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Dialing While Fishtailing: How Mobile Phones, Hands-Free Laws, and Driving Conditions Interact to Affect Traffic Fatalities

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ABSTRACT

Most rich countries in the world and four US states require drivers talking on mobile phones to use hands-free devices. However, previous research has failed to arrive at a consensus on the effect of mobile phones on traffic accidents yet has concluded that the effect of hands-free and hand-held phones on accidents is similar.

This paper uses state-level data from 1997-2005 on mobile phone ownership, traffic fatalities, and hands-free laws and finds that (1) mobile phones contribute to traffic fatalities and (2) hands-free laws appear to reduce fatalities. Specifically, mobile phone ownership results in a large and statistically significant increase in traffic fatalities in bad weather or wet road conditions, with no effect in good weather or dry road conditions. Laws requiring drivers to use hands-free technologies while talking reduce traffic fatalities in adverse conditions, and the effect grows stronger and becomes statistically significant the longer the law is in effect, although these longer-term effects are based solely on New York, which in 2001 became the first state to have a hands-free law.

The analysis relies on microdata from the Fatality Analysis Reporting System to estimate effects for traffic fatalities in different conditions and to isolate fatalities unlikely to be affected by confounding changes in alcohol policies or graduated licensing laws.

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Has the rapid increase in mobile phone usage contributed to traffic fatalities in the United States? Does legislation requiring drivers to use hands-free technology while talking have any effect on traffic fatalities? This paper answers these two questions, both central to the public debate over regulations on mobile phone usage while driving. Using state-level panel data on mobile phone ownership, hands-free laws, and traffic fatalities, this paper finds that mobile phones are associated with higher traffic fatalities, but only in bad weather or wet road conditions. The limited experience of a few states suggests that hands-free laws reduce traffic fatalities, but the results are sensitive to the time period studied and the specification. The findings give a nuanced picture of the relationship between mobile phones, hands-free laws, and traffic fatalities and suggest that the benefits of hands-free laws depend on driving conditions.

Traffic accidents are a leading cause of death in the US. In 2005, crashes resulted in over 43,000 fatalities. Motor vehicle accidents rank 8th among causes of death for the overall population and are the leading cause of death at ages 5-34.[1] Globally, traffic mortality rates are even higher in the developing world, and the World Health Organization calls traffic injuries a "public health epidemic" (WHO, 2007).

Concern about phone usage contributing to driver distraction and traffic accidents has led countries throughout the world to require hands-free technology for talking while driving. Japan became the first large country to ban mobile usage while driving without hands-free technology in 1999. As of 2006, every G7 country except the US banned non-

[1] See data from the Center for Disease Control's National Center for Injury Prevention and Control, http://webappa.cdc.gov/sasweb/ncipc/leadcaus10.html. CDC data include motor vehicle accidents in the category "unintentional injury." The ranking of motor vehicle accidents mentioned in the text describes how those accidents would rank if all subcategories of "unintentional injury" were broken out separately. For the overall population, motor vehicle accidents are the highest ranking subcategory within "unintentional injury," accounting for 39% of the category. Data from 2004.

hands-free mobile usage while driving, with penalties ranging from fines for using handheld devices while driving to exclusion from insurance coverage if drivers crash while talking on a handheld phone.[2]

In the US, only New York, New Jersey, the District of Columbia, and Connecticut have hands-free laws in effect; California and Washington have passed laws that will take effect in 2008. While many other states have considered hands-free legislation or have allowed municipalities to pass local hands-free laws, the US is clearly an outlier among nations on mobile phone legislation.

RESEARCH ON MOBILE PHONE SAFETY

Previous research has found conflicting evidence about the extent to which mobile phones contribute to traffic accidents.[3] However, despite different research methodologies, these studies agree that the risks to safety are similar for hands-free and handheld mobile phone usage. These previous studies rely on laboratory driving simulations, which do not necessary reflect behavioral changes people make in response to a hands-free law, and surveys in which drivers recall past accidents, which are subject to measurement error and selection issues.

An early study from the medical literature is Redelmeier and Tibshirani (1997), which uses the case-crossover method to compare mobile phone call-logs for drivers in

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[2] See list at www.iii.org/media/hottopics/insurance/cellphones, the Insurance Information Institute's website, and at www.cellular-news.com/car_bans, an online aggregator of news about mobile phones.

[3] At the same time, there are reasons why mobile phone ownership could reduce traffic accidents as well as contribute to them. Redelmeier and Tibshirani (1997) note that, in 39% of non-serious accidents (those with property damage but no bodily injury), mobile-phone-owning drivers called emergency services afterward. Being in possession of a mobile phone to call for assistance immediately could reduce the share of traffic accidents that result in fatalities. It is even theoretically possible that the positive effect of calling for assistance could outweigh the effect of driver distraction so that, on balance, drivers with mobile phones are less likely to die from accidents than those without mobile phones, though no study has drawn that conclusion.

non-serious accidents just before the event and at a comparable time in the past. They estimate the likelihood of an accident to be 4.3 times higher when using a phone, though this estimate is based only on drivers who have had accidents and own mobile phones and may not reflect the broader population if people are heterogeneous in mobile phone usage and in how mobile phone usage affects their driving. Hahn and Prieger (2006) use data on recent and retrospective traffic accidents and mobile phone usage. They construct a panel from respondents' recollections over two years and model the effect of cell phone usage on traffic accidents. Their estimates of the effect of mobile phone usage on accidents is smaller than those of Redelmeier and Tibshirani, in part because they find that individuals with higher mobile phone usage are riskier drivers independent of their mobile phone usage.[4] They attribute the higher rates of relative risk found in other studies to others' looking at a particularly accident-prone sample whose behavior is not generalizable to the population. However, their method relies on recall and identifies the effect of mobile phone usage on accidents from differences in the recalled levels of phone usage over time, and it is unclear how serious a problem measurement error might be. Further, their data were collected online, and their sample – like all online samples – could be biased on unobservables whose relationships to variables of interest are unknown.

Two studies based on laboratory and naturalistic experiments have found that mobile phones create significant distractions that result in driving less safely. Strayer, Drews, and Crouch (2006) find that the distraction from mobile-phone usage makes drivers more accident-prone than having a blood-alcohol concentration at the level of

[4] Hahn and Prieger identify this heterogeneity using individual effects, taking advantage of data on respondents' recollections of mobile phone usage and traffic accidents at multiple points in time. On average, survey respondents provided 3.7 quarters' worth of observations.

.08%, based on a driving simulator, even though driving while intoxicated leads to more aggressive driving behaviors than mobile phone usage does. The National Highway Traffic Safety Administration's (NHTSA 2006) naturalistic driver study outfitted 100 cars with sensors and data collection hardware to test the effect of numerous possible distractions on crashes and near-crashes. This methodology has the distinct advantage of separately estimating the effect of both dialing and talking/listening on a phone while driving. Dialing increased accident risk by 180% compared to the baseline of no distractions; talking/listening raised accident risk by 30%. Since drivers spend more time talking and listening than dialing, the two activities contribute nearly equally to incidents.

