

Driving Under the (Cellular) Influence*

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Abstract

Previous research in the laboratory and by epidemiologists has compared the danger of cell phone use while driving to that of illicit levels of alcohol. This paper investigates the causal link between driver cell phone use and crash rates by exploiting a natural experiment—the discontinuity in marginal pricing at 9pm on weekdays from 2002 to 2005 when cellular plans transitioned from “peak” to “off-peak” pricing. We first document that the pricing threshold induced a 7.2% jump on Mondays to Thursdays in call likelihood for a large and proprietary sample of drivers in California from 2005. Two additional datasets of calls, drawn from drivers and non-drivers nationwide, affirm the price sensitivity of cell phone users. We next document the corresponding change in the crash rate for California as well as the eight additional states for which we have the universe of crash data. Using a period prior to the prevalence of 9pm plans as a comparison group, we find no evidence for a relative rise in crashes after 9pm on Mondays to Thursdays in 2005, or during an extended period from 2002 to 2005. These results are robust to alternative estimation strategies and controls. Our preferred estimates imply an upper bound in the odds ratio of crash risk associated with cell phone use of 3.0, which rejects the findings of most existing research including the 4.3 asserted in the influential paper by Redelmeier and Tibshirani (1997). A panel analysis of regional trends in cell phone ownership and legislation banning driver cell phone use confirms our basic result. We present possible explanations for this counterintuitive finding, and discuss implications for policy.

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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 New York Times/ CBS News survey found that 80% of Americans believe that cell phone use should be banned.¹ This belief is echoed by recent research. Over the last few years, more than 125 published studies have examined the impact of driver cell phone use on vehicular crashes.² These studies include cross-sectional surveys, simulations in the laboratory, inspection of crash reports, observational studies using in-car cameras or confederate observers, longitudinal analyses of small samples of drivers, as well as correlations of aggregate cell phone ownership and crash records. In an influential paper published in the New England Journal of Medicine, Redelmeier and Tibshirani (hereafter, “RT”) concluded that cell phones increase the relative likelihood of a crash by a factor of 4.3 (1997). Laboratory and epidemiological studies have compared the relative crash risk of phone use while driving to that produced by illicit levels of alcohol (Redelmeier and Tibshirani 1997; Strayer and Drews and Crouch 2006).

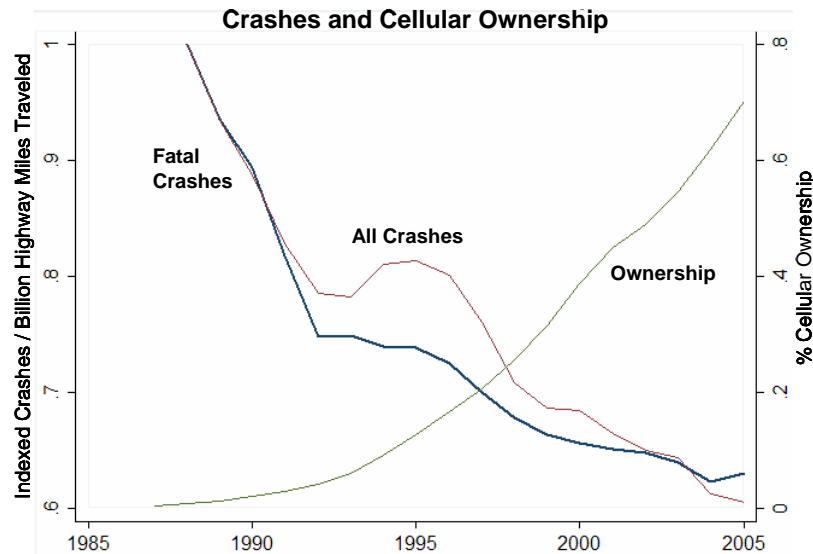


Figure 1, Cellular Ownership and Indexed Crashes/ Highway VMT for 1988 -2005

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. Cell phone ownership (i.e., cellular subscribers / population) has

¹The survey relied on a sample of 829 adults and was administered by phone in October 2009. The question referred specifically to handheld cellular use. The survey is reported at: <http://www.nytimes.com/2009/11/02/technology/02textingside.html>

²As counted by McCartt et. al. 2006.

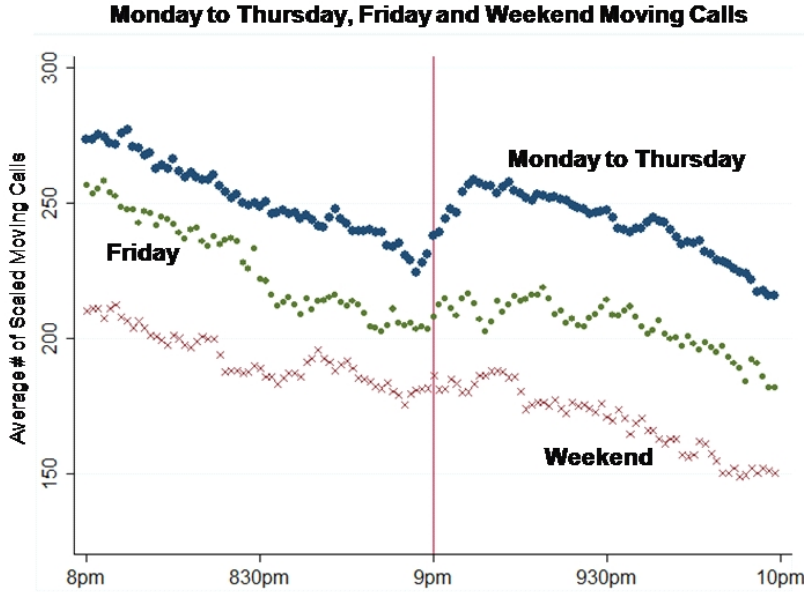


Figure 2, Average Scaled Moving Calls for California from 8pm to 10pm in 2005 (1 mn bin)

grown sharply since 1988, average use per subscriber has risen from 140 to 740 minutes a month since 1993, and surveys indicate that as many as 81% of cellular owners use their phones while driving—yet aggregate crash rates have fallen substantially over this period.³

No study has yet provided causal evidence of the relationship between cell phone use and crashes in the field. In this paper, we adopt a unique approach to estimate the causal link between cellular use and the crash rate. Specifically, we exploit a natural experiment which arises from a feature characterizing a large share of cellular phone plans from 2002 to 2005—a discontinuity in the marginal price of a phone call at 9pm on weekdays.

We first provide evidence that this discontinuity in prices drives a sharp increase in the likelihood of calling for drivers using a unique and proprietary dataset of calls from a prominent network provider. Our data is restricted to calls routed through multiple cell phone towers in a contiguous region just outside of a major California downtown area during an eleven day period in 2005. Given the mechanics of call routing and signal switching, the calls could have been placed only by callers in moving vehicles.⁴ While our data is

³Figure 1 plots fatal and all crashes nationwide from 1988 to 2005 per billion highway miles traveled using data from the General Estimates Survey and the Fatal Analysis Reporting System (see Appendix). Much of the drop in crash rates over this period is likely attributable to the increasing prevalence of safety devices and decline in driver alcohol use, while the NHTSA attributes the modest rise in crashes during the mid-1990s to relaxation of federal speeding regulations (2005). The survey indicating driver cell phone use was conducted by the Nationwide Insurance Company in 2008 and was based on a sample of 1241 American adults. See Figure A1 for a plot of average use per subscriber over time.

⁴An engineer at the major provider suggests that less than .1% of the calls in our sample are not generated

scaled for confidentiality, we estimate the data comprises 106,000 to 477,000 calls placed by moving callers within a region spanned by 300 to 400 cell phone towers. To our knowledge, our paper is the first in the literature to use a large call-level dataset directly from a major U.S. provider, and moreover, it is the first to feature call data from moving vehicles.

Figure 2 presents this distribution of cell phones calls by likely drivers across Mondays to Thursdays, Fridays and weekend evenings. Friday evenings are presented separately since callers appear to treat it distinctly from other weekdays and this distinction is statistically significant. The downward slopes reflect the pattern of traffic across evenings and we control for this explicitly in our regression analysis of call likelihood. These regressions confirm that driver call likelihood rises by 7.2% at the 9pm threshold when prices transition from “peak” to “off-peak” on Mondays to Thursdays. We find no comparable breaks in likelihood for neighboring hours or on the weekend at 9pm. Two more datasets of cell phone calls, this time made by drivers and non-drivers, confirm the price sensitivity of cell phone users as well as the differential treatment of Fridays. We present additional evidence on over 30,000 pricing plans across 26 markets by provider to demonstrate that from 2002 to 2005 a large fraction of callers were on plans with the 9pm price threshold. The rise in call likelihood at 9pm represents the first stage of our analysis.

We next test whether the rise in call likelihood at the threshold leads to a corresponding rise in the crash rate. In order to smooth crash counts that are subject to well recognized periodicity due to reporting conventions, we aggregate crashes into bins of varying sizes. While this strategy improves estimate precision, it introduces a bias due to potential covariate changes away from the threshold. To account for such movement in covariates, we compare the change in crashes at the threshold to the analogous change in a control period prior to the prevalence of 9pm pricing plans and characterized by low cellular use. We confirm the relative constancy of two critical covariates, traffic and reporting bias, across this period.

Figure 3 plots the universe of crashes for the state of California on Monday to Thursday evenings in 2005 and during the control period from 1995 to 1998.⁵ The plot, and subsequent regressions, indicate that crash rates in 2005, or in the extended time frame of 2002 to 2005, do not appear to change across the 9pm threshold relative to the pre-period. Assuming that drivers across the nation are comparable to those in California, we then generalize our crash analysis to include eight additional states for which we have the universe of crash data. Again we find no evidence for a relative rise in crashes across the threshold.

In a series of placebo checks, we estimate the same model for weekends and then for the proximal hours of 8pm and 10pm on Mondays to Thursdays. Finally, we demonstrate

by a caller from a moving vehicle.

⁵The periodicity evident in Figure 3 is due to the aforementioned reporting bias in the timing of accident reports.

that our results are robust to a variety of strategies through which one might deal with the bias that afflicts the reporting of crashes. Overall, the analysis suggests that cell phone use *does not* result in a measurable increase in the crash rate.

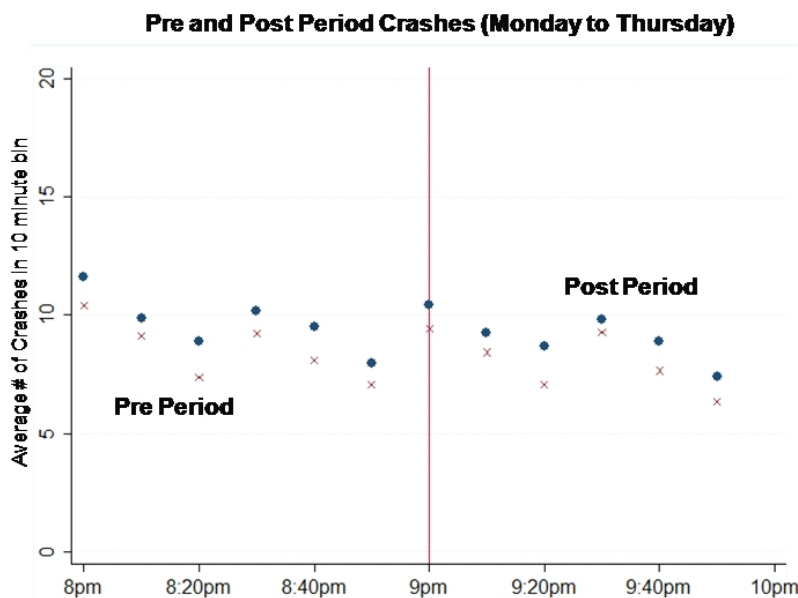


Figure 3, Average Crashes for California from 8pm to 10pm in Post (2005) and Pre (1998) Period (10 mn bins)

We calculate the upper bound of the relative rise in crashes at the 9pm threshold implied by our estimates. Our analysis of the expanded set of states for 2002 to 2005, as well as for just 2005, allows us to reject, with a 95% confidence interval, any relative rise in crashes larger than 1.0% and 1.2% respectively. Given the size of the observed discontinuity in call volume, and credible assumptions regarding evening driver cell phone use, the upper bound of each of these estimates translates to a 3.0 fold increase in crash risk. This not only rejects the 4.3 fold increase in crash risk estimated by RT, but the confidence interval of our estimates does not overlap with that of RT. Our rejection of the RT result is not sensitive to a reasonable range of assumptions regarding average call likelihood as well as the precise magnitude of the change in call likelihood at 9pm.

Is cellular use on the road really as dangerous as drunk driving? Our analysis suggests that it is not. The upper bounds easily rule out the crash risk of 7 associated with positive levels of blood alcohol, and the crash risk of 13 associated with illegal limits of blood alcohol (Levitt and Porter 2001a).

Our estimation is subject to at least two caveats. First, we assess only the local average treatment effect of cell phone use across all drivers and driving conditions around 9pm from Mondays to Thursdays. However, there is evidence that, even after accounting for lower

levels of traffic, crash risk is actually higher at night than during the day. Given the higher speeds and lower visibility that characterizes nighttime driving, one might expect cellular distraction to be quite hazardous at night. A second drawback is that while we find no evidence for a rise in fatal crashes using an analogous approach, estimates confined to fatal crash data are too imprecise to rule out RT.

While the natural experiment represents our most credible research design, we employ two additional empirical approaches to confirm the result and address these limitations. A first approach exploits the non-linear and heterogeneous take-up of cell phone technology across region by comparing yearly variation in regional ownership with yearly changes in the crash rate. Our unit of analysis, an “Economic Area,” represents the most disaggregated geographic unit for which ownership data is available. This analysis is the first, to our knowledge, to present region-year regressions of driver cell phone risk at this level of disaggregation. A second, related approach, estimates the impact of recent legislative bans on handheld cell phones by a number of states and municipalities on fatal crashes. Neither approach offers evidence associating cellular use with crashes.

We offer three main explanations to reconcile our results with the existing research. One possibility is that drivers compensate for the dangers of cell phone use by driving more carefully (Peltzman 1975). We review the mixed field and laboratory evidence on compensatory driving in the face of attentional distraction and present a simple model suggesting that compensation is a rational response to the dangers and benefits of cellular use. A second explanation, first suggested by Hahn and Tetlock (1999), is that the absence of an effect is caused because risk-loving drivers substitute one source of risk, such as speaking with others, listening to the radio or driving more aggressively, with another, such as using a cell phone. A third possibility is that cell phones may be dangerous for some drivers or under particular driving conditions, but are beneficial for other drivers or under alternative driving conditions (Kolko 2009).

Our findings have policy implications. Every state has considered some form of legislation to restrict the use of cell phones—or to require the use of hands-free devices—while driving for some or all groups of drivers. Twenty-seven states already have such legislation on the books.⁶ Yet researchers estimate that the economic value of cell phone use to drivers is considerable (e.g., Hahn and Tetlock 1999; Hahn and Tetlock and Burnet 2000; Lissy et al. 2000; Cohen and Graham 2003). In light of such benefits, our paper casts doubt on the desirability of at least some policies restricting driver cell phone usage. For instance, if drivers who currently use cell phones compensate for such use with more careful driving, then there may be a rationale for penalizing cellular use as a secondary, but not as

⁶Six states have banned hand-held cell phone use by all drivers and 21 other states have enacted partial bans primarily targeting younger drivers or those driving school busses (as reported by the Governors Highway Safety Association website in January 2010).

a primary, offense. If cellular use is the product of risk substitution, then any legislative ban is inefficient. Finally, if there is heterogeneity in the effect across drivers and driving conditions, then partial and targeted bans are appropriate.

Beyond contributing to the literature on the danger of cellular use, our paper is in the spirit of studies which use natural experiments to assess the effect of driver behavior on crash risk (Levitt and Porter 2001a; Levitt and Porter 2001b). This study also relates to the literatures that examine the theory of compensation in the face of driving risk factors (Peltzman 1975; Cohen and Einav 2003), the differences and potential complementarity between the laboratory and the field (e.g., Dahl and DellaVigna 2009; Levitt and List 2007), and the statistical value of life implicit in policy (e.g., Ashenfelter and Greenstone 2004).

The remainder of this paper proceeds as follows. The following section describes the background of research on the link between cell phones and crashes. The third section outlines the empirical approach and accompanying results. Next, we report the sensitivity of our findings to underlying assumptions, attempt to reconcile our estimates with the existing research, comment on policy implications, and discuss differences in the laboratory and the field in light of our results. The final section concludes.

2 Background

The sharp rise in cell phone ownership over the last several years has been paced by an equally impressive rise in research examining the effects of such ownership on vehicular crashes. Ignoring the substantial literature on the cognitive and neural underpinnings of limited attention and multi-tasking, one can classify analyses of crash risk due to cellular use into one of five major methodological categories: (i) Laboratory studies that focus on subject behavior in simulated, or highly controlled, driving conditions, (ii) Naturalistic studies of drivers on the actual road, (iii) Studies which inspect police annotations of crash records, (iv) Correlational analyses of aggregate crash records and cell phone ownership, and (v) Longitudinal analyses of individual level phone and crash records. Beyond estimating the impact of phone use on crashes, other researchers have measured the frequency of such use by drivers. Several excellent recent surveys of these literatures exist.⁷

Cell Phone Use and Crash Risk. In the standard experimental paradigm in the lab, 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 to traffic signals) under varying forms of distraction. These studies generally

⁷Examples of these surveys include Hahn and Prieger 2006; McCartt and Hellinga and Braitman 2006; Prieger and Hahn 2007; Caird et al. 2008.

conclude that instructing subjects to use cell phones impairs driving by a factor of 3 to 4 as compared to 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). Importantly, these studies generally find no differences between handheld and hands-free devices (Caird et al. 2008).

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

A second set of approaches, naturalistic studies, employ visual and audio recording devices to monitor behavior in authentic driving conditions. In one such study, “The 100-Car Naturalistic Study,” researchers equipped 100 vehicles with cameras and sensors and tracked 241 primary and secondary drivers for over 1 year (NHTSA 2006). After amassing nearly 43,000 hours of driving data, the authors find 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 calculate that dialing a cell phone multiplies crash risk by a factor of 3, while listening or speaking with a cellular device make drivers 1.3 times (with a 95 percent confidence interval of .93 to 1.90) more likely to crash. Cellular use was also associated with the majority of near-crashes.

