

Driving Under the (Cellular) Influence: Online Appendix

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1 Detailed Description of Data

Call Likelihood. Beyond the primary data on callers from moving vehicles, two additional datasets of calls confirm the price sensitivity of a broader population of cellular callers that extends beyond drivers. First, complete logs of cell phone activity for approximately 65 students and faculty over the academic years from 2004 and 2005 was obtained from the Reality Mining Project at the MIT Media Lab (MIT).¹ As part of a study examining the evolution of social networks and the transmission of information, researchers embedded surveillance technology in the cellular phones of each subject in their sample. Approximately 80,000 outgoing calls were logged over the course of the surveillance period.² Electronic logs ensure that the timing of calls are accurately documented to the second. The data may not be representative of the larger population across a variety of dimensions given that the subjects are primarily students.

Figure A2 depicts the distribution of calls, aggregated in 10 minute bins, from 8 to 10pm for Mondays to Thursdays, Fridays and weekends. In order to formally estimate the size of the rise in call volume at the price threshold, the upper panel of Table A3 reports results of a Poisson regression of minute level calls from 8 to 10pm with fixed effects that control for day of week, month and year of the call. The results indicate a rise in call likelihood of 22.6% in the hour after 9pm on Mondays and Thursdays and no significant rise in the comparable period on Fridays or weekends.³ The placebo checks for other hours indicate a rise at 8pm of about 12% and no rise at 10pm. However, the estimated rise at 8pm is not due to a discontinuous break at 8pm, but rather a gradual rise in calls from 8 to 9pm that may be idiosyncratic to this academic population.

A second additional dataset (TNS) comprises over 741,000 calls made by 9,864 cell phone users from households across the country in 2000 and 2001.⁴ The data was harvested from cellular phone bills voluntarily submitted from households randomly selected to participate in an earlier survey of telecommunications behavior and attitudes.⁵ The data are hourly data and are from a

¹The data is described in the publication: Eagle, Nathan and Alex Pentland, "Reality Mining: Sensing Complex Social Systems," *Personal and Ubiquitous Computing*, Vol. 10, No. 4, pp. 255-268, 2006.

²This period reflects the fact that most subjects joined and remained in the sample during the academic year. A small fraction of calls were made in summer months and these were not included.

³A negative binomial model, which one might advocate due to the high number of 0 call hours, produces similar estimates (i.e., a 23.4% call rise for Mondays to Thursdays, and nearly identical point estimates for Fridays and weekends).

⁴The dataset, *Residential Quarterly Tracking Data: Bill Harvesting*, is commercially distributed by TNS Telecom. While the firm continued to harvest cellular phone bills after 2001, we were unable to acquire this data for a more recent period due to prohibitive costs.

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

period characterized by either the absence of a cell phone plan or plans with non-uniform switching thresholds across the weekday evenings. The data usefully provides peak and off-peak designations for each call, and allows for the analysis of individual call patterns.

A sizable share of the 9,864 callers in the data have plans with thresholds that either do not exist or cannot be inferred.⁶ We therefore retain a subsample of callers that satisfy each of the following conditions: (i) Callers are in the sample for at least 30 or more calendar days and had calls on at least half of these days, (ii) Callers log at least one call in the evening hours (i.e., 5pm or after) in each of Monday to Thursdays, Fridays, and the weekend, (iii) Callers have no calls that are ambiguously tagged (i.e., each call is tagged as either “peak” or “off-peak” rather than “unclear”), and (iv) Callers have a mix of peak and off-peak calls which allows us to infer the switching hour of the caller’s plan.⁷ The remaining 287 callers have plans with switching thresholds at 6pm (65), 7pm (104), 8pm (78), 9pm (23) or 10pm (17). These individuals make a total of 16,900 evening calls.

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

$$E[Calls_{hsi} | \cdot] = \exp[\alpha + \gamma Switch_s + \theta AfterSwitch_{hsi} + \eta_h + \delta_i]$$

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

The coefficient estimates in the bottom panel of Table A3 indicate a rise in call volume of 23% in the hour following the switching threshold on Mondays to Thursdays, and no significant

and participation in a “special cash prize raffle” for their bills.

