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Driving Under the (Cellular) Influence: The Link Between Cell Phone Use and Vehicle Crashes[*]

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Working Paper 07-15

July 2007

[*] PRELIMINARY VERSION. We would like to thank David Card, Stefano Della Vigna, Robert Hahn, Michael Greenstone, Botond Koszegi, Prasad Krishnamurthy, Ritu Mahajan, Enrico Moretti, James Prieger, Matthew Rabin, Aman Vora, Glenn Woroch and participants of the IGERT workshop at the Goldman School of Public Policy for their thoughtful comments. Gregory Duncan, Nathan Eagle, Jeff May, and Econ One Research made important data contributions. We would also like to thank the IBER for providing funding for this project.

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Executive Summary

The link between cell phone use while driving and crash risk has in recent years become an area of active research. The most notable of the over 125 studies has concluded that cell phones produce a four-fold increase in relative crash risk—comparable to that produced by illicit levels of alcohol. In response, policy makers in fourteen states have either partially or fully restricted driver cell phone use. We investigate the causal link between cellular usage and crash rates by exploiting a natural experiment induced by a popular feature of cell phone plans in recent years—the discontinuity in marginal pricing at 9 pm on weekdays when plans transition from “peak” to “off-peak” pricing. We first document a jump in call volume of about 20-30% at “peak” to “off-peak” switching times for two large samples of callers from 2000-2001 and 2005. Using a double difference estimator which uses the era prior to price switching as a control (as well as weekends as a second control), we find no evidence for a rise in crashes after 9 pm on weekdays from 2002-2005. The 95% CI of the estimates rules out any increase in all crashes larger than .9% and any increase larger than 2.4% for fatal crashes. These estimates are at odds with the crash risks implied by the existing research. We confirm our results with three additional empirical approaches—we compare trends in cell phone ownership and crashes across areas of contiguous economic activity over time, investigate whether differences in urban versus rural crash rates mirror identified gaps in urban-rural cellular ownership, and finally estimate the impact of legislation banning driver cell phone use on crash rates. None of the additional analyses produces evidence for a positive link between cellular use and vehicle crashes.

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This version: July 17, 2007

Abstract

The link between cell phone use while driving and crash risk has in recent years become an area of active research. The most notable of the over 125 studies has concluded that cell phones produce a four-fold increase in relative crash risk– comparable to that produced by illicit levels of alcohol. In response, policy makers in fourteen states have either partially or fully restricted driver cell phone use. We investigate the causal link between cellular usage and crash rates by exploiting a natural experiment induced by a popular feature of cell phone plans in recent years– the discontinuity in marginal pricing at 9 pm on weekdays when plans transition from "peak" to "off-peak" pricing. We first document a jump in call volume of about 20-30% at "peak" to "off-peak" switching times for two large samples of callers from 2000-2001 and 2005. Using a double difference estimator which uses the era prior to price switching as a control (as well as weekends as a second control), we find no evidence for a rise in crashes after 9 pm on weekdays from 2002-2005. The 95% CI of the estimates rules out any increase in all crashes larger than .9% and any increase larger than 2.4% for fatal crashes. These estimates are at odds with the crash risks implied by the existing research. We confirm our results with three additional empirical approaches– we compare trends in cell phone ownership and crashes across areas of contiguous economic activity over time, investigate whether differences in urban versus rural crash rates mirror identified gaps in urban-rural cellular ownership, and finally estimate the impact of legislation banning driver cell phone use on crash rates. None of the additional analyses produces evidence for a positive link between cellular use and vehicle crashes.

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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 sentiment 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 identified clear links between cellular usage and crash risk.

Experimental and epidemiological studies have even equated 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 (NHTSA), then Figure 1 illustrates a puzzle (2006). Cell phone ownership 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 (CTIA 2006)– yet aggregate crash rates have fallen substantially over this period.[2]

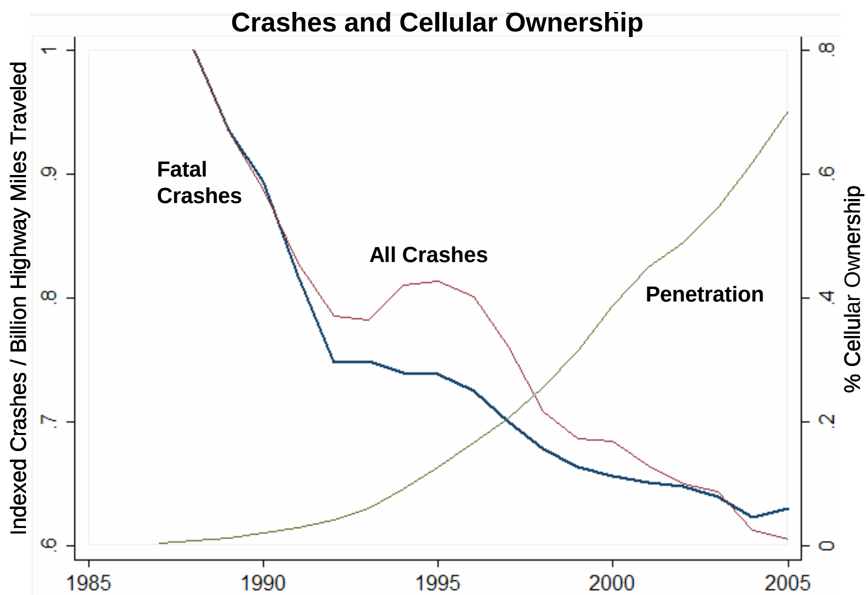

Figure 1, Cellular Ownership and
Indexed Crashes/ Highway VMT for 1988 -2005

[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. Fatal crashes were from collected from the Fatality Analysis Reporting System (FARS) database, while all crash data was estimated from the General Estimates System (GES). Penetration data was collected from CTIA wireless surveys. Volume data is from the Federal Highway Administration.

In this paper, we exploit a natural experiment in order to cleanly identify the impact of cell phone use on both fatal and non-fatal crashes. Our most credible research design suggests that current cell phone use *does not* result in a measurable increase in vehicular crashes. We are able to reject, with a 95% confidence interval, any rise in fatal crashes larger than 2.4% and any rise for all crashes of .9%. It should be noted, however, that our result is not inconsistent with the claim that cell phones are a source of attentional distraction. One possible explanation 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). We explore other rationale for the absence of a link between current cellular use and crashes in the discussion below.

Past attempts to link cell phone use and driver safety have relied on a variety of methodologies. These range from cross-sectional surveys of large groups of drivers, simulations in the lab, inspection of crash reports, observational analyses using in-car cameras or confederate observers, longitudinal studies of small samples of drivers, as well as correlative analyses of aggregate cell ownership and crash records. This research has located the percentage of crashes attributable to cell phone use anywhere from 0 to 33% of the 6 million crashes and 45,000 fatal crashes which occur each year which is equivalent to as many as 4 crashes a minute for each minute of the day.[3] However, despite the value of these studies, because of the difficulties associated with causal inferences in this setting, much of the existing research is open to scrutiny due to either questionable econometric identification or doubtful external validity.

The need to accurately gauge the detrimental influence of cell phones resonates far 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. According to the American Automotive Association, fourteen states already have such legislation on the books (AAA 2007).[4]

If the media coverage is to be believed, much of the political dialogue produced by legislative initiatives has centered around, and often even been prompted by, one or many of the published estimates. In a 1997 issue of the New England Journal of Medicine, Redelmeier and Tibshirani (hereafter "RT") published perhaps the most frequently cited estimate of the increase in crash risk due to cell phone use (Redelmeier and Tibshirani 1997; Hahn and Prieger 2006). Using an epidemiological design, they concluded that cell phones produced a 4.3 fold increase in the relative likelihood of a crash. This implies an 20%

[3] We discuss how we arrived at this characterization of the literature below.

[4] Three states have complete bans on hand-held cell phone use by drivers, while an additional 11 states have partial bans primarily targeting younger drivers. Several other states ban cell phone use by those driving school busses.

increase in annual crashes.[5] Given the strength of our research design, we believe that our paper may meaningfully add to the discourse regarding the efficacy of policies restricting driver cell phone usage.

We identify and size driver sensitivity to changes in cell phone use through a series of distinct estimation strategies. Our favored empirical approach combines a regression discontinuity design with a double difference estimator in order to assess the relative increase in crashes around the time of the day– 9pm on weekdays– when cell phone providers systematically transition from "peak" to "off-peak" pricing. Based on the assumption that a discontinuous rise in prices drives sharp increases in call volume – an assumption for which we provide evidence– we exploit the presence of pricing thresholds across weekdays and not weekends, as well as the recency of plans with the 9pm pricing threshold to assess the relative impact of additional call volume on crash rates. Figure 2 illustrates the distribution of cellular call volume across weekdays and weekends for a sample of callers in 2005. The plot depicts an approximately 24% relative rise in phone calls at the weekday 9pm threshold.[6]

Figure 3 conveys our basic result for fatal crashes from all states in 2005.[7] Crash rates do not appear to change across the 9pm threshold on weekdays relative to weekends during this period. Given the RT estimates of relative crash risk, the size of the observed discontinuity in call volume, and a conservative assumption of driver cell phone use, we would expect to see an 2 to 6% rise in crashes in the hour following the threshold.[8]

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

[6] 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.

