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Driving under the (Cellular) Influence†

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We investigate the causal link between driver cell phone use and crash rates by exploiting a natural experiment induced by the 9 PM *price discontinuity that characterizes a majority of recent cellular plans. We first document a* 7.2 *percent jump in driver call likelihood at the* 9 PM *threshold. Using a prior period as a comparison, we next document no corresponding change in the relative crash rate. Our estimates imply an upper bound in the crash risk odds ratio of* 3.0*, which rejects the* 4.3 *asserted by Redelmeier and Tibshirani* (1997)*. Additional panel analyses of cell phone ownership and cellular bans confirm our result.* (*JEL* R41)

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 percent 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] In an influential paper published in the *New England Journal of Medicine*, Redelmeier and Tibshirani (1997)—henceforth, RT—concluded that cell phones increase the relative likelihood of a crash by a factor of 4.3. Laboratory and epidemiological studies have further compared the relative crash risk of phone use while driving to that produced by illicit levels of alcohol (RT; Strayer, Drews, and Crouch 2006).

If alcohol, however, is responsible for 40 percent of fatal and 7 percent of all crashes each year, as reported by the National Highway Traffic Safety Administration

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[1] The 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

[2] As counted by McCartt, Hellinga, and Bratiman 2006.

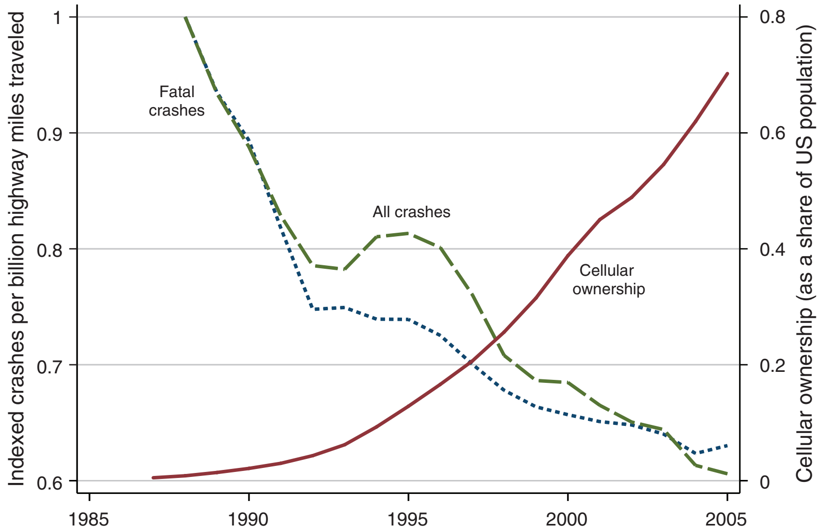

Figure 1. Cellular Ownership and Crashes Per Vehicle Mile Traveled in the United States for 1988 to 2005

(NHTSA), then Figure 1 illustrates a puzzle. Cell phone ownership (i.e., cellular subscribers/population) has 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 percent of cellular owners use their phones while driving—yet aggregate crash rates have fallen substantially over this period.

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, and novel data, 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 9 PM on weekdays.

We first provide evidence that this discontinuity in prices drives a sharp increase in the likelihood of calling for drivers using a proprietary dataset of calls from a leading network provider. Our data are 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. While 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 US 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. While the downward slopes reflect the pattern of traffic across evenings, driver call likelihood rises by 7.2 percent at the 9 PM threshold when prices transition from "peak" to "off peak." We find no comparable breaks in likelihood for neighboring hours or at 9 PM on weekends. We

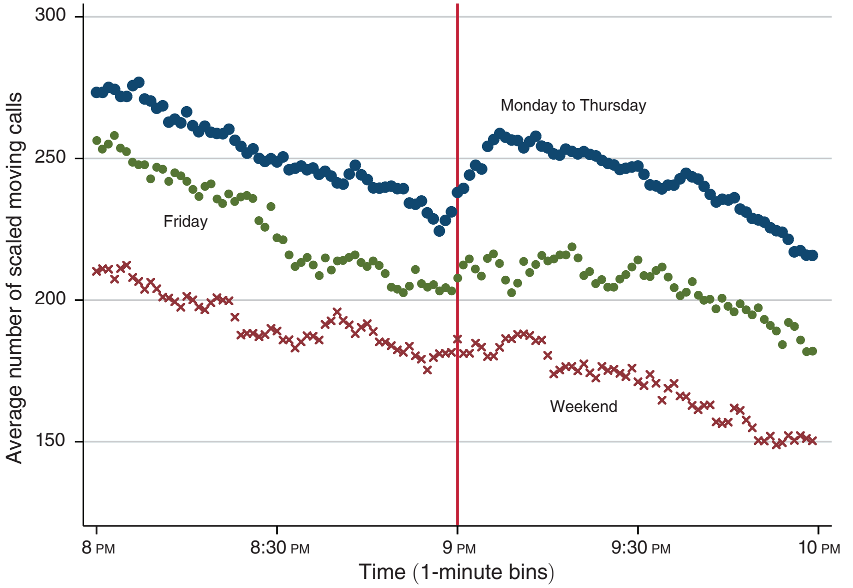

Figure 2. Cell Phone Call Volume from Moving Vehicles for California from 8pm to 10pm in 2005

present additional evidence on cell phone calls (this time by drivers and nondrivers) and 30,000 pricing plans across 26 markets to affirm the sensitivity of cellular users to the 9 PM price threshold. The rise in call likelihood at 9 PM 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 adopt a double-difference approach to compare the change in crashes at the threshold to the analogous change in a control period prior to the prevalence of 9 PM pricing plans and characterized by low cellular use.

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.[3] 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 9 PM threshold relative to the preperiod. We then generalize our crash analysis to include eight additional states for which we have the universe of crash data. Placebo tests of weekends and proximal hours, as well as robustness checks to account for the reporting bias in crashes, confirm that cell phone use does not result in a measurable increase in the crash rate.

Our estimates of the relative rise in crashes and call likelihood at 9 PM imply a 3.0 upper bound in the crash risk odds ratio (and a 1 s.e. upper bound of 1.4) under

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

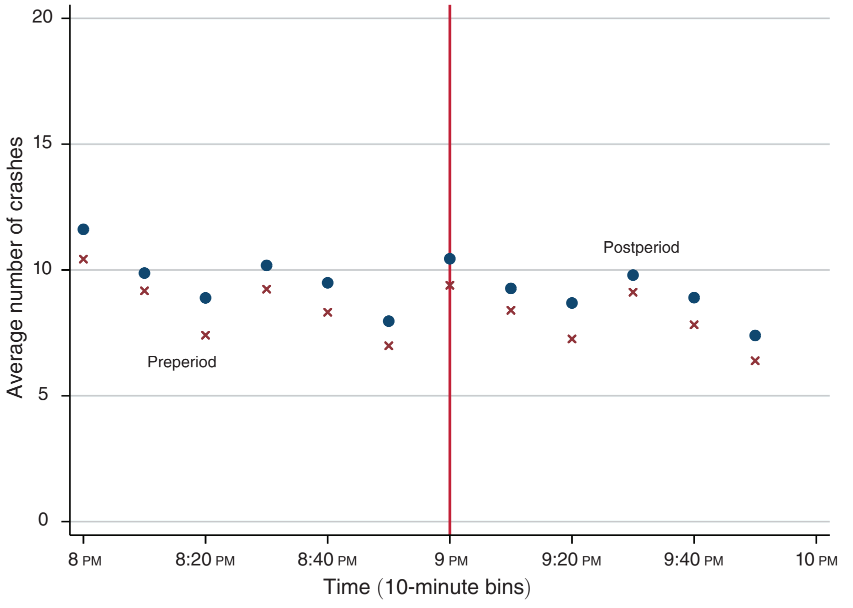

Figure 3. Crash Rate for California from 8pm to 10pm in Preperiods (1995 to 1998) and Postperiods (2005) (*Monday to Thursday*)

credible assumptions regarding evening cell phone use. This not only rejects the 4.3 fold increase in crash risk estimated by RT, but the confidence interval of our estimate fails to overlap with that of RT. The analysis further suggests that cellular use is not analogous to drunk driving as some policymakers and academics have averred. The upper bounds of this study 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 finding is subject to caveats. First, we assess only the local average treatment effect of cell phone use across all drivers and driving conditions around 9 PM from Mondays to Thursdays. Comparisons to other studies, including RT, should be tempered by the recognition that different estimates may reflect distinct local treatment effects. While we observe no obvious threats to the external validity of the present study, such validity rests on complicated differences in traffic patterns, driver composition, and the mix of call type at night from the remainder of the day. Second, while the upper bounds of our study may reject prevalent estimates of the literature, they may still constitute an economically significant level of crash risk. In light of this, we document the substantive implications of our confidence interval for the cost-benefit calculations of policymakers. Finally, our research design does not distinguish between handheld and hands-free use. However, we note that hands-free use was quite uncommon during our estimation period and that laboratory research has generally not found differences in crash risk across these technologies.