Despite the different magnitudes that the above studies find on the effect of mobile phone usage on traffic safety, they reach a consensus that driving while using a hands-free device is no safer than driving while using a standard hand-held device. The consensus is especially striking because of the widely varying methodologies that the different studies use. Redelmeier and Tibshirani (1997) find no significant difference between the increased accident risk when using hands-free and hand-held units. Hahn and Prieger (2006) find that hands-free device users are more careful drivers, and after controlling for these selection effects find no significant reduction in accidents from hands-free devices relative to handheld devices, conditional on minutes of mobile phone usage. And Strayer, Drews, and Crouch (2006) find no significant differences in the impairments introduced by handheld and hands-free devices. These consistent results about hands-free devices have been used to call into question the value of hands-free laws.

However, it does not follow from these studies' findings that hands-free laws have no effect on traffic safety. These studies have found that hands-free devices offer no reduction in driver inattention or crash risk relative to handheld mobile devices *conditional on using a phone while driving*. But drivers choose whether and when to use a phone, and it is theoretically possible that a hands-free law could change drivers' likelihood of using a phone while driving. On one hand, a hands-free law could make drivers less likely to talk while driving if hands-free technology is cumbersome or lower-quality or if the hands-free law serves as an educational warning about the danger of talking while driving; in these ways, a hands-free law could increase safety. On the other hand, a hands-free law could make drivers believe that hands-free devices are safer than handhelds and therefore raise drivers' likelihood to use a phone while driving; in this way, a hands-free law could reduce safety. The effect of a hands-free law on traffic safety is therefore unanswered by previous research; this paper answers this question directly, below.

APPROACH AND DATA

This paper uses state-level panel data on mobile phone ownership, hands-free laws, and traffic fatalities to assess the overall effect of phone ownership and hands-free laws on fatalities. Applying these methods to mobile phones and hands-free laws has advantages over laboratory studies and over self-reported or administrative data on mobile phone usage while driving. First, mobile phone usage can not be consistently observed or measured at the time of a traffic incident. A driver might purposely hang up so not to appear negligent, and police do not have immediate or comprehensive access to mobile phone records. These concerns make data on observed phone usage, including police reports on mobile phone involvement in traffic accidents, questionable. Second, hands-free laws might have effects on driving behavior and mobile phone usage behavior that are not captured in laboratory settings. Hands-free laws could affect the likelihood of whether drivers use a phone at all while driving, regardless of whether the device is hand-held or hands-free. Furthermore, the people who switch from hand-held phones to hands-free phones in places where a law is in effect could differ from the people who voluntarily switch from hand-held phones to hands-free phones in the absence of a law. The presence of driver heterogeneity (as Hahn and Prieger 2006 show) means that we can not judge the effect of hands-free laws based on the behavior of people who voluntarily switched from hand-held to hands-free phones in the absence of a law. To understand how mobile phone ownership and hands-free laws affect traffic fatalities, it is important to analyze how fatalities have changed in places where hands-free laws are in effect.

Using state-level panel data is a standard framework in the economics literature on traffic safety issues, which exploits changes in laws or other exogenous forces that affect states at different times or rates. This method has been used to study the effect on traffic fatalities of seat belt laws (Cohen and Einav 2003), of compulsory insurance and no-fault auto insurance laws (Cohen and Dehejia 2004), and of alcohol taxes and policies (Dee 1999 and Levitt and Porter 2001). These papers all use traffic fatality data from the NHTSA's Fatality Analysis Reporting System (FARS), which reports traffic fatality data comprehensively and consistently across all states for many years. The fatality data cover deaths from traffic incidents of drivers, passengers, and others, including pedestrians.

This paper also uses FARS fatality data aggregated by state and year. But rather than relying on aggregated FARS data published by NHTSA, this paper uses FARS microdata on persons, vehicles, and conditions to create customized state-year totals. Customized aggregation has two advantages. First, it is possible to exclude fatalities that could be related to policy factors that are difficult to summarize using state-year control variables. For instance, over the time period covered in this paper, many states changed the minimum ages for getting a learner's permit and a driver's license; states might also have changed the level of enforcement or penalties for driving under the influence of alcohol. Both young and drunk drivers are involved in a considerable share of traffic fatalities, and rather than attempt to develop crude proxies for state policies, this paper will look at aggregate fatality measures that exclude accidents involving young or drunk drivers. Second, it is possible to look separately at fatalities under specific conditions, such as when roads are wet or at nighttime. Since mobile phone usage could vary by conditions (if, say, people use mobile phones more during rush hour) or different conditions could alter the effect of mobile phones on fatalities (if, say, mobile phones aggravate the risk of fatalities on icy roads), looking separately at fatalities under different conditions might help explain not only whether mobile phones affect traffic fatalities but also why.

Ideally, this paper would investigate the effect of mobile phones on both fatalities and non-fatal accidents. However, FARS only includes accidents that result in fatalities, and there are no data for non-fatal accidents measured consistently across states and over time.[5] Although fatal accidents constitute a small percentage of total accidents, they bear

[5] The NHTSA's initiative to coordinate non-fatal crash data, the State Data System (SDS), included 29 states as of 2004, up from 17 states in 1999. Each state reports non-fatal crash data using its own data

a much larger share of media and policy attention.[6] Even a conservative economic measure of the value of a human life is far in excess of the monetary cost of replacing the most expensive car totaled in a non-fatal accident. Still, it is plausible that the distractions of mobile phone usage affect the likelihood of non-fatal accidents differently than that of fatal accidents. If anything, mobile phones are likely to increase fatalities by less than they increase non-fatal accidents if (as mentioned above) having a mobile phone makes it easier to get emergency assistance quickly after an accident and reduce the likelihood that an accident results in a fatality. Unlike this paper, Redelmeier and Tibshirani (1997) and Hahn and Prieger (2006) look only at non-fatal accidents because they rely on surveys of drivers' past accident history and obviously cannot include drivers who have died as a result of an accident.

For this study, data on mobile phone ownership come from Forrester Research, a technology research and consulting firm. Each year Forrester conducts its Technographics benchmark survey of 60,000-100,000 households about their technology adoption and behaviors.[7] Over the period 1997-2005, Forrester's Technographics survey asked about mobile phone ownership in the household. State-level mobile-phone

definitions and reporting standards, whereas the FARS system for fatalities is consistent across states, includes all states, and covers many years. See http://www-nrd.nhtsa.dot.gov/departments/nrd-30/ncsa/sds.html.

[6] In 2004, there were 38,000 fatal crashes, 1.9 million injury crashes, and 4.3 million non-injury crashes (property damage only). Fatal crashes therefore account for about 2% of crashes in which there is bodily harm, and .6% of all crashes (NHTSA 2005).