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

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

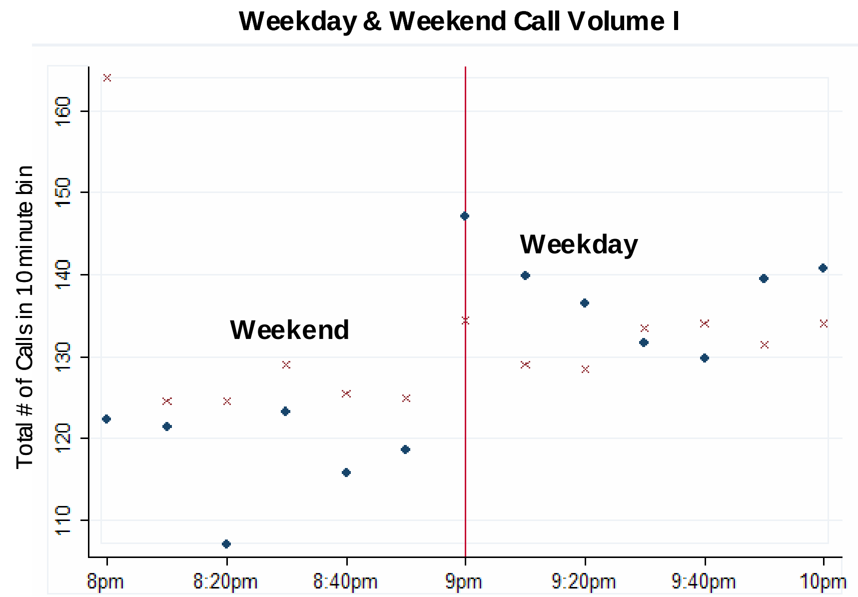

Figure 2, Outgoing Calls from 8pm – 10pm in 2005
(10 mn bins)

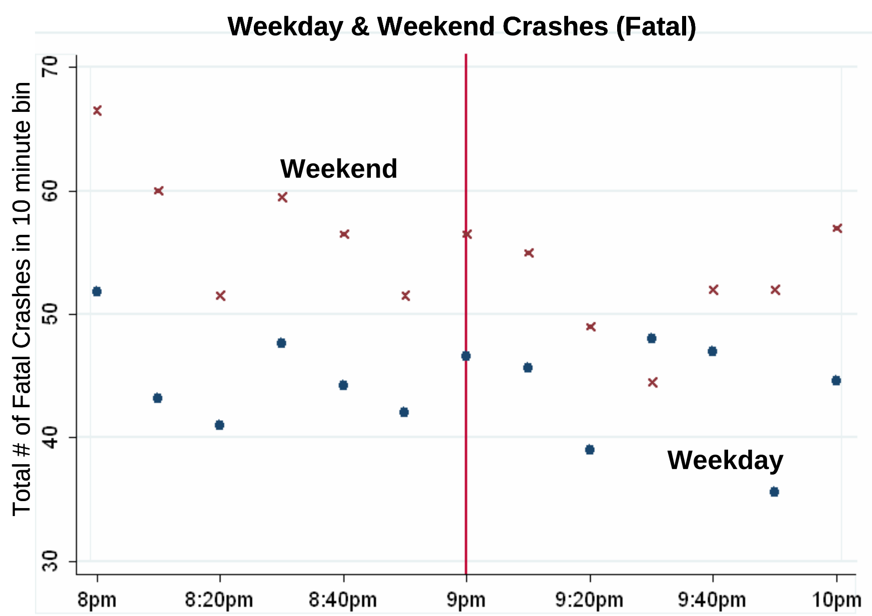

Figure 3, Fatal Crashes from 8pm – 10pm in 2005
(10 mn bins)

As a second, and arguably more reliable control, Figure 4 compares the rate of all crashes from weekday evenings during the "post" period characterized by high cellular plan conformity around the 9pm threshold (2002-2004) with a period preceding the introduction of tiered pricing plans (1990 - 1998) across the six states for which crash records are available.[9] Again, the plot offers no noticeable evidence for a rise in relative crash rates at 9pm during the post period.

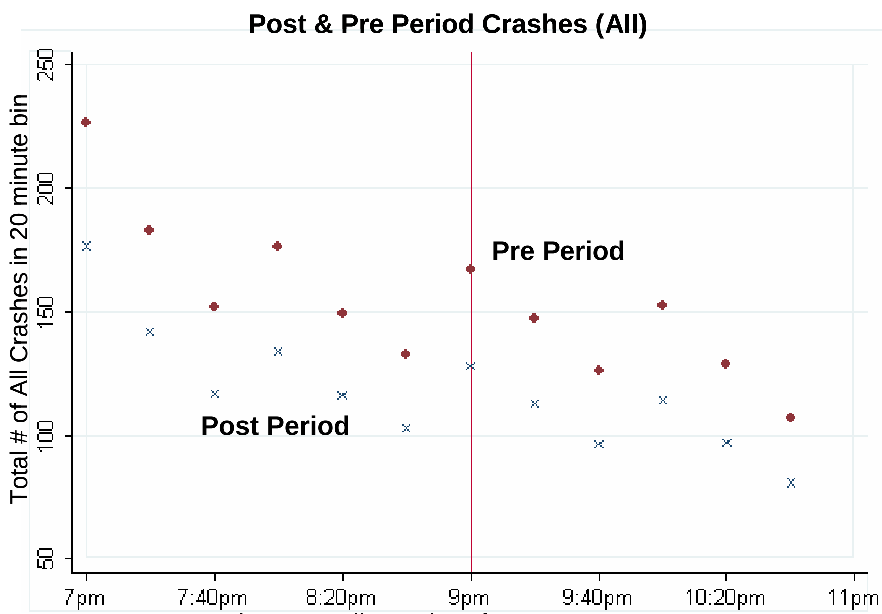

Figure 4, All Crashes from 7pm – 11pm
in 1990-1998 & 2002-2004 (20 mn bins)

As robustness checks, we present three additional empirical analyses. A first approach involves comparing 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.

Due to the absence of data on cell phone ownership more highly disaggregated than an EA, a second estimation exploits the additional variation provided by historical differences in the rate of cell phone 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 assess the change in crashes in largely urban areas over time from those of more rural controls. Finally, recognizing that some states have recently enacted complete bans on hand-held cellular usage we analyze the impact of this legislation on crash rates.

[9] The figure displays aggregated crash rates for CA, IL, MD, MO, NM, PA for 20 minute bins from 7pm to 11pm. However, given the exclusion of particular states in particular years due to data availability, the figure does not represent a balanced panel across state-years.

None of these strategies suggests a significantly positive relationship between cell phone use and vehicular crash rates.

This paper relies on an extensive dataset of fatal and non-fatal crashes, as well as data on provider pricing plans, cell phone subscriptions at the EA level (since 2001) and national level (since 1985), the quality of cell phone reception across time and across regions, and perhaps most novel, data on the actual distribution of calls from two samples of callers from 2000-2001 and 2005. The crash data includes a census of all crash records for California, Florida, Illinois, Maryland, Missouri, New Mexico and Pennsylvania, for most of the years from 1990 - 2004, as well as the census of fatalities from all states from 1987 – 2005. We investigate both fatal and all crashes due to the suspicion that these crash types may be differentially sensitive to use of cellular phones, as well as the increased statistical power provided by the far higher frequency of non-fatal crashes.

Beyond contributing to the literature on the danger of cellular use, our paper is very much in the spirit of studies which use natural experiments to assess the effect of driver behavior on crash risk (Levitt & Porter 2001a; Levitt & Porter 2001b). This study can also be linked to the literature examing the theory of compensating behavior with respect to driving risk factors (Cohen and Einav 2003; Peltzman 1975).

Though our paper represents a departure from past studies, it is not without its limitations. Our strongest estimation strategy– that of the pricing regression discontinuity– is handicapped by considerable heaping in the time at which crashes are reported. Additionally, the analysis yields only a local treatment effect of cell phone use by drivers at the 9pm pricing threshold. This complicates efforts to translate estimates to an aggregate welfare effect. While the panel analysis across region-years is not a local estimation, it does suffer from the absence of ownership data for the critical period of pickup, as well as possible bias through omitted confounds. Further, the comparison between urban and rural areas involves imputed rather than computed differences in ownership. Finally, the analysis of legislative bans is impaired by a lack of power due to the few regions which have enacted bans, as well as to the recency of such bans. We address each of these issues in the course of our discussions below.

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 main and alternative empirical approaches and presents the results of the analysis. In Section IV, we translate our estimates into annual crash rates and discuss our results in the context of existing research. Finally, Section V 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 an equally rapid 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, explicit analyses of crash risk due to cellular use generally fall into one of five major methodological categories: (1) Experimental studies that focus on subject behavior in simulated, or highly controlled, driving conditions, (2) naturalistic studies of drivers on the actual road, (3) studies which inspect police annotations of crash records, (4) correlative analyses of aggregate crash records and cell phone ownership, and (5) longitudinal analyses of individual level phone and crash records. Beyond estimating the impact of phone use on crashes, other researchers have attempted to accurately measure the extent to which driver cell phone use occurs. Several excellent surveys of these literatures exist (Hahn and Prieger 2006; McCartt et. al. 2006; Hahn and Dudley 2002; Lissy et. al. 2000; Hahn and Tetlock and Burnett 2000).

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 subjects instructed to use cell phones while driving are 3-4 times more "impaired" than 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 effect for younger drivers (McCartt et. al. 2006). Importantly, these studies find no differences between hand-held and hands-free devices.[10]

A benefit of simulations is that they are able to assess relative levels of impairment across various forms of distractions, as well as to illuminate the specific capacities that are likely to be impaired. Indeed, given the sophistication of simulation environments, as well as the precision of the measurements, studies in the lab may be best positioned to precisely size the levels of impairment attributable to any form of distraction including cellular use. A shortcoming of such studies, however, is the questionable external validity– i.e. it is unclear whether cell phone use in simulations is analogous to use in environments where driver well-being, or, even survival, is at stake. It is plausible, for instance, that drivers

[10] Not all of the experimental evidence points to increased crash risk. Some experimental studies have documented compensatory responses to cell phone use such as drivers slowing down or allowing more distance between vehicles. Subjects in other studies have been shown to adapt to distractions through repeated trials.

in the field compensate for cellular use with more cautious driving. Finally, experimental studies tend to produce estimates of relative, but not absolute, crash risk.

A second set of naturalistic studies employ visual and audio recording devices to monitor driver behavior in ecological conditions. In an example of one such study, "The 100-Car Naturalistic Study," researchers equipped 100 vehicles with five cameras and a series of sensors distributed throughout the vehicle and tracked 241 primary and secondary drivers for over 1 year (NHTSA 2006). Having collected 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 were 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 caused drivers to be 1.3 times more likely to have a crash. The majority of near-crashes were associated with cellular use. Much like the experimental studies, naturalistic approaches highlight the specific causes of driver impairment as well as their relative dangers. It is unclear, however, given the nature of the monitoring involved, 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.[11] 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 drawbacks including source unreliability (NHTSA 1997), and the increasing salience of cell phones as a reported determinant over time (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 mentioning 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.

One strategy through which to generate absolute estimates of crash risk is by comparing 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, trend analyses which aspire to identify the possibly modest effect

[11] 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).

of cellular use are not considered persuasive (Min and Redelmeier 1998). An additional complication the aggregate approach is that often there is not very much variation in rates of cell phone ownership to exploit, and appropriately disaggregated data on ownership is difficult to obtain.