We employ two additional empirical approaches that confirm our finding that cell phone use is not associated with higher crash rates. A first approach exploits the nonlinear and heterogeneous take up of cell phone technology across the smallest geographic regions for which data on cellular ownership is available. A second, related, approach estimates the impact of recent legislative bans on handheld cell phones on fatal crashes in a number of states and municipalities.

We offer three main explanations to reconcile our findings with existing research. One possibility is that drivers compensate for the dangers of cell phone use by driving more carefully (Peltzman 1975). Hahn and Tetlock (1999) suggest a second explanation for the absence of an observable effect: drivers with some affinity for risk-taking may be substituting one source of risk (e.g., speaking with a passenger or listening to the radio) with another (i.e., cell phone use). 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, and 37 states already have such legislation on the books.[4] Yet given the economic value of cell phone use to drivers (e.g., Hahn and Tetlock 1999; Hahn, Tetlock, and Burnet 2000; Lissy et al. 2000; Cohen and Graham 2003), our paper casts doubt on at least some policies restricting driver cell phone usage. For instance, if current driving compensate for their phone use with more careful driving, then there may be a rationale for penalizing cellular use as a secondary, but not as a primary, offense. If cellular use is the product of risk substitution, then any legislative ban is inefficient. And if there is heterogeneity in the effect across drivers and driving conditions, then partial and targeted bans are appropriate. More broadly, we document how the confidence intervals from this study sharply alter the value of statistical life that is implicit in such legislation.

The remainder of this paper proceeds as follows. Section I describes the background of research on the link between cell phones and crashes. The following section outlines the empirical approach and accompanying results. In Section III, we report the sensitivity of our findings to underlying assumptions, attempt to reconcile our estimates with the existing research, and comment on policy implications. The final section concludes.

I. 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. One can classify most analyses of crash risk due to cellular use into one of four 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) correlational analyses of aggregate crash records and cell phone ownership; and (iv) longitudinal analyses of individual phone and crash records. Beyond estimating the impact of phone use on crashes,

[4] Nine have banned handheld cell phone use by all drivers and 28 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 February 2012).

other researchers have measured the frequency of such use by drivers. Several excellent recent surveys of these literatures exist.[5]

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 conclude that instructing subjects to use cell phones impairs driving by a factor of three to four (Strayer, Drews, and Johnston 2003) and compare the effects to illicit levels of intoxication (Strayer, Drews, and Crouch 2006). Importantly, this research finds no differences between handheld and hands-free devices (Caird et al. 2008). Simulations illuminate relative levels and types of impairment across distractions, but a shortcoming of such studies, however, is that it is unclear whether cell phone use in simulations is at all analogous to use in environments where driver well-being, or survival, is at stake.

A second set of approaches, naturalistic studies, employ visual and audio recording devices to monitor behavior in authentic driving conditions. In the largest example of this approach, researchers equipped 100 vehicles with cameras and sensors and tracked 241 primary and secondary drivers for over one year (NHTSA 2006). After amassing nearly 43,000 hours of driving data, the authors find no evidence that listening or speaking with a cellular device make drivers more likely to crash (i.e., a modest 1.3 relative crash-risk ratio, with a 95 percent CI of 0.93 to 1.90).[6] Like laboratory studies, naturalistic approaches pinpoint specific causes of driver impairment and characterize their relative danger. Given the high costs, however, the sample sizes are often too small and volunteer drivers too unrepresentative to infer crash risk (Lissy et al. 2000). Additionally, given the lack of exogenous variation in phone use, cellular use in this context may be endogenous to unobserved factors, (e.g., stress), that may be correlated with other forms of inattention or crash risk.

A third strategy, which generates absolute estimates of crash risk, is the comparison of aggregate trends in cell phone ownership with trends in crash rates at the local, state or national level. In a very credible example of this design, Kolko (2009) compares state-year variation in cellular ownership with fatal car crashes from 1997 to 2005. After controlling for various covariates including state and year fixed effects, Kolko's (2009) point estimates, while not statistically significant, imply that the introduction of cell phones led to a roughly 16 percent increase in the annual fatal crash rate (with a 95 percent CI of -7 to $+39$ percent).[7] Kolko (2009) finds a smaller, but statistically significant, correlation between ownership and fatal crashes involving only poor driving conditions (i.e., wet roads or bad weather).

[5] Examples of these surveys include Hahn and Prieger 2006; McCartt, Hellinga, and Bratiman 2006; Prieger and Hahn 2007; Caird et al. 2008. A working paper version of the present paper features a more detailed exposition.

[6] The study does find that 78 percent of the 69 crashes and 65 percent of the 761 "near-crashes" committed by drivers in their sample were due to some form of driver inattention. The study also concludes that dialing a cell phone leads to a relative crash-risk ratio of 3.0.

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

Kolko (2009) also examines the impact of state bans restricting handheld cell phone use with the same framework and 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 three years old), before and after the enactment of bans in California, Connecticut, New York, and Washington DC, to claims in nearby regions (HLDI 2009). Overall, the authors find no evidence that the legislation led to a subsequent decrease in claims.[8]

The high level of aggregation and the strong secular and nonlinear trend in overall crashes in the 1990s (see Figure 1) complicate this correlational approach. 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.[9] 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 (2009) estimates of the correlation between ownership and crashes, as well as the effects of legislation, indicate that the inclusion of region specific time trends or a control period eliminates evidence for a positive correlation.

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. In their influential paper, the authors inspect crash records and detailed phone bills for 699 Toronto drivers recently involved in a minor car crash.[10] 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 driver specific crash free control period before the crash occurred. 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 CI of 3.0 to 6.5) and no statistical difference between handheld (5.3) and hands-free devices (3.9).[11] 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 and, again, finds no significant difference between handheld (4.9) and hands-free devices (3.8) (McEvoy et al. 2005).

While RT is considered perhaps the most influential of this, or any class, of studies, the study suffers from three principle drawbacks. First, the study relies on a very

[8] The study reports ten regression coefficients which correspond to specifications of various driver populations 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.

[9] While Kolko (2009) 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. Table 3 of Kolko (2009) does not provide enough information to infer a 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.

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

[11] The study fails to find significant differences in increased crash risk across age or gender.

unrepresentative sample of drivers recently involved in a crash (Hahn and Prieger 2006). As evidence for such selection, Prieger and Hahn (2007) and Wilson et al. (2003) survey drivers and find that handheld cell phone users are actually more likely to crash even when not on the phone. Second, 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.[12] Finally, 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 immediately after, rather than before, a crash occurred.

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 and further records the times of all calls including those automatically placed in an emergency. The study finds that from 2001 to 2003 hands-free calling among the nearly 3 million OnStar subscribers actually lowered crash risk by a factor of 0.62 (with a 95 percent CI of 0.37 to 1.05). While the study critically records the time of each crash accurately, 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 handful of studies have attempted to estimate the frequency of cell phone use on the road. 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 8 AM to 6 PM 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 percent of drivers were on hands-free phones resulting in a total usage of 10 percent (NHTSA 2005).[14] NOPUS estimates that total use has been steadily increasing over the last several years: from 6 percent in 2002, 7 percent in 2003, 8 percent in 2004 and 10 percent in 2005 (NHTSA 2002 to 2005). NOPUS

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

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

[14] NOPUS also reports the incidence of observed "head-set" use which, in 2005, was 0.7 percent. 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 (9 PM discontinuity)	1.0 times collision risk (3.0 upper bound)	0% increase in crashes (20% upper bound)
Experimental studies	3 to 4 times impairment (Strayer 2003, 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 (RT)	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 estimate of 4.3 relative crash risk, we use the baseline NOPUS usage in 2005 of 10 percent and then add the cellular and non-cellular driver crash risks (i.e., $(0.9 \times 1) + (0.1 \times 4.3)$) to produce a 33 percent increase.

also hints at heterogeneity in cellular use across driver age—but not gender—with handheld use alone approaching as high as 10 percent for drivers from 16 to 24 years in 2005 (Glassbrenner 2005).[15]

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. These studies suggest that cellular use in early night-time hours is not different from use during the day. In the first, conducted in 2006, authors equipped observers with night vision technology at 113 randomly selected intersections in Indiana from 9:30 PM to 5:45 AM (Vivoda et al. 2008). The study finds handheld use to be 6.9 percent among drivers from 9:30 PM to 12 AM ($N = 3774$) which is higher than the corresponding NOPUS estimate of daytime use.[16] A second study, conducted in 2001, specifically assesses cell phone use among high-speed drivers during various points in the day using photographic evidence from 40,000 drivers on the NJ Turnpike (Johnson et al. 2004). On average, only 1.5 percent 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 8 PM to 12 AM) and the afternoon (i.e., from 12 PM to 4 PM) for

[15] A second large-sample study of cellular use tracked long-term legislative compliance in Washington, DC, Maryland and Virginia (McCartt and Hellinga 2007). The study found 5.8 percent daytime handheld use in 2004. This figure is higher than the 4 percent handheld use estimated by NOPUS for 2004.