Like laboratory studies, naturalistic approaches pinpoint specific causes of driver impairment and characterize their relative danger. However, given that drivers may be aware of being monitored, it is unclear whether such studies improve upon the external validity of studies conducted in the lab. Further, given the high costs, the sample sizes are often too small and unrepresentative to 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.⁸ These studies attribute roughly one percent of crashes to phone use (Lissy et al. 2000). However, attempts to infer the causal effects of cell phone use from crash reports suffer from source unreliability (NHTSA 1997) and reporting bias due to recent increases in police awareness of cell phones as a possible crash cause (McCartt and Hellinga and Braitman 2006). Most importantly, one cannot infer causality from correlations between police reports and crashes since the growth in cell phone ownership amongst drivers should mechanically increase the observed fraction of police reports which cite such use during a crash.

A fourth strategy, which generates absolute estimates of crash risk, is the comparison

⁸As of 2007, 16 states provide crash statistics in which cell phones or CB radios are listed as a causal factor (Sundeen 2007).

of aggregate trends in cell phone ownership with trends in crash rates at the local, state or national level. In an analysis concurrent with the present study, Kolko (2009) compares state-year variation in cellular ownership with fatal car crashes from 1997 to 2005. After controlling for various covariates as well as state and year fixed effects, Kolko’s point estimates, while not statistically significant, imply that the introduction of cell phones led to a roughly 16% increase in the annual fatal crash rate (with a 95 percent confidence interval of -7 to +39%).⁹ Kolko finds a slightly smaller, but statistically significant, correlation between ownership and fatal crashes involving only poor driving conditions (i.e., wet roads or bad weather).

Finally, Kolko examines the impact of state bans restricting handheld cell phone use with the same framework. He finds a statistically significant negative impact of this legislation on the fatal crash rate. Another recent study compares collision claims for new vehicles, (i.e., under 3 years old), before and after the enactment of bans in California, Connecticut, New York and Washington D.C., to claims in nearby regions (HLDI 2009). Overall, the authors find no evidence that the legislation led to a subsequent decrease in claims.¹⁰

The high level of aggregation and the strong secular and non-linear trend in overall crashes in the 1990s (see Figure 1) complicates such analyses. For example, panel analysis at the state-year level leaves open the possibility that unobserved state-specific and time-varying risk-factors—such safety technology or speeding laws—might also influence the crash rate. While Kolko exploits weather and road conditions during a crash as an additional source of variation, it does not appear that the difference in point estimates between the examined subpopulations is statistically significant.¹¹

The present analysis attempts to address some of these shortcomings with more disaggregated data on ownership, an extended time-series using years prior to the widespread introduction of cell phones as a control period, and controls for region specific linear and quadratic trends. Our attempts at replicating the Kolko estimates of the correlation between ownership and crashes, as well as the effects of legislation, imply that the inclusion of region specific time trends or a control period eliminates evidence for a positive correlation. While our analysis is not immune from the disadvantages inherent in this approach, we do

⁹The extrapolation to absolute crash risk assumes linearity in the influence of increasing cellular ownership on crashes.

¹⁰The study reports ten regression coefficients which correspond to specifications of various driver populations (i.e., all and drivers < 25 years) and control groups over an unspecified number of months. The regressions control for linear trends in both the control and treatment groups. None of the specifications yielded statistically significant evidence for a drop in claims.

¹¹Table 3 of Kolko (2009) does not provide enough information to infer statistical difference in point estimates for each of the four regressions whose results are reported (i.e., “good weather”, “dry road”, “bad weather”, and “wet road” conditions). Given the difference in the magnitude of the point estimates, and the size of the estimated standard errors, it seems unlikely that the difference between coefficients is statistically significant.

not find significant evidence for a link between ownership and crashes or for a significant impact of legislation.

A final class of studies tracks individual level phone use and driving behavior for a small number of drivers. The most widely cited of these is the analysis by RT (1997). In their influential paper, the authors inspect crash records and detailed phone bills for 699 Toronto drivers recently involved in a minor car crash.¹² To control for heterogeneity in driver quality, the paper relies on a technique commonly employed in epidemiological research—the “case cross-over method”—to study the health effects of transient exposure to a risk factor. For each driver, the authors compare exposure to cell phone use immediately prior to the crash, with exposure during a crash free control period before the crash occurred. By examining the relative use of cell phones during the two periods, the authors control for driver specific variation in crash likelihood. Using a conditional logit regression, the paper infers that cell phone use increases the relative likelihood of a crash by a factor of 4.3 (with a 95 percent confidence interval of 3.0 to 6.0). The study fails to find significant differences in increased crash risk across age or gender. A more recent application of the case-crossover method in Australia finds that the use of cell phones increases crash risk by a factor of 4.1 (McEvoy et al. 2005). This study finds no significant difference in the crash risk associated with handheld (4.9) and hands-free devices (3.8).

While the RT paper is considered perhaps the most influential of this, or any class, of studies, Hahn and Prieger point out that the study relies on a very unrepresentative sample of drivers recently involved in a crash (2006). As evidence for such selection, Prieger and Hahn (2007) conclude from a survey of 7,268 drivers that handheld cell phone users are actually more likely to crash even when not on the phone. Wilson et al. arrive at a similar conclusions from collision records of 3,869 cell-phone using and non-using drivers (2003). An additional concern is that while the RT methodology controls for fixed driver characteristics, it does not control for time varying unobservables such as boredom or stress that may cause both cell phone use and poor driving. Finally, other researchers have noted that the lack of precision with which RT infer the timing of crashes means that observed cell phone calls may have been placed after, rather than before, a crash occurred. This is reasonable if, for instance, calls were placed to authorities or loved ones immediately after a crash.

In another epidemiological approach, Young and Schreiner (2009) investigate the risks associated with hands-free use of a popular voice-activated communication device embedded in select vehicles called OnStar. OnStar automatically places an emergency call in the event of a crash in which an airbag is deployed. OnStar records the times of phone conversations

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

as well as calls notifying operators of an airbag deployment. The study finds that from June 2001 to November 2003, hands-free calling amongst the nearly 3 million OnStar subscribers actually lowered crash risk to a factor of .62 (with a 95 percent confidence interval of .37 to 1.05) relative to driving without calling. A benefit of this design is that OnStar measures the time of each crash accurately. However, because the study does not directly observe the driving time during the comparison period for which there are no calls placed, calculations of relative risk are sensitive to the assumptions that underlie the inference of such driving duration. If driving time is underestimated, the study inflates the crash risk in the comparison period and biases the relative risk estimate downwards. A second concern is that drivers in the comparison period may be using other types of cellular devices to make calls.¹³

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

Frequency of Cellular Use by Drivers. A number of studies have attempted to estimate the frequency of cell phone use on the road. These include surveys of driver usage, as well as observational studies with experimenters or cameras stationed at intersections. The most widely cited of these is the National Occupant Protection Use Survey (NOPUS) administered and published (almost) every year since 2000 by the NHTSA. For the 2005 NOPUS, trained observers were dispatched from 8am to 6pm to 1,200 probabilistically sampled intersections nationwide in June 2005. Six percent of the 43,000 observed drivers were using a handheld cell phone. The authors estimate, using existing survey data, that an additional 4% of drivers were on hands-free phones resulting in a total usage of 10% (NHTSA 2005).¹⁴

NOPUS estimates total use has been steadily increasing over the last several years from 6% in 2002, 7% in 2003, 8% in 2004 and 10% in 2005 (NHTSA 2002 to 2005). NOPUS also hints at considerable heterogeneity in cellular use across driver age and location—but not gender—with handheld cell phone use alone approaching as high as 10% for drivers from 16 to 24 years in 2005 (Glassbrenner 2005). Independently, a study tracking long-term legislative compliance in Washington, D.C., Maryland and Virginia, found that daytime handheld use in 2004 was 5.8% (McCartt and Hellinga 2007). This figure is slightly higher than the 4% handheld use estimated by NOPUS for 2004.

¹³These criticisms were outlined by Braver, Lund and McCartt in their critique published on the Insurance Institute for Highway Safety website in March 2009.

¹⁴NOPUS also reports the incidence of observed “head-set” use which, in 2005, was .7%. The NOPUS estimate of total hands-free usage combines observed head-set usage with driver survey results (a survey by Stutts et al. 2003 entitled “Distractions in Everyday Driving”).

Table 1

EFFECT OF CELLULAR USE ON CRASH RISK: COMPARISON BY METHODOLOGY

	RELATIVE RISK	ABSOLUTE RISK
Present Analysis (9pm Discontinuity)	0 times collision risk (3.0 upper bound)	0% increase in crashes (20% upper bound)
Experimental Studies	3 to 4 times impairment (Strayer 2003; Strayer 2006)	20 to 30% increase in crashes (Extrapolated for 2005)
Naturalistic Studies	1.3 times collision risk (NHTSA 2006)	3% increase in crashes (Extrapolated for 2005)
Police Annotations	1.25 times collision risk	1% increase in crashes (Lissy et. al. 2000)
Ownership and Crash Trends	2.6 times collision risk	16% increase in fatal crashes (not significant) 11% increase in bad weather fatal crashes (Kolko 2009)
Individual Crash Records	4.3 times collision risk (Redelmeier and Tibshirani 1997)	33% increase in crashes (Extrapolated for 2005)

Notes: The table displays the relative and absolute crash risk implied by selected examples of each class of studies. In cases where relative or absolute crash risk was not explicitly calculated, we extrapolate such risk using basic assumptions of cell phone ownership, baseline usage, and in the case of the present analysis, the equivalence of volume increases and ownership increases. Extrapolations in absolute crash risk are made for 2005, and extrapolations for relative risk are made using the assumption values associated with the year of the study. For example, to generate the absolute crash risk in 2005 associated with the RT (1997) estimate of 4.3 relative crash risk, we use the baseline NOPUS usage in 2005 of 10% and then add the cellular and non-cellular driver crash risks (i.e., $(9 * 1) + (.1 * 4.3)$) to produce a 33% increase.

Our calibrations ultimately rely on assumptions regarding nighttime cellular use. We are aware of only two studies that explicitly consider cell phone use at night. Jointly these studies suggest that cellular use in early night-time hours is not different from use during the day. In the first study, conducted in 2006, authors equipped observers with night vision technology at 113 randomly selected intersections in Indiana from 9:30pm to 5:45am (Vivoda et al. 2008). The study finds handheld use to be 6.9% amongst drivers from 9:30pm to 12am ($N = 3774$) which is slightly higher than the corresponding NOPUS estimate of daytime use.¹⁵ A second study, conducted in 2001, specifically targets cell phone use amongst high-speed drivers during various points in the day. In this effort, researchers assessed 40,000 photographs of vehicles traveling on the high-speed NJ Turnpike (Johnson et al. 2004). On average, only 1.5% of the high-speed drivers are on handheld phones which is half of the comparable NOPUS estimate. Again, authors find no significant difference between cellular usage during the late evening (i.e., from 8pm to 12am) and the afternoon (i.e., from 12pm to 4pm) for this particular class of drivers.

Perhaps the most convincing evidence of cell phone use by drivers at night, relative to during the day, comes from the present analysis. Our data of cellular use by drivers in California in 2005, after controlling for traffic, provides a relative index of use by drivers over the course of the day. An analysis of this data, described below, further suggests that

¹⁵The study did find that usage dropped significantly after 2am (i.e., they estimate 3.1% usage from 2 to 4am and 1.3% usage from 4 to 5:45am).

estimates of daytime usage are legitimate, and possibly even conservative, proxies for use during the late evening.

Table 1 also compares the extrapolated or calculated relative and absolute crash risk for representative studies in the literature. Extrapolation of absolute crash risk assumes the 10% NOPUS rate of cellular use in 2005, randomization in usage across driver type, and linearity in the influence of ownership on crashes.¹⁶ The first row of the table reports the point estimates for the present analysis as well as the implied relative and absolute upper bounds. We elaborate on the calculation of the relative crash risk produced by the 9pm analysis below.

3 Empirical Analysis

This section presents the estimation strategy, identifying assumptions, and the empirical findings for the 9pm analysis and briefly discusses two supplementary approaches. Our analysis relies on a wide array of data sources which are described in the Appendix and summarized in Table A1. First, we provide evidence for the sensitivity of call volume to systematic and transparent discontinuities in the marginal price of cellular calls. We document that, from 2002 to 2005, most cellular users subscribe to plans which feature near zero marginal costs for a phone call after 9pm on weekdays. We then provide evidence for a jump in 9pm call likelihood on Mondays to Thursdays, but not weekends, or proximal hours, for a region in California. Additional data confirms that this price sensitivity of cellular use generalizes across years, geography and providers.

Second, we estimate the effect that increased call likelihood has on the likelihood of a crash. We first compare the difference in the crash rate in California before and after 9pm on Mondays to Thursdays in 2005 to a control period prior to the prevalence of 9pm plans and characterized by low average driver call likelihood. In principle, one might advocate a standard regression discontinuity estimate without the control period. However, the change in crashes at 9pm is jointly attributable to an on-hour change in the likelihood of a crash being reported as well as a change in crash risk due to heightened call likelihood. While we can eliminate the problem of the reporting conventions by aggregating crashes into 30 or 60 minute bins, lengthening the estimation window invalidates the standard regression discontinuity design by permitting other covariates, such as traffic and driver composition, to vary across the threshold. We therefore rely on a double difference, with a suitable control period, to mitigate any such bias.

¹⁶ Assuming for example that cell phone use occurs during 10% of total driving time, then, ignoring selection, a relative crash risk of 4.3 translates to a 33% increase in total crashes. Relative crash risks can be calculated conversely. Accordingly, estimates of the effect of cell phone use on the change in total crashes range from 1 to 33% in absolute terms.

For additional precision, we extend the analysis to a larger set of states and to an extended treatment period from 2002 to 2005. We examine the change in critical covariates, perform a series of placebo checks with alternative controls including weekends and proximal hours, and we subject our estimates to a number of robustness checks. Overall, we find no evidence for an increase in the relative crash rate at the 9pm threshold.

3.1 Estimation Strategy and Identifying Assumptions

Let $\ln(\text{Crash}_{rpwt})$ refer to the log number of reported crashes in region r in either a “post” or “pre” period p , during weekdays (i.e., Mondays to Thursdays) or weekends as indicated by w , at time of the day t . “Post” refers to the period characterized by high cell phone ownership and high plan conformity around a specific threshold (e.g., 2002 to 2005), while “pre” refers to the period of low average call likelihood and prior to the prevalence of 9pm pricing plans (e.g., 1995 to 1998). In this framework, reported crashes are jointly determined by the traffic level, bias in the reporting of crashes, and the covariate of interest, the number of cell phones in use.

Cellular use, CellUse_{rpwt} is a function of the likelihood that a driver makes a call, CallLike_{pwt} , as well as traffic. CallLike_{pwt} varies both across years and time of day. Average call likelihood across years is determined by a host of factors including levels of cell phone ownership, legislation, long run change in cellular pricing, and the sophistication of handset technology. Call likelihood may change during the course of a day due to changes in pricing, which is sharply discontinuous at 9pm on weekdays, as well as changes in the composition of drivers. We infer average levels of cellular use from the rich collection of observational studies discussed above. We also include a vector of additional covariates, X_{rpwt} , that may influence the rate of vehicular crashes. Such covariates include speeding regulations, weather conditions, and the availability and adoption of safety technology:

$$\ln(\text{Crash}_{rpwt}) = \alpha + \theta_1 \text{Traffic}_{rpwt} + \theta_2 \text{RepBias}_{rpwt} + \theta_3 X_{rpwt} + \lambda \text{CellUse}_{rpwt}(\text{CallLike}, \text{Traffic}) + \varepsilon_{rpwt}$$

It is possible that drivers who use cell phones have a greater affinity for risk, and that the risk affinity, R , of drivers on the road produces a higher likelihood of entering into a crash: $E(\varepsilon | R) \neq 0$. Since CellUse_{rpwt} may also be a function of the risk affinity of drivers, $\hat{\lambda}$ will be biased. One strategy through which to circumvent this bias is to assume that the distribution of unobserved driver risk is the same immediately before and after the 9pm pricing threshold, such that $\lim_{\Delta \rightarrow 0^+} E(\varepsilon | R_{9pm+\Delta}) = \lim_{\Delta \rightarrow 0^+} E(\varepsilon | R_{9pm-\Delta})$.