A final class of studies tracks both phone use and driving behavior for a small number of drivers over a particular period (Dreyer and Loughlin and Rothman 1999; Violanti 1998; Redelmeier and Tibshirani 1997; Violanti and Marshall 1996). The most widely cited of these is the analysis by Redelmeier and Tibshirani (RT) (1997). In the paper, the authors inspect the 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 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 such 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 are able to control for driver specific variation in crash likelihood. They then used a conditional logit regression to infer the relative risk of a crash due to cell phone use. The paper concludes that the use of cell phones increased 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 a major drawback with the RT result is that the study relies on a very unrepresentative sample (2006). Any simple correlation of crashes and phone use for only those drivers recently involved in a crash is confounded by selection. If drivers with greater risk of crashing while using cell phones are overrepresented in the RT sample, the relative risk estimate would then be an upper bound for the broader population of drivers. An additional concern with the RT study is that while the case crossover method does control for fixed driver characteristics, it does not control for time varying unobservables. For instance, bored or stressed drivers may be likely, to both, use cell phones and drive poorly due to mental distraction. In this case, the observed relative risk could simply reflect correlation of cell phone usage and crashes which are both derived from 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 paper used a nearly identical methodology to investigate the effects of cell phone exposure for drivers in Perth, Western Australia (McEvoy et. al. 2005). The authors find 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.[12]

In summary, researchers have adopted a number of methodological approaches to estimate the influence of cellular phones on crashes. Table 1 summarizes the range of effect sizes estimated for analyses from each methodological class. In order to meaningfully compare estimates of increased relative risk with those of increased absolute risk, it is necessary to translate the former figures into predicted changes in aggregate crash counts. Such a translation critically relies on the accuracy of assumptions regarding the frequency with which drivers use their cell phones.

Table 1

EFFECT OF CELLULAR USE ON CRASH RISK: COMPARISON BY METHODOLOGY

	RELATIVE RISK	EXTRAPOLATED ABSOLUTE RISK
9pm Price Discontinuity	X	0 to 1.4% increase in crashes
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

A number of studies attempt to estimate the frequency of such use. These include surveys which query drivers regarding patterns of usage, as well as observational studies where experimenters stationed at an intersection, for example, record behavior of ongoing traffic.[13] An example of the latter, the 2006 National Occupant Protection Use Survey (NOPUS) observes some 126,000 vehicles at 1,878 probabilistically sampled roadways and finds that 6% of drivers were using handheld cell phones at any point during the day and that an additional 4% were on hands-free phones (NHTSA 2006b). Earlier surveys 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). The NOPUS survey also

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

[13] See McCartt et al. 2006 for a thorough review of the literature.

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 involves an assessment of driver behavior in 40,000 photographs taken of vehicles 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) from the afternoon (i.e. from 12pm to 4pm).

Given these estimates of driver cell phone use, Table 1 presents extrapolations of the increased absolute crash risk implied by studies of relative crash risk. Our 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, abstracted from selection effects, 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%.[14]

3 Empirical Analysis

This section describes the data, the experimental design for each estimation strategy, and presents the empirical findings. Four sets of empirical results are provided. First, we provide evidence for the sensitivity of call volumes to discontinuities in marginal cellular call prices, and then measure how drivers respond to the time thresholds which mark such sharp price changes. Specifically, we document that, since 2002, most cellular users subscribe to plans with a 9 pm weekday pricing threshold after which time usage carries a near zero marginal cost. We then provide evidence for a jump in weekday, but not weekend, call volume immediately after 9 pm. 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 after and before 9pm on weekdays since 2002, to the same period during the era prior to the introduction of pre-paid plans (prior to 1998).

As additional evidence, we present results of yearly regressions of both fatal and all crashes on cell phone ownership across EAs over the period from 1987 to 2005. We then argue that rural areas within an EA lag urban areas in cellular ownership, and we investigate whether urban-rural differentials in crash rates mirror the urban-rural gap in cellular ownership. Finally, we attempt to estimate the effect of recent legislative bans on

[14] These calculations do not take into account possible heterogeneity of cell phone use across drivers. If only very risky drivers use cell phones, for example, and the use of cell phones is merely a substitute of one form of distraction from another, then our extrapolations may represent upper bounds of the predicted effect ranges.

handheld phones.

3.1 Summary of Data

Several data sources were used in this analysis (they are summarized in Table A1 in the Appendix). Each of the empirical approaches in this paper relies on crash count data, as well as data on changes in cell phone ownership. Data for the entire universe of fatal crash records from 1987 to 2005 for each of the 50 states was attained from the Fatality Analysis Reporting System (FARS). Any vehicular crash which results in the death of a motorist or non-motorist within 30 days of the crash is captured in the FARS database. Data for the universe of total crashes for varying periods from 1990 to 2005 was acquired for California, Florida, Illinois, Maryland, Missouri, New Mexico, and Pennsylvania through the State Data System (SDS). SDS and FARS are administered by the NHTSA which collects crash 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 crashes.

Figure 1 depicts the rate of fatal and all crashes, indexed to highway traffic volume, for each year from 1988 to 2005. Data on all crashes in this figure is based on nationwide sample conducted by the General Estimates Survey (GES) of the NHTSA. The plot indicates a decrease in fatal and all crashes over the last fifteen years, with a slight rise in the mid 1990s. The increasing prevalence and usage of safety devices as well as the decline in driver alcohol use is likely to have contributed to the drop in fatal and non-fatal crashes over this period (NHTSA 2005). The mild rise in the mid 1990s can be at least partially attributed to relaxation in nationwide speeding regulations. In recent years, there have been about 40,000 fatal crashes a year, and approximately 6 million total crashes each year nationwide.

An important unit of analysis in this paper is at the level of the Economic Area. 172 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 EA 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." In 2000, the BEA uniquely mapped counties to an Economic Area. We use these mappings in order 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.

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 increased exponentially over this period. By 2005, 2 of every 3 residents in a typical state owned a cell phone despite only 1 of 3 owning a cell phone just six years earlier.

Data on annual highway traffic volume for all states from 1989 to 2005 was obtained from the Federal Highway Traffic Administration. Traffic volume data was collected 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.

The central empirical strategy in this paper is based on the presumption 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, complete logs of cell phone activity for approximately 65 students and faculty over the course of 2005 were obtained from the Reality Mining Project (RMP) at the MIT Media Lab (Eagle 2006). As part of a broader project examining the evolution of social networks and the transmission of information, the RMP embedded surveillance technology in the cellular phones of each subject in the sample. Approximately 80,000 outgoing calls were logged over the course of the surveillance period. The benefit of electronic logs is that call timings were 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 9,406 cell phone users from U.S. households in 2000 and 2001 (TNS 2001).[15] The latter data was harvested from cellular phone bills voluntarily submitted from households that had been randomly selected to participate in an earlier, broader survey of telecommunications behavior and attitudes. The broader survey was administered by TNS Telecom, a firm which specializes in Telecom data collection. While this data is likely to be representative, it is hourly data, and is from a period characterized by non-uniform plan thresholds ranging from 6 to 10pm, or no threshold at all. However, the data usefully provides "peak" and "off-peak" designations for each call, and allows for the analysis of individual call patterns.

Data on historical cellular pricing plans was obtained through surveys of cell phone provider websites conducted monthly from 2002 to 2005 by Econ One Research.[16] The surveys detail 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. While the survey targeted New York City, we assume that the pricing details of national calling plans available to New York subscribers were similar to those available to other users nationwide. Market shares for each provider were collected from

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

[16] Data gathered from the *Econ One Wireless Survey.* Survey is available at www.econone.com.

S&P Industry Analysis Reports (S&P 2001-2006).

An alternative empirical approach in this paper exploits the differential cellular coverage in rural as compared to urban regions. Classifications of the urban/rural character of counties was collected from the U.S. Department of Agriculture. Finally, for the analysis of legislative bans, descriptions of state legislation was gathered from the American Automative Association as well as the National Conference of State Legislatures (AAA 2007; NCSL 2005).

3.2 Vehicle Crashes & Price Discontinuities

In our first empirical approach, we rely on a regression discontinuity (RD), as well as a series of counterfactuals to identify the change in crashes after a sharp and exogenous increase in the usage of cell phones. If cell phone use does cause crashes, then an exogenous rise in such use should be associated with a corresponding rise in crash rates. Discontinuities in the marginal price of a cell phone call represent one source of exogenous variation in usage. Accordingly, we first outline our estimation strategy and identifying assumptions. Next we document the existence of a systematic, and transparent discontinuity in marginal call prices during weekday evenings at 9pm. We then provide evidence that this price discontinuity produces a discontinuous rise in cell phone usage. Finally, we estimate the effect that increased usage has on the frequency of fatal and non-fatal crashes.

3.2.1 Estimation Strategy and Identifying Assumptions

Let $Crash_{r,p,wk,h,w}$ refer to the number of reported crashes in region r, hour h,minute window w either on weekdays or weekends as signalled by wk, in either the "post" period, p, characterized by high cell phone ownership and high plan conformity around a threshold, or a "pre" 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 $\operatorname{Re}pBias_{r,p,wk,h,w}$, 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:

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

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 of drivers on the road (R) 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 if we assume that the distribution of unobserved driver risk is the same immediately before the 9pm pricing threshold as it is immediately after the pricing threshold:

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

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 are able to rewrite the above equation (2) as:

$$\begin{aligned} (3) \quad Crash_{r,p,wk,h,w} = \alpha + \theta_1 Traffic_{r,p,wk,h,w} + \theta_2 \operatorname{Re} pBias_{r,p,wk,h,w} \\ + \theta_3 X_{r,p,wk,h} + \lambda Cell_{r,p,wk,h,w} + g(R) + v_{r,p,wk,h,w} \end{aligned}$$

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:

$$\begin{aligned} (4) \quad D_{r,1,1,h} = Crash_{r,1,1,h,w} - Crash_{r,1,1,h,w'} = \theta_1' \Delta Traffic_{r,1,1,h} \\ + \theta_3' \Delta \operatorname{Re} pBias_{r,1,1,h} + \lambda' \Delta Cell_{r,1,1,h} + v'_{r,1,1,h} \end{aligned}$$

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 contrasting the difference (D) in crashes around the time threshold during the "post" period from a "pre" period prior to the threshold era:

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

If we assume that the difference in traffic as well as the difference in the reporting bias around the threshold in the "pre" and "post" periods compared to weekends 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.

3.2.2 Price Discontinuities and Cell Phone Call Volumes

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.[17] Marginal fees for excess usage commonly vary from \$.35 to \$.45 per minute.