[16] The study did find that usage dropped significantly after 2 AM (i.e., they estimate 3.1 percent usage from 2 to 4 AM and 1.3 percent usage from 4 to 5:45 AM).

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 and is outlined in the Discussion.

Table 1 also compares the relative and absolute crash risk for representative studies in the literature as well as the present analysis. Calculation of absolute crash risk assumes the 10 percent NOPUS rate of cellular use in 2005, randomization in usage across driver type, and linearity in the influence of ownership on crashes.[17]

II. Empirical Analysis

A. *Description of Data*

This paper relies on a wide array of data on cell phone ownership, cellular pricing plans, call likelihood, and crash records. These sources are enumerated in Table A1 of the online Appendix. We briefly describe the most important data here and relegate remaining detail to the online Appendix.

Cellular Ownership.—Measures of cell phone ownership require data on the number of subscribers as well as the population in a given region. We collect data on subscribers by state for 1999 to 2007, nationally for 1985 to 2005, and by the FCC defined "Economic Area" for 2001 to 2005 and 2007 from the FCC and the Cellular Telephone Industry Association (CTIA).[18] Figure A1 in the online Appendix depicts trends in cell phone ownership nationwide as well as the growth in the average usage of each phone per user.[19] Overall, both ownership and usage increase exponentially over this period. By 2007, five of every six residents owned a cell phone despite only one of three owning a cell phone just eight years earlier.

Cellular Pricing Plans.—The central empirical strategy of the paper exploits the possibility that a discontinuity in call price leads to a change in call likelihood. We estimate the market shares of pricing plans by provider from historical plan data collected from 1999 to 2005 by Econ One Research, and from market share and plan turnover data from the FCC and S&P Industry Reports.[20] The historical plan data covers 26 major markets, 30 providers, and over 30,000 pricing plans, and details each plan's schedule of marginal call prices and the time threshold at which tiered plans transition from peak to off-peak pricing.

[17] Assuming for example that cell phone use occurs during 10 percent of total driving time, then, ignoring selection, a relative crash risk of 4.3 translates to a 33 percent 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 percent in absolute terms.

[18] Historical population data was downloaded from the Bureau of Labor Statistics.

[19] Data on average usage is reported in the annual CMRS Competition Reports published by the FTC.

[20] The historical plan data comes from the "Econ One Wireless Survey: An Internet Survey of Cellular and PCS Pricing Plans." It is generated from screen-shots of provider websites taken each year. Econ One Research provided this data to the authors as a courtesy for academic use. The FCC report is entitled "Annual Report to Congress on the State of Competition in the Commercial Mobile Radio Services Industry," and is available on the FCC website.

Call Likelihood.—To illustrate the relationship between call volume and call pricing, we primarily rely on a large and proprietary dataset of calls by likely drivers during an eleven day period in 2005 acquired from a major network provider. The data are restricted to calls routed through multiple cell phone towers in a contiguous, highly populated, region in California spanned by the coverage of approximately 300 to 400 towers (a single "switch"). The mechanics of signal switching are such that a call is originally routed by the tower emanating the strongest signal (typically the tower in closest proximity to the caller). A call in progress is 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 switching design, signals from stationary or even ambulatory callers are almost always routed by a single tower.[21] Rare exceptions exist when a caller is walking through a region with large buildings that interfere with a given tower's reception. However, our data are from a switch servicing 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.[22] 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.[23] At an average estimated duration of 2.6 minutes per call (calculated from a second dataset of calls from TNS Telecom and described below), this translates to 106,000 to 477,000 phone calls.

Two additional datasets of calls permit assessments of the price sensitivity of a broader population of cellular users that extends beyond drivers. The first additional dataset (hereafter, MIT) was acquired from researchers at the MIT Media Lab who implanted surveillance technology in cell phones to track subject movements, interactions, and communication over the course of the academic year.[24] A total of 65 subjects placed approximately 80,000 outgoing cell phone calls from August 2004 to May 2005. A second, more representative, dataset, features over 741,000 calls made by 9,864 cell phone users in 2000 and 2001 and is assembled from cell phone bills submitted by households randomly selected as a part of wider survey of telecommunications behavior administered by TNS Telecom (hereafter, TNS). The online Appendix provides greater detail on these data.

Crash Records.—Our analysis principally relies on two sources of crash data. First, the State Data System (SDS) provides data for the universe of reported crashes from 1990 to 2005 for California, Florida, Illinois, Kansas, Maryland, Mississippi, Missouri, Ohio, and Pennsylvania.[25] A well recognized drawback of using a crash database based on self-reports is the presence of substantive periodic heaping. The

[21] Engineers from the network provider estimate that a given caller must travel at least approximately two miles before a call will switch towers. Therefore, our dataset almost certainly comprises calls made by callers in moving vehicles.

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

[23] The provider multiplied the data by some integer from two to nine to preserve the anonymity of the call volumes.

[24] Eagle, Nathan, and Alex Pentland. 2006. "Reality Mining: Sensing Complex Social Systems." *Personal and Ubiquitous Computing* 10 (4): 255–68.

[25] 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. A handful of state-years suffer from data limitations ranging from complete

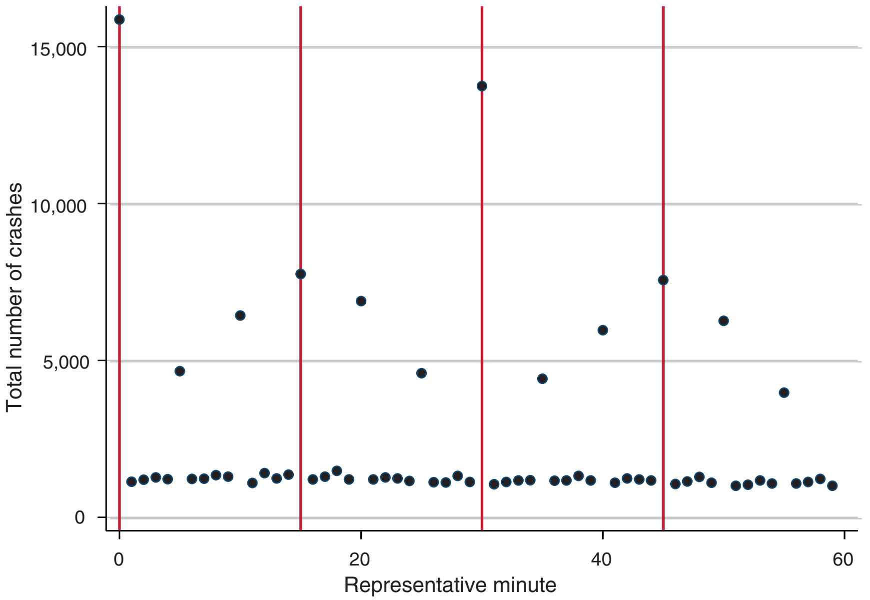

Figure 4. Periodicity in SIDS Crashes across Representative Hour in 2005 for All States in Sample

trajectory of a crash record helps to illuminate the origins of this bias. Once a vehicular crash is reported, police at the scene document various details of the incident, including the minute of the crash occurrence, and submits the paperwork to one of several possible state agencies. While states 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.[26] Figure 4 illustrates the nature of the heaping in reports that characterizes a representative hour in 2005 across the states in our sample. A close examination indicates that nearly 11 percent of crash reports fall exactly on the hour, 31 percent are on the hour, half hour, or quarter hour, and 61 percent reside in a minute ending in either zero or five.

Second, the Fatality Analysis Reporting System (FARS), also administered by the NHTSA, 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. Like the SDS data, FARS suffers from severe periodicity in the specific minute of the crash reports.

Figure 1 depicts the trends in crashes, indexed to highway traffic volume, for each year from 1988 to 2007.[27] 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

unavailability to state-years for which a critical variable is not reported (e.g., Pennsylvania in 2002; Illinois in 2004 and 2005).

[26] States differ in the criteria used to qualify a crash for reporting. Minor crashes below a minimum dollar value (typically \$400 to \$500) or not requiring a tow-away may not be reported.

[27] Crash data for this plot is from the General Estimates Survey, a national probability sample calculated by the NHTSA, and FARS.

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). In recent years, there have been about 40,000 fatal crashes, and approximately six million total crashes reported each year nationwide.

B. *Estimation Strategy and Identifying Assumptions*

We articulate the estimation strategy and identifying assumptions through a conceptual model. Let $\ln(Crash_{rpwt})$ refer to the log number of reported crashes in region r in either a "post" or "pre" period, indicated by p, during weekdays (i.e., Mondays to Thursdays) or weekends, 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) $(p = 1)$, while "pre" refers to the period of low average call likelihood and prior to the prevalence of 9 PM pricing plans (e.g., 1995 to 1998) $(p = 0)$. In this framework, reported crashes are jointly determined by the traffic level or changes in driver composition, $Traffic_{rpwt}$, bias in the timing of the crash report, $RepBias_{rpwt}$, and the covariate of interest, $CallVol_{rpwt}$, which indicates the number of cell phones in use.