[7] Forrester's Technographics surveys are conducted by mail; the samples are selected from national market research panels to be representative of US households demographically and are weighted to correct for differences in response rates. Forrester has used TNS/NFO's market research panel since 2001 and used NPD's panel in earlier years. Forrester collects data in the 48 contiguous states and the District of Columbia, but not in Alaska or Hawaii.

ownership in each year is tabulated from this question. The survey does not ask mobile phone owners whether they use their mobile phones specifically while driving.[8]

An alternative, less suitable source of data on mobile phone ownership is the Federal Communications Commission (FCC), which reports mobile subscriptions annually based on wireless carriers' reporting requirements. The FCC data have the disadvantage of identifying the location of where the mobile phone subscription is registered, which might not be where the subscriber lives and drives most often. This is not just a hypothetical concern: the disadvantage of this can be seen most clearly in the data for the District of Columbia (DC), where in 2004 the FCC reported 1.2 mobile subscriptions per resident, compared to 68% household ownership in DC according to Forrester. The discrepancy between the FCC and Forrester data for DC is most likely explained by commuters who live in Maryland and Virginia and work in DC, do little of their driving in DC (or none, if they commute by transit), but have their mobile phone subscriptions registered in DC. Furthermore, the FCC does not require carriers to report subscriptions for any state in which they have fewer than 10,000 subscribers. In some small states in some years, aggregate mobile subscription data are suppressed to maintain confidentiality of individual provider data.[9] Finally, the FCC reports mobile subscription by state starting only in 1999. For all of these reasons, this paper relies on the Forrester data.[10]

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[8] In the US, talking on mobile phones has long been associated with driving. In the early years of mobile phones, they were regularly called "car phones." In 2003 mobile phone users spent 44% of their usage minutes in cars (Dizon 2004). In 2006, 73% of adults who drive and have a cell phone reported in an online survey that talk on their cell phone while driving at least some of the time (Harris 2006).

[9] This potentially introduces a form of bias that would not be eliminated using state or fixed effects. With the rapid growth of mobile phone subscriptions, underreporting at the state level would be greatest in low-population states in earlier years.

[10] Other academic research has relied on Forrester's Technographics data, including Brown and Goolsbee (2002) Goolsbee (2000) and Stevenson (2003).

MOBILE PHONE OWNERSHIP AND TRAFFIC FATALITIES

In aggregate, mobile phone ownership and traffic fatalities have moved in opposite directions. Traffic fatalities in the US fell from 15.4 per 100,000 population in 1997 to 14.7 per 100,000 population in 2005. At the same time, mobile phone ownership grew from 38% of households in 1997 to 76% of households in 2005.[11] In states were mobile phone ownership increased more over the period, traffic fatalities increased more (or decreased less), but this relationship is not statistically significant. The correlation between 1997-2005 changes in household mobile phone ownership and per capita traffic fatalities is .07, (p=.62, n=49). Figure 1 depicts this relationship. States vary considerably in both the levels and changes in fatalities and mobile phone ownership. In 2005, traffic fatalities per 100,000 population ranged from 33.4 in Wyoming to 6.9 in Massachusetts. The change in traffic fatalities over 1997-2005 ranged from an increase of 5.4 per 100,000 in Wyoming to a decrease of 5.8 per 100,000 in Utah. Mobile phone ownership in 2005 was highest in Georgia (84%) and Connecticut (83%) and lowest in West Virginia (53%); the change in mobile phone ownership over 1997-2005 ranged from an increase of 45 percentage points in New Hampshire to an increase of 29 percentage points in Illinois and Nebraska.

Many factors affect traffic fatalities, and, over this period when mobile phone ownership rose rapidly, numerous other trends contributed to the reduction in overall fatalities, including improvements in vehicle technology and the laws and public awareness about factors, like drunk driving and seat belts, as shown in the studies

[11] The Cellular Telecommunications Industry Association first reports mobile phone subscriptions in January 1985, estimating 92,000 subscribers, approximately one per thousand households.

discussed above. In addition, there is significant heterogeneity among states that could be correlated with changes in both traffic fatalities and mobile phone ownership, such as vehicle miles traveled per capita, economic conditions, and demographic changes, which the economics literature has identified.

To isolate the effect of mobile phones on fatalities, the empirical strategy involves both state and year fixed-effects as well as additional independent variables not absorbed in the fixed effects.[12] The model to be estimated is this:

$$fatalities_{it}^{j} = \alpha + \beta mobile_{it} + \sum_{k} \gamma_k controls_{itk} + \sum_{i} \lambda_i state_i + \sum_{t} \delta_t year_t + \varepsilon_{it}$$

The dependent variable is per capita traffic fatalities of type j in state i and year j. Results are presented for all traffic fatalities as well as certain types of traffic fatalities, as explained below. The explanatory variable of interest is mobile phone ownership in the state-year, $mobile_{it}$.[13] Controls include vehicle-miles traveled per capita, unemployment, age distribution, and weather conditions, including precipitation and heating-degree days.[14] Table 1 presents summary statistics. Other factors that could affect fatalities but are relatively constant within a state over the short time period are absorbed in the state fixed-effects. Robust standard errors are used and are clustered by state.

[12] Because of possible correlation of errors for observations for the same state across years, standard errors will be corrected by clustering on state in all specifications in this paper.

[13] "Mobile phone ownership" always refers to the percentage of households with a mobile phone.

[14] Monthly vehicle miles traveled (VMT) by state were kindly provided by Steven Jessberger and Paul Svercl at the Federal Highway Administration and are available online at http://www.fhwa.dot.gov/ohim/tvtw/tvtpage.htm. Monthly weather data by state are available from the National Climatic Data Center and can be downloaded at http://www1.ncdc.noaa.gov/pub/data/cirs/. Annual estimates of state population by age are available from the Population Division of the U.S. Census Bureau and can be downloaded at http://www.census.gov/population/www/socdemo/age.html. Monthly unemployment data by state are available from the Bureau of Labor Statistics and can be downloaded at ftp://ftp.bls.gov/pub/time.series/la/.

Two additional factors, alcohol policies and youth driving restrictions, have been shown in other research to affect fatalities. Because alcohol policies and youth driving restrictions could be correlated with mobile phone adoption or with the promulgation of hands-free laws, they need to be accounted for. First, alcohol: there is no doubt that alcohol affects fatality rates. Levitt and Porter (2001) find legally drunk drivers are 13 times more likely to have fatal accidents than sober drivers, and that in 1993 approximately 15% of drivers on the road between 8 pm and 5 am have been drinking. NHTSA reports that 34% of traffic fatalities in 2004 were alcohol-related. However, some policies, like minimum drinking ages and severity of punishment for drunk-driving, affect fatality rates; others, like beer taxes and the legal limit on blood-alcohol content, appear not to (see Levitt and Porter (2001), Dee (1999), and Cohen and Einav(2003)).

Second, youth driving: Simons-Morton and Ouimet (2006) review the relevant literature, which shows that graduated licensing programs, which put restrictions on young drivers (typically 16 and 17 year-olds) in the form of a "junior license," reduce crash rates. During the period 1997-2005 most states toughened their graduated licensing program, for instance by forbidding driving during certain hours or in certain locations or requiring driving with adult passengers.