Table 2 documents the evening thresholds at which major providers distinguished between peak and off-peak usage for national calling plans offered to New York subscribers from 2002 to 2005.[18] The table also describes the estimated share of new users associated with each threshold in a given year. Unfortunately, calculating the share of users tied to a particular threshold is difficult because providers do not disclose plan level market shares and turnover rates.

[17] Actual plans often specify some large, but finite, limit for non-peak usage. Cingular for example, establishes usage limits even for non-peak periods that are marketed as allowing for "unlimited" usage. This limit is often 5,000 or 10,000 minutes.

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

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 cases 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 believe 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.

Table 2

PRICING PLAN THRESHOLDS FOR NYC, 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 plan listed by each provider on their website for New York City subscribers as recorded monthly in the EconOne Wireless Survey. 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, some providers in subsequent years began offering plans with earlier switching times of 7pm. However, we estimate that at least 75% of new subscribers in each year since 2002 had enrolled in 9pm plans. Assuming a 1-2 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 minute 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.[19] In another survey of 30,000 cell phone users, only 7% admitted to exceeding their monthly allotment of minutes. Such users were subject to "overage" fees which, on average, amounted to 50-60% of their usual bill.[20] It seems then that two-part tariff pricing, coupled with the tiered allotment of peak and off-peak minutes, produces a salient discontinuity in price for many users during weekday evenings.

We explicitly test for the correspondence between call price and usage at the plan switching threshold by using two rare datasets of actual calls.[21] 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.[22] 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 larger 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%

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

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

[21] Data on call volume is very 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.

[22] Not all of the subjects remained in the sample for the course of the calendar year. Additionally, many of the subjects surrendered their phones, or left the sample, over the summer. Consequently, call volumes are much lower in summer months than during the rest of the year.

and seems to persist at least until midnight. Table 3 explicitly enumerates the change in call 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 exhibit 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, much larger, 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).[23] 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.[24] While the data includes callers whose plans feature switching thresholds from 6pm to 10pm, or, in many cases, ambiguous or single tiered pricing, we are able to infer the time of the switching threshold, if it exists, given that individual calls are labeled as being "peak" or "off-peak".

From the 9907 callers in the original sample, we retain a subsample of callers that satisfy each of the following conditions: (1) Callers are in the sample for at least 30 consecutive days (2) Callers log a minimum of at least 30 calls (incoming and outgoing) (3) Callers have no calls that are ambiguously tagged (i.e. each call is tagged as either "peak" or "off-peak" rather than "unclear") (4) Callers have a mix of peak and off-peak calls which allows us to infer the switching hour of the caller's plan.[25] Of the remaining 500 callers in this subsample, most have plans with switching thresholds at either 7 pm (139), 8 pm (166) or 9 pm (102).

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 at each plan's respective threshold hour is in contrast to the general decline in calls for all other hours across all the plans over the depicted period.[26]

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

[24] The "ReQuest Consumer Survey" is a quarterly survey of about 30,000 households on consumer behavior and attitudes related to telecommunications. It is administered by TNS Telecom and is primarily marketed to telecom clients to help them better understand consumer attitudes and product preferences. 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.

[25] We impute the switching hour by computing the change in the average hourly 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 but jumps of just under 1 unit. Given 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.

[26] 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.

Weekday Call Volume by Plan Switching Time

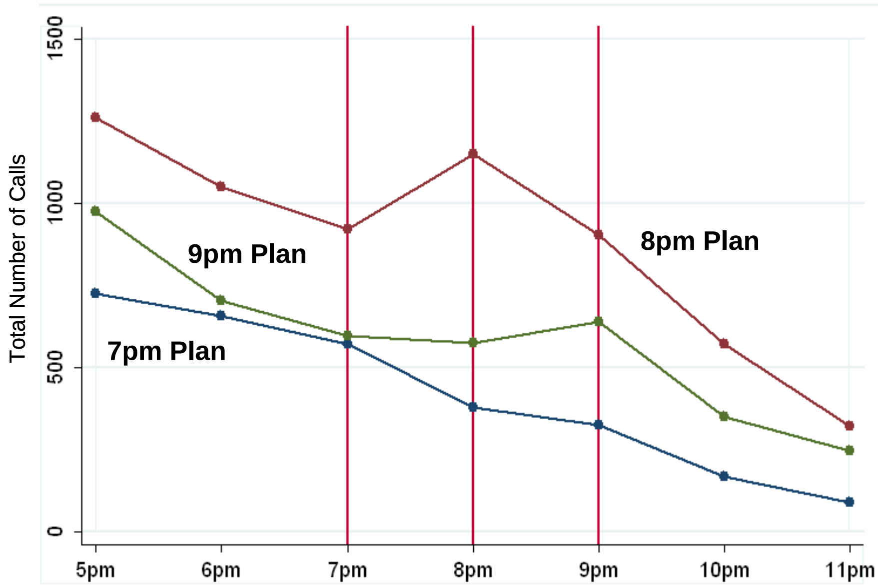

Figure 5, Outgoing Hourly Weekday Calls for 7, 8 and 9 pm Plans in 2000 - 2001

A panel regression at the level of the individual caller, sizes and confirms this sensitivity of callers to their respective 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 made in hour h under a calling plan which switches to "off-peak" pricing at hour s by caller i. Fixed effects are included to control for hour specific variation, as well as for each individual caller. $Switch_s$ refers to the hour when a caller transitions from "peak" to "off-peak" pricing, while $AfterSwitch_{h,s,i}$ denotes hours after (but not inclusive of) the switching threshold. The regression is estimated for all weekday outgoing and incoming calls made from 5pm to 12am for those callers included in the sample.

Table 4 reports that outgoing calls increase by 33.1% at the switching threshold while incoming calls sustain a smaller, and insignificant, increase of 9.7%. To address the concern that the rise in calls at the switching threshold may be counteracted by a fall in duration, the final column of the table shows that duration is unchanged 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	1.331***	1.097	-0.095
	(0.102)	(0.102)	(0.429)
After Switching Threshold	1.075	0.957	-0.102
	(0.134)	(0.158)	(0.551)
Individual Fixed Effects	X	X	X
Hour Fixed Effects	X	X	X
N	N = 3256	N = 2736	N = 2270

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. Coefficients are presented as incident rate ratios. 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. Standard errors are robust and clustered by the individual caller.

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

3.2.3 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 price discontinuity on weekdays during the period characterized by both high cell phone ownership and high price threshold conformity with such behavior on weekends as well as a 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 the FARS database of fatal crashes. Once a fatality linked to a vehicular crash occurs in a given state, it is documented by a variety of state agencies, and is then translated onto standardized paperwork and inputted into the FARS database by a trained analyst at a federally sponsored state agency. Consequently, the actual crash statistics themselves are derived from one of several existing state files such as police crash reports, death certificates, or hospital records. 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.

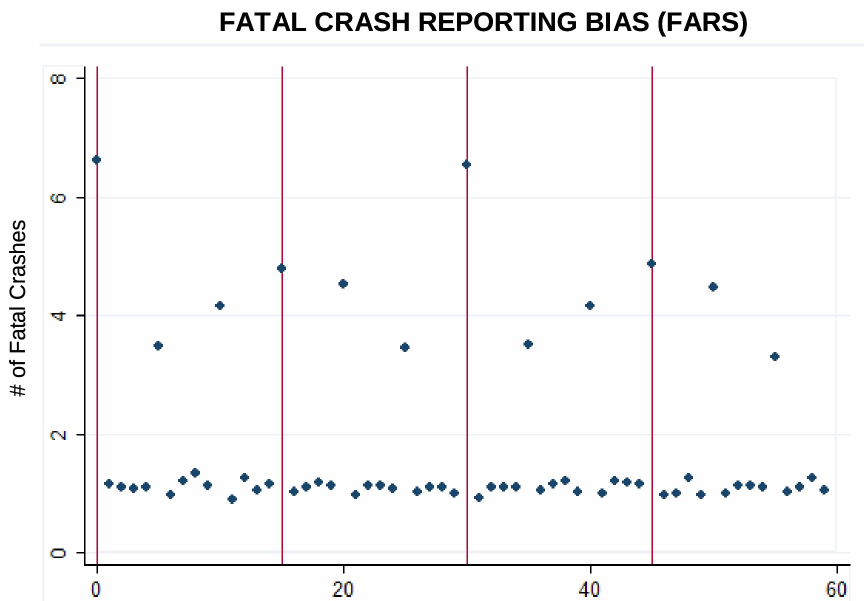

Figure 6, Average FARS Crashes by Minute in 2005

A closer 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. We control flexibly for these biases in the regression analysis below through three primary strategies. The first strategy entails choosing a unit of analysis which is aggregated across multiple minute bins. For example, if all crashes were reported at the nearest five minute interval, then the use of fifteen minute bins should be bias free. Consequently we report results for bins of 5, 15 and 30 minutes. A second strategy is implicit in the double differencing approach. Assuming no systematic change of biases across time, then, as the model above outlined, the double difference across the pre and post period should mitigate the impact of any reporting bias. Finally we use fixed effects for each minute bin interval which help to control for equivalent biases across control and treatment periods.

Fatal crashes. We turn first to the distribution of fatal crashes around the pricing plan threshold. Again, our estimation draws on variation in such crashes across all EAs over a 19 year period from 1987 to 2005.

The natural experiment produced by the price discontinuity lends itself to a number of control comparisons. As outlined above, 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 the "pre" period before 1998 when pre-paid plans were first introduced. As a robustness check, we examine this same double difference (DD) for weekends when the pricing thresholds are not in effect.

Figure 7 illustrates this iterative difference 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 circumvent the considerable on-hour bias in reporting. Treating each year as an independent draw allows us to calculate standard errors, which are displayed parenthetically. Surprisingly, the figure indicates a rise in crashes across the 9pm threshold in all quadrants except that within which the pricing discontinuity actually occurs. None of the differences, however, are significant given the calculated standard errors.