If we define a control function $g(R) = E(\varepsilon_{rpwt} | R)$ which is continuous through the

9pm threshold, we can rewrite the original equation as:

$$\begin{aligned} \ln(\text{Crash}_{r_{pwt}}) = & \alpha + \theta_1 \text{Traf}f\text{ic}_{r_{pwt}} + \theta_2 \text{RepBias}_{r_{pwt}} \\ & + \theta_3 X_{r_{pwt}} + \lambda \text{CellU}se_{r_{pwt}} + g(R) + v_{r_{pwt}} \end{aligned}$$

where the error term $v = \varepsilon - E(\varepsilon|R)$ is now independent of $\text{CellU}se_{r_{pwt}}$. Given our assumption of a continuous risk function at the pricing threshold, any break that we observe in crashes can be attributed to a change in the remaining covariates. We formalize this regression discontinuity at the threshold then, by calculating a first difference, $D_{r_{11t}}$, which represents the change in crashes during some time window immediately before the threshold, t' , from some window immediately after the threshold, t . Initially, we restrict focus to the post period. Assuming that $X_{r_{11t}}$ is unchanged locally around the threshold, it drops out of the first difference:

$$\begin{aligned} D_{r_{11t}} = \ln(\text{Crash}_{r_{11t}}) - \ln(\text{Crash}_{r_{11t'}}) = & \theta'_1 \Delta \text{Traf}f\text{ic}_{r_{11t}} \\ & + \theta'_2 \Delta \text{RepBias}_{r_{11t}} + \lambda' \Delta \text{CellU}se_{r_{11t}} + v'_{r_{11t}} \end{aligned}$$

Additionally, assuming that average call likelihood is unchanged across the threshold, $\Delta \text{CellU}se_{r_{11t}}$ will be a function of traffic as well as changes to call likelihood linked to price. In theory, we allow traffic patterns and reporting bias to vary across this first difference. In the face of covariates that vary across the threshold, we can calculate a second difference, DD_{rp1t} , by comparing the first difference in crashes around the time threshold during the post period from a similar difference calculated for the pre-period¹⁷:

$$DD_{rp1t} = D_{r_{11t}} - D_{r_{01t}} = \lambda'' (\Delta \text{CellU}se_{r_{11t}} - \Delta \text{CellU}se_{r_{01t}}) + v''_{rp1t}$$

If we assume that the change in reporting bias and traffic across the threshold in the pre and post period do not systematically differ (we test this assumption below), then the double difference in crash rates is simply a function of the residual change across the threshold in call likelihood due to price in the post relative to the pre period. If the change in likelihood due to price is absent in the pre-period, then the double difference in price reduces to a single difference in price at 9pm during the post-period. Importantly, if one believes that call likelihood does change across the threshold in the pre-period, due to some unobserved factor, then the double difference in cell phone use must be scaled by the difference in the average level of call likelihood over the years. For example, if average likelihood is 5 times higher in the post relative to the pre-period, then a 2% rise in 9pm

¹⁷ An example of a factor that might systematically change across the 9pm threshold, but whose double difference should not change systematically across the pre and post periods, is daylight.

call likelihood in the pre-period, is only equivalent to a .4% change across the threshold in the post period.

Finally, to allay the concern that the differences in reporting bias or other unobserved factors may systematically vary across the pre and post period, as a placebo check, we can calculate additional double differences that compare the first difference in crashes across the threshold on weekends at 9pm, as well as on weekday proximal hours, in the pre and the post period. Next we discuss details of the pricing discontinuity and document the subsequent change in call likelihood.

3.2 Change in Call Volume at Price Discontinuity

Pricing Plans. In recent years, contracts for cell phones have been characterized by a flat monthly fee which entitles subscribers to a specified number of minutes depending on the time of use. Any use in excess of this allotment is subject to relatively high marginal fees. For instance, a “900 Nation” plan offered by Cingular in 2006 allows 900 minutes of peak usage from 6am to 9pm each weekday, unlimited use for off-peak periods after 9pm and before 6am on weekdays, and unlimited use all day on weekends.¹⁸ Marginal fees for excess usage commonly range from \$.35 to \$.45 per minute.

Figure 4 documents the share of cellular subscribers associated with each hourly threshold at which providers distinguish between peak and off-peak usage across major national markets from 1999 to 2005 (i.e., “legacy share”). We calculate annual legacy shares for each plan threshold with data on new subscribers, inferred market shares for each category of pricing plans, and data on plan turnover (Table A2). Specifically, we first calculate the unweighted proportion of provider plans associated with each threshold for each year and then weight these proportions by the yearly market share of each provider as reported by the FCC. While we expect plans within a provider to vary in popularity, our estimation assumes that a proliferation of offerings is correlated with actual plan popularity. We assume new subscribers—including both new adopters and existing owners that transition from existing plans—allocate themselves across providers and into plans in a distribution dictated by each year’s market share. For simplicity, we treat all subscribers in 1995 as new and conservatively assume that, from 1995 to 1999, market shares and provider plans are constant. The basic pattern of Figure 4 is not highly sensitive to such assumptions. All told, the figure is a product of data on over 30,000 cell phone plans from 1999 to 2005 across 26 major markets and 30 providers. This data is obtained from a market research firm, Econ One Research, and is described in the Appendix.

Figure 4 suggests that from 2002 to 2005, 9pm pricing plans were the most popular

¹⁸ Actual plans often specify some large, but finite, limit for non-peak usage. These limits, sometimes marketed as “unlimited,” are typically 5,000 to 10,000 minutes.

category of cellular plans. Two-tiered plans with peak and off-peak pricing began gaining in popularity in the late 1990s. 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, by 2004, at least one major provider introduced plans with an earlier switching time of 7pm. Our model finds that during the period from 2002 to 2005 about 55% of subscribers had 9pm plans. The prevalence of 9pm plans during this period is even more striking if one were to plot the number, as opposed to share, of subscribers—or drivers who regularly use their cell phone while driving—with 9pm phone plans. Indeed, cellular ownership and usage by drivers exploded over this period as ownership expanded by a factor of 2.5 and average call likelihood by drivers grew by an even larger factor.

While plan data from Econ One Research does not exist prior to 1999, numerous analyst and industry reports, as well as news articles, offer no evidence for a national 9pm calling plan of any popularity in the years prior to 1999. The first national one-rate pricing plan was introduced by AT&T in mid 1998 according to an S&P Industry Survey. Other major providers quickly followed suit. It was after this innovation that national two-tiered plans proliferated and only gradually did plans converge to a 9pm switching threshold. Moreover, due to low ownership and low usage (due, for example, to unwieldy handsets, poor coverage, and high prices), the absolute number of subscribers, as well as absolute minutes of cellular use, associated with any plan prior to 1999 is modest. We discuss the implications of low ownership and low monthly usage below. Accordingly, we treat the years prior to 1999 as a control for the analysis.

Next we turn to direct evidence on the change in call likelihood from data on actual cellular calls. We demonstrate that calling patterns conform to the patterns in marginal pricing suggested by the plan counts above.

Call Likelihood. Does the existence of a sharp change in marginal pricing lead to a corresponding change in the propensity to call? A Pew Research Center survey of 1,503 people in 2006, reports that 44% of cell phone users delay their calls until they did not count against their allotment of peak minutes.¹⁹ In another survey of 30,000 cell phone users, those who exceeded their allotment were subject to “overage” fees which, on average, amounted to 50 to 60% of their usual bill.²⁰ These surveys suggest that the price threshold during weekday evenings was salient for many users.

We explicitly test for the correspondence between the change in call price and usage at the plan threshold with a rare, proprietary, dataset of cellular calls made by callers in

¹⁹Survey conducted by the Pew Research Center and published online in the Pew Internet and American Life Project in April 2006.

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

moving vehicles during an eleven day period in 2005.²¹ The region is bounded by coverage of a single cell phone “switch” which consists of 300 to 400 cell phone towers in a highly populated area of California. The data is then restricted to calls routed through multiple cell phone towers.

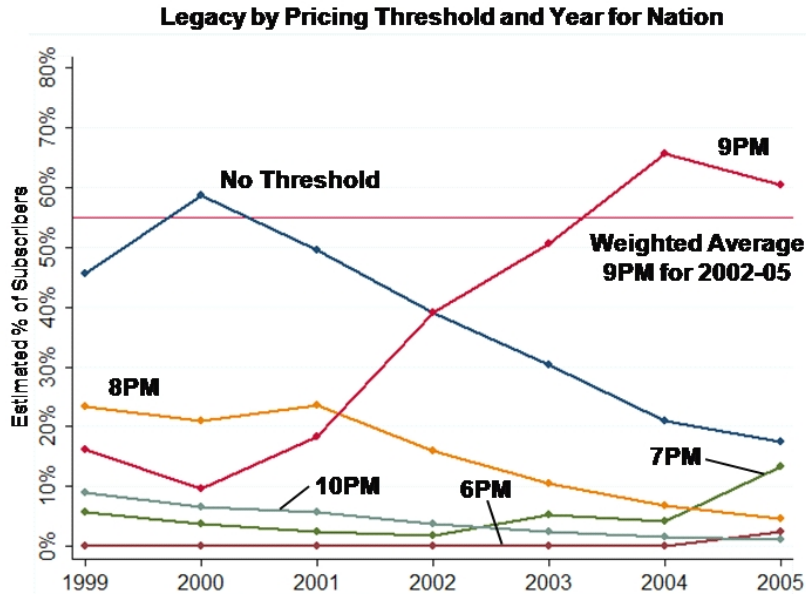


Figure 4, Estimated Legacy across National Markets for Plans by Pricing Threshold and Year for 1999 to 2005

The mechanics of signal switching are such that a call will originally be routed by the tower emanating the strongest signal. A call will be rerouted through a second tower only when the differential in signal strength between the old and a new tower exceeds a certain threshold. Due to this threshold switching design, signals of stationary or even ambulatory callers are almost always routed by a single tower. Engineers from the network provider estimate that a given caller must travel at least approximately 2 miles before a call will switch towers.²² Rare exceptions to this rule may exist when a caller is walking through a region with large buildings that interfere with a given tower’s reception. However, our data is from a switch which covers a region just outside of a downtown area and thus avoids calls made by ambulatory callers within the city center. According to a provider engineer,

²¹Data on call volume is difficult to acquire. Providers 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. Our data, filtered as it is to reflect only moving callers, is, to our knowledge, the first such data made available to academic researchers.

²²This implies that longer duration calls and calls from drivers on speedier roads may be overrepresented in our call data. Usage evidence from the NJ turnpike study (Johnson et al. 2004) suggests that drivers on high speed roads may be more cautious in their use of cell phones. If so, the observed rise in call volume in our data may be understating the effect for drivers on slower roads.

an expert in call traffic, less than .1% of the calls in our sample are from callers outside of moving vehicles.

Therefore, our dataset almost certainly comprises calls made or received by callers in moving vehicles. While volumes are scaled for confidentiality, we can calculate that the data consists of 275,000 to 1.24 million minutes of cell phone use.²³ Assuming an average duration of 2.6 minutes per call, this translates to 106,000 to 477,000 calls made during the course of the 11 days.²⁴ The eleven days of calls represents the longest near-continuous period in 2005 during which data could be retrieved from the archives.²⁵

Importantly, the fraction of users that subscribe to 9pm plans for the provider for which we have direct data in 2005 is both lower than the same fraction for other providers in 2005 and is also lower than the overall fraction across all providers in 2002 to 2005 according to our analysis of plan counts and our calculation of legacy shares. Our provider began offering a highly publicized alternative pricing plan in 2004 which featured an earlier switching hour. While we cannot disclose the details of this calculation for confidentiality, the ratio of the rise in call volume at 9pm and at this alternative hour, in our first stage data, is in approximate proportion to our estimate of the ratio of the legacy share of callers associated with both of these thresholds. Therefore, our observed first stage is, in this sense, a lower bound of the rise in call likelihood for the broader range of providers and years.

Figure 2 depicts call volume for moving callers for each minute from 8 to 10pm for Mondays to Thursdays, Fridays, and the weekend across the sample. A vertical line marks the 9pm threshold at which time the marginal price of calls on weekdays—but not weekends—drops sharply. Critically, the figure reveals a discontinuity in the likelihood of making a call on Mondays to Thursdays at 9pm as compared to weekends and Fridays.²⁶

Why might callers treat Friday as distinct from other weekdays? This behavior is evident in two additional datasets of cellular calls documented in the Appendix. One speculates that this pattern in calls may be due to the lessened salience of the price change on Fridays as compared to other weekdays. If callers are mindful that on 9pm on Friday they are on the precipice of 50 to 60 hours of off-peak pricing, they may choose to smooth calling moreso than at other weekday thresholds where they face far fewer (particularly waking) hours of off-peak calls.

A plausible skeptic might contend that some fraction of the callers in our dataset are

²³The calculation is based on knowing that the scaling factor is an integer from 2 to 9.

²⁴The average call duration of 2.6 minutes is calculated from another large dataset of cellular calls that is described in detail in the appendix (TNS).

²⁵More precisely the calls are from a continuous 14 day period, but there are three days for which no data could be extracted.

²⁶A regression analysis confirms that we can reject the null that the rise in call likelihood on Fridays is equal to the analogous rise on other weekdays.

passengers as opposed to drivers. While this is likely true, it is important to note that we rely on this data only as a measure of relative likelihood amongst moving callers across the day and specifically at 9pm. We infer average call likelihood from the extensive literature that surveys such use. The composition of the data is a concern if passenger callers are *differentially* more sensitive at 9pm to price changes than driver callers.

We can calculate the robustness of the driver first stage to the possibility that passengers are more responsive to prices than their driving counterparts. While we do not have direct data on the price sensitivity or baseline call likelihood of passengers, we do have extensive evidence on drivers in single as compared to multiple occupant vehicles. NOPUS reports that drivers in vehicles without passengers are 4 times more likely to be cellular users (NHTSA 2006). If phone use is heightened, as both intuition and data suggest, for single occupants as compared to accompanied occupants, then we can initially assume that the baseline call likelihood of passengers is equivalent to that of drivers in multiple occupant vehicles. One may reasonably have competing intuitions as to whether passengers are more or less price sensitive at 9pm than such drivers. However, using data on average vehicular occupancy, a calibration indicates that the magnitude of the first stage for drivers effectively drops from 7.2% to 6.8% if passengers are twice as price sensitive as drivers. If passenger baseline likelihood is also twice as high as assumed, then the effective first stage drops to 6.4%. Finally, if baseline likelihood and price sensitivity are both *three times* as high as driver reference points, the magnitude of the first stage drops to 5.4%. As outlined in the discussion, an attenuation of this degree does not alter the interpretation of the second stage results.²⁷ To the extent that calls by passengers also serve as distraction to drivers, even differential price sensitivity between drivers and passengers may not be cause for concern with respect to the research design.

To formally estimate the size of the break in call likelihood in the hour following the Mondays to Thursdays pricing threshold, we estimate the following OLS model:

²⁷We calculate the effective driver first stage in the case of differential price sensitivity with (1) the share of single and multiple occupant vehicles on the road (2005 crash data for California indicates that 23% of the 970,000 vehicles in the sample are multiple occupant), (2) the baseline call likelihood of drivers in both vehicle types from NOPUS (13.3% and 3.3%, respectively, after handheld figures are scaled to account for handheld and hands-free use), (3) an initial assumption that passengers share the calling norms of their accompanying drivers, (4) and finally the assumption that multiple passengers are not on the phone simultaneously. The calibrations imply that even if baseline likelihood and price sensitivity are 2x higher for passengers than their driver counterparts, the effective first stage is 6.4%. If likelihood and price sensitivity are 3x higher for passengers, then the effective first stage is 5.4%. To illustrate the calculation for a 2x increase in both parameters, note that the passenger share of mobile individuals on the road is 19% (i.e., $.23 / (.77 + .23 + .23)$). Given the baseline likelihood across occupants, and an assumption of 2x higher passenger likelihood, the passenger share of total cellular usage is 13% (i.e., $2*(.19*.033) / (2*.19*.033 + .19*.033 + .63*.134)$). Next, if x is the rise in driver call likelihood at 9pm, and we further assume that passengers are 2x as price sensitive as drivers, then, $0.87x + 0.13*2x = 7.2\%$ which implies an effective driver first stage of $x = 6.4\%$. It is worth noting that if one believes that cellular use by passengers is distracting, the figure should be treated as a lower bound of this exercise.

$$\ln(\text{Calls}/\text{Traffic})_t = \alpha + \gamma \text{After9pm}_t + \varepsilon_t$$

where Calls_t denotes scaled calls for each minute t , and Traffic_t represents the traffic count for the region of consideration at each minute. We acquire traffic data from several thousand traffic counters located on roadways in the California region corresponding to the call data.²⁸ After9pm_t is a dummy variable indicating whether the crash occurred on or after 9pm and is the explanatory variable of interest. The model is estimated from 8 to 10pm separately for Mondays to Thursdays, Fridays and weekends.²⁹ Note that, due to the log specification, the scaling of the cellular call data now becomes immaterial to the estimated coefficient of interest.

The top panel of Table 2 reports the results of this analysis. The upper panel of the table confirms the pattern evident in the figures—call likelihood increases by 7.2% from 9 to 10pm on Mondays to Thursdays. There is a sharp local rise in call likelihood at 9pm and this rise appears to persist until at least 10pm. While changes to call likelihood away from the threshold could potentially be due to changes in factors such as driver composition or driver willingness to call that are unrelated to price, the size and stability of the increase is consistent with price playing a sustained role in the heightened likelihood. Moreover, comparing likelihood from 9 to 10pm on weekends to Mondays to Thursdays suggests that, if anything, the price change may temper what might otherwise have been a late evening decline in call likelihood. Fridays feature a smaller, but still statistically significant, rise in call likelihood.

Our analysis relies on comparisons between the treatment period to an earlier control period from 1995 to 1998. While we cannot display the equivalent plots nor perform the equivalent analysis of likelihood for the control period, we are persuaded that driver call likelihood did not sharply rise at 9pm for two reasons. First, the control period is characterized by the absence of 9pm calling plans. Second, generally, there is not evidence for a rise in call likelihood across hours not associated with a price change.

To persuade the reader that the observed rise in call likelihood in 2005 is due to price changes, as opposed to other on-the-hour phenomena present even during the unobserved pre-period (e.g., the need to coordinate plans), the lower panel of Table 2 examines the local change in call likelihood for a series of placebo hours not associated with a pricing change. These placebos include weekends at 9pm, proximal hours on Mondays to Thursdays, as well as across a composite of evening hours, excluding 9pm, on all days. While the estimates

²⁸We download traffic data at the 30 second level from a California traffic database, called PEMS, for the relevant region and time. The PEMS database is described in the Appendix.

²⁹The analysis of Fridays relies on traffic data at the 5 minute level since the more disaggregate data was not available for these days. Aggregating calls and estimating this regression at five minute intervals produces a virtually identical point estimate for the coefficient of interest.

are not highly precise, there is no systematic evidence for a rise in call volume for evening hours not associated with a price change.