Explanatory variables related to alcohol policies and graduated licensing laws are excluded from the specification even though both are known to affect fatality rates. The variables for alcohol-related policies are often crude measures and, in many cases, exhibit little state variation over the recent period.[15] Graduated licensing laws do exhibit variation across states over the time period under study, but graduated licensing laws are

[15] For instance, the 1984 National Minimum Drinking Age Act made federal highway funding contingent on states raising the drinking age to 21. Since 1988 all states have met the federal rules, although there is some state variation on exceptions like underage drinking with parental consent.

multi-faceted and are not easily compared across states or reduced to quantitative measures.

As an alternative way to handle alcohol and youth driving, this paper focuses on fatalities that involve neither alcohol nor a youth driver – fatalities which should not be affected by alcohol policies or graduated licensing laws.[16] In fact, it is likely that any effect of mobile phone ownership on traffic fatalities would be more pronounced for fatalities not involving alcohol or youth drivers. This is in part because the peak times for mobile phone usage while driving are different than the peak times for drunk driving. NHTSA reports that 54% of weekend night fatalities are alcohol-related, compared with 11% of weekday daytime fatalities. In contrast, NHTSA observations of mobile phone usage while driving are higher on weekdays than on weekends, and highest at weekday rush-hours; the NHTSA does not conduct its mobile phone usage observation studies outside of daylight hours (see Tables 2 and 3). Furthermore, many states forbid drivers with learners' permits from using mobile phones, regardless of whether the phone incorporates hands-free technology.[17]

The first results using the empirical model are presented in Table 4; the effect of mobile phone ownership on fatalities is inconclusive. Column 1 of Table 4 shows the coefficient on mobile phone ownership in the absence of controls other than state and year fixed-effects to be positive though statistically insignificant; adding controls (shown in column 2) lowers the coefficient value by one-third. The coefficient on unemployment

[16] Following the NHTSA definition, this paper defines a fatality as "alcohol-related" if any driver or non-motorist (a pedestrian, usually) involved had a blood alcohol concentration (BAC) of 0.08 grams per deciliter (g/dl) or above. FARS reports BAC measurements or estimates for all participants in a fatal crash. The procedure NHTSA uses to estimate missing BAC values is described in NHTSA (2002). This paper defines "youth drivers" as younger than 18, and fatalities are classified as involving youth drivers regardless of who was killed in the crash.

[17] See mobile phone restrictions on youth drivers at www.ghsa.org/html/stateinfo/laws/cellphone_laws.html.

rate is negative and statistically significant.[18] To test for a non-linear relationship between mobile phone ownership and traffic fatalities, column 3 of Table 4 includes the square of mobile phone ownership, which is statistically insignificant. Column 4 repeats the specification in column 2, but using only fatalities not involving alcohol or youth drivers as the dependent variable. The coefficients on the age variables fall, and in particular the coefficient on the share of adults age 16-24 moves from being significant at the 10% level in column two to insignificant and barely one-fifth the magnitude in column four. Excluding fatalities involving youth drivers naturally affects the coefficients on the age variables, and excluding alcohol-related fatalities matters as well since the likelihood of alcohol involvement in a crash varies by age of victim.[19]

The effect of mobile phones on traffic fatalities, however, depends on weather and road conditions.[20] Table 5 presents the same specification as before but for specific types of traffic fatalities. All columns exclude fatalities involving alcohol or young drivers. The effects of mobile phone ownership on fatalities in good weather or dry road conditions are indistinguishable from zero. The coefficients of mobile phone ownership on fatalities in bad weather or wet road conditions are positive, statistically significant, and large. The coefficient of 2.22 in the regression of bad weather fatalities (column 2), when compared to the average bad-weather fatality rate of 1.37, implies that every ten-percentage point

[18] Other control variables tested and found to have no effect were average commuting times (only available by state annually since 2000), population growth, and income. These were not included in the final analysis.
[19] Among 25-44 year-olds killed in traffic accidents, 48% were involved in an alcohol-related crash (i.e. someone in the crash, not necessarily the driver or the person killed, was intoxicated), compared with 38% of 16-24 year-olds, 33% of 45-64 year-olds, 17% of 0-15 year-olds, and 12% of people 65 years or older.
[20] In this paper, “bad weather” is defined as “rain,” “sleet,” “snow,” “fog,” or “other” (smoke, smog, blowing sand, or dust). In this paper, “wet road conditions” are defined as “wet”, “snow or slush”, “ice”, or “sand, dirt, or oil.” The FARS coding manual enumerates these weather and road conditions, which guides analysts in transferring data from police accident reports into the FARS system. FARS data cover attributes of persons involved, vehicles involved, and the accident itself. The accident variables include weather conditions and road surface conditions at the time of the crash, as well as numerous other attributes.

increase in mobile phone ownership raises bad-weather traffic fatalities by .222 per 100,000 – a 16% increase.[21] The coefficient of 2.48 in the regression of wet-road fatalities (column 4), when compared to the average wet-road fatality rate of 1.90, implies that a ten-percentage point increase in mobile phone ownership raises wet-road traffic fatalities by 13%. In sum, mobile phone ownership increases fatalities in bad weather and in wet road conditions by a large amount, though not in good weather or dry road conditions.

Mobile phone ownership does not have a statistically significant effect on traffic fatalities at a particular time of day. The coefficients on mobile phone ownership are positive but statistically insignificant for daylight, darkness, and dawn or dusk fatalities, as well as rush-hour fatalities (which can be in daylight, darkness, or dawn or dusk, depending on the season), as shown in Table 6.

Mobile phone ownership, therefore, has a large and statistically significant effect on traffic fatalities, but only under specific conditions. In bad weather or on wet roads, mobile phone ownership is associated with a large increase in fatalities, suggesting that the distraction of mobile phone usage aggravates the challenge of driving under difficult conditions while having no effect during optimal driving conditions. Mobile phone ownership does not, however, contribute to fatalities in either the increased congestion of rush-hour traffic or the decreased visibility of darkness.

[21] One should not make estimates of how much the increase of mobile phones ownership from zero to current levels have affected fatalities since the time period covered by this paper begins only when mobile phone ownership was at 38% nationally. Even though the results show that a linear relationship between mobile phone ownership and traffic fatalities is a reasonable hypothesis, the relationship could be non-linear if years prior to 1997, when mobile phone ownership was lower, were included.

HANDS-FREE LAWS AND TRAFFIC FATALITIES

To test the effect of hands-free laws, the analysis now looks at how fatalities have changed in the few states with hands-free laws relative to the rest of the US. Although laws requiring drivers to use hands-free devices when using phones are the norm in rich countries, within the US only New York, New Jersey, the District of Columbia (DC), and Connecticut have state-wide hands-free laws in effect, with similar laws passed in California and Washington. Some cities in other states, like Santa Fe, NM, and Brookline, MA, require hands-free devices while talking and driving. In many states, some types of drivers – such as school-bus drivers – are required to use hands-free devices. On the other hand, eight states forbid their municipalities from restricting mobile phone usage while driving.[22]

Only New York had a hands-free law in effect during a significant period covered by this paper. New York's law took effect in November, 2001, so the period under study (1997-2005) includes four years and two months when New York's law was in effect. New Jersey's and the District of Columbia's laws took effect in July, 2004, and Connecticut's in October, 2005. California's and Washington's laws take effect in July, 2008 (see Table 7). Any evidence of the longer-term effect of hands-free laws on fatalities will therefore be based on data from New York only, while the short-term effect of hands-free laws can take into account the experience of New Jersey, DC, and Connecticut.