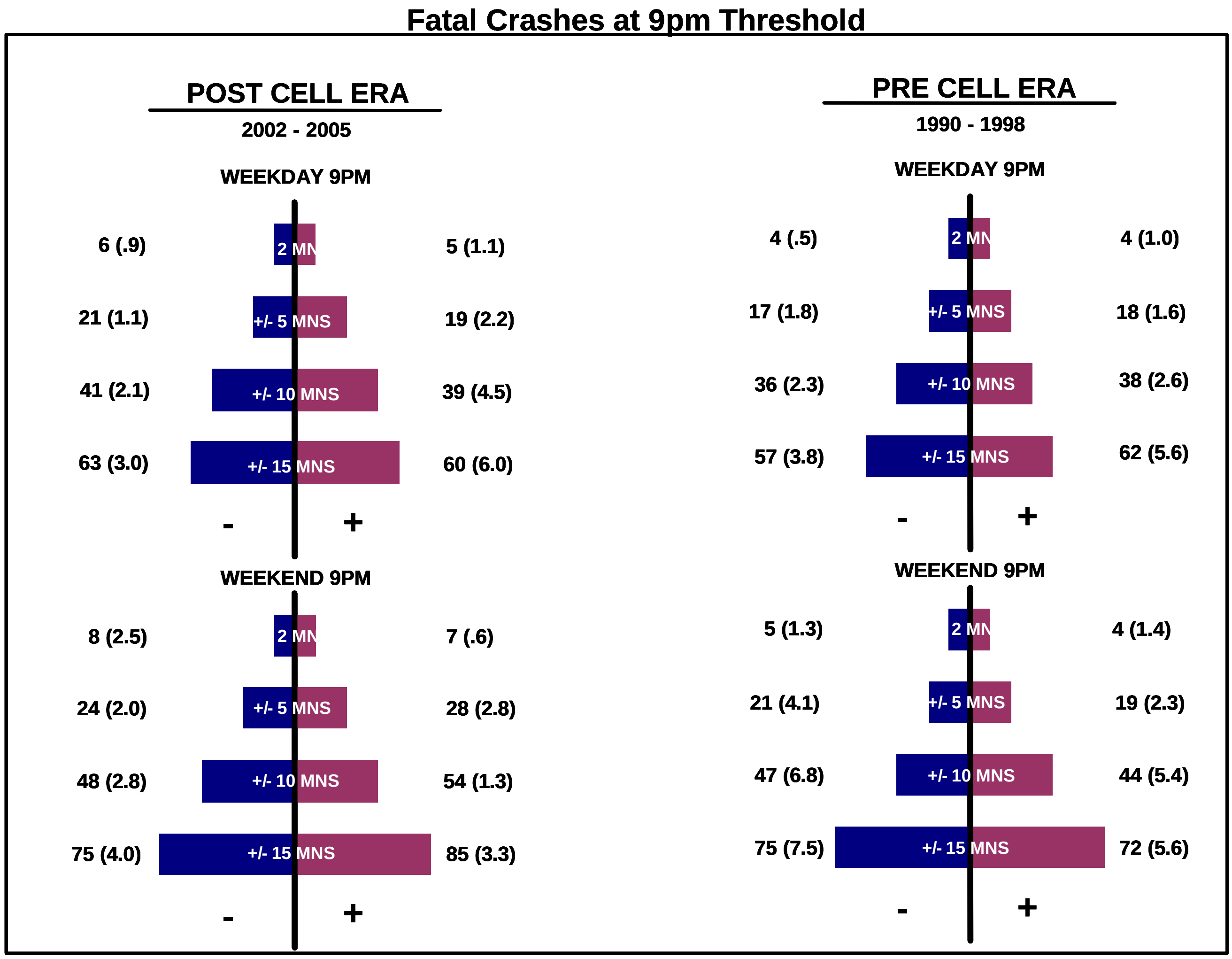

Figure 7, Average Fatal Crashes across 9pm for Pre & Post Era

Figure 3 depicts time trends in 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 do not display any discernible break in weekday crashes around the 9pm threshold.

We formally estimate the relative change in crashes around 9pm in the period of interest with the following model:

(7)

$$crash_{y,m,d,b} = \alpha + \beta(Post * After\ 9pm)_{y,b} + \gamma_1 After\ 9pm_b + \gamma_2 Post_y + \eta_y + \eta_m + \eta_d + \eta_b + \varepsilon_{y,m,d,b}$$

where $crash_{y,m,d,b}$ denotes the sum of crashes in year y, month m, day of the week d, and in minute bin b. $Post_y$ 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, our experimental comparison in this regression are differentials around the threshold in the "post" period from 2002 to 2005 compared to the same differential in a control period preceding 1998. We estimate the model for the years from 1987 to 1998 as well as from 2002 to 2005. Further, we restrict the regression to hours between 7 and 11pm.[27] Fixed effects for each minute bin control for non-linear movement in crashes across the estimation period as well as help control for reporting biases.[28] The model also includes fixed effects to control for year, month and day of the week specific variation.

The regression is estimated with a Poisson distribution.[29] Table 6 presents the results. The first three columns report marginally negative but insignificant point estimates for the interaction term of interest for the weekday 6pm to 10pm estimation. The fourth column extends the treatment window to two hours after 9pm. The results indicate no evidence for a positive increase in crashes, and our favored specification, reported in the first column, implies a upper bound of 2.4%. As a robustness check, the final column estimates the double difference using 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 positive weekday differentials. Overall, the results provide no evidence for a positive relative change in fatal crash rates across the threshold.

[27] Alternatively, we estimate the model for both shorter and longer time bands around 9pm. While the standard errors are modestly sensitive to the choice of estimation window, the basic results are substantively unchanged.

[28] We experimented with other, more granular, controls for the reporting bias. The choice of such controls does not seem to qualitatively alter the results.

[29] 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).

Table 5

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

	DEPENDENT VARIABLE - FATAL CRASHES PER MINUTE BIN				
	WEEKDAY				WEEKEND
	6 PM - 10PM			6 PM - 11PM	6 PM - 10PM
	5 MN (1)	15 MN (2)	30 MN (3)	5MN (4)	5MN (5)
Post x After 9pm	-0.0092 (0.017)	-0.0116 (0.022)	-0.0107 (0.024)	-0.0419** (0.018)	0.0267 (0.030)
After 9pm	0.0308 (0.033)	0.0443* (0.023)	0.0604*** (0.017)	-0.0284 (0.037)	-0.0021 (0.082)
Post	-0.0317* (0.019)	-0.0729** (0.035)	-0.0897** (0.037)	-0.0372* (0.019)	-0.0835*** (0.030)
Minute Bin Fixed Effects	x	x	x	x	x
Yr, DOW, Month Fixed Effects	x	x	x	x	x
N	N = 29846	N = 11364	N = 6093	N = 36914	N = 13374

Notes: The table presents poisson regression results for the pre (1990-1998) and post (2002-2004) difference in aggregate fatal crashes for each minute bin on a particular day of the week-month-year. The first three columns present the basic specification for weekdays from 6pm to 10pm for 5, 10 and 15 minute bins while column (4) estimates the model for 6pm to 11pm, and column (5) provides estimates for the weekend. The After 9pm dummy variable is coded as 1 for any crash occuring after nine 9pm in the estimation period. Constants and fixed effects are not reported. All estimations use robust standard errors and are clustered by day of the week X year.

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

All Crashes. 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 of all crashes is limited to seven states in the period ranging from 1990 to 2004.[30] Figure 4 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 in the period of interest through a panel regression. The estimated model is identical to the equation

[30] 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 Mexico and Maryland. Illinois reports the time of crash only beginning in 1996, and Illinois and Pennsylvania did not make crash records available to the SDS for 2002. There are a total of 55 EAs across the seven states of which 2 were eliminated because they spanned state borders and problematized county-EA matching. The variability in data availability is understandable given that the SDS must ultimately rely on each state to provide its own crash records.

outline above, but with the crashes no longer restricted to just fatal crashes, and the dependent variable $\ln(crash_{y,m,d,b})$. The model is estimated with an OLS regression for the period from 7pm to 11pm for the years from 1990 to 1998 and 2002 to 2004.

Table 6 reports the results of the estimation. The table points to a negative double difference $(Post * After\ 9pm)_{y,b}$ across each of the weekday specifications. The results are similar to the above estimates for fatal crashes– the weekday double difference estimates for the interaction are non-positive across specifications, and the weekend estimate offers no evidence that an unobserved confound is masking a positive effect. Our favored specification, presented in the first column, suggests an upper bound for the change in all crashes of .9%.

Table 6

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

	DEPENDENT VARIABLE - LN(ALL CRASHES PER MINUTE BIN)				
	WEEKDAY				WEEKEND
	6 PM - 10PM			6 PM - 11PM	6 PM - 10PM
	5 MN (1)	15 MN (2)	30 MN (3)	5 MN (4)	5 MN (5)
Post x After 9pm	-0.0244 (0.017)	-0.023 (0.016)	-0.0227 (0.016)	-0.0338 (0.028)	0.0235 (0.021)
After 9pm	-1.519*** (0.019)	-0.897*** (0.014)	-0.571*** (0.015)	-0.412*** (0.018)	-0.0736 (0.049)
Post	-0.164*** (0.024)	-0.175*** (0.023)	0.345*** (0.023)	-1.916*** (0.028)	-2.015*** (0.022)
Minute Bin Fixed Effects	x	x	x	x	x
Yr, DOW, Month Fixed Effects	x	x	x	x	x
N	N = 34410	N = 11472	N = 5743	N = 43030	N = 13765

Notes: The table presents OLS regression results for the pre (1990-1998) and post (2002-2004) difference in ln(all crashes) data aggregated across the seven states for which data is available for each minute bin on a particular day of the week-month-year. The first three columns present the basic specification for weekdays from 6pm to 10pm for 5, 10 and 15 minute bins while column (4) estimates the model for 6pm to 11pm, and column (5) provides estimates for the weekend. after having eliminated crashes reported on each hour. The After 9pm dummy variable indicates any crash occuring after nine 9pm in the estimation period. Constants and fixed effects are not reported. All estimations use robust standard errors and are clustered by day of the week X year.

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

In summary, the 9pm pricing analysis provides upper bounds of the relative change in fatal crashes of about 2.4% and an upper bound of .9% for all crashes. This upper bound compares to the 4% increase that one would expect to see 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% discontinuity in call volume at the threshold implied by the MIT data. We comment on the implications of these estimates further in the discussion below.

While the pricing discontinuity provides a relatively clean research design, one 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. Moreover, the estimated behavioral response at 9pm is prompted by changes in cellular usage rather than changes in cell phone ownership. To address these concerns, we provide an additional set of analyses in the next section.