⁶We impute the switching hour by computing the change in the average peak/off-peak rating for each evening hour. Peak calls are tagged with the value “1” while off-peak calls are tagged with the value “2”. In principle, if a caller has a 7 pm switching threshold, then the average peak/off-peak rating should jump cleanly from 1 to 2 at 7 pm on weekdays. However, due to the presence of holidays or calls made in excess of the allowed quota for that month, we do not always observe unit jumps in the rating. In the absence of clean rating jumps, we tag the evening hour with the largest jump in average peak/off-peak rating as the switching hour for each caller.

⁷The rationale for employing a minimum day and call threshold is to ensure sufficient power for a fixed effects estimation, as well as to minimize any potential miscategorization of switching time thresholds. The basic results and figures are robust to less strict selection criteria.

comparable rise in calls on other days or other hours (note that the volume increase in the hour following the early placebo threshold (-1) is attributable to the call rise at the actual switching threshold). There is likely higher persistence in the call volume increase following the pricing threshold in this data, relative to other data, because many callers are on plans that switch fairly early in the evening. Finally, to test for the concern that the rise in calls at the switching threshold may be counterbalanced by a fall in call duration, we check and find no evidence for a statistically significant fall in duration at the threshold.

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

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

2 Supplementary Analyses

While the analysis of call volume and crash rates at 9pm constitutes the primary approach, two additional empirical approaches confirm our basic result. In the first approach, we compare aggregate national trends in crashes and cellular ownership at the EA and state level. Next, using a region-month panel, we examine whether legislative bans on handheld driver cell phone use reduced the fatal crash rate.

2.1 Panel Estimation of Crashes and Ownership

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

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

EAs are associated with considerable variation in ownership. Ownership rates ranged from 19 to 57 percent across EAs in 2001 and from 61 to over 100 percent by 2007.⁸ We estimate the following model with an OLS regression:

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

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

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

⁸In rare cases, such as in Washington D.C., the FCC reports ownership as being greater than 100% due to either multiple subscriptions by some residents or the fact that the FCC records location of registration rather than of residence.

Table A4 presents the results of the estimations. The first two columns report results of the panel analysis of the crash rate across the approximately 60 EAs in nine states from 1990 to 2005 for which we have the universe of crash data. The point estimate of interest indicates the percent change in the crash rate given a 1% point increase in average EA ownership after controlling for EA and year fixed effects. To control for the possibility that omitted factors that cause crashes within a state over time are correlated with cellular ownership, the next column includes more flexible controls which allow for EA specific time trends.⁹ Columns 3 and 4 repeat the exercise for fatal crashes for all 172 EAs from 1989 to 2007. None of the estimates suggest a statistically significant positive link between ownership and fatal crashes.

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

The final columns of the table provide a robustness check of the results by controlling for changes in traffic volume across regions and time. Since traffic volume is only coded at the state level, this regression is limited to fatal accidents at the state, rather than the EA, level.¹⁰ The estimation, admittedly imprecise, again provides no evidence for a statistically significant correlation between ownership and crashes.

Importantly, if we restrict our state-year analysis of fatal crashes to 1999 to 2005 we can approximately replicate the effect sizes reported in Kolko (2009). Kolko reports positive but insignificant estimates of the effect of cellular ownership on crashes, adjusted for traffic volume, after controlling for state and year fixed effects in a state-year panel regression from 1997 to 2005.¹¹ His favored estimates imply, under the previously stated assumptions, that the introduction of cell

⁹Silva and Tenreiro point out that log-linear estimations can be inconsistent if the true underlying model is characterized by a Poisson distribution (2006). We re-estimate our baseline model using a Poisson specification and a population offset. The point estimates are substantially similar and insignificant.

¹⁰Regressions are confined to fatal accidents because of the limited number of states in the SDS dataset. As opposed to EA level penetration which is available only since 2001, state level ownership data is available since 1999.