The short-term effects of hands-free laws have been mixed, with DC experiencing a decline in fatalities, New York an increase, and New Jersey little change, in a

[22] These include Florida, Kentucky, Louisiana, Mississippi, Nevada, Oklahoma, Oregon, and Utah. New Jersey and New York also forbid municipalities from introducing mobile phone bans, but statewide bans are in place in those states. See www.ghsa.org/html/stateinfo/laws/cellphone_laws.html.

difference-in-differences analysis. Table 8 compares fatality rates for each state in the year prior to the hands-free law taking effect and the first year the law was in effect; this change over time is compared with the change in fatalities for the same time period for the U.S. excluding the four states with laws in effect at any time. In New York, the difference-in-differences was positive for overall fatalities, those not involving alcohol and youth drivers, and those in bad weather and in wet road conditions. In DC, the difference-in-differences was negative for all four types of fatalities, and dramatically so: fatalities not involving alcohol or youth drivers fell by close to half in the year starting July 2004, when the hands-free law took effect. Connecticut was excluded from this analysis because the law was in effect for only three months during the period this study covers.

Over a longer time period after the hands-free law took effect, however, New York's fatality rate has fallen relative to that of the US overall. Since 1997 New York has had lower fatality rates – both overall and for specific types of fatalities – than the US, but the gap widened after 2002 for fatalities not involving alcohol or youth drivers. The gap widened after 2002 for fatalities in bad weather or wet road conditions, not involving alcohol or youth drivers, as well. The fatality rate not involving alcohol or youth drivers in New York fell from 5.56 per 100,000 population in 2001, the year at the end of which the law went into effect, to 4.89 in 2005.

To include the experience of all states with hands-free laws in effect, the empirical framework is expanded to include a variable equal to the share of the year that a state had a hands-free law in effect. The model of the effect of hands-free laws to be estimated is:

$$fatalities_{it}^{j} = \alpha + \beta mobile_{it} + \theta law_{it} + \sum_{k} \gamma_k controls_{itk} + \sum_{i} \lambda_i state_i + \sum_{t} \delta_t year_t + \varepsilon_{it}$$

The *law* variable for New York equals .17 in 2001 and 1 in 2002, 2003, 2004, and 2005; for New Jersey and DC, .5 in 2004 and 1 in 2005; for Connecticut, .25 in 2005; and 0 for all other state-years. In this model, the coefficient on the existence of a hands-free law in state i at time t is interpreted as a one-time permanent shift in fatalities due to the law. This model implies that the law has an immediate and persistent effect on traffic fatalities.

The coefficient on the hands-free law dummy is negative and significant for all three measures of fatalities that exclude those involving alcohol or youth drivers. In column 1 of table 9, the coefficient on the hands-free law for all fatalities is negative but not statistically significant (t=1.6). For fatalities not involving alcohol or youth drivers, the coefficient is -.907 and statistically significant; this large magnitude implies a reduction in fatalities not involving alcohol or youth drivers of nearly 10%.[23] The coefficients on hands-free law is negative and significant for both the bad-weather and wet-road-condition fatalities; having the law in effect implies a 17% reduction in bad-weather fatalities and a 16% reduction in wet-road-condition fatalities. Comparing the coefficients on hands-free law with those on mobile phone ownership, the effect of a law is roughly equivalent to reducing mobile phone ownership by more than 10 percentage points.

However, a more plausible and less restrictive model would allow both for an initial change in the level of fatalities as well as an ongoing change in the trend of

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[23] The mean of the dependent variable is 9.88 annual fatalities per 100,000 population (see summary statistics in Table 1).

fatalities. The difference-in-differences analysis showed that fatalities in New York rose the first year; reductions in fatalities came in subsequent years. A second model, which captures this, is:

$$fatalities_{it}^{j} = \alpha + \beta mobile_{it} + \theta_1 law_{it} + \theta_2 law_{it} * lawyears_{it} + \sum_{i \in M} \rho_i state_i * year + \sum_k \gamma_k controls_{itk} + \sum_i \lambda_i state_i + \sum_t \delta_t year_t + \varepsilon_{it}$$

In this model, the impact of the hands-free law is captured both by the initial (intercept) shift, represented by θ_1, and by the change in the trend, represented by θ_2. The lawyears variable equals the number of years since the passage of the hands-free law in state i at time t. Since this model tests whether the law changes the trend in fatalities, a linear trend is included as well for every state i in the set of states M which have a law in effect for at least a year during the period 1997-2005. This applies to New York, DC, and New Jersey, so the model includes a linear time trend for those three areas.

For the three fatality types that do not involve alcohol or youth drivers, the coefficients on both the hands-free-law intercept-shift and the hands-free-law and year interaction are negative, though not statistically significant at the 5% level. The law and year interaction is negative and significant at the 10% level for fatalities in wet-road conditions (see column 4, Table 10). None of the three state-specific time trends is statistically significant.

Because all four hands-free laws took effect mid-year, the above analysis introduces measurement error by using annual fatality measures and the fraction of the year that the law was in effect. To remove this measurement error, the analysis was repeated with monthly data.[24] The FARS reports the day and time of each fatality, so it is possible to generate fatality rates by month rather than by year. Forrester's mobile phone

[24] The hands-free law in all four states went into effect on the first day of the month.

ownership measure, however, comes from an annual survey at the end of each year, so the monthly level of mobile phone ownership was linearly interpolated using the figure from the Decembers preceding and following the month in question. All four of the other explanatory variables – vehicle miles traveled, unemployment rate, precipitation, and heating degree days – are available monthly by state as well. The Census reports the age distribution only annually, and because none of the age variables has any effect on fatalities not involving alcohol or youth drivers, the age variables were dropped from the month-level analysis. The specifications remain the same as for the annual-level analysis.[25] Monthly fatalities are reported per million population, whereas annual fatalities were reported per 100,000 population. Summary statistics for monthly variables are presented in Table 11.

The results of the month-level analysis are very similar to those in the annual-level analysis. Under the restrictive model of the law having an immediate and persistent effect (i.e. only an intercept shift), the coefficient on hands-free law is negative and statistically significant for the three fatality types not involving alcohol or youth drivers (see Table 12). The coefficient magnitudes are nearly identical to the annual analysis after taking into account the rescaling of the fatalities variable from per 100,000 to per million.