3.3 Additional Analyses

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. Each EA consists of one or more economic nodes - a metropolitan or micropolitan statistical area that serves as a regional economic center. The 172 EAs identified by the BEA range in population from 61,285 (North Platte, Nebraska) to 25 million (the EA spanning New York City, Northern New Jersey and Long Island). EAs represent a greater level of disaggregation than data at the state (or national) level, and, as such, are closer to the ideal for this type of analysis. We then exploit implied differences in cellular ownership in predominantly urban versus rural counties within each EA, as an even more precise test of the link between ownership and crashes. Finally, using a state-month panel design, we examine whether complete legislative bans on driver cell phone use in a number of states have had any impact on reducing the fatal crash rate.

3.3.1 Aggregate Trends in Crashes and Cell Phone 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 across 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 changes in cell phone usage per month, depicted in Figure A1, were additionally considered.

However, given that the rise in cell phone ownership across regions is sufficiently heterogeneous, we can exploit variation across regions as well as years to more accurately pin down the link between cellular ownership and crashes. Indeed, EAs are associated with considerable variation in ownership. Ownership rates ranged from 19 to 57% across the

EAs in 2001. By 2005, this range grew to 41 to 95% ownership. Accordingly, we estimate the following model with OLS:

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

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 which has an additional control, $Vol_{r,y}$, for 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 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% point increase in cell phone ownership in a representative EA after controlling for EA and year fixed effects. The next column includes more flexible controls which allow for EA specific time trends. Columns (3) and (4) repeat the exercise for all crashes for the six states for which data is available. The point estimates in these specifications fail to suggest a link between ownership and fatal crashes, and if anything, suggest a marginally negative relationship.

The final column includes a regression of fatal accidents controlling for state-year highway volume and provides an important robustness check of the results. This regressions is limited to fatal accidents at the state, rather than the EA, level.[31] Changes in traffic volume over time and region do not seem to alter the earlier results.

Given the favored specification for all crashes, in column (4), our estimation allows us to reject any effect size larger than -.0032 + 1.96*.0031 = .0029. That is, we can place the upper bound of the effect on the all crash rate of a 1% point increase in cellular ownership at .29% given a 95% confidence 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 used. An analogous calculation, using the regression result for fatal

[31] 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.

Table 7

TRENDS IN CELLULAR OWNERSHIP AND CRASHES ACROSS REGION-YEAR

	DEPENDENT VARIABLE - LN(CRASHES PER 100,000 POP)				
	Fatal Crashes (FARS)		All Crashes (SDS)		Fatal Crashes
	Economic Area		Economic Area		State
	(1)	(2)	(3)	(4)	(5)
Cell Phone Ownership	-0.0010	-0.0004	-0.0018	-0.0032	-0.1274
	(0.0013)	(0.0002)	(0.0024)	(0.0031)	(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 in a given year for a particular region from 1987 to 2005 for fatal crashes, and from 1990 to 2004 for all crashes. For the first two columns, crashes are confined to fatal crashes, while the next two columns report all crash data. The explanatory variable of interest is the rate of cell phone ownership (i.e. cell phone subscribers / population) for the corresponding year and region. Constants are excluded. All estimations use robust standard errors and are clustered by EA.

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

crashes in column (2), suggests that the introduction of cell phones did not cause any increase in fatal crashes as compared to the counterfactual.[32]

There are multiple 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 that there are unobserved variables which are correlated with the growth in cell phone ownership across regions and time. The likelihood for this bias in results is more pronounced given the lack of EA level ownership data before 2001. A final possibility is that our test lacks power to detect the size of the true effect.

The lack of precision in our all crash estimates can partially be attributed to the high

[32] 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. For example, 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 effect size bounds might be even lower.

level of aggregation of our unit of analysis. 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 Economic Area. Given systematic historical differences in ownership across rural and urban areas, one strategy through which to engage this variation, is to infer county specific cellular ownership through the rural/urban character of each county. We turn to this analysis next.

3.3.2 Crashes & Urban/Rural Variation in Coverage

One drawback of the region-year regressions is that cellular ownership and crashes are compared at a high level of aggregation. This aggregation introduces imprecision in the estimated correlation between the two trends. As an alternative, more precise, estimation strategy, we exploit the heterogeneity in the spread of cellular coverage in urban as compared to rural areas. We find no additional evidence for a link between vehicle crashes and cell phone ownership.

Urban/Rural Coverage Gap. Policy makers have long recognized the existence of an urban-rural gap in telecommunications infrastructure. The Federal Communications Commission (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] Based on their analysis, the FCC has consistently concluded that rural consumers have far less choice in providers– and therefore inferior coverage– than their urban counterparts.

There have been attempts to redress this urban-rural imbalance. Most prominent amongst such efforts is the Universal Service Fund (USF) which levies a tax on all interstate and international telecommunications providers to fund telecommunications infrastructure in rural areas. Between 2000 and 2005, the USF increased spending from $1.8 billion to $3.8 billion on programs to subsidize capital and operating costs for telecommunications service provision in rural areas. Much of that increase was earmarked to rural wireless providers. Despite these gains, rural areas still tend to be characterized by less provider choice, more dead zones and worse service quality. Such factors cause– or perhaps reflect– the lower ownership levels 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.[34]

[33] These classification schemes included "Cellular Market Areas" (i.e. "Metropolitan Statistical Areas" vs "Rural Service Areas"), population density, and "Economic Area nodal" versus "Economic Area non-nodal" counties. The FCC reports clearly 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 some of the concerns of rural consumers and the factors

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 the 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.[35] However, we are able to confirm the suggested evidence 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. Accordingly, 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.[36] Appendix Table A2 enumerates the nine categories of counties on the urban-rural continuum and also displays the distribution of counties and population across these categories in the year 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 10 presents an EA level scatter plot of cellular ownership against the fraction of the EA population that resides in metropolitan counties (corresponding to codes 1 to 3) in 2001– the first year for which EA level ownership data is available. The figure illustrates that metropolitan EAs tend to have higher levels of cellular ownership.

determining choice of cell tower location in rural markets (Davidson, USA Today, December 20th, 2005).

[35]Since 2001, the FCC has been collecting subscription data at a far more disaggregated geographical level - the so-called rate centers. Rate centers are small geographical areas used by local carriers for a number of purposes including toll determination. Urban rate centers are usually a few square miles while very rural areas have rate centers encompassing hundreds of square miles. All service providers must report total number of subscribers at the rate center level to the Number Resource Utilization/Forecast (NRUF) database. There are 18,000 rate centers or on average 6 per county. This detailed data are not available to the authors.

[36]This coding scheme was originated in 1975 by David L. Brown, Fred K. Hines, and John M. Zimmer for a report Social and Economic Characteristics of the Population in Metro and Nonmetro Counties: 1970. It was updated after both the 1980 and 1990 censuses. The current coding is from 2003 and is similar in spirit to the earlier approach. However, in the 2000 census there were major changes to delineating metro areas and measuring urban and rural areas and so the current coding is not comparable to that of the earlier period.

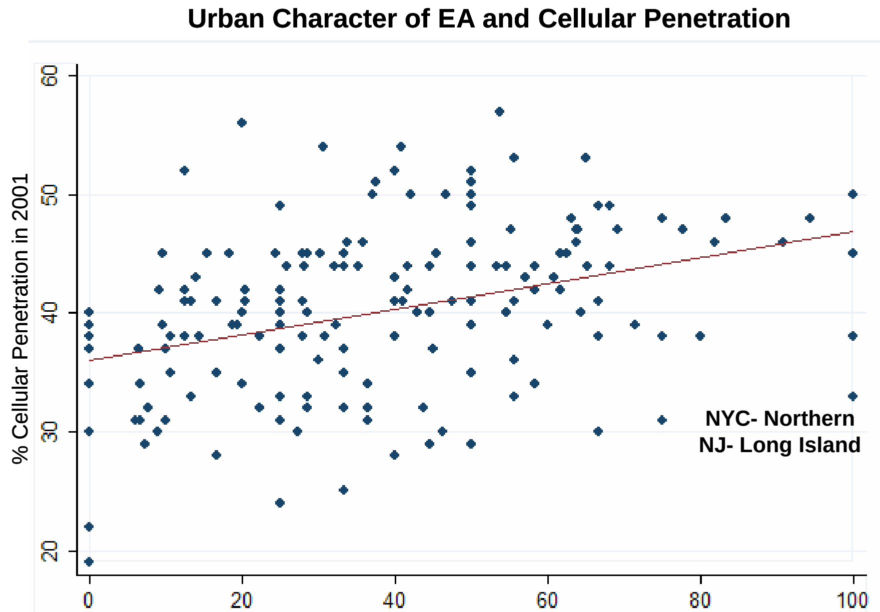

Figure 8, % Share of Metro Population by EA (U/R Codes <= 3)

To test this relationship more formally, Table A4 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.[37] 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% points lower for every 1 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 EAs in 2001.[38] 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 suggests that the most urban counties made further gains relative to their counterparts from 2001 to 2005.

Vehicular Crashes & 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. We classify fatal crashes as occurring in either metro, urban, or rural counties using the urban-rural continuum codes. 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, the more urban counties should have a higher level

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

[38] 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.

of cellular ownership than suggested by their EA average, while the most rural counties should have ownership levels lower than that indicated by their EA average. For ease of exposition, we aggregate counties into three groups. Counties with urban rural continuum codes 1-3, 4-7, and 8-9 in 2000 are grouped as metropolitan, urban/suburban, and rural areas respectively.[39]

To test the relationship between county type and crashes, we regress county fatal crash rates on EA level ownership as well as its interaction with the county type t:

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

Table 8 presents the results of the OLS regressions. If increasing cellular ownership has a sufficiently large impact on fatal crashes, one would expect the metropolitan crash rate to rise relative to the rural rate. While crashes in rural areas are far less common than in more populous urban and suburban areas, rural crashes are actually more likely to be fatal because such crashes involve higher average speeds, fewer average safety restraints, and relative delays in the arrival of medical care[40]. The first three columns provide estimation results for fatal crashes, while the remaining columns provide results for all crashes. Fixed effects and EA specific linear and quadratic time trends are used to control for possible confounds.

The first column confirms the large differences in levels across the three county types. On average, urban/suburban and rural counties have fatal crash rates 31% and 68% higher than that of metropolitan counties. The second and third columns indicate that higher EA-level ownership is associated with a *reduction* in the fatal crash rate in metropolitan counties. Within an EA ownership is decreasing across the urban-rural continuum. Because the dummy variables are coded with the metro county type as the base case, if cellular ownership is linked to the fatal crash rate, then one would expect negative coefficients on the interactions terms ($urban * cell\ own$) and ($rural * cell\ own$). Yet, relative to the effect of increasing ownership for metropolitan counties, there is an increase in the urban crash rate, and no significant difference for rural counties. A F-test on the joint significance of cellular ownership and its interaction with county type yields an F-statistic of 29.9, indicating significance at the 1% level. These results are robust to the inclusion of linear and quadratic time trends across the three county types. While the results are clearly

[39] We have run the analyses at a more disaggregate level - using the 1 to 9 urban rural continuum code - and the results remain substantively similar.