$CallVol_{rpwt}$ describes the volume of cell phone use by drivers on the road. This measure is a product of the number of vehicles on the road, $Traffic_{rpwt}$, and the likelihood of a given driver making a phone call, $CallLike_{rpwt}$. This likelihood of making a call is determined by a set of long-run factors including the level of cell phone ownership, legislation, average rates of cellular pricing, and the sophistication of handset technology, as well as by short-run factors including variation in call price. A vector of additional covariates, $\mathbf{X}$, such as speeding regulations, weather conditions and visibility, and the availability and adoption of safety technology may also directly influence the rate of crashes:

$$\begin{aligned}\ln(Crash_{rpwt}) = \alpha &+ \theta_1 Traffic_{rpwt} + \theta_2 RepBias_{rpwt} + \theta_3 X_{rpwt} \\ &+ \lambda CallVol_{rpwt}(CallLike, Traffic) + \varepsilon_{rpwt}.\end{aligned}$$

It is possible that drivers who use cell phones have a greater affinity for risk, and that the risk affinity, R, of drivers on the road produces a higher likelihood of entering into a crash: $E(\varepsilon \mid R) \neq 0$. Since $CallLike_{rpwt}$ 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 9 PM pricing threshold. Any break that we now observe in crashes can be attributed to a change in the remaining covariates.

One can characterize the change in crashes during some time window immediately before the threshold, t', from one immediately after the threshold, t, by calculating a first difference, D_{r11_t}. We initially restrict focus to the postperiod and assume that **X** and long-run determinants of $CallLike_{rpwt}$ are unchanged locally around the threshold to produce

$$D_{r11t} = \ln(Crash_{r11t}) - \ln(Crash_{r11t'}) = \theta_1 \Delta Traffic_{r11t} + \theta_2 \Delta RepBias_{r11t} + \lambda \Delta CallVol_{r11t} + v_{r11t}.$$

The change in call volume across the threshold, $\Delta CallVol_{r11t}$, is now a function of changes linked to traffic and driver composition and short-run variation in call likelihood driven by price.

One might advocate the use of a standard regression discontinuity approach to estimate the effect of cellular use across the threshold. This approach would describe the change in crashes, induced by a fall in prices, by fitting higher order polynomials on either side of 9 PM on weekdays in recent years. Such a design assumes, however, that covariates other than cellular price change smoothly across the 9 PM threshold. The reporting bias, as well as the possibility of on-hour changes in traffic and driver mix, complicates a standard regression discontinuity design.[28]

In the face of covariates, such as traffic patterns, driver composition, or reporting bias, that may vary across this first difference, we calculate a second difference, DD_{rp1t}, by comparing the first difference in crashes around the time threshold during the postperiod from a similar difference calculated for the preperiod. Covariates whose variation is stable across each difference fall out of the equation:[29]

$$DD_{rp1t} = D_{r11t} - D_{r01t} = \lambda(\Delta CallVol_{r11t} - \Delta CallVol_{r01t}) + v'_{rp1t}.$$

The double difference in crash rates is now simply a function of the residual change across the threshold in call volume. We can attribute the change in call volume, in turn, solely to relative changes in call price in the preperiod and postperiod. If the change in likelihood due to price is absent in the preperiod, then the double difference in price reduces to a single difference in price at 9 PM in the postperiod.[30] Finally, to allay the concern that the differences in reporting bias or other unobserved factors may systematically vary across the preperiod and postperiod, as a placebo check, we can calculate analogous double differences for the weekend, DD_{rp0t}, and for proximal hours.

In the exposition that follows, we explicitly estimate the postperiod first difference in call volume, due strictly to an exogenous change in price, $\Delta CallVol_{r11t}|_{-price}$, and provide evidence that the comparable difference in the preperiod, $\Delta CallVol_{r01t}|_{-price}$, is either zero or negligible. We next estimate the change in the relative crash rate across the threshold in the post as compared to the preperiod,

[28] We estimate parametric RD fits around 9 PM and these results are available upon request. While estimates for the treatment period are sensitive to the choice of the fitted polynomial, we find nearly identical estimates for the treatment and control period for a given specification. This is consistent with the present analysis (and suggests that the RD is not fully accomodating either the heaping in crash reports or an unobserved change in other covariates across the threshold).

[29] An example of a factor that might systematically change across the 9 PM threshold, but whose double difference should not change systematically across the preperiod and postperiods, is daylight.

[30] Importantly, if one believes that call likelihood does change across the threshold in the preperiod, due to some unobserved factor, than 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 five times higher in the postperiod relative to the preperiod, then a 2 percent rise in 9 PM call likelihood in the preperiod, is only equivalent to a 0.4 percent change across the threshold in the postperiod.

DD_{rp1t}. These two estimates ultimately permit us to back out the parameter of interest, λ, as well as upper bounds for the crash risk associated with cellular use.

C. *Change in Call Volume at 9 PM Threshold*

Pricing Plans.—Over the past decade, 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 6 AM to 9 PM each weekday, unlimited use for off-peak periods after 9 PM and before 6 AM on weekdays, and unlimited use all day on weekends.[31] Marginal fees for excess usage commonly range from \$0.35 to \$0.45 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 (Econ One Research), inferred market shares for each category of pricing plans (FCC), and data on plan turnover (S&P Industry Reports). 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 (see online Appendix Table A2). 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 new adopters and those switching 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 5 is not highly sensitive to such assumptions. 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.

The figure suggests that from 2002 to 2005, 9 PM pricing plans were the most popular category of cellular plans with an approximate 55 percent share of all subscribers. The prevalence of 9 PM 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 9 PM 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 does not exist prior to 1999, numerous analyst and industry reports, as well as news articles, offer no evidence for a national 9 PM calling plan of any popularity in the years prior to 1999.[32] Accordingly, we treat the years prior to 1999 as a control for the analysis.

[31] Actual plans often specify some large, but finite, limit for off-peak usage. These limits, sometimes marketed as "unlimited," are typically 5,000 to 10,000 minutes.

[32] 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 9 PM switching threshold. Moreover, due to low ownership

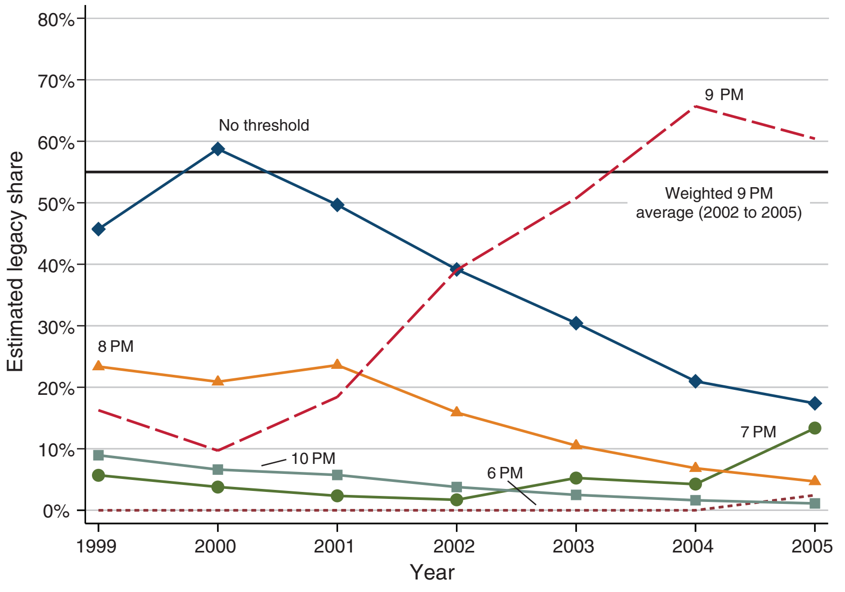

FIGURE 5. ESTIMATED LEGACY SHARE FOR NATIONAL PLANS BY PRICE THRESHOLD AND YEAR FOR 1999 TO 2005

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 percent of cell phone users delay their calls to avoid peak usage.[33] 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 percent of their usual bill.[34] 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 the dataset of 106,000 to 477,000 cellular calls made by callers in moving vehicles in California during an 11-day period in 2005. Figure 2 depicts call volume for callers for each minute from 8 to 10 PM for Mondays to Thursdays, Fridays, and the weekend across the sample. A vertical line marks the 9 PM 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 9 PM as compared to weekends and Fridays.[35] Why might callers treat Friday as distinct from other weekdays? One speculates that this pattern in calls may be due to the lessened salience of the price

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.

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

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

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

change on Fridays—when a weekend of off-peak pricing awaits—as compared to other weekdays. This behavior is also evident in two additional datasets of cellular calls documented in the online Appendix.

Importantly, the fraction of users that subscribe to 9 PM 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 lower than the overall fraction across all providers in 2002 to 2005 according to our analysis of legacy. Our provider began offering a highly publicized alternative pricing plan in 2004 which featured an earlier switching hour.[36] 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.