Adding the variable for the number of months that the law has been in effect yields the same results as before: for all fatality types, both the law dummy (representing an intercept shift) and the law-and-month interaction (representing a slope change) are

[25] The month-level panel covers 97 months: December 1997 to December 2005. Earlier months in 1997 are excluded because December 1997 is the earliest that mobile phone ownership is available, and interpolation for the rest of 1997 would require an earlier value. With 49 states (including DC and excluding Alaska and Hawaii), the number of observations is 97 x 49 = 4753.

negative yet statistically insignificant. The one exception is that the interaction term in the regression for fatalities not involving alcohol or youth drivers (column 2 of Table 13) is negative and statistically significant. The time trends for New York and DC are not statistically significant for any fatality type; the time trend for New Jersey is significant for only fatalities in wet road conditions not involving alcohol or youth drivers (column 4 of Table 13) and for Connecticut, which had a law in effect only for the last three months of the period under study.

The final specification replaces the intercept-and-shift terms with dummies for six periods after the law took effect. This allows the effect to be non-linear; it also makes explicit which states' experiences are contributing to the estimates. The first of the six periods covers 1-6 months of the law in effect, and all four states contribute to this estimate. The next two periods, which cover months 7-12 and 13-18, include the experience of New York, New Jersey, and DC, and the last three periods, covering months 19-24, 25-36, and 37+, include only New York. This specification also includes a dummy indicating whether the law had been passed but had not yet taken effect.[26]

The coefficients on the hands-free laws are consistently negative for all six periods and all four fatality types, but only sometimes statistically significant.[27] In the regressions on all fatalities and on fatalities not involving alcohol or youth drivers, the coefficients on months 19-24 are statistically significant; in the regressions on fatalities in bad weather and fatalities in wet road conditions (columns 3 and 4 of Table 14), the coefficients on months 25-36 and 37+ are all statistically significant. The only other

[26] Table 7 shows when hands-free laws were passed and went into effect for each state.
[27] The one coefficient that is non-negative is the .004 value for months 13-18 in the regression of fatalities in bad weather (column three, Table 14).

statistically significant coefficient is that on months 7-12 in the regression of fatalities in wet road conditions.

DISCUSSION AND CONCLUSION

Mobile phone ownership is associated with more traffic fatalities, but only in bad weather or wet road conditions. In effect, mobile phone ownership interacts with these kinds of dangerous driving conditions to increase traffic fatalities. Mobile phone ownership has no statistically significant effect on fatalities in good weather or dry road conditions; nor does mobile phone ownership interact with darkness or the congestion of rush-hour to increase fatalities during those times. Because a small share of fatalities occur in bad weather or wet road conditions, the effect on mobile phone ownership on fatalities in these conditions are masked when fatalities are examined in aggregate. Just as the effect of mobile phones on traffic incidents may be heterogeneous across individuals, as Hahn and Prieger (2006) find, the effects are also heterogeneous with regard to driving conditions.

The findings about hands-free laws on traffic fatalities are less definitive, but they suggest that laws probably reduce traffic fatalities, at least in the longer-term. Looking across all the entire analysis of hands-free laws, the findings consistently point to a reduction in traffic fatalities when the laws are in effect. In the most restrictive specification, where the hands-free law variable is a dummy representing a one-time shift in the level of fatalities, the estimates are statistically significant: it is clear that fatality rates fell after hands-free laws went into effect. In other specifications, many of the estimates are not statistically different from zero. However, even in the most flexible specification, with non-linear effects, some estimates are statistically significant, and essentially all are negative.

The analysis is limited, of course, by the fact that only New York has had a law in effect for longer than 18 months of the period under study. New York is the only state contributing to these statistically significant (and large) declines in fatalities in bad weather and in wet road conditions more than two years after the law went into effect. Because these significant results include a New York specific time trend, control for weather variables, and exclude fatalities involving alcohol or youth drivers whose rates could have been affected by other policy changes in New York, there is no other apparent explanation for the drop in these fatalities in New York other than the hands-free law.

The big question is why hands-free laws appear to reduce some types of fatalities when other research has consistently found that drivers using hands-free devices are no less likely to be in accidents or be distracted than drivers using hand-held phones. Recall that these previous studies found a similar level of distraction among drivers using both types of devices, *conditional on using the device*. In practice, however, drivers decide whether and when to use mobile phones. The question, then, is how hands-free laws might change drivers' likelihood of using mobile phones when driving, regardless of whether the device is handheld or hands-free.

It may be that drivers in states with hands-free laws are shifting their talking minutes to when they are not driving, or to those times while driving when talking is less likely to contribute to a fatal accident, or reducing their overall phone usage.[28] Why

[28] Hands-free laws do not decrease mobile phone ownership or average spending on mobile phones. A regression of mobile phone ownership on demographics for the same panel of states and years yields an insignificant coefficient on the law variable; this is the case for both the level of mobile phone ownership and the average household mobile phone bill. It is possible, however, that drivers in hands-free states could

might hands-free laws reduce usage under some driving conditions? It could be that drivers find hands-free technology more cumbersome to use if making a call requires handling two devices (the headset or earpiece and the phone) rather than one, or drivers might find a decrease in sound quality from a bad headset or undependable Bluetooth wireless connection. Unfortunately, the state-level data on mobile phone ownership and fatalities used in this paper cannot assess any of these speculations.

Yet another explanation why hands-free laws could reduce mobile phone usage is that the law in itself serves as an educational warning about the danger of talking while driving, thus discouraging usage or at least encouraging more discretion in deciding when to talk. A test of this is whether fatalities decrease in the period after a state has passed the law but before it has taken effect; the passage of the law itself signals the government's belief of the danger of hand-held phones, so any education effect of the law could begin at the time of passage rather than enforcement. The model presented in Table 14 includes a dummy indicating whether a hands-free law was passed but not yet in effect. Table 7 shows that this period lasted five months in New York, six months in New Jersey and DC, and three months in Connecticut. The effect of passage prior to enforcement is ambiguous: the effect is negative and statistically significant on all fatalities (column 1 of Table 14) and positive and statistically significant on fatalities in wet road conditions not involving alcohol or youth drivers.

A recent Harris Poll tries to address these open questions. Among respondents in states with hands-free laws, 61% use a mobile phone while driving all or some of the

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reduce their overall phone usage without lowering their average monthly mobile phone bill since mobile phone users with a monthly usage allotment do not pay for marginal minutes unless they exceed the allotment.

time, compared with 73% of respondents overall. Respondents in states with hands-free laws are more likely to think that using a mobile phone while driving in dangerous than respondents overall, 64% vs 56%. However, these results cannot be used to assess the effect of hands-free laws because the poll was conducted only at one point in time, rather than before-and-after hands-free laws came into effect. Therefore these poll data cannot rule out the possibility that respondents in states with hands-free laws exhibit different behaviors and attitudes, regardless of the law being in effect.[29]

The results show that the effect of New York's hands-free law on reducing traffic fatalities strengthens over time: after two years the effects on fatalities in wet road conditions and bad weather conditions are negative, statistically significant, and large. Several explanations are consistent with the law's effect strengthening over time, although the available data cannot be used to test these explanations. One is that drivers wait to comply with the law until the probability and severity of enforcement is known. Another is that if hands-free laws have an educational effect or are introduced with a program of education, the education takes time to sink in and change behaviors. Yet another could be that over time drivers replace more cumbersome hands-free technologies, like a corded headset, by investing in less obtrusive and plausibly less distracting technologies like Bluetooth headsets or phones that are integrated into the car's stereo system.