[40] 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.

inconsistent with a simple story of increasing cellular ownership leading to increasing fatal crash rates, they could be indicative of heterogeneous treatment effects within county types. It is unclear, however, why such heterogeneity would exist.

Columns (4-6) present results from the same specifications using the SDS data for all crashes for seven states from 1990 to 2004. Column (4) indicates that the level of all crashes is lower in rural as opposed to metro areas. The final two columns indicate a small negative effect of ownership on metropolitan crash rates, but the interaction terms again offer no evidence for a differential effect on crashes across urban and rural counties. An F-test on the joint significance of cellular ownership and its interaction with county type yields an F-statistic of 1.39 which is insignificant at the 10% level. These results are robust to the inclusion of flexible time trends across the county types. In summary, there appears to be no evidence of differential trends in urban-rural crash rates linked to increasing cellular ownership.

3.3.3 Legislative Ban on Cell Phones & Crashes

In our final approach, we estimate the impact of legislative bans which restrict cellular use by drivers. Three states have legislated complete bans on hand-held phones: 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. Six additional states have legislated partial bans on driver cellular use, but each of these bans targeted a modest fraction of drivers (Table A5 in the Appendix enumerates the states and large municipalities with complete or partial bans).[41]

Figure 9 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. 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 ($t-1$). An examination of longer horizons reveals no significant dip in crashes for any region other than, at first glance, the state 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,

[41] Chicago is the largest municipality to enact a complete ban against driver cell phone use. Other smaller municipalities have also enacted bans. However, of these bans, many are subject to minimal enforcement. An enumeration of municipalities with bans can be found in the "Phones and Highway Safety: 2005 Legislative Update" published by the National Conference of State Legislatures (available at: *www.ncsl.org/programs/transportation/cellphoneupdate05.htm#stateCell*)

Table 8

LINK BETWEEN CRASHES AND URBAN/RURAL CHARACTER

	DEPENDENT VARIABLE - LN(CRASHES PER 100,000 POP)					
	Fatal Crashes (FARS)			All Crashes (SDS)		
	(1)	(2)	(3)	(4)	(5)	(6)
Urban/Suburban County	0.3134***	0.3306***	0.3094***	0.0390	0.0234	0.1370**
	(0.0254)	(0.0332)	(0.0378)	(0.0419)	(0.0735)	(0.0548)
Rural County	0.6776***	0.5898***	0.6818***	-0.2004***	-0.2518**	-0.2243***
	(0.0428)	(0.0779)	(0.0678)	(0.0613)	(0.1023)	(0.0821)
Cell Phone Ownership		-0.0019***	-0.0019***		-0.0008*	-0.0008*
		(0.0002)	(0.0002)		(0.0004)	(0.0004)
Urban/Suburban x Ownership		0.0053***	0.0055***		0.0005	-0.0012
		(0.0011)	(0.0015)		(0.0020)	(0.0027)
Rural x Ownership		0.0010	-0.0003		-0.0004	-0.0011
		(0.0030)	(0.0040)		(0.0047)	(0.0057)
EA Fixed Effects	X	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X	X
EA Fixed Effects x Year		X	X		X	X
EA Fixed Effects x $Year^2$			X			X
N	N = 46758	N = 23226	N = 23226	N = 6345	N = 2617	N = 2617
R^2	0.31	0..32	0.32	0.48	0.50	0.50

Notes: The dependent variable for the first three columns is the natural log of the number of fatal crashes per 100,000 in population in a given year for a particular Economic Area. For the next three columns the dependent variable is the per capita log of all crashes for EAs in SDS states. Counties with urban-rural continuum codes of <= 3, 4 to 7, and >= 8 are respectively designated as metro, urban/suburban and rural. All errors are robust and clustered at the EA level.

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

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.

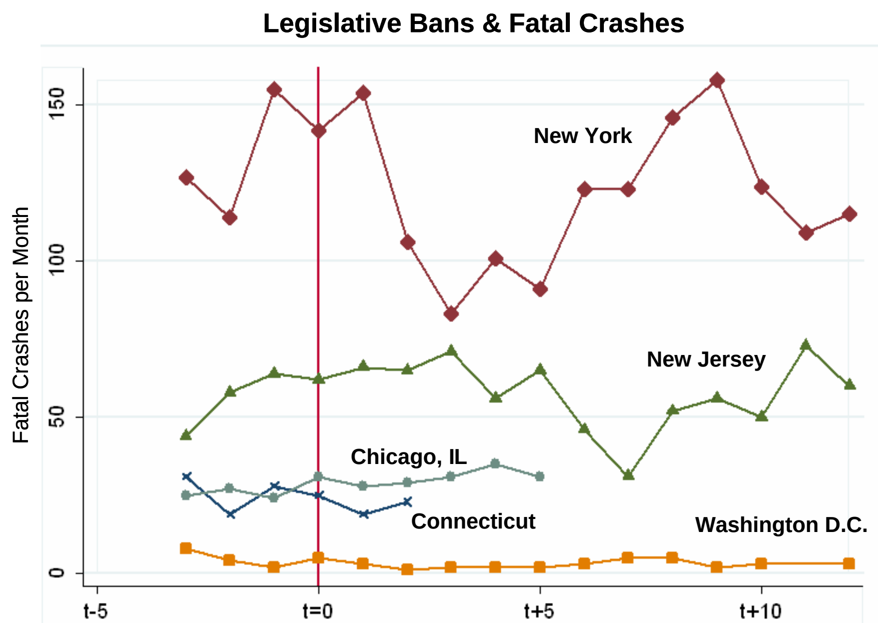

Figure 9, Monthly Fatal Crashes Before and After Legislative Bans

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:

$$Crash_{r,m,y} = \alpha + \lambda Ban_{r,m,y} + \eta_r + \eta_m + \eta_y + \varepsilon_{r,m,y} \tag{9}$$

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 results of the estimated coefficient $\widehat{\lambda}$ (1.38%, p=.93) confirms the general intuition of Figure 9– legislative bans on cellular use do not seem to reduce fatal crash counts.

4 Discussion

The present analysis implies lower crash rates than suggested by popular or academic belief. Table 9 enumerates the absolute risk rates for aggregate crashes implied by the RT study– the most widely cited study on cellular use and relative crash risk– under varying assumptions of cell phone usage by drivers, as well as the estimates of call volume increase produced by our two first stage data sets. For example, using the 2006 NOPUS estimate of handheld and handsfree driver cell phone use of 10%, and the estimatde call volume increase from 9 to 10pm of 16% from the MIT sample, the 4.3 fold increase in relative crash risk of RT implies a 4% relative increase in crashes during the hour following the weekday 9pm threshold.

Since the TNS sample is larger and more representative than the MIT sample, the rise in call volume is arguably closer to 33% than to 16%. A key assumption relates to cellphone usage during nighttime driving. However, we were unable to find any accurate and recent assessments. The NOPUS estimate of 10% cellular usage was conducted during the day. The only nighttime assessment of cell phone usage, the 1.5% estimate of drivers on the NJ Turnpike, was published in 2004, but relied on data collected between March and July of 2001 and focused explicitly on drivers on high speed roadways (Johnson et. al. 2004). As such, the estimates are from a period with minimal cellular ownership, and near-zero hands-free usage.[42] It is reasonable then 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. Yet it is also likely that combined usage (handheld and handsfree) during the late evening hours in 2005 is well above 1.5%.

Our most convincing evidence suggests that cellular use *does not* increase crash risk. That is, point estimates for the increase in both fatal and all crashes are approximately 0 across specifications. In this sense, 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 possible cell phone use, the upper bounds of our estimates for all crashes fall below the RT estimates. Given the lower frequency of fatal crashes, the resulting standard errors, as well as upper bounds, of the estimation are higher than for the all crash analysis. Nevertheless, the upper bound of 2.4% for the fatal crash rate still falls below most plausible RT estimates.

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

Table 9

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

	RT STUDY				9PM THRESHOLD ANALYSIS	
					UPPER BOUND	
	Driving Time on Cell Phone				Fatal	All
9pm Call Volume Rise	1.5%	4%	7%	10%		
16% (MIT)	0.8	1.9	3.0	4.0	2.4	0.9
33% (TNS)	1.6	3.8	6.2	8.2	2.4	0.9

Notes: This table presents the increase in aggregate crash risk due to driver cell phone use implied by the RT (1997) study as compared to the present study. The table reports the risk increase implied by varying estimates of driver cell phone use, as well as the estimates of call volume increase from 9pm to 10pm indicated by the two first stage samples. The mean RT relative crash risk estimate of 4.3 was used to calculate the figures.

An important caveat of our analysis is that the estimated effects represent a *local* treatment effect. That is, while our research design allows for a relatively precise estimation of the driver response at 9pm on weekdays after 2002, mappings to an absolute crash risk presume that the local average treatment effect is in fact the average treatment effect. One way through which we dealt with the issue of generalizability is to examine aggregate EA level trends in both ownership and crashes as well as urban-rural differentials in crashes. While these analysis provide directional support for a zero effect, they suffer from greater imprecision than that produced from the natural experiment. Importantly, the analysis reveals no salutary effect of existing state-wide cell phone bans. Additionally, we do not estimate the rise in call volume exclusively for drivers. It is possible that driver sensitivity to the 9pm 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 quite modest for the upper bounds of our analysis for all crashes to fall below the RT estimates.

What might then explain the departure of our results from that of RT? As mentioned earlier, the RT study, inventive as it was, suffers from two principle drawbacks. The first is that it relies on an unrepresentative sample of those involved in a recent crash. 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. Our study for the most part avoids these problems. Finally, it is possible that the findings of RT, generated in 1997, may no longer apply to the seasoned cell phone drivers of recent years. More recent studies, however, have very 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.

Interpreting the Effect Magnitude. If cell phones are a source of distraction, given limits to attentional capacity, how is it that our estimates suggest that such phones have zero, or perhaps even a mitigating, influence on crashes? Indeed, there are a number of plausible explanations for why cell phone use may appear to reduce rather than 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 the riskier driving of those who substituted one form of risk for another (1975). While compensatory responses to the imposition of seat-belts may seem far-fetched, it is more plausible to imagine drivers who slow down, pull over, shift to uncongested lanes or roadways, or simply allocate more attention to driving in response to making or receiving a cell phone call.