Table 2
CHANGE IN CALL LIKELIHOOD AT 9PM THRESHOLD

	DEPENDENT VARIABLE - LN(SCALED CALLS / TRAFFIC) PER MINUTE				
	MON - THU			FRIDAY	
	8:00 - 9:59 60 minutes (1)	8:30 - 9:29 30 minutes (2)	8:45 - 9:14 15 minutes (3)	8:55 - 9:04 5 minutes (4)	8:55 - 9:04 5 minutes (5)
After 9pm	0.072*** (0.004)	0.067*** (0.005)	0.082*** (0.006)	0.070*** (0.009)	0.041*** (0.008)
N	N = 600	N = 300	N = 150	N = 50	N = 20

	MON - THU		ALL DAYS	WEEKEND
	8PM	10PM	5 to 10PM (no 9PM)	9PM
	7:55 - 8:04 5 minutes	9:55 - 10:04 5 minutes	x:55 - x:04 5 minutes	8:55 - 9:04 5 minutes
After 9pm				0.025 (0.021)
After 8pm	0.027* (0.015)			
After 10pm		0.006 (0.018)		
After Hour			-0.016 (0.027)	
N	N = 50	N = 50	N = 400	N = 20

Notes: The table estimates the change in call likelihood for moving callers across the pricing threshold and presents a series of placebo and robustness checks. The dependent variable is ln(scaled calls / traffic). Dummy variables denote a crash occurring on or after the hour indicated. The upper panel presents regression results for the change in call likelihood of moving callers from Mondays to Thursdays using varying windows across the 9pm threshold, as well as the local change at 9pm for Friday callers. The lower panel estimates the local change in moving call likelihood for proximal hours (i.e., 8 and 10pm) during Mondays to Thursdays, evening hours from 5 to 10pm, excluding 9pm, across all days, and 9pm on Weekends. All specifications are estimated with OLS at the minute level and control for ln(traffic) which is at the minute level with the exception of the Friday estimates where traffic is only available at the 5 minute level. Robust standard errors clustered by date are reported parenthetically.

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

Note that average call likelihood in the control period is minimal due in part to low ownership (26%), low monthly average usage (300 minutes) and the scarcity of hands-free technology. Given minimal average call likelihood prior to 1999, an increase in call likelihood at 9pm due to some on-the-hour phenomenon does not pose a major concern for the research design. So long as the crash risk associated with cell phone use due to this phenomena is comparable to the crash risk associated with cellular use due to a price

change, one can be agnostic about the cause of the call likelihood increase. To illustrate, suppose that in 1998 the rise at 9pm in call likelihood amongst drivers is 2%. Conservatively allowing for an average call likelihood during this period of 2% yields a net change in call volume of .04%. Producing an equivalent net change in 2005 would require only a .4% rise in 9pm call likelihood.³⁰

Generalizability of First Stage. To what extent is the price sensitivity exhibited in our data generalizable across years, providers, and geography? Two datasets provide additional direct evidence that the price sensitivity of cellular users can be generalized across time, geography, and provider. These data also suggest, as one might intuit, that drivers are less responsive to changes in marginal price than stationary callers. We briefly describe the behavior of callers in these data below and leave the descriptive and analytic detail for the Appendix.

The first additional data set was acquired from researchers at the MIT Media Lab (hereafter, MIT) who implanted surveillance technology in cellular phones in order to track subject movements, interactions, and cellular communication over the course of the academic year.³¹ A total of 65 subjects placed approximately 80,000 outgoing cell phone calls from August 2004 to May 2005.³² Figure A2 depicts a sharp increase in calls made at the 9pm pricing threshold on Mondays to Thursdays but not Fridays or weekends. Regressions, reported in the top panel of Table A3, confirm the pattern evident in the figures—call volume rises by 23% in the hour after 9pm on Mondays to Thursdays but not on Fridays or weekends.³³ Moreover, there are no prominent breaks evident at other surrounding hours. It is worth noting that because the dataset is comprised primarily of students and faculty, it may overstate the price sensitivity of the broader population of contemporaneous callers.

Finally, we appeal to a second, more representative, dataset, assembled by TNS Telecom, of over 741,000 calls made by 9,864 cell phone users in 2000 and 2001 (hereafter, TNS). The data was extracted from cellular phone bills voluntarily submitted from households randomly selected as part of wider survey of telecommunications behavior and attitudes and consequently allows for a within-subject estimation of call usage. While details of the inference are presented in the Appendix, for the subsample of 287 callers (16,900 evening

³⁰We arrive at this calculation by scaling the hypothetical pre-period 9pm rise in likelihood of 2% by the ratio of the 2005 and 1998 average call likelihood (10% / 2%). Average call likelihood in 1998 is not known. However, given the 2000 NOPUS estimate of 4% likelihood, and considering changes in ownership, monthly usage and availability of hands-free technology during the previous two years, one can assume that average likelihood in the pre-period was no more than 2%.

³¹Eagle, Nathan and Alex Pentland, “Reality Mining: Sensing Complex Social Systems,” *Personal and Ubiquitous Computing*, Vol. 10, No. 4, pp. 255-268, 2006.

³²This period reflects the fact that most subjects joined and remained in the sample during the academic year. A small fraction of calls were made in summer months and these were not included.

³³A negative binomial model, which one might advocate due to the high number of 0 call hours, produces similar estimates (i.e., a 23.4% call rise for Mondays to Thursdays, and nearly identical point estimates for Fridays and weekends).

calls) with a cleanly identifiable switching threshold from 6 to 10pm, we find that callers in the TNS data are also highly sensitive to call pricing during non-Friday weekdays. The bottom panel of Table A3 reports that the relative rise in call volume in the hour subsequent to the pricing thresholds is 23% on Mondays to Thursdays and is smaller and statistically insignificant on other days.³⁴

Collectively, these data document the price sensitivity of cell phone users across a variety of caller types, geographies, providers, time periods and even pricing plans. While drivers may be less sensitive to a change in prices than the more general population of cellular users, we have no reason to believe that such sensitivity is a particular artifact of the region and time which characterizes our first stage data.

3.3 Change in Crash Rate at Price Discontinuity

Do crash rates respond to the increased cellular usage induced by a change in prices? We turn to this question next.

Reporting Bias. A well recognized drawback of using a crash database based on self-reports is the presence of substantive periodic heaping. Our data on the universe of crashes for selected states comes from the State Data System (SDS) administered by the NHTSA and described in the Appendix. The trajectory of a crash record helps to illuminate the origins of this bias in the SDS database. Once a vehicular crash is reported, a police at the scene documents various details of the incident, including the minute of the crash occurrence, and submits the paperwork to one of several possible state agencies. States, however, vary in the specifics that govern data collection and crash qualification criteria. Crash records are ultimately centralized and sent once a year to the NHTSA where they are standardized and maintained. Any bias which is likely to occur then, may vary in severity across states as well as over time. Figure 5 illustrates the nature of the heaping that characterizes a representative hour in 2005 across the states in our sample. A close examination indicates that nearly 11% of crashes are reported to have occurred exactly on the hour. About 31% 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.

The periodic heaping in crash reports complicates a standard regression discontinuity design. In principle, one should be able to describe the change in crashes induced by a fall in prices at the threshold by fitting lines, or higher order polynomials, on either side of 9pm on weekdays in recent years. The challenge, however, is to disentangle the on-hour

³⁴To test for the concern that the rise in calls at the switching threshold may be counterbalanced by a fall in call duration, we test for and find no evidence for a statistically significant fall in call duration at the threshold.

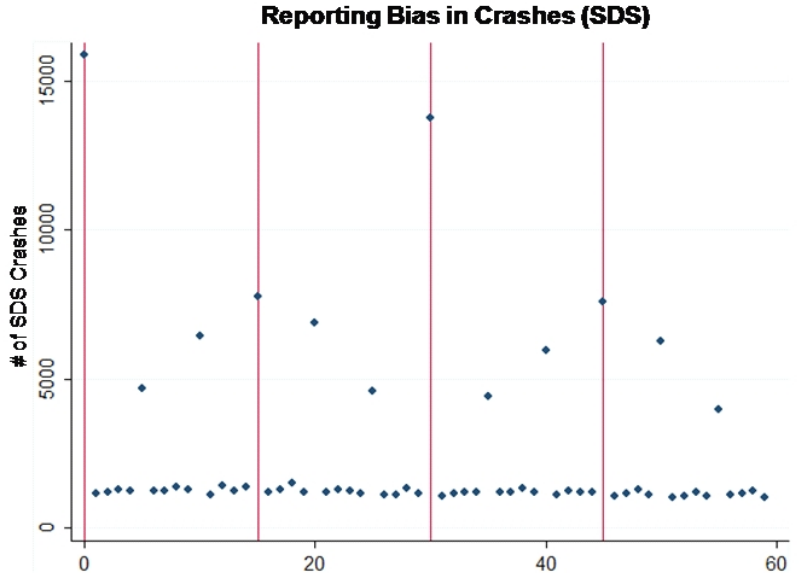


Figure 5, SDS Crashes by Minute in 2005

spike in reported crashes from changes in actual crashes at the threshold. One might be tempted, in a regression framework, to control flexibly for the reporting bias by including dummy indicators for minutes ending in 0 or 5. However, such a strategy yields imprecise estimates of the change in crashes at 9pm. The reason for this imprecision is that the on-hour spike due to the reported bias is an order of magnitude larger than any plausible change in the crash count induced by higher cellular use.

One strategy through which to deal with this complication is to smooth the count data by choosing a unit of analysis which aggregates crashes into larger minute bins (e.g., intervals of 30 or 60 minutes). However, aggregation introduces its own imprecision in the estimates due to changing patterns in driving behavior away from the threshold. As a result, we rely on a double difference approach in addition to smoothing in order to adjust for the observed heaping and to increase the precision and accuracy of the estimates. In a series of robustness checks we alter the strategy used to bin crashes and show that our results are insensitive to the treatment of reporting spikes at each hour, half-hour, or even five minute intervals.

Crash Analysis. We turn first to the distribution of crashes around the pricing threshold in California for 2005. Implicit in this initial test is the assumption that the driver behavior reflected in the first stage sample is representative of the broader population of drivers across all of California in 2005. A comparison of ownership, using FCC data, reveals that, in 2005, ownership in the region associated with our first stage (78%) is slightly higher than statewide ownership (68%) and is on par with other metropolitan regions within

the state.

Figure 3 displays the pattern of average crashes across 10 minute intervals from Mondays to Thursdays in California from 8 to 10pm in 2005 as compared to the analogous pattern for the pre-period from 1995 to 1998. The cyclicity of the plot is due to the reporting biases discussed above. The vertical line marks the 9pm pricing plan threshold. Figure A3 in the Appendix compares Monday to Thursday crashes in California from 8 to 10pm in 2005 to a second control of weekend crashes. Neither plot provides evidence for a relative rise in crashes at the pricing threshold.

We formally estimate the relative change in crashes around 9pm on Monday to Thursdays with the following Poisson model:

$$E[Crash_{symdtb} | \cdot] = \exp(\alpha + \beta(Post_y * After\ 9pm_b)_{yb} + \gamma_1 After\ 9pm_b + \gamma_2 Post_y + \phi_s + \eta_y + \delta_m + \xi_d)$$

where $Crash_{symdtb}$ denotes the fatal crashes in state s , year y , month m , day of week d , date t , and minute bin b . $Post_y$ indicates whether the crash occurred in the treatment period where there is a shift in pricing at 9pm, and $After\ 9pm_b$ is a dummy variable indicating whether the crash occurred on or after 9pm. The interaction term $(Post * After\ 9pm)_{yb}$ is the explanatory variable of interest. The model controls for state, year, month and day of the week specific variation. The regression is estimated with a Poisson specification.³⁵

Intuitively, the experiment simulated by this regression is a comparison of the difference in pre and post crashes around the threshold for symmetric estimation windows around 9pm from Mondays to Thursdays. We initially estimate a baseline regression of daily crash counts for 60 minute windows before and on/after the threshold in California from 8 to 9:59pm each day in 2005 as well as the control period from 1995 to 1998. In addition, we estimate the model for 30 minute windows from 8:30 to 9:29pm. The narrower estimation window around 9pm is less likely to be confounded by unobservable changes in pre and post trends before or after the threshold, but is more sensitive to the problems raised by the reporting biases documented above. As such, standard errors actually increase for the tighter estimation windows.

The choice of the control period is dictated by the low prevalence of 9pm plans and low average call likelihood prior to 1999, as well as the trade-off between the added precision, and the possibility of introducing bias, associated with a lengthier period. Our estimation results are robust to control periods of alternative lengths.³⁶

³⁵The estimation choice is dictated by the highly non-normal shape of the crash count distribution. Many of the cells contain 0 fatal crashes. Our results are also robust to estimations based on alternative specifications (e.g. the linear probability model, and negative binomial regression).

³⁶Results of these estimations are available from the authors upon request. Note that 1990 is the earliest

Next, to heighten the precision in the regressions, we estimate the model for an extended period from 2002 to 2005 during which we are confident that most individuals were on cell phone plans characterized by the 9pm break and for which our first stage is representative. Finally, we estimate the model for the full set of states for which we have crash data. This extended analysis includes California, Florida, Illinois, Kansas, Maryland, Michigan, Missouri, Ohio and Pennsylvania.³⁷ Assuming driver behavior with respect to crashes and cellular use is not qualitatively different across geographic regions, including additional states in the model allows us to add even more precision to the results. Figure A4 depicts the distribution of crashes in the pre and post period for the expanded sample.

The extension of the analysis to drivers outside of California in 2005 is, at least in part, justified by the MIT and TNS call data that asserts that price sensitivity of callers generalizes across geography and time periods. Moreover, cellular ownership for the region associated with the first stage (78% in 2005) is in the same range of ownership nationally (71%).

The upper panel of Table 3 provides regression results for crashes in California. The first two columns report near-zero and insignificant point estimates for the interaction term of interest for both the baseline and the more narrow 30 minute window. The next two columns present analogous, but much more precise, results for a broader time period stretching from 2002 to 2005.

Our estimation approach relies on the constancy of important covariates across the threshold in the post-period relative to the pre-period. We can explicitly test this assumption for traffic and reporting bias in California. First, we estimate the double-difference of log traffic counts in a manner consistent with the above analysis, for 2005 as well as 2002 to 2005. We find no evidence for a significant change in traffic across the 9pm threshold relative to the control period.³⁸ Second, to verify the constancy of reporting bias, at least for the 30 minute estimation, we test for a change in the fraction of total crashes reported within the first 30 minutes of each hour in the post as compared to the pre-period. Again, we cannot reject the null that this fraction is identical across periods.

possible bound for a control period due to data availability.

³⁷Some state-years are missing from the SDS data or do not report the time of accident which is required for our analysis. Specifically, Illinois is available only from 1996 to 2003, and Pennsylvania is missing data for 2002. The variability in data availability is in part due to the fact that the SDS must ultimately rely on each state to provide its own crash records.

³⁸We estimate the double-difference regressions of log hourly traffic counts at the traffic station x date level in California for 8 to 10pm, Monday to Thursday, in the pre and post period(s). The coefficient of interest is $b = .0040$, $se = .0045$ for 2005 and $b = -.0012$, $se = .0042$ for 2002 to 2005. The regressions include fixed effects to control for station, year, month, and day of week specific variation. Errors are robust and clustered at the date level.

Table 3
RELATIVE PRE-POST (MON TO THURS) CHANGE
IN CRASH RATE AT 9PM THRESHOLD

DEPENDENT VARIABLE - CRASHES PER MINUTE BIN				
CALIFORNIA				
	2005		2002 to 2005	
	8:00 - 9:59 60 mn bin (1)	8:30 - 9:29 30 mn bin (2)	8:00 - 9:59 60 mn bin (3)	8:30 - 9:29 30 mn bin (4)
Post x After 9pm	-0.001 (0.019)	0.004 (0.024)	-0.010 (0.012)	-0.012 (0.016)
N	N = 2088	N = 2088	N = 3342	N = 3342
EXPANDED STATES				
	2005		2002 to 2005	
	8:00 - 9:59 60 mn bin	8:30 - 9:29 30 mn bin	8:00 - 9:59 60 mn bin	8:30 - 9:29 30 mn bin
Post x After 9pm	-0.014 (0.013)	-0.003 (0.015)	-0.006 (0.008)	-0.002 (0.010)
N	N = 17960	N = 13784	N = 28410	N = 21726

Notes: The table presents the double difference estimate of the change in crashes at 9pm on Mondays to Thursdays in the post (i.e., 2005, and 2002 to 2005) relative to the pre-period (1995 to 1998). The "Post*After9pm" dummy denotes crashes occurring on or after 9PM in the post period. The upper panel presents the results for California. The first two columns estimates the model using 60 and 30 minutes bins respectively for 2005, while the next two columns presents analogous results for 2002 to 2005. The bottom panel provides comparable results for the expanded set of states for which data is available: California, Florida, Illinois, Kansas, Maryland, Michigan, Missouri, Ohio and Pennsylvania. Pennsylvania is missing data for 2002 and Illinois data is missing 1995, 2004, and 2005. Michigan and Ohio are excluded from the 30 minute specifications due to the absence of minute level crash data. All specifications are Poisson regressions run at the state x date level. Fixed effects control for state, month, year and day of week specific variation in crash rates where appropriate. Robust standard errors clustered by date are reported parenthetically.

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

Finally, in the lower panel of Table 3, we present regressions for the expanded set of states. Illinois is excluded from the 2005 analysis since no data is available for that year. The last column, with estimates for 30 minute windows, excludes Michigan and Ohio since these states provide the hour, but not minute, of each crash prior to 2000. Extending the sample to multiple years reduces estimated standard errors but does not substantively change the point estimates.