¹¹Kolko uses proprietary survey data from Forrester Research to infer state-year cell phone ownership from 1997 to 2005. Our ownership data, taken from the FCC, is only available as of 1999 which prevents a closer replication.

phones produces a 15% increase in the aggregate fatal crash rate.¹² Our analogous and also insignificant estimates imply a 12% increase in the fatal crash rate.¹³ However, we find that the introduction of an early control period with no cellular ownership or the introduction of linear and quadratic state time-trends each—as well as both jointly—eliminate the positive point estimates for cellular ownership.¹⁴

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

Though the EA represents a disaggregated unit of analysis, the present approach ignores the potential variation of cell phone usage over time due to the recent introduction of bans on handheld cell phone use in selected regions. We explore this additional source of variation next.

2.2 Analysis of Legislative Bans on Handheld Cell Phones

In a third approach, we estimate the influence of legislative bans that restrict cellular use by drivers. Six states had banned handheld phones (almost) without exception at the time of our analysis.¹⁵ New York’s ban went into effect in November 2001, followed by New Jersey in July 2004, Connecticut in October 2005, California and Washington in July 2008 and Oregon in early 2010. Beyond these states, a number of municipalities have enacted complete bans. The largest of these municipalities are Chicago, whose ban went into effect in July 2005, and Washington D.C. which banned cellular use by drivers beginning in July 2004. Several additional states have legislated partial bans on cellular use but these bans typically target a modest fraction of drivers. Table A5 in the Appendix enumerates the states and large municipalities with complete or partial

¹²Originally reported as 16%, the Kolko estimate is taken from Column 2 of Table 2 and is discussed in the subsequent text and footnote. We adjust the figure to 15% to account for the 70% ownership rate for 2005 which we use throughout the text.

¹³Specifically, we estimate the model presented in Column 5 after restricting the sample to 1999 to 2005. We find a coefficient estimate of ownership equal to .0016 (with a standard error of .0022).

¹⁴The introduction of linear and quadratic time trends reduces the point estimate of cell phone ownership (%) from .0016 to -.0031. The introduction of an early control period with no cellular ownership reduces the point estimate from .0016 to -.0008. The inclusion of both a control period and the time trends reduces the point estimate to -.0001. None of these estimates are statistically significant.

¹⁵One common exception is the use of cell phones for emergency calls.

bans.¹⁶ Note that to the extent that drivers substitute hands-free devices for banned handheld phones, our analysis tests for the difference in crash risk between hands-free and handheld use.

Our data on fatal crashes, from 1989 to 2007, allows us to explore the effects of the legislation in New York, New Jersey, Connecticut, as well as the large municipalities of Chicago and Washington D.C. The analysis is at the state, rather than EA, level since states are actually a more disaggregated unit of analysis for these regions, and EA ownership data are not available for 2006. The ban in Chicago is treated as if it were for the entire state of Illinois in this analysis.¹⁷ Since the bans are generally enacted during the year, the analysis is at the monthly, rather than yearly, level. Unfortunately, our data on all crashes fails to cover the regions and time periods of interest.

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

While Figure 6 presents graphical evidence of the impact of cellular bans, to formally test for the effects of the legislation, we estimate the following OLS regression at the region level for fatal crashes each month from 1989 to 2007:

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

where Ban_{rmy} is a dummy variable which indicates that a complete handheld ban was in effect for any part of a given state r , in month m , and year y . As before, we include a control period with 0% ownership prior to 1993. Region, year, and month, fixed effects are included along with linear and quadratic time trends by region and year to flexibly control for time and region specific variation in crashes.

¹⁶The table excludes numerous states which ban cellular use by school bus drivers. A list of municipalities with bans can be found in “Cell Phones and Highway Safety: 2006 Legislative Update” published by the National Conference of State Legislatures (Sundeen 2007).

¹⁷One might expect this to bias the results against finding any effect of the legislation but our basic results are not sensitive to the inclusion of Illinois.

