	0.05
5	Urban population of 20,000 or more, not adjacent to a metro area	105	0.02
6	Urban population of 2,500 to 19,999, adjacent to a metro area	609	0.05
7	Urban population of 2,500 to 19,999, not adjacent to a metro area	450	0.03
8	Completely rural or less than 2,500 in urban population, adjacent to a metro area	235	0.01
9	Completely rural or less than 2,500 in urban population, not adjacent to a metro a	435	0.01

Table A5

CELLULAR OWNERSHIP & URBAN-RURAL CHARACTER

	DEP VAR - CHANGE IN % CELLULAR OWNERSHI			
	1992 - 2001		2001 - 2005	
	(1)	(2)	(3)	(4)
EA Rural Code (Type)	-1.929***		-0.629	
	(0.3120)		(0.7590)	
% Pop - County Type 1		0.139***		0.205*
		(0.0470)		(0.1190)
% Pop - County Type 2		0.026		0.108
		(0.0420)		(0.1610)
% Pop - County Type 3		-0.026		-0.007
		(0.0490)		(0.2100)
% Pop - County Type 4		0.016		0.105
		(0.0470)		(0.1120)
% Pop - County Type 5		-0.024		0.138
		(0.1130)		(0.1080)
% Pop - County Type 6		-0.031		0.191*
		(0.0490)		(0.1060)
% Pop - County Type 7		-0.128		0.059
		(0.0790)		(0.1150)
% Pop - County Type 8		0.026		
		(0.0760)		
% Pop - County Type 9				0.135
				(0.1930)
R^2	0.17	0.27	0.01	0.08
N	N = 168	N = 168	N = 172	N = 172

Notes: The dependent variable is the change in cellular ownership over the indicated period. EA rural code refers to the county average urban-rural continuum code weighted by the population for an EA in 2000. Higher values denote more rural EAs.

* significant at 10%; ** significant at 5%; *** significant at 1%

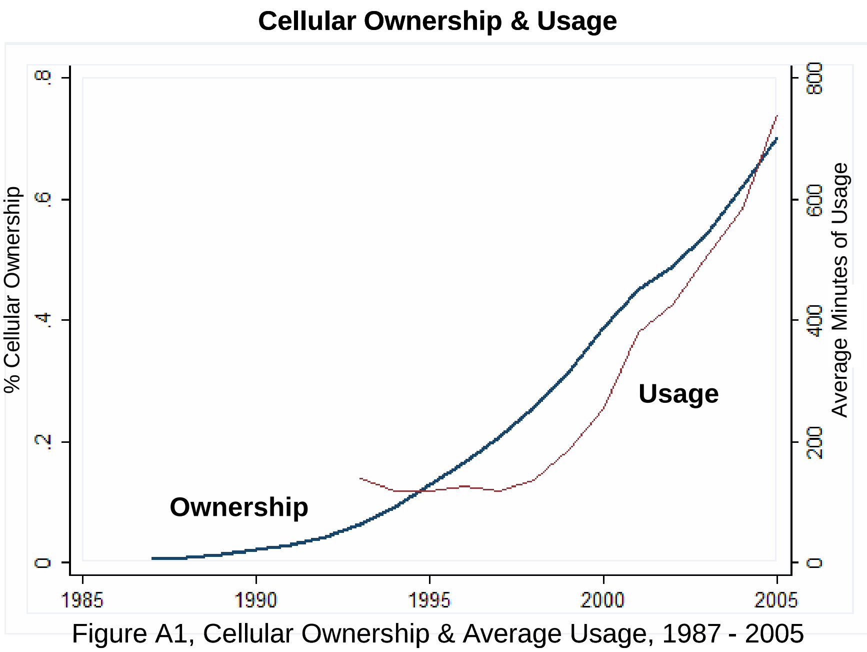

Figure A1, Cellular Ownership & Average Usage, 1987 - 2005

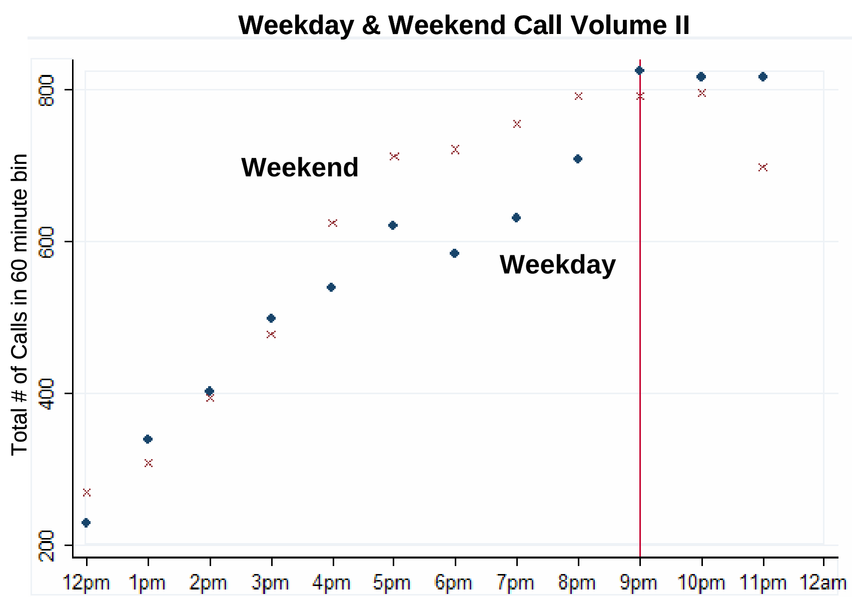

Figure A2, Outgoing Calls from 12pm – 12am in 2005
(60 mn bins)