Our favored specifications are for the expanded set of states and imply an upper bound of the relative change in the crash rate of .97% for 2002 to 2005 and 1.18% for 2005.³⁹

³⁹Note that for coefficients near zero, the interpretation of a Poisson regression is similar to that of a percent change. Upper bounds of point estimates using a 95% confidence interval were produced by our

Overall, the results provide no evidence for a positive relative change in the crash rate.⁴⁰

We repeat our benchmark analysis for the subset of fatal crashes. Our data is from the NHTSA’s Fatality Analysis Reporting System (FARS) and is described in the Appendix. A benefit of expanding focus to fatal crashes is that, unlike the SDS data, it extends to all 50 states. A (statistical) drawback is that fatal crashes are 150 times less frequent than their non-fatal counterparts with just under 40,000 incidents per year. Moreover, the recording of fatal crashes suffers from the same reporting bias with large spikes on the hour and the half hour. Consequently our estimates are substantially noisier. The double difference estimate for change in fatal crashes at 9pm on Mondays to Thursdays in 2002 to 2005 compared to 1995 to 1998 is actually negative and marginally significant ($b = -0.058$, $se: 0.033$) The corresponding placebo estimate for weekends is slightly positive and insignificant ($b = 0.028$, $se: 0.042$).⁴¹

Placebo and Robustness Checks. Table 4 reports the results of a series of placebo and robustness checks for the expanded year and state model. The first four columns of the upper panel present results of the baseline crash analysis for the 8 and 10pm hours for 30 and 60 minute windows. The final two columns of the panel report estimates of the model for weekends, using 30 and 60 minute windows around 9pm. The analysis confirms the absence of a strong negative change in the crash rate around the threshold for weekday proximal hours, or weekends at 9pm, that could mask a potential effect of cellular use at 9pm. Additionally, we estimate, but do not report in the table, triple difference estimates, using the change across proximal hours and 9pm on weekends, as additional controls.⁴²

The first column of the lower panel of the table estimates the baseline specifications for a smaller window of 15 minutes. Despite being subject to considerable on-hour reporting biases, the estimate for the smaller window is comparable to estimates for the lengthier statistical program but can also be calculated manually using the delta method.

⁴⁰One important assumption in the difference-in-difference analysis is that the trend in crashes is parallel in the pre and post periods. As evidence for this identifying assumption, we test whether the crash rate in the post and pre period have similar linear trends for varying windows around 9pm. Poisson regressions test this assumption by modeling crashes across 1, 15, 30 and 60 minute bins as a function of pre and post period specific linear time-trends and controls for day of week, month and year specific variation. We fail to reject the null of differential trends for any reasonable level of significance and for varying time windows around 9pm. Results of these estimations are available from the authors (also see Figures 3 and A4).

⁴¹Just as in our benchmark analysis, a Poisson model estimates regressions at the state-date-bin level. We examine 60 minute bins before and after 9pm in 2002 to 2005 using 1995 to 1998 as a control period. We include fixed effects to control for variation across state, year, month and day of the week. Due to the large number of zero crash counts, we also estimate a negative binomial model and the results remain largely unchanged.

⁴²We amend the expanded year and state model to calculate these triple difference estimates. The resulting coefficient of the net change across the 30 minute window around 9pm is $b = -.0082$, $se = .0138$, when using the 10pm hour as a double difference control, and is $b = .0004$, $se = .0135$, when using 8pm as a double difference control. We cannot produce the analogous triple difference using a 60 minute window for proximal hours without overlapping estimation periods. The triple difference estimate when using weekends as a double difference control, across 60 minute windows, is $b = -.0177$, $se = .0147$.

windows but is less precise.

Table 4
RELATIVE PRE-POST (MON TO THURS) CHANGE AT 9PM -
PLACEBO AND ROBUSTNESS CHECKS

DEPENDENT VARIABLE - CRASHES PER MINUTE BIN						
PLACEBO CHECKS EXPANDED STATES, 2002 to 2005						
	8PM		10PM		WEEKEND	
	7:00 - 8:59 60 mn bin (1)	7:00 - 8:59 30 mn bin (2)	9:00 - 10:59 60 mn bin (3)	9:30 - 10:29 30 mn bin (4)	8:00 - 9:59 60 mn bin (5)	8:31 - 9:29 30 mn bin (6)
Post x After 9pm	0.006 (0.007)	0.002 (0.009)	-0.006 (0.007)	0.007 (0.010)	0.011 (0.012)	0.010 (0.013)
N	N = 28410	N = 21726	N = 28410	N = 21726	N = 14176	N = 10840
ROBUSTNESS CHECKS EXPANDED STATES, 2002 to 2005						
	8:45 - 9:14 15 mn bin	8:01 - 10:00 60 mn bin Start Bin :01	8:01 - 9:59 59 mn bin No :00	8:01 - 10:00 58 mn bin No :00, :30	8:01 - 9:59 48 mn bin No :05s	8:31 - 9:29 24 mn bin No :05s
Post x After 9pm	0.001 (0.013)	-0.013 (0.008)	-0.011 (0.009)	-0.004 (0.009)	-0.006 (0.011)	-0.017 (0.015)
N	N = 21726	N = 21726	N = 21726	N = 21726	N = 21726	N = 21726

Notes: The table presents results from placebo and robustness checks of the double difference crash estimates for the baseline specification of expanded states from 2002 to 2005. The upper panel presents results from a series of placebo estimates for Mondays to Thursdays at 8pm and 10pm as well as for 9pm on weekends. All specifications are presented for both 60 and 30 minute windows across the threshold. The lower panel presents results from a series of robustness checks. The first column provides the double difference in relative crash change for a smaller 15 minute window around the 9pm threshold on Mondays to Thursdays. The remaining columns present the baseline estimate but after modifying the way in which the reporting bias is handled. In the second column, the 9pm spike is included in the bin preceding rather than following 9pm for a 60 minute estimate, while the final four columns drop crashes at intervals as specified (these estimations *exclude* on-hour crashes) for small and large windows around the threshold. The expanded sample is identical to that described in Table 3. Robust standard errors clustered by date are reported parenthetically.

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

The remaining columns of the lower panel present estimates for the standard windows after modifying the strategy used to allocate crashes to bins before and after the threshold. The new allocations are meant to address the possibility that the double difference approach does not adequately correct for the reporting bias. Accordingly, in Column 2, we shift the minute bin so that crashes reported from 8:01 to 9:00 are treated as having occurred prior to the threshold while crashes reported from 9:01 to 10:00 are treated as having occurred after the threshold. The next three columns of the panel estimates the baseline specification but after *eliminating* crashes reported at regular intervals that may be subject to reporting bias. First, crashes at exactly at 8:00 and 9:00 in both the pre and post periods are eliminated,

then crashes occurring at 8:30 and 9:30 are also eliminated, and finally, crashes occurring at every 5 minute increment are eliminated. Omitting these data points, in which many of the crash reports are concentrated, does little to change the underlying pattern in point estimates but does produce greater imprecision. The final column of the second panel eliminates each 5 minute increment but for the shorter 30 minute window.

As a final test of robustness, we conduct, but do not report, separate regressions for each day of the week from Monday to Thursday and find no evidence for positive and significant crash increases. The robustness and placebo checks offer no evidence for the existence of a confounding factor or measurement idiosyncrasy that might mask a positive change in the relative crash rate at 9pm.

In summary, the 9pm pricing analysis provides no evidence for a relative increase in crashes at the threshold. The point estimates for the change in relative crash rates across the threshold are consistently near zero. The upper bound of the estimated relative change is .97% in the fully expanded specification and 1.18% for the expanded set of states when considering only 2005. These figures both imply that the relative crash risk associated with cellular use is 3.0. This assumes an average call likelihood of 7.8% across 2002 to 2005 and 10% in 2005, as well as the 7.2% increase in driver call volume indicated by the first stage analysis.⁴³ These upper bounds reject the RT estimate of a 4.3 fold increase in crash risk due to cell phone use. We now describe two additional empirical strategies that produce results consistent with these findings.

3.4 Panel Analysis of Ownership, Crashes and Legislation

Two alternative empirical approaches supplement our basic results. Full details of these approaches are provided in the Appendix. In the first, we compare aggregate trends in crashes and cellular ownership at the level of the state and Economic Area (EA). EAs are used by the FCC to denote regions of contiguous economic activity and represent the most disaggregated geographic units for which data on cellular ownership data is available. Our data includes the universe of crashes for approximately 60 EAs across nine states from 1990 to 2005, and for the universe of fatal crashes for all states from 1989 to 2007. Using a panel regression with flexible controls for region and time trends, and a control period during which we know that ownership is trivial, we show that there is no statistically significant link between change in ownership and crashes.

In an second, related, approach, we estimate the influence of recent legislative bans restricting handheld cellular use by drivers in New York, New Jersey, Connecticut, as well

⁴³It is worthwhile to note that the estimated behavioral response at 9pm is based on changes in cellular usage rather than changes in cell phone ownership. This complicates the translation of the regression estimates to a relative crash risk. This concern can be allayed with a simple assumption equating the effects of increased usage with increased ownership.

as the large municipalities of Chicago and Washington D.C. Recognizing that the effect of legislation on crashes is determined by both compliance as well as crash risk associated with handheld cellular use, we use a panel analysis to trace the monthly time-path of fatal crashes following the imposition of the bans. The analysis suggests that the legislation did not lead to a significant reduction in the fatal crash rate over short or longer run horizons.

4 Discussion

Sensitivity of Results to Assumptions. The present analysis suggests the counter-intuitive finding that cell phone use by drivers is not associated with higher crash rates. Whether the upper bounds from our analysis are able to reject existing research depends on the value of two key parameters associated with drivers—average call likelihood and the increase in call likelihood at 9pm.

A first key parameter relates to the average call likelihood at 9pm during the treatment period. Evidence exists that usage during the evening is no lower than average use across the day. Data on use during the day is extensive. NOPUS reports daytime usage, defined from 8am to 6pm, is 10% by 2005. Average daytime use across 2002 to 2005 is 7.8%.

There are three pieces of evidence that speak to the relationship between evening and daytime use. Two studies, to our knowledge, have considered evening usage and both suggest that cellular use in the evening is no different than it is during the day (Vivoda et al. 2008, Johnson et al. 2004). The most direct evidence of relative cellular usage across times of the day is from our own first stage data of 106,000 to 477,000 phone calls from 2005. A minute level regression of the natural log of indexed call volume divided by traffic, for the hours from 8am to 6pm and 8 to 9pm on Mondays to Thursdays, on an indicator signalling inclusion in the 8 to 9pm hour, suggests that cellular usage, as a fraction of traffic, from 8 to 9pm is significantly higher than the average use during the NOPUS day ($b = .317$, $se: .004$). A similar estimation indicates that usage at precisely 9pm is also significantly higher than over the NOPUS period ($b = .353$, $se: .020$). Together, the evidence suggests that the NOPUS estimates of daytime usage are legitimate, and even conservative, proxies for use during the late evening.

A second key parameter regards the rise in cellular call likelihood at 9pm. The first stage data indicates a 7.2% increase in call likelihood from Mondays to Thursdays at the pricing threshold for likely drivers. Our calculation of legacy shares indicate that this data is from a provider and a period which almost certainly underrepresents the fraction of users at 9pm as compared to other providers in 2005 or across 2002 to 2005. Even allowing for differential price sensitivity across vehicular passengers and drivers, we believe that the 7.2% represents a conservative estimate of the change in call likelihood for drivers at the

pricing threshold.

As additional context for the magnitude of this price sensitivity, it does appear that drivers are less price sensitive than the broader population of cellular users. While driver and non-driver elasticities may not be strictly comparable, two additional data sets of callers (MIT and TNS) provide evidence for an over 20% rise in general call likelihood at pricing thresholds in the period under consideration.

To explore the sensitivity of our findings to the assumptions laid out above, Table 5 compares the relative crash risk implied by the upper bounds of our estimates across a range of values for average driver call likelihood and the change in call likelihood at 9pm. The table relies on the analysis of the set of expanded states during 2002 to 2005 as well as just 2005. The latter estimates, while less precise, correspond to a period of higher average call likelihood. The table is centered at our best estimate for each parameter. For example, if average call likelihood is 7.8%, 9pm call likelihood rises by 7.2%, and the estimated upper bound for the change in the crash rate at 9pm is .97%, then our analysis implies an upper bound in the relative crash risk of 3.0.⁴⁴

Shaded regions of the table indicate upper bounds which reject the 4.3 suggested by RT. Fixing the change in likelihood at 7.2%, given an average call likelihood as low as 6%, the 2002 to 2005 analysis would imply a crash risk of 3.6 which is below RT. Fixing average call likelihood at 7.8%, a first stage rise in likelihood of 5.2% would imply an upper bound of 3.9. One can similarly gauge the sensitivity of the calibrations for the 2005 estimates. Moreover, to the extent that dialing intensity jumps discontinuously at 9pm, assuming dialing is more dangerous than simply talking, then the pertinent baseline crash risk from the existing literature may be higher than 4.3.

What might explain the departure of our results from RT? The RT study suffers from three principle drawbacks. The first is that it relies on an unrepresentative sample of those involved in a recent crash (Hahn and Prieger 2006). Selection implies that the RT result is at best an upper bound for the population of drivers as a whole. Second, there is the possibility that the RT result is confounded by a factor such as driver anxiety which prompts both cellular use as well as higher crash risk.⁴⁵ Finally, given the lack of precision

⁴⁴The change in the indexed crash rate at 9pm, is the sum of the change due to cellular users and non-users: $\Delta\%CrashRate_t^{UB} * CrashRate_t = \Delta CrashRate_t^{Cell} + \Delta CrashRate_t^{Non-Cell} = [x * \Delta CellUse_t] + [1 * \Delta NonCellUse_t]$. $\Delta CrashRate_t^{Cell}$ is the product of the relative crash risk associated with cellular use, x , and the change in normalized cellular use, $\Delta CellUse_t$, which is itself a product of the change in 9pm call likelihood and average call likelihood. $\Delta CrashRate_t^{Non-Cell}$ is simply the product of the crash risk of drivers not on cellular phones, normalized to 1, and the change in the share of drivers that are non-users at 9pm (i.e., $\Delta NonCellUse_t$). Similarly, the baseline crash rate, $CrashRate_t$, is a sum of the crash rates of cellular and non-cellular users prior to 9pm. The populated equation is $.0097[1*(1-.078) + x(.078)] = [x(.072*.078) + 1*(-.072*.078)]$. Solving for x yields 3.0.

⁴⁵Hahn and Tetlock (1999) suggest the possibility of worsening traffic conditions (e.g., poor weather or traffic congestion) as a possible example of this problem.

in timing of crashes, it could be that observed calls may have been placed in response to a crash as opposed to serving as the cause of an incident. We turn next to the mechanisms which might explain the absence of a correlation between crashes and cellular use.

Plausible Explanations for the Effect. If cell phones are a source of distraction, given limits to attentional capacity, how is it that such phones have no, or perhaps very little, influence on crashes? There are a number of plausible explanations for why cell phone use may not raise crash frequency.

One explanation is that drivers who use cell phones compensate for the added distraction by modifying their driving behavior. This so called “Peltzman Effect” was popularized by Sam Peltzman who suggested that the benefits of seat-belt regulations might be offset by riskier driving (1975). It is plausible to imagine drivers who slow down, pull over, shift to uncongested lanes or roadways, or simply devote more attention to driving in response to making or receiving a cell phone call. In the Appendix, we present a simple model that illustrates how compensation is a rational response for drivers who both benefit from, and are distracted by, cellular use.

The evidence for driver compensation under the influence of cell phones is mixed. In driver simulations in the lab, several studies have found that drivers reduce their speeds slightly when subject to either handheld or hands-free use (see Caird et al. 2008 for a meta-analysis of 33 studies).⁴⁶ However, some studies find a higher variance in such speeds (e.g., Rakauskas and Gugerty and Ward 2004), while others find that cellular users actually increase speed (Rosenbloom 2006).⁴⁷ The few studies which examine cell phone distraction in repeated trials find evidence for learning (e.g., Shinar and Tractinsky and Compton 2005).

Given concerns over external validity, one can turn to field studies for alternative insight. There is field evidence consistent with compensation. In a study looking at cellular driving in both field and experimental settings, Mazzae et al. find significant degradation in various driver outcomes in simulated, but not real-life, driving (2004). While this difference may be due to the lack of statistical power, the study also finds, consistent with compensation, that cellular usage is lower when traffic is more congested. The NJ Turnpike study also reports cellular usage at very high speeds (i.e., 15 mph over the speed limit) is 20% lower than moderately speeds and this difference is statistically significant (Johnson et al. 2004).

⁴⁶Caird et al. (2008) estimates that the standardized mean weighted effect size of handsfree use relative to a baseline control is $r = .23$ (with 95% CI of .06 to .40 and composite $N = 495$), while the mean effect size of handheld use relative to the same baseline control is $r = .39$ (with 95% CI of .26 to .52 and composite $N = 160$). The authors, however, characterize this level of compensation as not “appreciable.”

⁴⁷We thank an anonymous referee for bringing this study to our attention.

Table 5

SENSITIVITY OF CRASH RISK IMPLIED BY UPPER BOUNDS OF PRESENT ANALYSIS

9PM Increase in Call Likelihood	CRASH RISK IMPLIED BY UPPER BOUNDS									
	EXPANDED STATES (2002 to 2005)					EXPANDED STATES (2005)				
	Baseline 9PM Call Likelihood					Baseline 9PM Call Likelihood				
	6%	7%	7.8%	9%	10%	6%	7%	7.8%	9%	10%
5.2%	4.8	4.3	3.9	3.5	3.3	5.9	5.2	4.8	4.3	3.9
6.2%	4.1	3.6	3.4	3.1	2.9	4.9	4.4	4.0	3.6	3.4
7.2%	3.6	3.2	3.0	2.7	2.6	4.3	3.8	3.5	3.2	3.0
8.2%	3.2	2.9	2.7	2.5	2.3	3.8	3.4	3.2	2.9	2.7
9.2%	3.0	2.7	2.5	2.3	2.2	3.5	3.1	2.9	2.6	2.5

Notes: This table presents the relative crash risk due to driver cell phone use implied by the upper bound of our benchmark analysis of the 9pm price discontinuity for expanded states and 60 minute windows. The table displays the relative crash risk associated with varying estimates of baseline call likelihood as well as estimates of the increase in call likelihood at 9pm. The relative risk of crashing if using a cell phone while driving can be calculated by solving for x in the following expression: $ub[1*(1-b) + x(b)] = [x(bc) + 1(-bc)]$ where ub is the upper bound on our benchmark result, b is the baseline likelihood of cellular use by drivers, and c is the % jump in likelihood at 9pm. An illustrative calculation is outlined in the text.