[29] This survey was conducted online by Harris Interactive, a polling firm, in May, 2006. The sample size is 2,085. Many caveats apply. Online surveys aren't generalizable to the offline population and, depending on the sampling methodology, might not be representative of the online population. Survey results were reported without sample sizes in each cell, so tests of significance for differences between groups aren't possible to conduct. Finally, these results do not control for individual characteristics, and microdata from the survey were not made public for researchers to assess whether these differences in attitudes and behavior among respondents in hands-free states hold true when after controlling for individual characteristics.

Another question raised by the results is why a hands-free law could have a negative and statistically significant effect on fatality types for which mobile phone ownership does not have a statistically significant positive effect in the first place? Column 1 of Table 14 shows that the coefficient on months 19-24 is significant for all fatalities; the coefficients on months 7-12 and 37+ are significant at the 10% level. Yet the effect of mobile phone ownership on all fatalities, though positive, is insignificant with a t-statistic well under one (column 4, Table 4). One possible explanation is that the effects of mobile phone ownership and hands-free laws are heterogeneous, as Hahn and Prieger (2006) argue. During the period that this study covers, 1997-2005, mobile phone ownership rose from 38% to 76% of the population, so the estimated effect of mobile phone ownership on traffic fatalities reflects behavior of later adopters of mobile phones. However, hands-free laws apply to all mobile phone users, not only later adopters. With heterogeneous effects, we would not necessarily get consistent results for mobile phone ownership among later adopters and hands-free laws among all mobile phone users.

Despite the many remaining unanswered questions about how and why mobile phones affect traffic fatalities, the evidence suggests that the effects of mobile phones and hands-free laws on traffic fatalities vary across people, across conditions, and over time. The effect of mobile phone ownership on traffic fatalities depends on road conditions and weather; the effect of hands-free laws on traffic fatalities appears to depend on how long the laws have been in effect; and, with more years of data from other states with hands-free laws, it may emerge that the effect of hands-free laws on traffic fatalities varies by state as well.[30] It is premature to conclude that hands-free phones are no safer than hand-

[30] The effect of mobile phones and hands-free laws on traffic fatalities could also depend on how mobile phone technology evolves: phones with breathalyzers (made by LG Electronics, sold in Korea) could

helds and that hands-free laws do not affect traffic fatalities. The results suggests that there is room to design mobile phone policies and enforcement strategies that could, in fact, save lives, and the cost of these policies and their enforcement could be reduced by targeting situations in which mobile phones increase traffic fatalities.

reduce drunk driving, and phones with location-sensing capabilities could help emergency services respond more quickly after crashes.

Table 1: Summary statistics for variables used in the annual-level analysis

	Mean	Standard deviation
Mobile phone ownership	.550	.134
VMT per capita, 000's	10.42	1.82
Unemployment rate	.047	.011
% adults 16-24	.165	.018
% adults 25-44	.376	.027
% adults 45-64	.296	.021
Precipitation inches	36.96	15.18
Heating degree days, 000's	5.04	1.95
Fatalities (per 100,000 pop.)	16.85	6.12
Fatalities not involving alcohol or young drivers (per 100,000 pop.)	9.88	3.63
Fatalities **in good weather** not involving alcohol or young drivers (per 100,000 pop.)	8.46	3.12
Fatalities **in bad weather** not involving alcohol or young drivers (per 100,000 pop.)	1.37	.74
Fatalities **in dry road conditions** not involving alcohol or young drivers (per 100,000 pop.)	7.91	3.01
Fatalities **in wet road conditions** not involving alcohol or young drivers (per 100,000 pop.)	1.90	.95
Fatalities **in rush hour** not involving alcohol or young drivers (per 100,000 pop.)	2.39	1.00
Fatalities **in daylight** not involving alcohol or young drivers (per 100,000 pop.)	6.57	2.58
Fatalities **in darkness** not involving alcohol or young drivers (per 100,000 pop.)	2.81	1.07
Fatalities **in dawn or dusk** not involving alcohol or young drivers (per 100,000 pop.)	.46	.27
Hands-free law (dummy)	.017	.121
Hands-free law * years in effect	.027	.268

N=441
49 states (incl. DC, excluding AK and HI) over 9 years

Table 2:
Observed mobile-phone usage, daylight hours, at stop lights or stop signs, 2005

	Holding mobile phone	Using hands-free mobile phone
Weekday rush hour	8%	.8%
Weekday non-rush hour	6%	.8%
Weekend	4%	.2%

Source: Driver Cell Phone Use in 2005, NHTSA, DOT HS 809 967

Table 3:
Driver traffic fatalities: % with driver's BAC>=.08, 2004

Weekdays (M-F, 6 am – 6 pm)	11%
Weeknights (M 6 pm – F 6 am)	45%
Weekend days (Sa-Su, 6 am – 6 pm)	19%
Weekend nights (F 6 pm – M 6 am)	54%

Source: Alcohol, NHTSA, DOT HS 809 905

Table 4: Mobile phones and traffic fatalities, all conditions

Dependent variable: Annual traffic fatalities per 100,000 population

	(1)	(2)	(3)	(4)
	All fatalities	All fatalities	All fatalities	Fatalities not involving alcohol or youth drivers
Mobile phones (%)	4.674 (2.839)	3.059 (2.791)	5.558 (8.307)	2.137 (2.442)
Mobile phones (%), squared			-2.415 (7.264)	
VMT per capita, 000's		0.188 (0.236)	0.184 (0.232)	0.141 (0.188)
Unemployment rate (%)		-34.755*** (11.747)	-33.848*** (12.442)	-29.475*** (8.215)
% adults 16-24		53.292* (27.809)	55.124** (25.517)	11.431 (24.134)
% adults 25-44		56.583 (43.688)	57.700 (42.806)	17.167 (33.624)
% adults 45-64		85.524* (44.887)	85.383* (45.664)	26.915 (34.131)
Precipitation inches, year		-0.020 (0.016)	-0.020 (0.016)	-0.007 (0.011)
Heating degree days, 000's, year		-0.289 (0.296)	-0.273 (0.292)	-0.278 (0.238)
Observations	441	441	441	441
R-squared	0.96	0.96	0.96	0.95

Notes:
- Regressions include state and year fixed effects
- Robust standard errors, clustered on state
- Omitted category: % of adults age 65+
- *** denotes significance at 1% level; ** at 5%; * at 10%

Table 5: Mobile phones and traffic fatalities, by weather and road conditions

Dependent variable: Annual traffic fatalities per 100,000 population, excluding fatalities involving alcohol or young drivers