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:

$$\ln(Calls/Traffic)_t = \alpha + \gamma After\ 9\ \text{PM}_t + \varepsilon_t,$$

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.[37] $After\ 9\ \text{PM}_t$ is a dummy variable indicating whether the call occurred on or after 9 PM and is the explanatory variable of interest. The model is estimated from 8 to 10 PM separately for Mondays to Thursdays, Fridays and weekends.[38] 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 table confirms the pattern evident in the figures—call likelihood increases by 7.2 percent from 9 to 10 PM on Mondays to Thursdays. There is a sharp local rise in call likelihood at 9 PM and this rise appears to persist until at least 10 PM. While changes to call likelihood away from the threshold could potentially be due to changes in factors unrelated to price (e.g., driver composition or propensity to call), unrelated to price, the size and stability of the increase is consistent with price playing a sustained role in heightened 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 directly observe the change in call likelihood during the control period, we are persuaded that driver call likelihood did not sharply rise at 9 PM for two reasons. First, as previously noted, the control period is characterized by the absence of 9 PM calling plans. Second, while somewhat imprecisely estimated, there is no evidence for a rise in call likelihood across hours not associated with a price change. The lower panel of Table 2 examines the

[36] While we do not disclose the details of this calculation for confidentiality, the ratio of the rise in call volume at 9 PM 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.

[37] We 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 online Appendix.

[38] The analysis of Fridays relies on traffic data at the five-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.

TABLE 2—CHANGE IN CALL LIKELIHOOD AT 9 PM THRESHOLD

	Dependent variable—ln(scaled calls/traffic) per minute				
	Monday to Thursday				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 9 PM	0.072*** (0.004)	0.067*** (0.005)	0.082*** (0.006)	0.070*** (0.009)	0.041*** (0.008)
Observations	$N = 600$	$N = 300$	$N = 150$	$N = 50$	$N = 20$
	Monday to Thursday		All days	Weekend	
	8 PM	1 PM	5 to 10 PM (no 9 PM)	9 PM	
	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 9 PM				0.025 (0.021)	
After 8 PM	0.027* (0.015)				
After 10 PM		0.006 (0.018)			
After hour			−0.016 (0.027)		
Observations	$N = 50$	$N = 50$	$N = 400$	$N = 20$	

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 occuring 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 9 PM threshold, as well as the local change at 9 PM for Friday callers. The lower panel estimates the local change in moving call likelihood for proximal hours (i.e., 8 and 10 PM) during Mondays to Thursdays, evening hours from 5 to 10 PM, excluding 9 PM, across all days, and 9 PM on weekends. All specifications are estimated with OLS at the minute level, although Friday estimates rely on traffic data at the 5-minute level. Robust standard errors clustered by date are reported parenthetically.

*** Significant at the 1 percent level.

* Significant at the 10 percent level.

local change in call likelihood for a series of placebo hours not associated with a pricing change including weekends at 9 PM as well as proximal and "composite" hours on other days. Moreover, given the low baseline call likelihood prior to 1999—due to low cell phone ownership, low monthly average usage, and the scarcity of hands-free technology during this period—any incidental rise in on-the-hour calling does not threaten the research design.[39]

[39] To illustrate, suppose that in 1998 the rise at 9 PM in call likelihood among drivers is 2 percent. Allowing for an average baseline call likelihood during this period of 2 percent (given the 2000 NOPUS estimate of 4 percent, and considering changes in ownership, monthly usage and availability of hands-free technology during the prior two years, we believe that 2 percent is a conservative estimate) yields a net change in call volume at 9 PM of 0.04 percent (i.e., 0.02×0.02). Given that by 2005, average usage grew to 10 percent, producing an equivalent net change in the absolute number of cellular users in the treatment and control period would require only a 0.4 percent

A skeptic might contend that some fraction of the callers in our dataset are 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 among moving callers across the day and specifically at 9 PM. We infer average call likelihood from the extensive literature that surveys such use (e.g., NOPUS). The composition of the data are a concern if passenger callers are *differentially* more sensitive at 9 PM 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 lack 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 for single, 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 9 PM 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 percent to 6.8 percent 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 percent. 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 percent.[40] If calls by passengers also distract drivers, even differential price sensitivity between drivers and passengers would not be cause for concern with respect to the research design.

Generalizability of First Stage.—We next assess whether the exhibited price sensitivity generalizes across years, providers, and geography. A comparison of cellular ownership, using FCC data, reveals that 2005 ownership in the region associated with our primary first stage (78 percent) was roughly comparable to statewide ownership (68 percent) and national ownership (71 percent).

rise in calling at 9 PM in the treatment period. We arrive at this calculation by scaling the hypothetical preperiod 9 PM rise in likelihood of 2 percent by the ratio of the 2005 and 1998 average call likelihood (10 percent/2 percent).

[40] We calculate the effective driver first stage in the case of differential price sensitivity with (i) the share of single and multiple occupant vehicles on the road (2005 crash data for California indicates that 23 percent of the 970,000 vehicles in the sample are multiple occupant); (ii) the baseline call likelihood of drivers in both vehicle types from NOPUS (13.3 percent and 3.3 percent, respectively, after handheld figures are scaled to account for handheld and hands-free use); (iii) an initial assumption that passengers share the calling norms of their accompanying drivers; (iv) 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 percent. If likelihood and price sensitivity are 3x higher for passengers, then the effective first stage is 5.4 percent. To illustrate the calculation for a 2x increase in both parameters, note that the passenger share of mobile individuals on the road is 19 percent (i.e., $0.23/(0.77 + 0.23 + 0.23)$). Given the baseline likelihood across occupants, and an assumption of 2x higher passenger likelihood, the passenger share of total cellular usage is 13 percent (i.e., $2 \times (0.19 \times 0.033)/(2 \times 0.19 \times 0.033 + 0.19 \times 0.033 + -0.63 \times 0.134)$). Next, if x is the rise in driver call likelihood at 9 PM, and we further assume that passengers are 2x as price sensitive as drivers, then, $0.87\text{x} + 0.13 \times 2\text{x} = 7.2$ percent which implies an effective driver first stage of $x = 6.4$ percent. 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.

The MIT and TNS datasets provide additional evidence through which one can generalize the price sensitivity of cellular use across time, geography, and provider (see online Appendix for full detail). Online Appendix Figure A2 plots 80,000 outgoing calls from the MIT data and depicts a sharp increase of 23 percent in calls made at 9 PM on Mondays to Thursdays but not Fridays, weekends or surrounding hours. Online Appendix Table A3 reports that, in the TNS data, the relative rise in call volume in the hour subsequent to a plan's pricing threshold is also 23 percent on Mondays to Thursdays, and is smaller and statistically insignificant on other days.[41]

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 an artifact of the region and time which characterizes the primary first stage data. We next turn to the question of whether crash rates respond to the increased cellular usage induced by a change in prices.

D. *Change in Crash Rate at* 9 PM *Threshold*

Reporting Bias.—An analysis of crash rates demands first addressing the reporting bias in crash reports. One strategy through which to deal with heaping in crashes 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). While aggregation accounts for misreporting within a bin, it does not remedy misreporting that may occur across bins. Aggregation additionally 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 heaping across bins and to increase the precision and accuracy of the estimates.[42] Additionally, in a series of robustness checks we alter the strategy used to bin crashes and show that our results are insensitive to the econometric treatment of reporting bias 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. Figure 3 displays the pattern of average crashes across ten-minute intervals from Mondays to Thursdays in California from 8 to 10 PM in 2005 as compared to the analogous pattern for the preperiod from 1995 to 1998. The cyclicality of the plot is due to the aforementioned reporting bias. The vertical line marks the 9 PM pricing plan threshold. Figure A3 in the online Appendix compares Monday to Thursday crashes in California from 8 to 10 PM in 2005 to a second control of weekend crashes. Neither plot provides evidence for a relative rise in crashes at the pricing threshold.

[41] To 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.

[42] In an analytic model, available from the authors, we show that the double difference largely remedies complications due to reporting bias so long as the bias, and the curvature of the crash trajectory, is comparable in the preperiod and postperiod.

We formally estimate the relative change in crashes around 9 PM on Monday to Thursdays with the following Poisson model:[43]

$$E[Crash_{symdtb} \mid .\,] = \exp(\alpha + \beta(Post_y \times After\ 9\ \text{PM}_b)_{yb} + \gamma_1 After\ 9\ \text{PM}_b$$

$$+ \gamma_2 Post_y + \phi_s + \eta_y + \delta_m + \xi_d),$$

where $Crash_{symdtb}$ denotes 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 9 PM, and $After\ 9\ \text{PM}_b$ is a dummy variable indicating whether the crash occurred on or after 9 PM. The interaction term $(Post \times After\ 9\ \text{PM})_{yb}$ is the explanatory variable of interest. The model controls for state, year, month, and day of the week specific variation.

Intuitively, the experiment simulated by this regression is a comparison of the difference in precrash and postcrash around the threshold for symmetric estimation windows around 9 PM 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 9:59 PM 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 9:29 PM. The narrower estimation window around 9 PM 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 bias. As expected, standard errors increase for the tighter estimation windows. The choice of the control period is dictated by the low prevalence of 9 PM 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.[44]

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.