A second explanation for our findings is that the drivers who use cell phones have an affinity for risk (Hahn and Tetlock 1999). 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 radios, televisions or DVDs). Prieger and Hahn suggest that driver heterogeneity in riskiness leads most research to significantly overestimate the impact of cell phone use on crashes (2007). Much like our study, they conclude that driver use of cell phones has close to a zero effect on crashes. In another study, authors classified 3,869 Canadian drivers as cellular users and non-users based on observed usage at a single point in time and then collected and analyzed collision records for these vehicles over a 2 to 3 year period (Wilson et al. 2003). The study finds a higher rate of violations for cellular users and attributes this differential to violations associated with aggressive driving, alcohol, non-moving violations, and seat belt non-use rather than inattention.

Finally, the effect of cellular use on crashes may be heterogeneous across drivers.⁴⁸ While the local average treatment effect may be zero, there may be drivers for whom the use of cell phones is detrimental, as well as some drivers for whom cell phones are beneficial. For example, cell phones may actually improve selective driver outcomes by alleviating boredom. The NHTSA reports that 100,000 crashes, and 1500 fatal crashes each year are attributable to driver fatigue or sleepiness (2004), and “The 100-Car Naturalistic Study” concluded that 20% of crashes and 12% of near-crashes were linked to driver fatigue

⁴⁸See Hahn and Prieger (2006) for a model of the heterogeneous effects of cellular use on crashes.

(NHTSA and Virginia Tech 2006). The dangers of fatigue may be particularly pronounced for drivers accustomed to driving long distances or long hours. To this point, the Federal Motor Carrier Safety Administration, in 2003, implicated fatigue as a factor in 13% of all fatal large-truck crashes.⁴⁹

Implications for Welfare and Policy. Cell phones provide economic value to drivers. The Harvard Center for Risk Analysis assessed the value of non-emergency cellular calls by drivers at \$43 billion annually (Cohen and Graham 2003). The Cellular Telephone Industry Association reports that more than 200,000 emergency calls are made by drivers using cell phones every day. Yet despite transparent benefits, a majority of Americans support bans of driver cell phone use and view such devices as a leading threat to public safety. Moreover, a large number of municipalities, states, and even Congress, have either considered or passed legislation restricting driver use of cell phones over the last several years.

In light of the benefits of cellular devices, our results suggest that blanket bans on cellular use are not desirable. A number of additional considerations exist. First, our results represent only local treatment effects of the influence of cellular use around 9pm. A plausible argument is that cellular use at 9pm may be less hazardous than during the day since there is less traffic at night. However, crash rates per vehicle mile travelled are actually significantly higher at night than during the day (NHTSA 2000). This elevated crash risk is likely due, in part, to lower visibility as well as higher average speeds.⁵⁰ To the extent that lower visibility and higher speeds exacerbate the crash risk associated with attentional distractions, cellular use may be just as, or more, deleterious at night as it is at other times of the day.

Second, if drivers compensate for cellular use with more careful driving, then there may be a rationale for penalizing cellular use as a secondary, but not as a primary, offense. Third, given that our results cannot rule out the detrimental influence of cell phones for certain subpopulations, partial bans which target specific drivers may be appropriate. Partial bans of cell phone use by teenagers in several states suggests that policy makers believe in such heterogeneity in risk. More research is needed to clarify whether the influence of cell phones is heterogeneous across drivers (as well as driving conditions).

A number of researchers have made the analogy between the dangers of cellular use and drunk driving. Our analysis is inconsistent with this claim. Our upper bounds easily rule out the crash risk of 7 associated with positive levels of blood alcohol, and the crash risk of 13 associated with illegal limits of blood alcohol (i.e., $> .10$ BAC) (Levitt and Porter

⁴⁹This statistic was reported as a part of the “Report to Congress on the Large Truck Crash Causation.”

⁵⁰Data on average hourly speeds for highway traffic in California from 2005, (collected from the PEMS website described in the Appendix), suggests that speeds from 9 to 10pm are about 6% higher than speeds throughout the rest of the day.

2001a).

Finally, policies aimed at regulating driver cell phone use trade off the benefits of usage against possible harm to person or property. As such, the estimates of our paper can be used to help calculate the statistical value of life implicit in such policies (e.g., Ashenfelter and Greenstone 2004). Our upper bound in relative crash risk of 3.0 implies an upper bound in aggregate crashes of 20% as reported in Table 1. Applying this figure to the annual fatal crash count produces an upper bound of 8,000 fatalities per year. The \$43 billion benefit of cellular use implies that implicit in cellular bans is a lower bound in the value of life of about \$5 million.⁵¹

Laboratory and Field Evidence. This paper is a part of a growing collection of studies which highlight differences and possible complementarity between the laboratory and the field. The finding that cell phone use does not cause crashes is not necessarily inconsistent with laboratory findings that such use distracts. The explanations offered above—compensation, risk substitution, and effect heterogeneity—each highlight differences and relative advantages across laboratory and field settings.

A first difference is suggested by the possibility that cell phones do distract but drivers offset such distraction by driving more carefully. If compensatory behavior is responsible for our results, then naive translation of the mechanisms posited by laboratory findings to policy prescriptions neglects the influence that possible alternative mechanisms, such as self-preservation, may have on behavior. Field studies may be useful in illuminating the presence of alternative mechanisms, or interactions between understood mechanisms and real-world variables, that might be otherwise neglected in the laboratory.

A second difference between the laboratory and the field is highlighted by the possibility that certain drivers substitute across different sources of risk when driving. Driver distraction experiments explicitly compute a treatment effect by comparing the relative likelihood of a crash with and without a cell phone. If real-world cell phone use is concentrated amongst risk-loving drivers who would otherwise engage in activities such as talking to fellow passengers, then the laboratory and field analyses rely on different comparison groups. In such an event, laboratory findings would lead to inflated inferences of the dangers of cellular use (see Dahl and Dellavigna 2009).

Finally, the possible heterogeneity in the magnitude of the effect across drivers hints at a third difference between the laboratory and the field. If some drivers rely on cell phones in order to counteract fatigue, then this self-selection in use will not be captured in laboratory settings where participant fatigue is not randomized.

Our analysis underscores a potential complementarity between laboratory and field

⁵¹ Hahn, Tetlock and Burnett (2000) outline a more detailed method to account for the lifetime costs associated with mortality, injuries, property damage, lost productivity, and medical expenses.

approaches. Specifically, modified experimental studies in the lab could help to arbitrate between the possible explanations suggested by the findings of our natural experiment. For example, by introducing appropriate incentives, one could at least begin to test the hypothesis of compensation in the laboratory. Other laboratory experiments could identify the presence of risk substitution or interactions between cellular use and fatigue.

5 Conclusion

This paper exploits a natural experiment—the discontinuity in the marginal price of a cell phone call during weekday evenings—to estimate the influence of driver cell phone use on vehicular crashes. Using a wide array of data on crashes, ownership, cell phone plans, average call likelihood, as well as rare datasets of actual cell phone calls, we find no evidence that an exogenous rise in call volume, induced by the change in cellular prices, leads to an increase in crashes. This result is at odds with much of the existing research. The most influential study on this topic (RT) suggests that cell phone use results in a 4.3 fold increase in relative crash risk and equates the danger of cellular use to that of illicit levels of alcohol. The upper bounds of our estimates allow us to rule out the crash risk implied by RT under plausible assumptions of average call likelihood. To corroborate our findings, we pursue two additional empirical strategies. Neither of these provide evidence to support the relationship between phone use and crashes.

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

In the least, we believe our findings should renew interest in empirical research examining the effects of cell phone use and reopen discussions on the costs and benefits of policy restricting such use. One direction of future research, which may prove particularly important to policy makers, is to investigate whether the influence of cellular use differs across types of drivers and driving conditions. Our research design allows for such an analysis of driver heterogeneity if one exploits differences in price sensitivity and average call likelihood across demographic groups as an additional source of treatment variation. Finally, our findings could be used to help design future laboratory studies which may shed added light on the link between cellular use and crashes.

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7 Appendix

7.1 Description of Data

Crash Data. This paper relies on a wide array of data. These sources are summarized in Table A1. Each empirical approach depends on data on crash records, as well as data on changes in cell phone ownership. The State Data System (SDS) provides data for the universe of crashes from 1990 to 2005 for California, Florida, Illinois, Kansas, Maryland, Michigan, Missouri, Ohio and Pennsylvania. A handful of these state-years suffer from data limitations ranging from complete unavailability to state-years for which a critical variable is not reported (e.g., Pennsylvania in 2002 and Illinois in 2004 and 2005). The Fatality Analysis Reporting System (FARS) provides data for the universe of fatal crash records from 1987 to 2007 for each of the 50 states. FARS captures any vehicle crash resulting in a death within 30 days of the collision. The SDS and FARS databases are administered by the National Highway Transportation Safety (NHTSA) Administration which collects records from participating state agencies. A total of eighteen states participate in the SDS, but only nine states release crash data which covers a significant portion of the desired time frame.

Figure 1 depicts the trends in crashes, indexed to highway traffic volume, for each year from 1988 to 2007. Data on all crashes in this plot is taken from the General Estimates Survey which is a national probability sample calculated by the NHTSA. The plot indicates a decrease in crashes over the last fifteen years, with a slight rise in the mid-1990s. Much of the drop in crash rates over this period is attributable to the increasing prevalence and usage of safety devices as well as a decline in driver alcohol use. The mild rise in the mid-1990s can be at least partially attributed to relaxation in nationwide speeding regulations (NHTSA 2005). Recently, there have been about 40,000 fatal crashes, and approximately 6 million total crashes reported each year nationwide.

Much of the analysis for the alternative empirical approaches is at the level of the EA. Defined originally by the Bureau of Economic Analysis (BEA), EAs are currently used by the Federal Communications Commission to denote regions of contiguous economic activity. Each of the 172 EAs consists of one or more economic nodes—a metropolitan or micropolitan statistical area that serves as a regional economic center. The BEA uniquely mapped counties to an Economic Area in 2000. We use these mappings to construct EA level crash and population data.

Cell Phone Ownership. Measures of cell phone ownership require data on the number of subscribers as well as the population in a region. Data on cell phone subscribers for each EA from 2001 to 2005 as well as 2007, each state from 1999 to 2007, and nationally from 1985 to 2005 was collected from the FCC and the Cellular Telephone Industry Asso-

ciation. 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.⁵² Overall, both ownership and usage increase exponentially over this period. By 2007, 5 of every 6 residents owned a cell phone despite only 1 of 3 owning a cell phone just eight years earlier.

Call Likelihood. The central empirical strategy in this paper is grounded in the claim that discontinuities in cell phone pricing prompt sharp increases in cell phone call volume. To illustrate this first stage relationship between call volume and call pricing, the analysis relies primarily on a large dataset of calls by likely drivers during an eleven day period in 2005 acquired from a major network provider. The data is restricted to calls routed through multiple cell phone towers in a contiguous, highly populated, region in California. The boundary of this region is defined by coverage of a single cell phone switch which consists of 300 to 400 towers. The mechanics of signal switching are such that a call will originally be routed by the tower emanating the strongest signal. A call will be rerouted through a second tower only when the differential in signal strength between the old and a new tower exceeds a certain threshold. Due to this threshold switching design, signals of stationary or even ambulatory callers are almost always routed by a single tower. Rare exceptions to this rule may exist when a caller is walking through a region with large buildings that interfere with a given tower’s reception. However, our data is from a switch which covers a region just outside of downtown and thus avoids calls made within the city center. The 11 days of calls represents the longest near-continuous period in 2005 during which data could be retrieved from the archives.⁵³

Engineers from the network provider estimate that a given caller must travel at least approximately 2 miles before a call will switch towers. Therefore, our dataset almost certainly comprises calls made by callers in moving vehicles. While volumes are scaled for confidentiality, we can estimate that the data consists of 276,000 to 1.24 million minutes of cell phone use over this period. Given an average duration of 2.6 minutes per call, calculated from a large dataset of cellular calls described below (TNS), this translates to 106,000 to 477,000 calls made during the course of the 11 days. The data also allows us to compare relative call likelihood at different points during the day.

Two additional datasets of calls confirm the price sensitivity of a broader population of cellular callers that extends beyond drivers. First, complete logs of cell phone activity for approximately 65 students and faculty over the academic years from 2004 and 2005 was obtained from the Reality Mining Project at the MIT Media Lab (MIT).⁵⁴ As part

⁵²Data on average usage is reported in the annual FCC CMRS reports.

⁵³More precisely the calls are from a continuous 14 day period, but there are three days for which no data could be extracted.

⁵⁴The data is described in the publication: Eagle, Nathan and Alex Pentland, “Reality Mining: Sensing

of a study examining the evolution of social networks and the transmission of information, researchers embedded surveillance technology in the cellular phones of each subject in their sample. Approximately 80,000 outgoing calls were logged over the course of the surveillance period. Electronic logs ensure that the timing of calls are accurately documented to the second. The data may not be representative of the larger population across a variety of dimensions given that the subjects are primarily students.

Figure A2 depicts the distribution of calls, aggregated in 10 minute bins, from 8 to 10pm for Mondays to Thursdays, Fridays and weekends. In order to formally estimate the size of the rise in call volume at the price threshold, the upper panel of Table A3 reports results of a Poisson regression of minute level calls from 8 to 10pm with fixed effects that control for day of week, month and year of the call. The results indicate a rise in call likelihood of 22.6% in the hour after 9pm on Mondays and Thursdays and no significant rise in the comparable period on Fridays or weekends. The placebo checks for other hours indicate a rise at 8pm of about 12% and no rise at 10pm. However, the estimated rise at 8pm is not due to a discontinuous break at 8pm, but rather a gradual rise in calls from 8 to 9pm that may be idiosyncratic to this academic population.

Our second additional dataset (TNS) comprises over 741,000 calls made by 9,864 cell phone users from households across the country in 2000 and 2001.⁵⁵ The data was harvested from cellular phone bills voluntarily submitted from households randomly selected to participate in an earlier survey of telecommunications behavior and attitudes.⁵⁶ The data is hourly data and is from a period characterized by either the absence of a cell phone plan or plans with non-uniform switching thresholds across the weekday evenings. The data usefully provides peak and off-peak designations for each call, and allows for the analysis of individual call patterns.

A sizable share of the 9,864 callers in the data have plans with thresholds that either do not exist or cannot be inferred.⁵⁷ We therefore retain a subsample of callers that satisfy each of the following conditions: (i) Callers are in the sample for at least 30 or more

Complex Social Systems,” *Personal and Ubiquitous Computing*, Vol. 10, No. 4, pp. 255-268, 2006.

⁵⁵The dataset, *Residential Quarterly Tracking Data: Bill Harvesting*, is commercially distributed by TNS Telecom. While the firm continued to harvest cellular phone bills after 2001, we were unable to acquire this data for a more recent period due to prohibitive costs.

⁵⁶The “ReQuest Consumer Survey” is a quarterly survey of about 30,000 households on consumer behavior and attitudes related to telecommunications. Households were offered a small payment in exchange for copies of one month’s worth of cellular, cable, TV and internet bills. In the fourth quarter of 2001, households were offered \$5 and participation in a “special cash prize raffle” for their bills.

⁵⁷We impute the switching hour by computing the change in the average peak/off-peak rating for each evening hour. Peak calls are tagged with the value “1” while off-peak calls are tagged with the value “2”. In principle, if a caller has a 7 pm switching threshold, then the average peak/off-peak rating should jump cleanly from 1 to 2 at 7 pm on weekdays. However, due to the presence of holidays or calls made in excess of the allowed quota for that month, we do not always observe unit jumps in the rating. In the absence of clean rating jumps, we tag the evening hour with the largest jump in average peak/off-peak rating as the switching hour for each caller.

calendar days and had calls on at least half of these days, (ii) Callers log at least one call in the evening hours (i.e., 5pm or after) in each of Monday to Thursdays, Fridays, and the weekend, (iii) Callers have no calls that are ambiguously tagged (i.e., each call is tagged as either “peak” or “off-peak” rather than “unclear”), and (iv) Callers have a mix of peak and off-peak calls which allows us to infer the switching hour of the caller’s plan.⁵⁸ The remaining 287 callers have plans with switching thresholds at 6pm (65), 7pm (104), 8pm (78), 9pm (23) or 10pm (17). These individuals make a total of 16,900 evening calls.

The data clearly demonstrates the responsiveness of callers to their particular weekday pricing thresholds. We specify the following Poisson model at the level of the individual caller to formally size the sensitivity of callers to their respective plan thresholds:

$$E[Calls_{h_{si}} | .] = \exp[\alpha + \gamma Switch_s + \theta AfterSwitch_{h_{si}} + \eta_h + \delta_i]$$

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

The coefficient estimates in the bottom panel of Table A3 indicates a rise in call volume of 22.5% in the hour following the switching threshold on Mondays to Thursdays, and no significant comparable rise in calls on other days. There is likely higher persistence in the call volume increase following the pricing threshold in this data, relative to other data, because many callers are on plans that switch fairly early in the evening. Finally, to test for the concern that the rise in calls at the switching threshold may be counterbalanced by a fall in call duration, we check and find no evidence for a statistically significant fall in duration at the threshold.

Pricing Plan Data. We obtain data on historical cellular pricing plans through monthly screen-shots of cell phone provider websites taken each year from 1999 to 2005 by Econ One Research.⁵⁹ The survey details the availability of pricing plans by provider, the schedule of marginal prices per call, as well as the time threshold at which tiered pricing plans switch from peak to off-peak pricing (Various Years). Our data covers 26 major markets and 30 providers over this period. Market shares for the top 25 wireless providers

⁵⁸The rationale for employing a minimum day and call threshold 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.

⁵⁹The survey is officially entitled: Econ One Wireless Survey: An Internet Survey of Cellular and PCS Pricing Plans. It is distributed each year by Econ One Research.

were collected from the CMRS Competition reports published each year by the FCC.⁶⁰ In many cases, mapping the plan counts to FCC market shares required knowledge of ownership changes, subsidiary relationships and regional brand names. We used a variety of resources including analyst reports and online news sources to assist in this mapping. Finally, in order to calculate legacy subscription rates for each threshold, we required the rate of turnover, or churn, for each year. Churn rates were acquired from S&P Industry Surveys from 2001 to 2005. We assume churn to be at 2001 levels for years prior to 2001. Table A2 details the data and calculations used to estimate legacy shares by pricing threshold, and Figure 4 displays these shares for years from 1999 to 2005.