In an initial specification with just month, year and state fixed effects, the estimated coefficient of interest, $\hat{\lambda}$ in Column 1 of Table A6 suggests a large and statistically significant 13% drop in fatal crashes after the enactment of legislation. This is broadly consistent with the findings of Kolko (2009). However, the inclusion of state specific linear and quadratic time trends reduce the point estimate to a statistically insignificant -.07. Additional checks reveal that the pattern of crashes in Washington D.C. is responsible for the negative point estimate. Given the modest fatal crash rate in Washington D.C. (about 4 per month), any small change in crashes strongly alters the estimated coefficients given the construction of the dependent variable. The exclusion of Washington D.C. eliminates the apparent negative effect of the legislation as reported in Column 3 as does a regression weighted by region population.¹⁸

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

3 Model of Compensatory Response

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

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

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

¹⁸One can also deal with the disproportionate influence of Washington D.C. by including population weights in the regression. We replicate Column 2 of Table A6 but this time weight each observation by regional population to produce a Post Legislation coefficient estimate of $b = 0.022$, $s.e. = 0.018$.

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

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

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

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

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

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

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

$$U_s : w_s - p_sL = 0$$

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

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

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

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

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

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

We can recast the above expression as:

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

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

4 Additional Tables and Figures

APPENDIX TABLE A1—SUMMARY OF DATA SOURCES

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

APPENDIX TABLE 2—PRICING THRESHOLDS FOR CALLING PLANS FROM 1999 TO 2005

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

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

**APPENDIX TABLE A3—CHANGE IN CALL VOLUME
AT PLAN THRESHOLD (MIT / TNS)**

Dependent Variable - Total Calls per Minute Bin (MIT)					
	9PM THRESHOLD ANALYSIS			8PM	10PM
	Mon - Thu (1)	Friday (2)	Weekend (3)	Mon - Thu (4)	Mon - Thu (5)
After 9pm	1.227*** (0.054)	0.958 (0.069)	0.994 (0.062)		
After 8pm				1.120*** (0.048)	
After 10pm					0.984 (0.042)
N	N = 20880	N = 5160	N = 10440	N = 20880	N = 20880

Dependent Variable - Hourly Calls by Caller (TNS)					
	SWITCHING THRESHOLD ANALYSIS			- 1 HR	+ 1 HR
	Mon - Thu	Friday	Weekend	Mon - Thu	Mon - Thu
Switching Threshold	1.230** (0.123)	1.141 (0.146)	1.137 (0.122)	1.096 (0.118)	0.870*** (0.043)
After Switching Threshold	1.054 (0.168)	0.819 (0.152)	1.233 (0.189)	1.342* (0.216)	0.813*** (0.057)
N	N = 2002	N = 1918	N = 1967	N = 2002	N = 2009

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

*** significant at the 1 percent level.

** significant at the 5 percent level.

* significant at the 10 percent level.

**APPENDIX TABLE A4—TRENDS IN CELLULAR OWNERSHIP
AND CRASHES ACROSS REGION-YEAR**

	Dependent Variable - ln(Crashes per 100,000 Persons)					
	All Crashes (1990 to 2005)		Fatal Crashes (1989 to 2007)		Fatal Crashes (1989 to 2007)	
	Economic Area		Economic Area		State	
	(1)	(2)	(3)	(4)	(5)	(6)
Cell Phone Ownership	-0.0018 (0.0015)	-0.0004 (0.0014)	-0.002 (0.001)	0.002 (0.001)	-0.001 (0.001)	0.001 (0.002)
ln(Traffic Volume)					0.132 (0.199)	0.229 (0.210)
Region Fixed Effects	X	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X	X
Region FE x Year		X		X		X
Region FE x Year ²		X		X		X
N	N = 476	N = 476	N = 2036	N = 2036	N = 642	N = 642
R ²	0.97	0.99	0.83	0.92	0.93	0.97

Notes: The table estimates a panel regression of cellular ownership and vehicular crashes over time. The dependent variable across each regression is the natural log of the number of crashes, per scaled capita, in a given year for a particular region for the stated time period. The explanatory variable of interest is the percent rate of cell phone ownership in the specified region (i.e., 100 * cell phone subscribers / population). For the regressions at the level of an Economic Area (one of 172 regions of contiguous economic activity nationwide as defined by the BEA (EA)), 2006 is excluded since penetration data is not available. Ownership prior to 1993 is assumed to be 0%, and ownership from 1993 to 2000 is coded as missing except for the state-year regressions which include ownership for 1999 and 2000. The first two columns report analysis of all crashes for nine states, while the next two columns report an analysis of fatal crash data for all states. A small number of EA-years were excluded due to lack of data on population or inability to match county and EA. In the all crash analysis, Michigan is excluded during the control period due to missing county identifiers and Pennsylvania is excluded in 2002 due to missing data. Columns 5 and 6 report a robustness check for fatal crashes at the state level after controlling for state-year traffic volume. The state-year series begins in 1989 to coincide with availability of traffic volume data. Robust standard errors clustered by EA or state are reported parenthetically.