Our estimation approach relies on the constancy of important covariates across the threshold in the postperiod relative to the preperiod. 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 using data from the PeMS traffic database.[45] We find no evidence for a significant change in traffic across the 9 PM threshold relative to the control period.[46] Second, to verify the constancy of reporting bias, at least for the 30-minute estimation, we test for a

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

[44] Results of these estimations are available from the authors upon request. Note that 1990 is the earliest possible bound for a control period due to data availability.

[45] We note that PeMS covers freeway traffic only. The call data is from an area featuring a mix of freeway and surface roads. However, our spot analysis of surface road traffic, using counts from city DOT websites, indicates that traffic patterns around 9 PM are very similar across the two road types.

[46] We estimate the double-difference regressions of log hourly traffic counts at the traffic station x date level in California for 8 to 10 PM, Monday to Thursday, in the preperiod and postperiod. The coefficient of interest is $b = 0.0040$, se $= 0.0045$ for 2005 and $b = -0.0012$, se $= 0.0042$ for 2002 to 2005. The regressions include fixed

TABLE 3—RELATIVE PREPOST (*Monday to Thursday*) CHANGE IN CRASH RATE AT 9 PM THRESHOLD

	Dependent variable—crashes per minute bin California			
	2005		2002 to 2005	
	8:00–9:59 60-minute bin	8:30–9:29 30-minute bin	8:00–9:59 60-minute bin	8:30–9:29 30-minute bin
	(1)	(2)	(3)	(4)
Post × *After* 9 PM	−0.001	0.004	−0.010	−0.012
	(0.019)	(0.024)	(0.012)	(0.016)
Observations	$N = 2{,}088$	$N = 2{,}088$	$N = 3{,}342$	$N = 3{,}342$
	Expanded states			
	2005		2002 to 2005	
	8:00–9:59 60-minute bin	8:30–9:29 30-minute bin	8:00–9:59 60-minute bin	8:30–9:29 30-minute bin
Post × *After* 9 PM	−0.014	−0.003	−0.006	−0.002
	(0.013)	(0.015)	(0.008)	(0.010)
Observations	$N = 17{,}960$	$N = 13{,}784$	$N = 28{,}410$	$N = 21{,}726$

Notes: The table presents the estimate of the change in crashes at 9 PM on Mondays to Thursdays in the post (i.e., 2005, and 2002 to 2005) relative to the preperiod (1995 to 1998) as specified in the text. The *Post* × *After* 9 PM dummy denotes crashes occurring on or after 9 PM in the postperiod. The upper panel presents the results for California. The first two columns estimate the model using 60- and 30-minute bins respectively for 2005, while the next two columns present analagous 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, Mississippi, Missouri, Ohio, and Pennsylvania. Pennsylvannia 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 × 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.

change in the fraction of total crashes reported within the first 30 minutes of each hour in the post as compared to the preperiod. Again, we cannot reject the that this fraction is identical across periods.

Next, to heighten the precision in the regressions, we estimate the model for an extended period from 2002 to 2005 which corresponds to the duration of the first stage evidence. The remaining two columns of the upper panel show no evidence for a positive double difference in the crash rate for either the 30- or 60-minute window. Finally, we estimate the model for the full set of states for which we have crash data: California, Florida, Illinois, Kansas, Maryland, Mississippi, Missouri, Ohio, and Pennsylvania.[47] The expanded state-year sample comprises approximately 8 million crashes. Online Appendix Figure A4 depicts the distribution of crashes in the pre and postperiod for the expanded sample of states.

effects to control for station, year, month, and day of the week specific variation. Errors are robust and clustered at the date level.

[47] Some 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 lower panel of Table 3 presents regressions for the expanded set of states. Illinois is excluded from the 2005 analysis since no data are 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.[48] Extending the sample to multiple years reduces estimated standard errors but does not substantively change the point estimates. Our two favored specifications, for the expanded set of states and 60-minute bins, generate an upper bound of the relative change in the crash rate of 0.97 percent for 2002 to 2005 and 1.18 percent for 2005.[49] Overall, the results provide no evidence for a positive relative change in the crash rate.[50]

We repeat our benchmark analysis for the subset of fatal crashes with FARS data. 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 nonfatal 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 9 PM on Mondays to Thursdays in 2002 to 2005 compared to 1995 to 1998 is actually negative and marginally significant ($b = -0.058$, s.e.: 0.033). The corresponding placebo estimate for weekends is slightly positive and insignificant ($b = 0.028$, s.e.: 0.042).[51]

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 10 PM 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 9 PM. The analysis confirms the absence of a strong negative change in the crash rate around the threshold for weekday proximal hours, or weekends at 9 PM, that could mask a potential effect of cellular use at 9 PM. Additionally, we estimate, but do not report in the table, triple difference estimates, using the change across

[48] We additionally estimate the model for a constant set of states across the 60-minute and 30-minute bins. Excluding Michigan, Ohio, and Illinois, the 60-minute coefficient for the *Post* $\times$ *After* 9 PM interaction for 2005 is $b = -0.008$, se $= 0.013$, and for 2002 to 2005 is $b = -0.003$, se $= 0.009$. The analogous estimate for the 30-minute coefficient for 2005 is $b = -0.003$, se $= 0.016$ and for 2002 to 2005 is $b = 0.000$, se $= 0.010$.

[49] Note 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 percent confidence interval were produced by our statistical program but can also be calculated manually using the delta method.

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

[51] Just as in our benchmark analysis, a Poisson model estimates regressions at the state-date-bin level. We examine 60-minute bins before and after 9 PM 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.

TABLE 4—RELATIVE PREPOST (*Monday to Thursday*) CHANGE AT 9 PM— PLACEBO AND ROBUSTNESS CHECKS

	Dependent variable—crashes per minute bin					
	Placebo checks expanded states, 2002 to 2005					
	8 PM		10 PM		Weekend	
	7:00–8:59 60-minute bin (1)	7:30–8:59 30-minute bin (2)	9:00–10:59 60-minute bin (3)	9:30–10:29 30-minute bin (4)	8:00–9:59 60-minute bin (5)	8:30–9:29 30-minute bin (6)
Post × *After* "X" PM	0.006 (0.007)	0.002 (0.009)	−0.006 (0.007)	0.007 (0.010)	0.011 (0.012)	0.010 (0.013)
Observations	$N = 28410$	$N = 21{,}726$	$N = 28{,}410$	$N = 21{,}726$	$N = 14{,}176$	$N = 10{,}840$
	Robustness checks Expanded states, 2002 to 2005					
	8:45–9:14 15-minute bin	8:01–10:00 60-minute bin Start bin :01	8:01–9:59 59-minute bin No :00	8:01–10:00 58-minute bin No :00, :30	8:01–9:59 48-minute bin No :05s	8:31–9:29 24-minute bin No :05s
Post × *After* 9 PM	0.001 (0.013)	−0.013 (0.008)	−0.011 (0.009)	−0.004 (0.009)	−0.006 (0.011)	−0.017 (0.015)
Observations	$N = 21{,}726$	$N = 21{,}726$	$N = 21{,}726$	$N = 21{,}726$	$N = 21{,}726$	$N = 21{,}726$

Notes: The table presents results from placebo and robustness checks of the crash estimates for the baseline specification of expanded states from 2002 to 2005, as specified in the text. The upper panel presents results from a series of placebo estimates for Mondays to Thursdays at 8 PM and 10 PM, as well as for 9 PM 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 9 PM 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 9 PM spike is included in the bin preceding rather than following 9 PM 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.

proximal hours and 9 PM on weekends, as additional checks which are consistent with these results.[52]

The first column of the lower panel of Table 4 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 windows but is less precise. 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 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

[52] We 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 9 PM is $b = -0.0082$, se = 0.0138, when using the 10 PM hour as a double difference control, and is b = 0.0004, se = 0.0135, when using 8 PM 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 $b = -0.0177$, se = 0.0147.

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, we exclude crashes at exactly at 8:00 and 9:00 in both the pre and postperiods; we then additionally exclude crashes occurring at 8:30 and 9:30; and, finally, we exclude crashes occurring at every five-minute increment. Omitting these data points does little to change the underlying pattern in point estimates but does produce greater imprecision. The final column of the second panel eliminates each five-minute increment but for the shorter 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.

In summary, the 9 PM 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 0.97 percent in the fully expanded specification and 1.18 percent for the expanded set of states in 2005.

E. *Panel Analyses of Ownership, Legislation, and Crashes*

We briefly describe two alternative empirical approaches which supplement and confirm our basic results. Full details of these approaches are provided in the online Appendix.

In the first alternative approach, we compare aggregate trends in crashes and cellular ownership at the level of the state and EA. EAs are used by the FCC to denote regions of contiguous economic activity (172 nationwide) and represent the most disaggregated geographic units for which data on cellular ownership data are 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 find no statistically significant link between change in ownership and crashes ($b = -0.0004$, s.e.: 0.0014 for all crashes; and $b = 0.002$, s.e.: 0.001 for fatal crashes).