A second, related, possibility is that the drivers who tend to use cell phones while driving are drivers who 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 which suggests that driver heterogeneity in riskiness has led 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.

A third possibility is that cell phones actually improve driver outcomes for some drivers 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). "The 100-Car Naturalistic Study" concluded that 20% of crashes and 12% of near-crashes were linked to driver fatigue (NHTSA 2006a). The dangers of fatigue may be particularly pronounced for drivers accustomed to driving long distances or long hours such as large truck or cab drivers. In 2003, the Federal Motor Carrier Safety Administration implicated fatigue as a factor in 13% of all fatal large-truck crashes.[43] It is possible that for a certain class of drivers, cell phone use actually reduces fatigue and leads to safer outcomes.

Finally, the effect of cellular use on crashes may be heterogeneous across drivers. That is, while the local average treatment effect may be marginally negative or zero, there may

[43] Reported as a part of the *Report to Congress on the Large Truck Crash Causation.*

be drivers for whom the use of cell phones is particularly detrimental, as well as some drivers for whom the effect is negligible or even 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 of the experimental results. One possible direction of future research is to explore the potential heterogeneity of this effect.

Implications for Welfare & 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 the 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). A large number of municipalities, every state in the nation, as well as Congress itself has either considered or passed legislation against driver use of cell phones during the last several years.

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 do not rule out heterogeneity in the riskiness of cell phones across driver type, then bans on certain demographics, or bans on cellular use in certain contexts may indeed 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 should be done to elucidate the subpopulations of drivers for whom the link to crashes may indeed be relatively high.

Moreover, policies aimed at regulating cellular usage while driving trade off the value to society of unfettered cell phone use against the risk to life, limb and property. As such, the estimates of our paper could then be used to make explicit the statistical value of life which is implicit in such policies and could further inform cost-benefit analyses of the same (see Kniesner and Viscusi 2003, Johansson 2002 for discussion of the statistical value of life implied by regulatory decisions).

5 Conclusion

The link between cell phone use and driver safety has emerged in recent years as a topic of considerable research and policy interest. Most studies have concluded that cell phone usage increases crash risk with some even comparing its danger to that of alcohol consumption. The most notable of these studies (RT) suggest that cell phones result in a four fold increase in relative crash risk. Policy makers in several states have responded by pushing through either partial or complete bans on cell phones while driving.

We investigate the link between driver phone use and crash rates by exploiting a natural experiment induced by a discontinuity in pricing in popular cell phone plans. We first document a jump in call volume immediately after 9 pm on weekdays– when most plans since 2002 allow for free calls– using two large, distinct set of call level data. No such jump occurs on the weekend. Given call sensitivity to the change in marginal prices, we then examine the corresponding change in crash rates around the 9pm threshold since 2002. In order to control for possible confounds, we compare the change in crashes around 9pm to the same change in the period prior to the introduction of pre-paid plans in 1998, as well as to weekends. While the RT results imply an approximately 1 to 8% rise in crashes across the pricing threshold, we find no evidence for a relative rise in crash rates. In fact, the upper bounds of our estimates allow us to rule out any rise in fatal crashes larger than 2.4% and any rise in all crashes larger than .9%. To corroborate our results, we pursue three additional empirical strategies. None of these provide evidence to support a link between crash rates and driver cell phone use.

Reconciling our findings with that of the 4.3 fold increase in relative crash risk observed by RT presents a challenge. However, a few hypotheses exist. Drivers for whom cell phones greatly increases the risk of a crash may be overrepresented in the RT sample. Such selection effects suggest that the RT result is at best an upper bound for the population of drivers as a whole. Further, risk loving drivers may simply treat cell phones as a substitute for other distractions (e.g. talking to a fellow passenger, or fiddling with their radios) (Hahn and Prieger 2006).

It is important to 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 could be that drivers who use such devices compensate for the added distraction by driving more carefully. This hypothesis is consistent with the theory put forth by Peltzman (1975). In the least, we believe that these findings should renew interest in empirical research examining the effects of cell phone use, and possibly reopen policy discussions on the costs and benefits of regulations where such dialogue has quieted. One direction of future research which may prove particularly important to policy makers involves examining whether the influence of cellular use differs across drivers and contexts. Our research design allows for such an analysis of driver heterogeneity if one uses differences in cell phone price sensitivity across demographic groups as an additional source of treatment variation.

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7 Appendix

Table A1.

SUMMARY OF DATA SOURCES

	DATA SOURCE **(1)**	**YEARS** **(2)**	**DESCRIPTION** **(3)**
ACCIDENT/TRAFFIC			
Fatal Crashes	Fatality Analysis Reporting System (FARS)	1987 - 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
PENETRATION DATA			
Cellular Subscribers	Cellular Telephone Industry Association Survey (CTIA)	1999 - 2005	Cellular subscribers by state by year
	Federal Communications Commission (FCC)	2001 - 2005	Cellular subscribers by Economic Area (EA)
Population	Bureau of Labor Statistics (BLS)	1987 - 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 60
	TNS Telecom	2000 - 2001	Data from cellular phone bills for 9000+ 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

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

CODE	DESCRIPTION (1)	NUMBER (2)	% POP (3)
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 ar	435	0.01

Table A4

CELLULAR OWNERSHIP & URBAN/RURAL CHARACTER

	CHANGE IN % CELLULAR OWNERSHIP			
	1992 - 2001		2001 - 2005	
	(1)	(2)	(3)	(4)
EA Rural Code (Type)	-1.929*** (0.3120)		-0.629 (0.7590)	
% Pop - County Type 1		0.139*** (0.0470)		0.205* (0.1190)
% Pop - County Type 2		0.026 (0.0420)		0.108 (0.1610)
% Pop - County Type 3		-0.026 (0.0490)		-0.007 (0.2100)
% Pop - County Type 4		0.016 (0.0470)		0.105 (0.1120)
% Pop - County Type 5		-0.024 (0.1130)		0.138 (0.1080)
% Pop - County Type 6		-0.031 (0.0490)		0.191* (0.1060)
% Pop - County Type 7		-0.128 (0.0790)		0.059 (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: Penetration refers to number of subscribers for every 100 in population. EA rural code refers to the county average urban/rural continuum code weighted by population for an EA in year 2000. More rural EAs are assigned higher value types.

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

Table A5

SUMMARY OF STATE BANS ON CELL PHONES

STATES	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)
Illinois		Complete ban for Chicago	
Colorado		Ban on permit drivers	Secondarily enforced, fine of $15
Deleware		Ban on permit drivers	Similar to reckless driving penalties
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
Texas		Ban on permit drivers*	Not Available

NOTE. Table compiled from National Conference of State Legislatures reports, as well as various other news sources. 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 Texas ban on permit drivers applies to drivers only for the first six months following the issuance of a permit.

Cellular Penetration & Usage

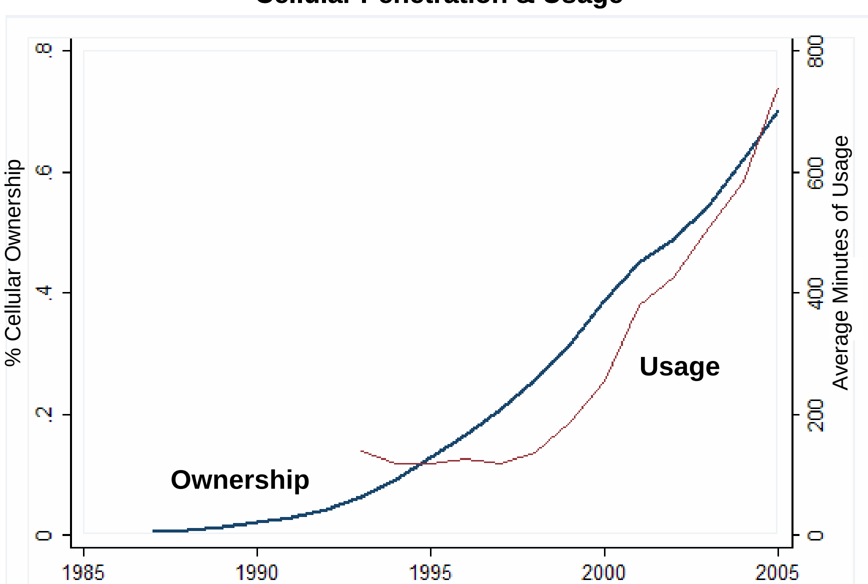

Figure A1, Cellular Ownership & Average Usage, 1987 - 2005

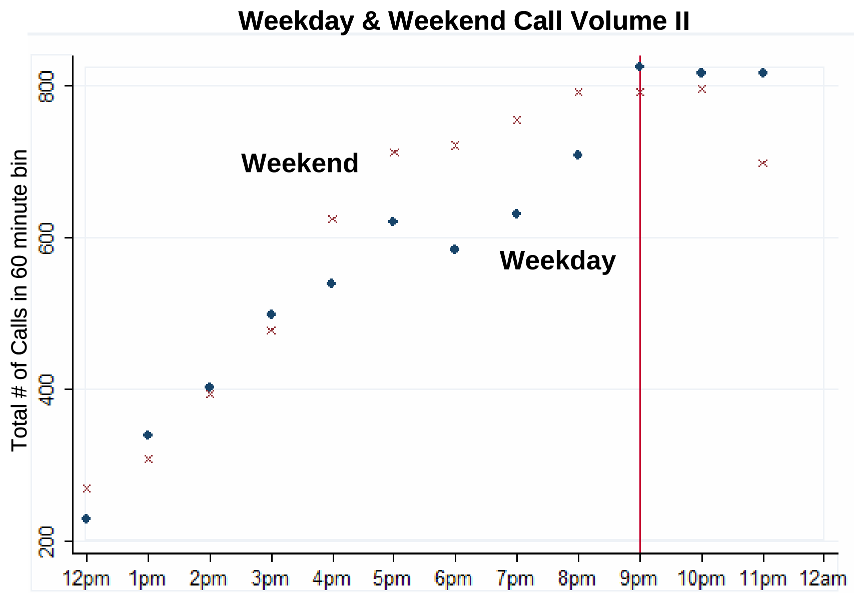

Figure A2, Outgoing Calls from 12pm – 12am in 2005
(60 mn bins)