APPENDIX TABLE A5—SUMMARY OF BANS ON HANDHELD CELL PHONES

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

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

**APPENDIX TABLE A6—CHANGE IN REGION-MONTH FATAL CRASHES
AFTER HANDHELD LEGISLATION**

	Dependent Variable - ln(Crashes per 100,000 Persons)					
	Fatal Crashes (1989 to 2007)			Fatal Crashes (1989 to 2007)		
	All Regions		Excluding DC	All Regions		Excluding DC
	(1)	(2)	(3)	(4)	(5)	(6)
Cell Phone Ownership	0.001 (0.001)	0.001 (0.001)	0.002 (0.002)	0.001 (0.001)	0.001 (0.001)	0.002 (0.002)
Post Legislation	-0.130** (0.050)	-0.070 (0.079)	0.019 (0.023)			
Post Legislation (1 month)				0.015 (0.044)	0.049 (0.041)	0.040 (0.057)
Post Legislation (2-3 months)				-0.217 (0.151)	-0.182 (0.179)	0.008 (0.064)
Post Legislation (4-6 months)				-0.150 (0.118)	-0.113 (0.137)	0.034 (0.033)
Post Legislation (> 6 months)				-0.127*** (0.046)	-0.060 (0.068)	0.017 (0.025)
ln(Traffic Volume)	0.133 (0.202)	0.326 (0.203)	0.289 (0.202)	0.133 (0.202)	0.323 (0.202)	0.289 (0.202)
Region Fixed Effects	X	X	X	X	X	X
Year, Month Fixed Effects	X	X	X	X	X	X
Region FE x Year		X	X		X	X
Region FE x Year ²		X	X		X	X
N	N = 7725	N = 7725	N = 7572	N = 7725	N = 7725	N = 7572
R ²	0.69	0.71	0.72	0.69	0.71	0.72