In a second related approach, we estimate the influence of recent legislative bans restricting handheld cellular use by drivers in New York, New Jersey, and Connecticut, as well as the large municipalities of Chicago and Washington, DC. Noting that the effect of legislation on crashes is determined by both compliance as well as relative difference in crash risk associated with handheld and hands-free use, we use a panel analysis to trace the relative monthly time-path of fatal crashes in regions following the imposition of the bans.

Figure 6 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 in part, to drops in traffic as a result of the attacks on September 11, 2001. In fact, the New York legislation, while

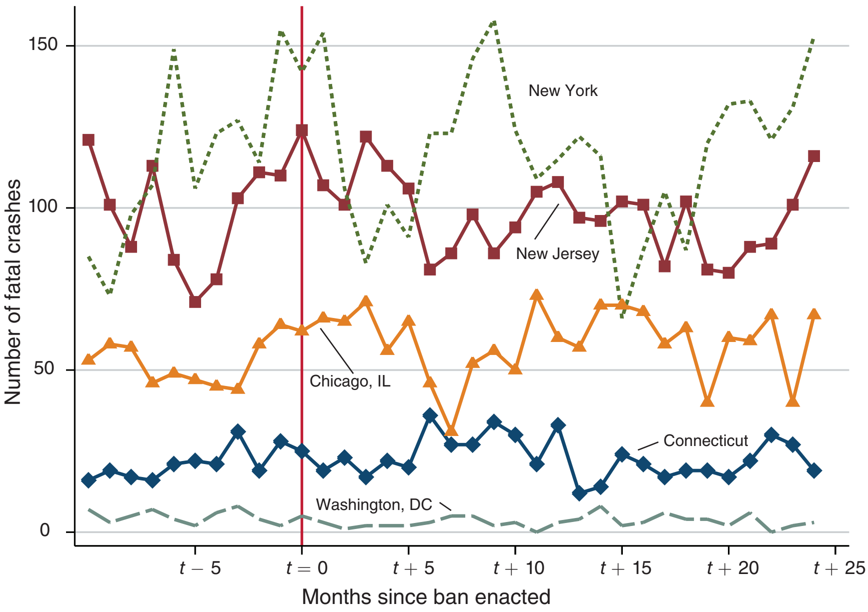

Figure 6. Fatal Crash Rate Before and After Cellular Ban

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. A more formal regression analysis, detailed in the online Appendix, also indicates that the legislation did not lead to a significant reduction in the fatal crash rate over short or longer run horizons.

III. Discussion

Sensitivity of Results to Assumptions.—The present analysis suggests the counterintuitive finding that cell phone use by drivers is not associated with higher crash rates. Whether the upper bounds from the analysis are able to reject relative risk rates from existing research, including the 4.3 odds-ratio of RT, requires that we translate estimates of the change in crash rate at 9 PM to estimates of crash risk associated with cellular use. This translation depends on two key parameters linked with driver calling behavior—average call likelihood and the increase in call likelihood at 9 PM.

A first key parameter relates to the average call likelihood at 9 PM during the treatment period. Evidence exists that usage during the evening is no lower than average use across the day. Beyond the two aforementioned studies that have concluded 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 8 AM to 6 PM and 8 to 9 PM on Mondays to Thursdays, on an indicator signalling inclusion in the 8 to 9 PM hour, suggests that cellular usage, as a fraction of traffic, from 8 to 9 PM is significantly higher than the average use during the NOPUS day ($b = 0.317$, s.e.: 0.004). A similar estimation indicates that usage at precisely 9 PM is also significantly higher than over the NOPUS period ($b = 0.353$, s.e.: 0.020). Together, the evidence suggests that the NOPUS estimates of daytime use (i.e., 7.8 percent from 2002 to 2005, and 10 percent in 2005) are legitimate, and even conservative, proxies for use during the late evening.

A second key parameter regards the rise in cellular call likelihood at 9 PM.[53] The first stage data conservatively indicates a 7.2 percent increase in call likelihood from Mondays to Thursdays at the pricing threshold for likely drivers. Our calculations of legacy shares indicate that these data are from a provider and a period which almost certainly underrepresents the fraction of users at 9 PM as compared to other providers in 2005 or across 2002 to 2005.

To explore the sensitivity of our findings to variation in the above parameters, Table 5 compares the upper bounds of relative crash risk implied by our preferred estimates (i.e., expanded states from 2002 to 2005, and expanded states for 2005) across a range of values for average driver call likelihood and the change in call likelihood at 9 PM. For example, if average call likelihood is 7.8 percent, and 9 PM call likelihood rises by 7.2 percent, the 0.97 percent upper bound for the estimated change in the crash rate (expanded states from 2002 to 2005) implies a 95 percent upper bound in crash risk of 3.0 (and a 1 s.e. upper bound of 1.4).[54] Fixing the change in 9 PM likelihood at 7.2 percent, an average call likelihood as low as 6 percent would reject RT (crash risk of 4.3) with an implied crash risk of 3.6 (expanded states from 2002 to 2005). Moreover, to the extent that dialing intensity jumps discontinuously at 9 PM, assuming dialing is more dangerous than simply talking, then the pertinent baseline crash risk from the existing literature may be higher than 4.3.

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. Similar to the "Peltzman Effect," popularized by Peltzman (1975) in the context of safety belts, drivers may slow down, pull over, shift to uncongested lanes, or simply heighten attention in response to making or receiving a cell phone call. In the online Appendix, we present a

[53] It is worthwhile to note that the estimated behavioral response at 9 PM is based on changes in cellular use 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.

[54] The change in the indexed crash rate at 9 PM is the sum of the change due to cellular users and nonusers: $\Delta\%CrashRate_t^{UB} \times CrashRate_t = \Delta CrashRate_t^{Cell} + \Delta CrashRate_t^{Non-Cell} = [x \times \Delta CellUse_t] + [1 \times \Delta NonCellUse_t]$. $\Delta CrashRate_t^{Cell}$ 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 9 PM call likelihood and average call likelihood during the period of interest. $\Delta CrashRate_t^{Non-Cell}$ 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 nonusers at 9 PM (i.e., $\Delta NonCellUse_t$). Similarly, the baseline crash rate, $CrashRate_t$, is a sum of the crash rates of cellular and noncellular users prior to 9 PM. The populated equation for the 2002 to 2005 estimate is $0.0097[1 \times (1 - 0.078) + x(0.078)] = [x(0.072 \times 0.078) + 1(-0.072 \times 0.078)]$. Solving for x yields 3.0.

Table 5—Sensitivity of Crash Risk Implied by Upper Bounds of 9 pm Analysis

	Crash risk implied by upper bounds							
	Expanded states (2002 to 2005)				Expanded states (2005)			
	Baseline 9 pm call likelihood				Baseline 9 pm call likelihood			
9 pm Δ call likelihood	**6%**	**7%**	**7.8%**	**9%**	**7%**	**7.8%**	**9%**	**10%**
5.2%	4.8	4.3	3.9	3.5	5.2	4.8	4.3	3.9
6.2%	4.1	3.6	3.4	3.1	4.4	4.0	3.6	3.4
7.2%	3.6	3.2	3.0	2.7	3.8	3.5	3.2	3.0
8.2%	3.2	2.9	2.7	2.5	3.4	3.2	2.9	2.7
9.2%	3.0	2.7	2.5	2.3	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 9 pm 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 9 pm. The relative risk of crashing if using a cell phone while driving can be calculated by solving for x in the following expression: $ub[1 \times (1-b) + x(b)] = [x(bc) + 1(-bc)]$, where ub is the upper bound on our benchmark result, b is the baseline likelihood of cellular use by drivers, and c is the percent jump in likelihood at 9 pm. An illustrative calculation is outlined in the text.

simple model that illustrates how compensation is a rational response for drivers who both benefit from, and are distracted by, cellular use. The laboratory evidence for such compensation 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).[55] However, some studies find a higher variance in such speeds (e.g., Rakauskas, Gugerty, and Ward 2004), while others find that cellular users actually increase speed (Rosenbloom 2006).[56] The few studies which examine cell phone distraction in repeated trials find evidence for learning (e.g., Shinar, Tractinsky, and Compton 2005).

There is field evidence consistent with compensation. In a study looking at cellular driving in both field and experimental settings, Mazzae et al. (2004) find significant degradation in various driver outcomes in simulated, but not real-life (as observed in a naturalistic study using camera equipped vehicles), driving. While measured imprecisely, the study also finds, consistent with compensation, that cellular usage is lower when traffic is more congested. The New Jersey Turnpike study also reports cellular usage at very high speeds (i.e., 15 mph over the speed limit) is 20 percent lower (statistically significant) than usage at moderate speeds (Johnson et al. 2004).

A second explanation is that the drivers who use cell phones have an affinity for risk (Hahn and Tetlock 1999). In this scenario, risk-loving drivers may simply use cell phones as a substitute for other distractions (e.g., talking to a fellow passenger, or

[55] Caird et al. (2008) estimates that the mean effect size difference in driver speed of hands-free use (positive number represents a slower speed) relative to a baseline control is $r = 0.23$ (with 95 percent CI of 0.06 to 0.40 and composite $N = 495$), while the mean effect size difference of handheld use relative to the same baseline control is $r = 0.39$ (with 95 percent CI of 0.26 to 0.52 and composite $N = 160$). The authors, however, characterize this level of compensation as not "appreciable."