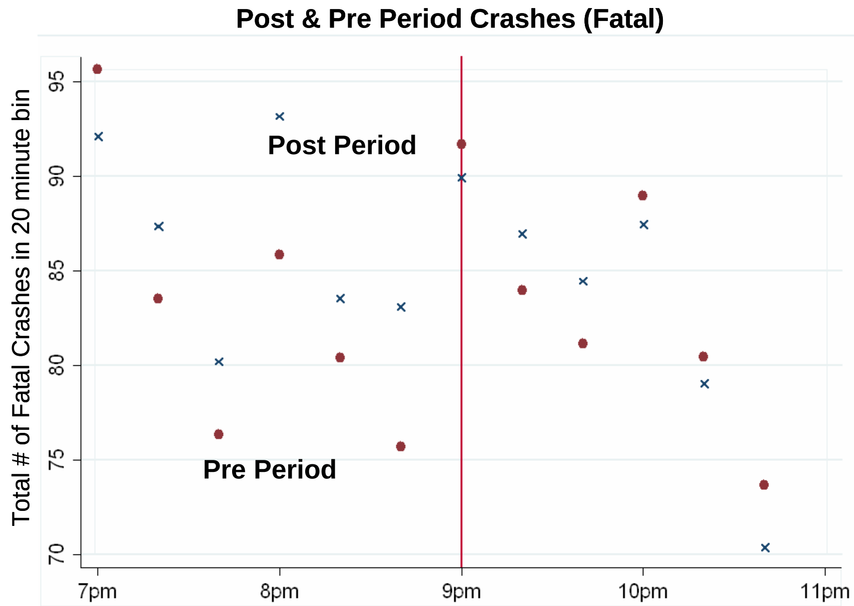

Figure A3, Fatal Crashes from 7pm – 11pm
in 1990-1998 & 2002-2005 (20 mn bins)

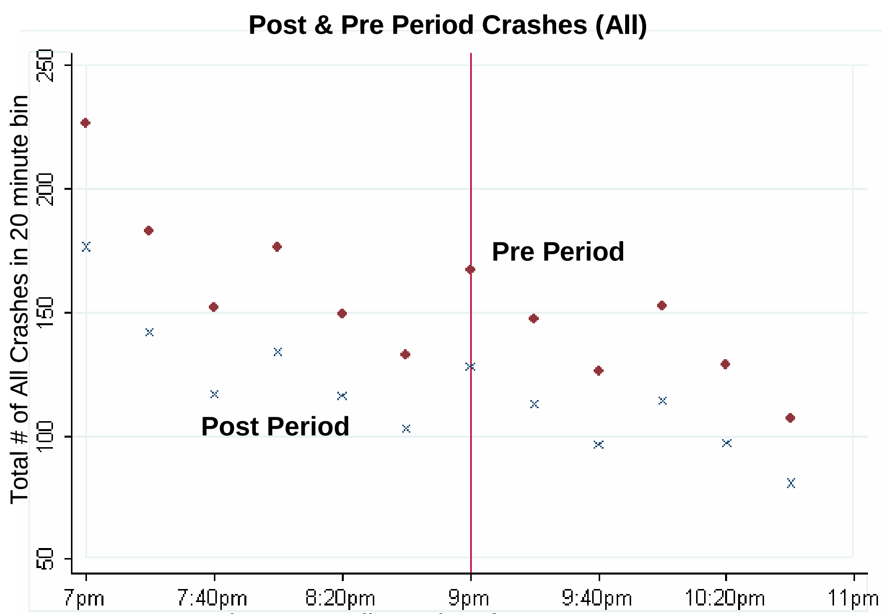

Figure A4, All Crashes from 7pm – 11pm
in 1990-1998 & 2002-2004 (20 mn bins)

10 Appendix – Derivation of Compensation Model

The first order conditions of the model are given by:

$$U_s : w_s - p_s L = 0$$

$$U_c : v_c - m - p_c L = 0$$

The SOC requires that the Hessian is negative semi-definite. While it is easily seen that $U_{ss} < 0$, a second requirement is that:

$$U_{ss}U_{cc} - U_{sc}^2 : (w_{ss} - p_{ss}L)(v_{cc} - p_{cc}L) - p_{sc}^2 L > 0$$

We can recast the above expression as:

$$U_{ss}U_{cc} - U_{sc}^2 : w_{ss}v_{cc} - w_{ss}p_{cc} - v_{cc}p_{ss}L + (p_{ss}p_{cc} - p_{sc}^2)L^2 > 0$$

The first three terms of the expression are positive while the last term is positive so long as p_{sc} is sufficiently small.