Legislation and Traffic. Traffic counts at the 30 second level for the region of interest in California was downloaded from the Performance Evaluation Monitoring System (PEMS) website administered by UC Berkeley and the state’s Department of Transportation. This data was aggregated to produce minute level counts and was used to calculate the change in call likelihood across the pricing threshold in the analysis of the first stage. The database was also the source of hourly level traffic counts from 1993 to 2005 used in the checks of traffic constancy in the second stage analysis. California has several thousand counting stations in place across major highways, freeways and local roadways and these produce highly disaggregated traffic counts that can be downloaded for one of several districts by which the state is segregated.

The first alternative analysis in the paper is a comparison of aggregate cellular ownership and crash rates. This analysis includes a robustness check which controls for state-level traffic data. We collected data on annual highway traffic volume for all states from 1989 to 2007 from the Federal Highway Traffic Administration. The agency compiles traffic data from approximately 4,000 counting stations positioned on roadways across the country. Total traffic volume on U.S. highways grew by nearly 1 trillion miles during this period reaching 3.0 trillion in 2007. A second alternative approach entails the analysis of legislation banning driver use of cell phones for which we rely on legislative descriptions published by the National Conference of State Legislatures as well as the Governors Highway Safety Association website (Sundeen 2007).

7.2 Supplementary Analyses

Two additional empirical approaches confirm our basic results. In the first approach, we compare aggregate national trends in crashes and cellular ownership at the EA and state level. Next, using a region-month panel, we examine whether legislative bans on handheld driver cell phone use reduced the fatal crash rate.

⁶⁰This report, entitled “Annual Report to Congress on the State of Competition in the Commercial Mobile Radio Services Industry,” is available for each year on the FCC website.

7.2.1 Panel Estimation of Crashes and Ownership

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

However, given the heterogeneous rise in cell phone ownership across regions, we can exploit variation across regions as well as years to more accurately pin down the relationship between ownership and crashes. Economic Areas (EAs) represent the most disaggregated geographic units for which data on cellular ownership data is available. EAs are currently used by the FCC to denote regions of contiguous economic activity. Each of the 172 EAs consists of one or more economic nodes—a metropolitan or micropolitan statistical area that serves as a regional economic center. Examples of EAs include “Minneapolis-St.Paul”, “Washington-Baltimore”, as well as the largest, “New York-Northern New Jersey-Long Island.”

EAs are associated with considerable variation in ownership. Ownership rates ranged from 19 to 57 percent across EAs in 2001 and from 61 to over 100 percent by 2007.⁶¹ We estimate the following model with an OLS regression:

$$\ln(\text{Crash Rate})_{ry} = \alpha + \gamma \text{Cell Own}_{ry} + \theta \ln(\text{Traffic})_{ry} + \eta_r + \delta_y + \varepsilon_{ry}$$

where $\ln(\text{Crash Rate}_{ry})$ denotes the log of the crash rate for region r and year y , while Cell Own_{ry} refers to the percent share of cell phone ownership for a given region-year. 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 additional specifications with a covariate, $\ln(\text{Traffic})_{ry}$, to control for highway traffic volume across region and year. All estimations are conducted at the EA level, with the exception of the robustness specifications which are estimated at the state level.

Since cellular ownership is only observed at the EA level from 2001 to 2007 (excluding 2006 for which ownership data is not available), and given that national ownership is less than 5% prior to 1993, we code region specific ownership as missing from 1993 to 2000 and as zero prior to this period. This strategy allows us to effectively construct a control period with near-zero ownership and contrast it with a treatment period for which ownership is

⁶¹In rare cases, such as in Washington D.C., the FCC reports ownership as being greater than 100% due to either multiple subscriptions by some residents or the fact that the FCC records location of registration rather than of residence.

both positive and known.

Table A4 presents the results of the estimations. The first two columns report results of the panel analysis of the crash rate across the approximately 60 EAs in nine states from 1990 to 2005 for which we have the universe of crash data. The point estimate of interest indicates the percent change in the crash rate given a 1% point increase in average EA ownership after controlling for EA and year fixed effects. To control for the possibility that omitted factors that cause crashes within a state over time are correlated with cellular ownership, the next column includes more flexible controls which allow for EA specific time trends.⁶² Columns 3 and 4 repeat the exercise for fatal crashes for all 172 EAs from 1989 to 2007. None of the estimates suggest a statistically significant positive link between ownership and fatal crashes.

In principal, we can calculate upper bounds for the above estimates and compare these to other effect sizes reported in the literature. Assuming that cellular influence is linear in ownership we can also calculate upper bounds for the overall influence of the introduction of cell phones compared to the counterfactual scenario in which cell phones were not introduced. In our favored specification for all crashes, reported in Column 2, the upper bound for the coefficient estimate is .0024 which implies that, in 2005, the upper bound of the influence of cell phones on the crash rate is 17% (i.e., $(.0024*.70)*100$). This upper bound rejects the 33% increase in crashes implied by RT. For fatal crashes, the upper bound for the coefficient estimate of Column 4, .0044, rejects any increase in aggregate crashes larger than 31%.

The final columns of the table provide a robustness check of the results by controlling for changes in traffic volume across regions and time. Since traffic volume is only coded at the state level, this regression is limited to fatal accidents at the state, rather than the EA, level.⁶³ The estimation, admittedly imprecise, again provides no evidence for a statistically significant correlation between ownership and crashes.

Importantly, if we restrict our state-year analysis of fatal crashes to 1999 to 2005 we can approximately replicate the effect sizes reported in Kolko (2009). Kolko reports positive but insignificant estimates of the effect of cellular ownership on crashes, adjusted for traffic volume, after controlling for state and year fixed effects in a state-year panel regression from 1997 to 2005.⁶⁴ His favored estimates imply, under the previously stated assumptions, that

⁶²Silva and Tenreyo point out that log-linear estimations can be inconsistent if the true underlying model is characterized by a Poisson distribution (2006). We re-estimate our baseline model using a Poisson specification and a population offset. The point estimates are substantially similar and insignificant.

⁶³Regressions are confined to fatal accidents because of the limited number of states in the SDS dataset. As opposed to EA level penetration which is available only since 2001, state level ownership data is available since 1999.

⁶⁴Kolko uses proprietary survey data from Forrester Research to infer state-year cell phone ownership from 1997 to 2005. Our ownership data, taken from the FCC, is only available as of 1999 which prevents a closer replication.

the introduction of cell phones produces a 15% increase in the aggregate fatal crash rate.⁶⁵ Our analogous and also insignificant estimates imply a 12% increase in the fatal crash rate.⁶⁶ However, we find that the introduction of an early control period with no cellular ownership or the introduction of linear and quadratic state time-trends each—as well as both jointly—eliminate the positive point estimates for cellular ownership.⁶⁷

There are several possible explanations for why our estimations do not yield statistically significant results. One, of course, is the absence of any genuine correlation between crashes and cellular ownership. A second possibility is that unobserved, time-varying determinants of crashes are correlated with the growth in cell phone ownership. The inclusion of controls for region and year fixed effects, and region specific time trends is meant to help guard against this possibility. A final possibility is that our test lacks power to detect the true effect size.

Though the EA represents the most disaggregated level for which subscription data is widely available, our analysis ignores the potential variation of cell phone usage over time due to the recent introduction of bans on handheld cell phone use in selected regions. We explore this additional source of variation next.

7.2.2 Analysis of Legislative Bans on Handheld Cell Phones

In a third approach, we estimate the influence of legislative bans that restrict cellular use by drivers. Six states have banned handheld phones (almost) without exception.⁶⁸ New York’s ban went into effect in November 2001, followed by New Jersey in July 2004, Connecticut in October 2005, California and Washington in July 2008 and Oregon in early 2010. Beyond these states, a number of municipalities have enacted complete bans. The largest of these municipalities are Chicago, whose ban went into effect in July 2005, and Washington D.C. which banned cellular use by drivers beginning in July 2004. Several additional states have legislated partial bans on cellular use but these bans typically target a modest fraction of drivers. Table A5 in the Appendix enumerates the states and large municipalities with complete or partial bans.⁶⁹ Note that to the extent that drivers substitute hands-free

⁶⁵Originally reported as 16%, the Kolko estimate is taken from Column 2 of Table 2 and is discussed in the subsequent text and footnote. We adjust the figure to 15% to account for the 70% ownership rate for 2005 which we use throughout the text.

⁶⁶Specifically, we estimate the model presented in Column 5 after restricting the sample to 1999 to 2005. We find a coefficient estimate of ownership equal to .0016 (with a standard error of .0022).

⁶⁷The introduction of linear and quadratic time trends reduces the point estimate of cell phone ownership (%) from .0016 to -.0031. The introduction of an early control period with no cellular ownership reduces the point estimate from .0016 to -.0008. The inclusion of both a control period and the time trends reduces the point estimate to -.0001. None of these estimates are statistically significant.

⁶⁸One common exception is the use of cell phones for emergency calls.

⁶⁹The table excludes numerous states which ban cellular use by school bus drivers. A list of municipalities with bans can be found in “Cell Phones and Highway Safety: 2006 Legislative Update” published by the National Conference of State Legislatures (Sundeen 2007).

devices for banned handheld phones, our analysis tests for the difference in crash risk between hands-free and handheld use.

Our data on fatal crashes, from 1989 to 2007, allows us to explore the effects of the legislation in New York, New Jersey, Connecticut, as well as the large municipalities of Chicago and Washington D.C. The analysis is at the state, rather than EA, level since states are actually a more disaggregated unit of analysis for these regions, and EA ownership data is not available for 2006. The ban in Chicago is treated as if it were for the entire state of Illinois in this analysis.⁷⁰ Since the bans are generally enacted during the year, the analysis is at the monthly, rather than yearly, level. Unfortunately, our data on all crashes fails to cover the regions and time periods of interest.

It is worth noting that the impact of handheld bans on the crash rate is multi-determined. For example, the effect of legislation on crashes is determined by the crash risk associated with handheld use, driver compliance with the legislation, possible compensatory use of hands-free devices, and in the event of such compensation, the crash risk associated with hands-free use. There is some evidence that drivers, at least in the short-run, comply to legislative bans although such compliance may dissipate in the long-run (McCartt and Hellinga 2007). While much laboratory evidence suggests that the distracting effects of hands-free cell phones are comparable to handheld counterparts (Caird et al. 2008), it is unclear to what extent drivers substituted to hands-free devices, particularly, during the early years of the technology.

Figure A5 depicts the raw monthly counts of fatal crashes for the months preceding and following the enactment of each complete ban for the regions of interest. With the possible exception of New York, the figure indicates no sharp drop in crashes for any of the regions during the five months following ban enactments ($t + 5$). We attribute the drop in crashes in New York at least partially to drops in traffic as a result of the attacks on September 11th, 2001. In fact, the New York legislation, while nominally enacted in November 2001, was not enforced with binding fines until March 2002 which corresponds to ($t + 4$) in the figure. Longer horizons reveal no systematic patterns across the regions.

In order to formally test for the effects of the legislation, we estimate the following OLS regression at the region level for fatal crashes each month from 1989 to 2007:

$$\ln(\text{Crash Rate})_{rym} = \alpha + \lambda \text{Ban}_{rmy} + \gamma \text{Cell Own}_{ry} + \theta \ln(\text{Traffic})_{ry} + \eta_r + \delta_y + \pi_m + \varepsilon_{rym}$$

where Ban_{rmy} is a dummy variable which indicates that a complete handheld ban was in effect for any part of a given state r , in month m , and year y . As before, we include a

⁷⁰One might expect this to bias the results against finding any effect of the legislation but our basic results are not sensitive to the inclusion of Illinois.

control period with 0% ownership prior to 1993. Region, year, and month, fixed effects are included along with linear and quadratic time trends by region and year to flexibly control for time and region specific variation in crashes.

In an initial specification with just month, year and state fixed effects, the estimated coefficient of interest, $\hat{\lambda}$ in Column 1 of Table A6 suggests a large and statistically significant 13% drop in fatal crashes after the enactment of legislation. This is broadly consistent with the findings of Kolko (2009). However, the inclusion of state specific linear and quadratic time trends reduce the point estimate to a statistically insignificant -.08. Additional checks reveal that the pattern of crashes in Washington D.C. is responsible for the negative point estimate. Given the modest fatal crash rate in Washington D.C. (about 4 per month), any small change in crashes strongly alters the estimated coefficients given the construction of the dependent variable. The exclusion of Washington D.C. eliminates the apparent negative effect of the legislation as reported in Column 3.

To better understand the time-path impact of the legislation, we estimate the above model with dummy variables indicating 1 month, 2 to 3 month, 4 to 6 month and > 6 month horizons. The estimates in the final three columns suggest that, without controlling for time trends, the legislation prompted a statistically significant reduction in the long-run crash rate. However, with time trends included, the ban appears to have no significant impact on fatal crashes. Excluding Washington D.C. eliminates the negative point estimates entirely.

7.3 Model of Compensatory Response

We consider a simple model which illustrates the conditions under which a rational driver might compensate in the face of beneficial, but distracting, cell phone use. Define driver utility as follows:

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

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

For a given unit cost, m , a driver chooses (s^*, c^*) to maximize utility (see Appendix for derivation of first and second order conditions). The effect of a change in the cost of

cellular usage, m , on the probability of an accident, $p(s^*, c^*)$ can be expressed as:

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

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

We can show that such compensation arises under the stated assumptions and preferences by solving for $\frac{ds^*}{dm}$ (derivation below):

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

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

Derivation of Solution. The first order conditions of the model are given by:

$$U_s : w_s - p_sL = 0$$

$$U_c : v_c - m - p_cL = 0$$

Total differentiation of the first order condition for (s^*, c^*) yields:

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

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

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

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

We can recast the above expression as:

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

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

7.4 Additional Tables and Figures

Table A1

SUMMARY OF DATA SOURCES

	DATA SOURCE	YEARS	DESCRIPTION
CRASH/ TRAFFIC RECORDS			
Crash Records	State Data System (SDS)	1990 to 2005	Crash records for all crashes for nine states
Fatal Crash Records	Fatality Analysis Reporting System (FARS)	1989 to 2007	Crash records for all fatal crashes for all 50 states
State-Year Traffic	Federal Highway Administration	1989 to 2007	Traffic volume by state by year
Minute Level Traffic	Performance Evaluation Monitoring System	2002 to 2005	Raw 30 second and 5 minute counts from several thousand traffic detectors on CA roadways
CELL PHONE OWNERSHIP			
Cellular Subscribers	Cellular Telephone Industry Association	1999 to 2007	Cellular subscribers by state by year
	Federal Communications Commission	2001 to 2007	Cellular subscribers by Economic Area (EA)
Population	Bureau of Labor Statistics	1990 to 2007	Yearly population by county
EA - County Codes	The Bureau of Economic Analysis	2000	EA codes for each county
CELL PHONE CALL VOLUME			
	Major Network Provider	2005	Cellular signals from moving users in a large contiguous CA region spanned by 300 to 400 cell phone towers over 11 days in 2005
	Reality Mining Project, MIT	2004 to 2005	Logs tracking 80,000 outgoing cellular calls for 65 students/faculty at MIT during academic year
	TNS Telecom	2000 to 2001	Data from cellular phone bills for 9864 households
CELL PHONE PRICING			
Provider Pricing Plans	Econ One Research	1999 to 2005	Monthly snapshots of historical pricing plan details for all providers across major national markets each year from 1999 to 2005
Provider Market Shares	FCC CMRS Competition Reports	1999 to 2005	Market shares for top 25 providers by year
Churn Rates	S&P Industry Surveys	2001 to 2005	Market shares for top 25 providers by year