[56] We thank an anonymous referee for bringing this study to our attention.

fiddling with radios, televisions or DVDs). Prieger and Hahn suggest that driver heterogeneity in riskiness leads most research to significantly overestimate the near zero impact of cell phone use on crashes (2007). In another study of 3,869 Canadian drivers, authors also find that cellular users are more likely to incur traffic violations for risk-taking behaviors unrelated to cell phone use such as alcohol consumption, non-moving violations, and seat belt non-use, than their counterparts (Wilson et al. 2003).

Finally, the effect of cellular use on crashes may be heterogeneous across drivers.[57] 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 in the 100 car naturalistic study described above, 20 percent of crashes and 12 percent of near-crashes were linked to driver fatigue (NHTSA 2006).[58]

Limitations to the Result.—We highlight two primary caveats to our main result. An initial caveat is that the finding reflects a local average treatment effect of the influence of cellular use around 9 PM. We discuss three ways in which the locality of the estimate might affect its generalizability to other periods of the day (or its ability to be contrasted with findings from other research settings). First, one could plausibly argue that, relative to earlier in the day, crash risk due to cellular use at night is less hazardous due to diminished traffic. However, crash rates per vehicle mile travelled are actually significantly higher at night than during the day (NHTSA 2000). The elevated crash risk is likely due, in part, to lower visibility, higher average speeds, and possibly greater driver fatigue. To the extent that these factors could exacerbate the detriment of attentional distractions, cellular use may be just as, or more, deleterious at night than at other times of the day.[59]

Second, in the event of heterogeneity in cellular crash risk by driver type (e.g., by age or gender) it is possible that the particular mix of drivers who choose to call at 9 PM may bias our results. For selection by driver type to downward bias our estimates, then the composition of drivers who call at 9 PM—which is determined by relative distribution of driver types on the road, the baseline rate of cellular use by type, and the price sensitivity at 9 PM by type—must have disproportionately lower crash risk associated with cellular use. While this bias is not simple to assess, we are not aware of evidence to suggest driver type is unrepresentative at night, and laboratory evidence does not find a significant difference in the relative impairment induced by cellular use between very young and old drivers (despite large differences in baseline crash risk).[60]

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

[58] 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 percent of all fatal large-truck crashes (included in the "Report to Congress on the Large Truck Crash Causation").

[59] Data on average hourly speeds for highway traffic in California from 2005, collected from the PeMS website described in the online Appendix, suggests that speeds from 9 to 10 PM are about 6 percent higher than speeds throughout the rest of the day.

[60] The laboratory evidence on young and old drivers is from a study by David Strayer and Frank Drews that can be found here: http://www.psych.utah.edu/AppliedCognitionLab/Aging.pdf.

Finally, an intriguing possibility is if calls vary in their capacity for distraction and our estimate is picking up an unrepresentative set of phone calls. Suppose calls are of two types—urgent and nonurgent and the latter type are more likely to be deferred until after 9 PM due to the price change. Then nonurgent calls would comprise a disproportionate share of the 9 PM jump in calls. If nonurgent calls are less anxiety provoking and otherwise less distracting, then this would cause our estimates to be downward biased.[61] While we have no direct evidence on such a possibility, we do note that to the extent that call duration reflects the content of a call, we check for and find no statistically significant difference in the duration of calls in the hour before and after the pricing threshold in the TNS data.

A second caveat is that while our point estimate suggests no link between cellular use and crash risk, our 95 percent upper bounds still allows for a crash-risk ratio of up to 3.0 (1.4 and 0.9 for our 1 s.e. upper bounds). We cite three reasons why this level of precision may be economically meaningful. First, we are able to rule out the point estimates of the most influential studies—i.e., the 4.3 risk ratio of RT, a more recent case-crossover estimate of 4.1 (McEvoy et al. 2005), and the range of risk ratios from 3 to 4 produced by laboratory findings (Strayer 2003 and Strayer 2006)—in a large and policy relevant literature. References to these studies, and their estimates of crash risk, are pervasive in policy discourse. Second, the inconsistency of our findings from those of laboratory studies offers a specific but important caution in the translation of findings from the laboratory to the field. If cell phones do distract but such distractions are offset by more careful driving, substitution away from other risky behaviors, or the beneficial effects in counteracting fatigue, then the naive translation of the mechanisms posited by laboratory findings to policy prescriptions neglects the influence of possible alternative mechanisms. Our result highlights how field studies may be useful in illuminating the presence of alternative mechanisms—such as compensation, risk substitution, or fatigue—or interactions between known mechanisms and real-world variables that are important for understanding actual behavior. Lastly, the point estimate and confidence interval generated by our analysis appears to seriously affect the cost-benefit calculations used to determine policy on cellular bans. We comment on the policy implications of the findings below.

Implications for Welfare and Policy.—While legislative bans on cellular use have become increasingly pervasive, the optimality of such policy depends on the mechanisms underlying crash risk as well as a weighing of pertinent costs and benefits. As an example of the importance of mechanisms, 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. Further, 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

[61] We thank an anonymous referee for suggesting this explanation.

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).

Abstracting from mechanistic understanding, bans regulating driver cell phone use balance benefits of use against possible harm to person or property. Other researchers have estimated the economic value of cell phones to drivers. The Harvard Center for Risk Analysis assessed the value of nonemergency cellular calls by drivers at $43 billion annually (Cohen and Graham 2003), while the Cellular Telephone Industry Association reports that more than 200,000 emergency calls are made by drivers daily. Using such estimates as a departure point, we can calculate the value of statistical life implicit in decisions to enact cellular bans under varying estimates of crash risk (e.g., Ashenfelter and Greenstone 2004).

As illustration, presented in Table 6, if we focus exclusively on fatalities attributed to cell phone use (and ignore costs associated with injury and property damage), then the 4.3 relative risk ratio of RT implies 13,000 fatalities and a value of life of $3.3 million, while the 6.5 upper bound of RT implies 22,000 fatalities with a value of life of $2.0 million.[62] Our point estimate of 1.0 implies no additional fatalities and an enormous valuation of life, while our 1 s.e. upper bound of 1.4 (expanded states from 2002 to 2005 sample) implies 1,600 fatalities at a statistical value of life of $27 million and our 95 percent upper bound implies 8,000 fatalities per year at a value of $5.4 million per life.

While estimates of life valuations implied by regulation vary considerably in the literature, in 2004 the US Department of Transportation reportedly employed a valuation of $3 million per life for regulation (Ashenfelter 2006). To the extent that the analogy to drunk driving motivates policy, we note that the odds-ratio of 7 (13) associated with positive (illicit) levels of blood alcohol implies a life valuation of $1.8 million ($0.9 million) for bans on drunk driving (Levitt and Porter 2001a). The final column of Table 6 compares the annual fatalities avoided from a ban on cellular use, implied by various parameter estimates, with the 13,582 fatalities attributable to illicit levels of alcohol use (NHTSA 2005). While societal tolerance for risk and uncertainty must also be considered given the imprecision of parameter estimates, these calculations illustrate the potentially high economic relevance of our confidence intervals for reassessing optimal policy regarding cell phone use.

IV. 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,

[62] 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.

Table 6—Value of a Statistical Life Implied by Cellular Bans

Analysis parameter	Relative risk odds ratio	Annual fatalities avoided by ban	Implied VSL (>)	Fatalities avoided versus drunk driving
9 PM point estimate	1.0	0	—	0%
9 PM point estimate + 1 s.e.	1.4	1,600	\$27m	12%
9 PM upper bound	3.0	8,000	\$5.4m	59%
RT	4.3	13,200	\$3.3m	97%
RT upper bound	6.5	22,000	\$2.0m	162%
Laboratory studies	3.0 to 4.0	8,000 to 12,000	\$3.6m to \$5.4m	59% to 78%

Notes: This table presents the value of statistical life (VSL) thresholds implied by legislative bans on cellular use and various crash-risk parameters. The table considers parameter estimates from the present 9 PM analysis, as well as the RT study and laboratory studies. The Levitt and Porter (2001a) crash risk pertains to any positive level of alcohol. Fatality calculations assumes 40,000 fatalities per year. VSL calculations assume an economic value of phone use of \$43 billion each year (Cohen and Graham 2003). The final column compares the annual fatalities avoided by a legislative ban and implied by the parameter estimates with the 13,582 annual fatalities caused by drunk driving at illicit levels of intoxication (NHTSA 2005).

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. Two additional empirical strategies confirm the absence of a 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 a local average treatment effect, it could be that cell phones are dangerous for certain drivers (or driving conditions) and are beneficial for others, or that our estimates reflect an unrepresentative time of day, mix of drivers, or composition of calls.

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. For example, by introducing appropriate incentives, one could attempt to test the hypothesis of compensation while other experiments could identify the presence of risk substitution or interactions between cellular use and fatigue.

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