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

*** significant at the 1 percent level.

** significant at the 5 percent level.

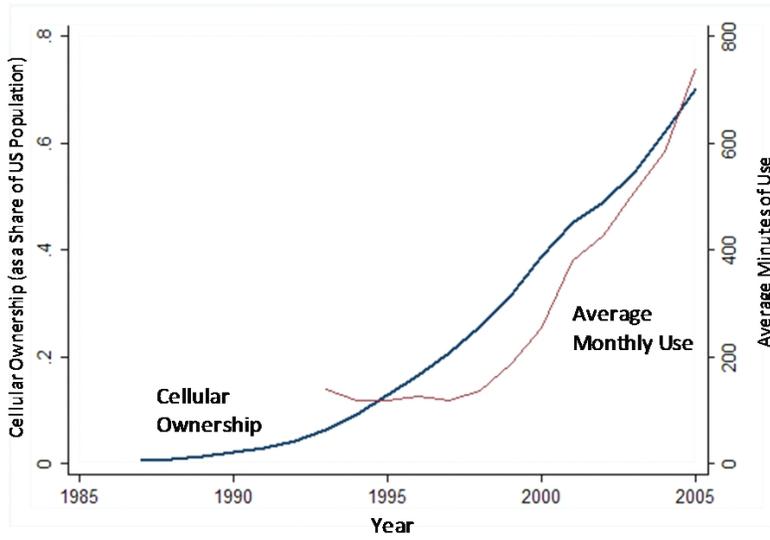


Figure A1. Cellular Ownership and Monthly Use in US from 1987 to 2005

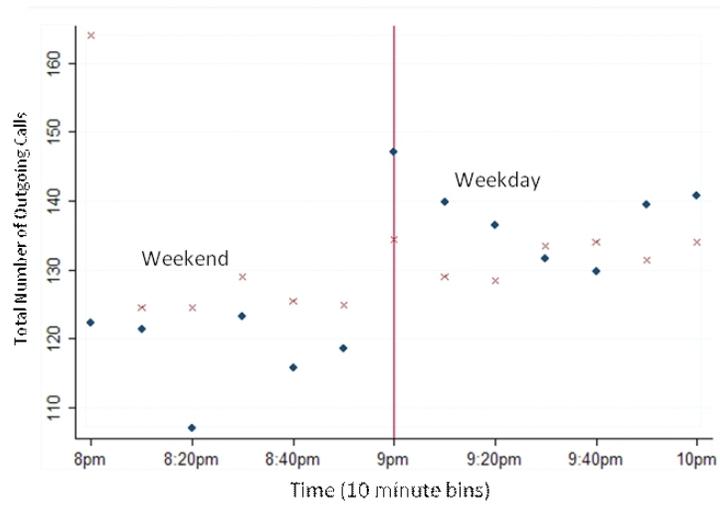


Figure A2. Outgoing Call Volume in MIT Data from 8pm to 10pm in 2005

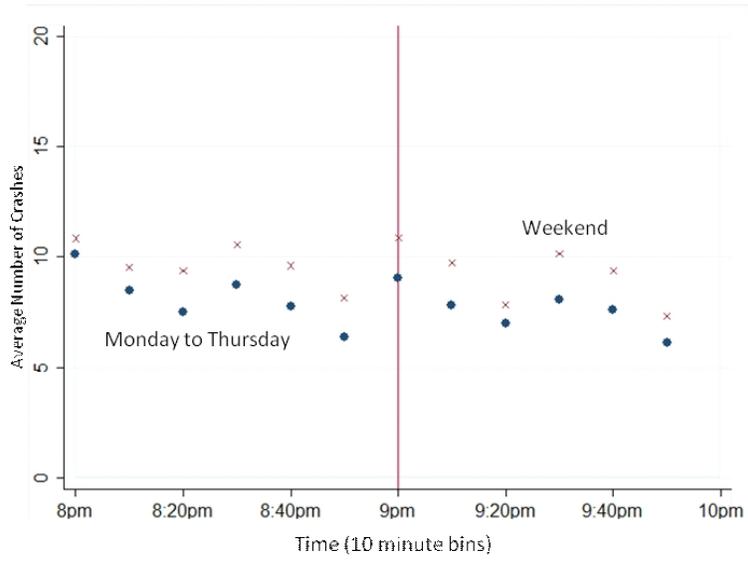


Figure A3. Crash Rate for California from 8pm to 10pm in Post-Period (2005)

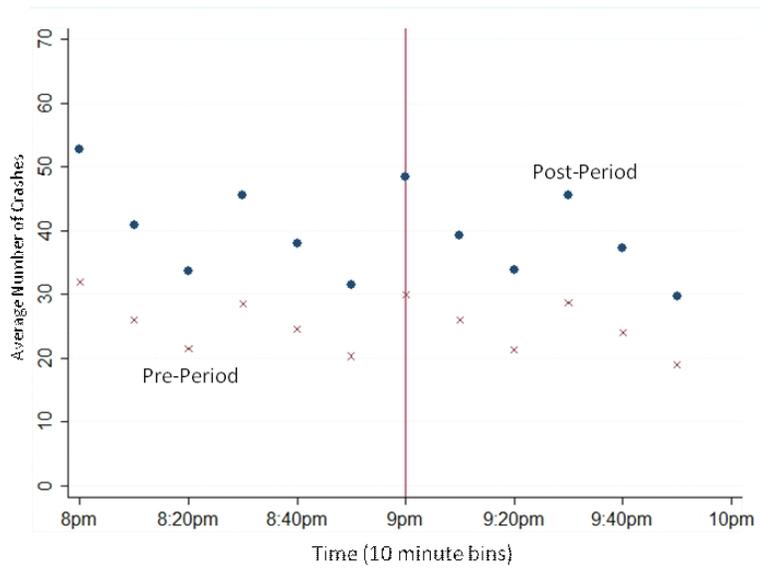


Figure A4. Crash Rate for Expanded States from 8pm to 10pm in Pre (1995 to 1998) and Post (2002 to 2005) Periods