Table A2

PRICING THRESHOLDS FOR CALLING PLANS FROM 1999 TO 2005

ALL NATIONAL MARKETS																	
	NONE	6PM	7PM	8PM	9PM	10PM	SUB	MKT SH		NONE	6PM	7PM	8PM	9PM	10PM	SUB	MKT SH
1999									2003								
Verizon	266	0	47	161	66	75		30%	Verizon	340	0	0	0	1634	0		24%
SBC	78	0	0	52	25	26		19%	Cingular	0	0	432	0	432	0		15%
AT&T	122	0	34	1	90	0		12%	AT&T	336	0	0	0	1050	0		14%
Sprint	74	0	5	124	64	0		7%	Sprint	0	0	390	0	780	0		10%
Voicestream	42	0	0	0	0	0		3%	T-Mobile	0	0	0	0	546	0		8%
Westem	62	0	10	28	0	0		1%	Nextel	104	0	0	0	156	0		8%
Powertel	10	0	0	0	0	0		1%	Alltel	0	0	0	0	67	0		5%
US West	60	0	0	0	0	0		1%	US Cellular	0	0	0	0	48	0		3%
Cincinnati Bell	15	0	0	0	0	0		0.2%	Metro PCS	8	0	0	0	0	0		1%
									Qwest	60	0	0	0	0	0		1%
									Cincinnati Bell	0	0	0	0	19	0		0.3%
New Share	50%	0%	6%	23%	15%	6%	86.0m	0.72	New Share	13%	0%	13%	0%	74%	0%	158.7m	0.88
Legacy Share	46%	0%	6%	23%	16%	9%			Legacy Share	30%	0%	5%	11%	51%	3%		
2000									2004								
Verizon	603	0	33	0	31	87		25%	Cingular	0	0	0	0	273	0		27%
Cingular	350	0	0	12	0	0		18%	Verizon	0	0	0	0	1952	0		24%
AT&T	282	0	0	140	0	0		14%	Sprint	0	0	390	0	2236	0		12%
Sprint	0	0	0	750	0	0		9%	T-Mobile	0	0	0	0	546	0		10%
Alltel	26	0	0	0	0	0		6%	Nextel	104	0	0	0	302	0		9%
T-Mobile	112	0	0	0	0	0		4%	Alltel	0	0	0	0	72	0		5%
Westem	70	0	0	0	11	0		1%	US Cellular	0	0	8	0	74	0		3%
Powertel	13	0	0	0	0	0		1%	Metro PCS	16	0	0	0	0	0		1%
Qwest	65	0	0	0	0	0		1%	Qwest	0	0	55	0	145	0		0%
Cincinnati Bell	20	0	0	1	0	0		0.3%	Cincinnati Bell	0	0	0	0	15	0		0.3%
New Share	60%	0%	1%	33%	2%	4%	109.5m	0.78	New Share	2%	0%	7%	0%	91%	0%	182.1m	0.90
Legacy Share	59%	0%	4%	21%	10%	7%			Legacy Share	0.89							
2001									2005								
Verizon	662	0	0	281	54	0		23%	Cingular	0	0	494	0	182	0		26.0%
Cingular	12	0	0	0	326	83		17%	Verizon	312	0	0	0	678	0		24.7%
AT&T	184	0	0	322	0	0		14%	Sprint	0	1298	1298	0	1298	0		21.6%
Sprint	0	0	0	0	550	0		11%	T-Mobile	0	0	0	0	390	0		10.4%
T-Mobile	108	0	0	0	0	0		5%	Alltel	0	0	66	0	66	0		5.1%
Alltel	0	0	0	62	0	0		5%	US Cellular	0	0	124	0	40	0		2.4%
Qwest	0	0	0	40	0	0		1%	Metro PCS	20	0	0	0	0	0		1.0%
Cincinnati Bell	10	0	0	0	6	0		0.4%	Cincinnati Bell	0	0	0	0	8	0		0.2%
PrimeCo	12	0	0	0	0	0		0.3%									
New Share	37%	0%	0%	26%	34%	3%	128.4m	0.77	New Share	5%	20%	32%	0%	43%	0%	207.9m	0.91
Legacy Share	50%	0%	2%	24%	18%	6%			Legacy Share	21%	0%	4%	7%	66%	2%		
2002									Estimated Churn								
Verizon	360	0	0	71	1568	0		23%	1999	2000	2001	2002	2003	2004	2005		
Cingular	11	0	0	0	432	0		16%	27%	27%	27%	26%	22%	22%	20%		
AT&T	204	0	0	0	1092	0		15%	NONE	6PM	7PM	8PM	9PM	10PM			
Sprint	0	0	0	0	500	0		10%	26%	1%	7%	9%	55%	2%			
T-Mobile	200	0	0	0	0	0		7%	Average Weighted Legacy, 2002-05								
Alltel	0	0	0	0	65	0		5%									
US Cellular	0	0	0	0	28	0		3%									
Leap Wireless	3	0	0	0	0	0		1%									
Qwest	23	0	25	0	0	0		1%									
Cincinnati Bell	9	0	0	0	9	0		0.3%									
New Share	18%	0%	1%	2%	80%	0%	140.8m	0.81									
Legacy Share	39%	0%	2%	16%	39%	4%											

Notes: The table displays the distribution of pricing plans associated with each switching threshold by provider and year as well as calculations which estimate the new and legacy share of subscribers associated with each threshold by year. The data on plan counts is from monthly snapshots of provider websites originally reported in the Wireless Survey administered by Econ One Research. All years are for the month of December except for 1999 which displays plan data from September. New Shares for each year reflect the unweighted fraction of plans associated with each threshold and provider weighted by national market shares for each provider. Market shares for the top 25 providers each year are collected from annual FCC CRMS reports. New Shares are scaled up to account for unknown market shares. Legacy shares are calculated by applying annual churn rates--listed above and gathered from S&P Analyst Surveys--to the threshold shares each year. Assumptions of the legacy calculations are outlined in the text. Legacy Shares weighted by total subscribers for 2002 to 2005 are listed as well.

Table A3

CHANGE IN CALL VOLUME AT PLAN THRESHOLD (MIT / TNS)

DEPENDENT VARIABLE - CALLS PER MINUTE BIN (MIT)					
	9PM THRESHOLD ANALYSIS			8PM	10PM
	Mon - Thu (1)	Friday (2)	Weekend (3)	Mon - Thu (4)	Mon - Thu (5)
After 9pm	1.227*** (0.054)	0.958 (0.069)	0.994 (0.062)		
After 8pm				1.120*** (0.048)	
After 10pm					0.984 (0.042)
N	N = 20880	N = 5160	N = 10440	N = 20880	N = 20880
DEPENDENT VARIABLE - HOURLY CALLS BY CALLER (TNS)					
	SWITCHING THRESHOLD ANALYSIS			- 1 HR	+ 1 HR
	Mon - Thu	Friday	Weekend	Mon - Thu	Mon - Thu
Switching Threshold	1.225*** (0.057)	1.143 (0.094)	1.116 (0.075)	0.871 (0.096)	1.084 (0.140)
After Switching Threshold	1.045 (0.069)	0.829 (0.095)	1.191* (0.110)	1.126 (0.210)	1.054 (0.220)
N	N = 2009	N = 2009	N = 2009	N = 1369	N = 1369

Notes: The table estimates the change in call likelihood for two additional sets of cellular call data. The upper panel presents the estimated rise in calls at 9pm and other placebo hours for a sample of MIT callers from 2004 and 2005. The first three columns estimate the rise in outgoing calls at 9PM for Monday to Thursdays, Fridays and the Weekend respectively. The final two columns present placebo estimates for changes in call likelihood at 8PM and 10PM on Monday to Thursdays. All specifications are Poisson estimates run at the minute level, and the reported estimates are incidence rate ratios. Robust standard errors clustered by date are presented parenthetically. The lower panel presents the estimates from the TNS sample of callers in 2000 for 2001. The first three columns report the estimated change in outgoing calls at the switching threshold for each caller for Monday to Thursdays, Fridays and Weekends respectively. The final two columns present placebo estimates that test for changes an hour before and an hour after the switching threshold. All specifications are Poisson estimates run at the hourly x caller level and the reported estimates are incidence rate ratios. Robust standard errors clustered by caller are reported parenthetically.

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

Table A4

TRENDS IN CELLULAR OWNERSHIP AND CRASHES ACROSS REGION-YEAR

	DEPENDENT VARIABLE - LN(CRASHES PER 100,000 POP)					
	All Crashes (1990 to 2005)		Fatal Crashes (1989 to 2007)		Fatal Crashes (1989 to 2007)	
	Economic Area		Economic Area		State	
	(1)	(2)	(3)	(4)	(5)	(6)
Cell Phone Ownership	-0.0018 (0.0015)	-0.0004 (0.0014)	-0.002 (0.001)	0.002 (0.001)	-0.001 (0.001)	0.001 (0.002)
ln(Traffic Volume)					0.132 (0.199)	0.229 (0.210)
Region Fixed Effects	X	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X	X
Region FE x Year		X		X		X
Region FE x Year ²		X		X		X
N	N = 475	N = 475	N = 2036	N = 2036	N = 642	N = 642
R ²	0.97	0.99	0.83	0.92	0.93	0.97

Notes: The table estimates a panel regression of cellular ownership and vehicular crashes over time. The dependent variable across each regression is the natural log of the number of crashes, per scaled capita, in a given year for a particular region for the stated time period. The explanatory variable of interest is the percent rate of cell phone ownership in the specified region (i.e., 100 * cell phone subscribers / population). For the regressions using Economic Area (EA), 2006 is excluded since penetration data is not available. Ownership prior to 1993 is assumed to be 0%, and ownership from 1993 to 2000 is coded as missing except for the state-year regressions which include ownership for 1999 and 2000. The first two columns report analysis of all crashes for nine states, while the next two columns report an analysis of fatal crash data for all states. A small number of EA-years were excluded due to lack of data on population or inability to match county and EA. In the all crash analysis, Michigan is excluded during the control period due to missing county identifiers and Pennsylvania is excluded in 2002 due to missing data. Columns 5 and 6 report a robustness check for fatal crashes at the state level after controlling for state-year traffic volume. The state-year series begins in 1989 to coincide with availability of traffic volume data. Robust standard errors clustered by EA or state are reported parenthetically.

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

Table A5

SUMMARY OF BANS ON HANDHELD CELL PHONES

REGION	DATE OF ENACTMENT	SCOPE OF BAN	PUNISHMENT
California	July 2008	Complete	\$20 fine for first offense, then escalates
Connecticut	Oct 2005	Complete	\$100 fine
New Jersey	July 2004	Complete	100*
New York	Nov 2001*	Complete	\$100 fine
Oregon	Jan 2010	Complete	\$142 fine
Washington	July 2008	Complete	Secondarily enforced, \$124+ fine*
Washington D.C.	July 2004	Complete	\$100 fine (first offense waivable)
Chicago, Illinois	July 2005	Complete	\$50-100 fines
Arkansas		< 18 year olds, 18 to 20 hands-free only	
Colorado	--	< 18 year olds	--
Delaware	--	Permit drivers	--
Illinois	--	< 19 year olds	--
Indiana		< 18 year olds	
Kansas		Permit drivers	
Louisiana		Permit drivers	
Maine	--	< 18 year olds	--
Maryland	--	< 18 year olds with Permit	--
Michigan	--	Permit drivers*	--
Minnesota	--	Permit drivers, first 12 months	--
Nebraska	--	< 18 year olds with Permit	--
North Carolina	--	< 18 year olds	--
Rhode Island	--	< 18 year olds	--
Tennessee	--	Permit drivers	--
Texas	--	Permit drivers*	--
Virginia	--	< 18 year olds	--
West Virginia	--	Permit drivers	--

Notes: Data was compiled from the Governors Highway Safety Association website as well as various other news sources. States with bans on only school bus drivers are not listed. "Complete" refers to bans on hand-held cell phones for all drivers. New York law was enacted in November 2001 but fines were not fully binding until March 2002. In Washington, cell phone use is ticketed only in combination with some other violation. New Jersey law was originally secondarily enforced with fines ranging from \$100 to \$250 but is now enforced as a primary violation. The Michigan ban applies to permit drivers on probation because of earlier cellular use that is said to have resulted in a crash or ticket. The Texas ban on permit drivers applies to drivers only for the first six months following the issuance of a permit. Utah has a law on the books which bans "careless driving" that could be caused by cell phone related distraction. Date of enactment and punishment are only reported for regions with complete bans.

Table A6

CHANGE IN REGION-MONTH FATAL CRASHES AFTER HANDHELD LEGISLATION

	DEPENDENT VARIABLE - LN(CRASHES PER 100,000 POP)					
	Fatal Crashes (1989 to 2007)			Fatal Crashes (1989 to 2007)		
	All Regions		Excluding DC	All Regions		Excluding DC
	(1)	(2)	(3)	(4)	(5)	(6)
Cell Phone Ownership	0.001 (0.001)	0.001 (0.001)	0.002 (0.001)	0.001 (0.001)	0.001 (0.001)	0.002 (0.001)
Post Legislation	-0.130** (0.050)	-0.080 (0.086)	0.010 (0.020)			
Post Legislation (1 month)				0.015 (0.044)	0.053 (0.051)	0.042 (0.061)
Post Legislation (2-3 months)				-0.217 (0.151)	-0.181 (0.185)	0.011 (0.067)
Post Legislation (4-6 months)				-0.150 (0.118)	-0.114 (0.137)	0.034 (0.028)
Post Legislation (> 6 months)				-0.127*** (0.046)	-0.063 (0.062)	0.000 (0.021)
ln(Traffic Volume)	0.133 (0.202)	0.171 (0.169)	0.096 (0.153)	0.133 (0.202)	0.173 (0.171)	0.097 (0.153)
Region Fixed Effects	X	X	X	X	X	X
Year, Month Fixed Effects	X	X	X	X	X	X
Region FE x Year		X	X		X	X
Region FE x Year ²		X	X		X	X
N	N = 7725	N = 7725	N = 7572	N = 7725	N = 7725	N = 7572
R ²	0.69	0.72	0.73	0.69	0.72	0.73

Notes: The table estimates the effects of legislation banning cellular use on the rate of vehicular crashes over time. The dependent variable across each regression is the natural log of the number of crashes, per scaled capita, in a given month for a particular region for 1989 to 2007. The explanatory variables of interest are the percent rate of cell phone ownership in the specified region (i.e., 100 * cell phone subscribers / population) as well as dummies which indicate the presence of a legislative ban in a given region and period. In the latter two columns, the legislative dummy variables refer to non-overlapping time periods. Ownership prior to 1993 is assumed to be 0%, and ownership from 1993 to 1998 is coded as missing. The analysis begins in 1989 to coincide with availability of traffic volume data. Columns 3 and 6 estimate the model excluding Washington D.C. Robust standard errors clustered by region are reported parenthetically.

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

Cellular Ownership & Usage

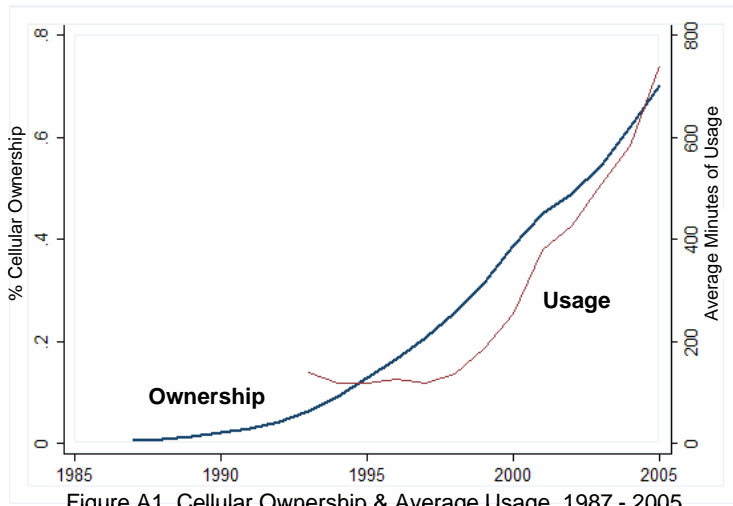


Figure A1, Cellular Ownership & Average Usage, 1987 - 2005

Monday to Thursday, Friday and Weekend Calls (MIT)

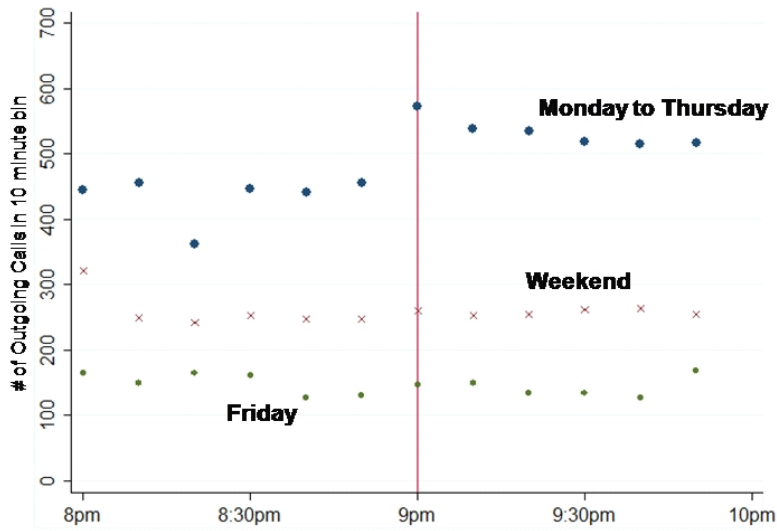


Figure A2, Outgoing Calls from 8pm to 10pm in 2004 to 2005 (10 mn bin)

Monday to Thursday and Weekend Crashes

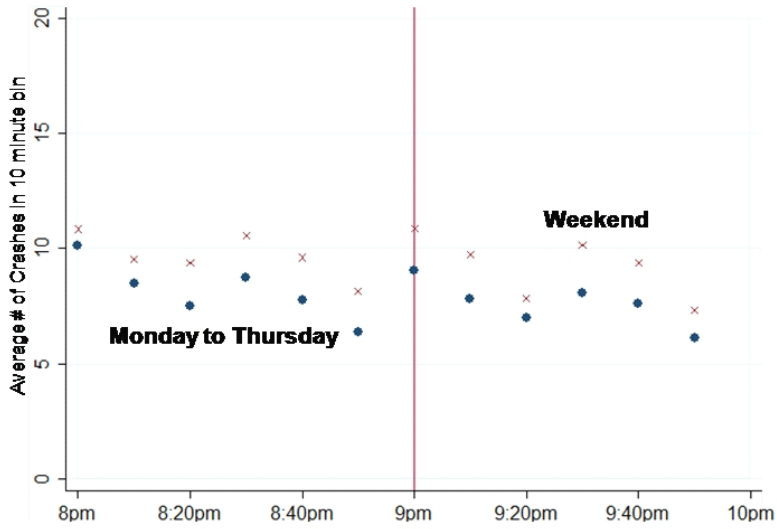


Figure A3, Average Crashes for California from 8pm to 10pm in 2005 (10 mn bins)

Pre and Post Period Crashes (Monday to Thursday)

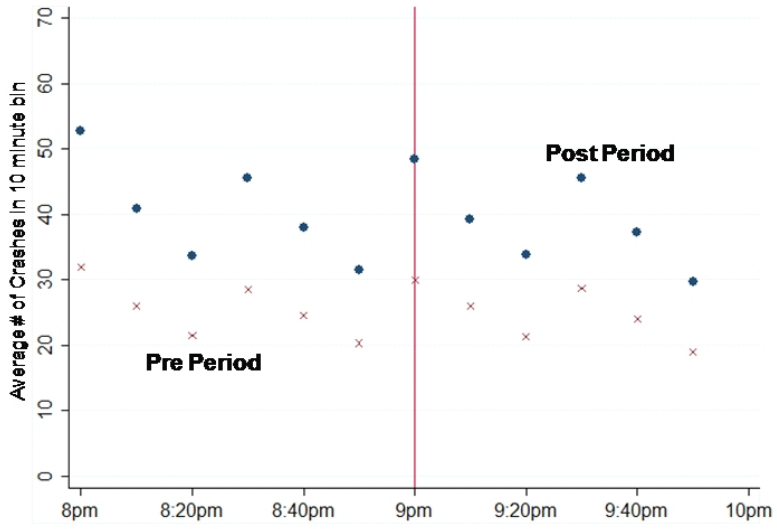


Figure A4, Average Crashes for Expanded States from 8pm to 10pm in Post (2002 to 2005) and Pre (1995 to 1998) Period (10 mn bins)