	(1)	(2)	(3)	(4)
	Fatalities in **good weather** not involving alcohol or youth drivers	Fatalities in **bad weather** not involving alcohol or youth drivers	Fatalities in **dry road conditions** not involving alcohol or youth drivers	Fatalities in **wet road conditions** not involving alcohol or youth drivers
Mobile phones (%)	0.103	2.172**	-0.067	2.477**
	(1.952)	(0.847)	(2.006)	(0.991)
VMT per capita, 000's	0.092	0.039	0.194	-0.032
	(0.144)	(0.063)	(0.176)	(0.045)
Unemployment rate (%)	-27.491***	-1.221	-24.115***	-4.544
	(9.121)	(3.376)	(7.687)	(3.071)
% adults 16-24	25.934	-16.172*	17.447	-6.464
	(21.297)	(9.011)	(20.074)	(7.456)
% adults 25-44	29.125	-13.617	18.749	-1.647
	(28.647)	(11.645)	(26.911)	(10.878)
% adults 45-64	38.127	-12.525	21.378	5.978
	(30.533)	(12.487)	(28.134)	(10.762)
Precipitation inches, year	-0.022**	0.015***	-0.029***	0.023***
	(0.009)	(0.003)	(0.009)	(0.004)
Heating degree days, 000's, year	-0.303	0.040	-0.355*	0.070
	(0.198)	(0.088)	(0.199)	(0.103)
Observations	441	441	441	441
R-squared	0.93	0.79	0.94	0.86

Notes:
- All fatality measures exclude fatalities involving alcohol or young drivers
- Regressions include state and year fixed effects
- Robust standard errors, clustered on state
- Omitted category: % of adults age 65+
- *** denotes significance at 1% level; ** at 5%; * at 10%

Table 6: Mobile phones and traffic fatalities, by time of day

Dependent variable: Annual traffic fatalities per 100,000 population, excluding fatalities involving alcohol or young drivers

	(1)	(2)	(3)	(4)
	Fatalities in **rush hour** not involving alcohol or youth drivers	Fatalities in **daylight** not involving alcohol or youth drivers	Fatalities in **darkness** not involving alcohol or youth drivers	Fatalities in **dawn or dusk** not involving alcohol or youth drivers
Mobile phones (%)	0.458 (0.975)	1.492 (1.423)	0.235 (1.458)	0.427 (0.292)
VMT per capita, 000's	0.102 (0.068)	0.076 (0.166)	0.113 (0.097)	-0.059 (0.049)
Unemployment rate (%)	-12.810*** (4.592)	-20.568*** (6.664)	-10.288* (5.570)	1.475 (2.133)
% adults 16-24	-10.979 (6.998)	3.980 (17.840)	5.095 (10.864)	-0.041 (3.944)
% adults 25-44	-9.526 (7.896)	16.343 (21.825)	-0.126 (16.612)	-1.578 (5.648)
% adults 45-64	-3.543 (8.697)	33.280 (24.854)	-5.454 (15.967)	-2.639 (6.477)
Precipitation inches, year	-0.002 (0.005)	-0.008 (0.008)	0.002 (0.005)	-0.001 (0.002)
Heating degree days, 000's, year	-0.004 (0.095)	-0.125 (0.166)	-0.119 (0.130)	-0.011 (0.051)
Observations	441	441	441	441
R-squared	0.85	0.94	0.82	0.58

Notes:

- All fatality measures exclude fatalities involving alcohol or young drivers
- Regressions include state and year fixed effects
- Robust standard errors, clustered on state
- Omitted category: % of adults age 65+
- *** denotes significance at 1% level; ** at 5%; * at 10%

Table 7: States with hands-free laws

State	Law passed	Law in effect	Months in 1997-2005 with law in effect
New York	June 2001	November 2001	50
New Jersey	January 2004	July 2004	18
District of Columbia	January 2004	July 2004	18
Connecticut	July 2005	October 2005	3
California	September 2006	July 2008	0
Washington	May 2007	July 2008	0

Table 8: Differences-in-difference analysis of hands-free laws: first-year effects

New York (hands-free law in effect November 2001)

	New York		United States*		Diff-in-diff
	11/00-10/01	**11/01-10/02**	**11/00-10/01**	**11/01-10/02**	
All fatalities	7.76	8.32	15.46	15.95	0.07
Fatalities not involving alcohol or youth drivers	5.31	5.59	9.07	9.23	0.12
Fatalities in bad weather not involving alcohol or youth drivers	.70	.76	1.18	1.09	0.15
Fatalities in wet road conditions not involving alcohol or youth drivers	1.16	1.15	1.62	1.49	0.12

DC (hands-free law in effect July 2004)

	DC		United States*		Diff-in-diff
	7/03-6/04	**7/04-6/05**	**7/03-6/04**	**7/04-6/05**	
All fatalities	11.47	6.50	15.80	15.36	-4.53
Fatalities not involving alcohol or youth drivers	5.85	3.32	9.44	9.38	-2.47
Fatalities in bad weather not involving alcohol or youth drivers	.39	.30	1.18	1.28	-0.19
Fatalities in wet road conditions not involving alcohol or youth drivers	.18	0	1.59	1.71	-0.3

New Jersey (hands-free law in effect July 2004)

	New Jersey		United States*		Diff-in-diff
	7/03-6/04	**7/04-6/05**	**7/03-6/04**	**7/04-6/05**	
All fatalities	7.95	8.41	15.80	15.36	0.9
Fatalities not involving alcohol or youth drivers	5.15	5.82	9.44	9.38	0.73
Fatalities in bad weather not involving alcohol or youth drivers	.76	.75	1.18	1.28	-0.11
Fatalities in wet road conditions not involving alcohol or youth drivers	.94	1.05	1.59	1.71	-0.01

*US average excludes NY, DC, NJ, CT

Table 9: Impact of hands-free laws: one-time shift only

Dependent variable: Annual traffic fatalities per 100,000 population

	(1)	(2)	(3)	(4)
	All fatalities	Fatalities not involving alcohol or youth drivers	Fatalities in **bad weather** not involving alcohol or youth drivers	Fatalities in **wet road conditions** not involving alcohol or youth drivers
Mobile phones (%)	3.324 (2.873)	2.497 (2.448)	2.263** (0.867)	2.599** (1.006)
Hands-free law	-0.669 (0.424)	-0.907*** (0.235)	-0.228** (0.106)	-0.306** (0.121)
VMT per capita, 000's	0.190 (0.235)	0.143 (0.186)	0.039 (0.063)	-0.032 (0.044)
Unemployment rate (%)	-36.078*** (11.775)	-31.271*** (8.046)	-1.672 (3.467)	-5.149 (3.087)
% adults 16-24	55.545* (28.153)	14.488 (24.679)	-15.403* (8.990)	-5.435 (7.566)
% adults 25-44	58.676 (44.123)	20.007 (34.270)	-12.903 (11.573)	-0.691 (10.895)
% adults 45-64	86.174* (44.962)	27.796 (34.578)	-12.303 (12.316)	6.274 (10.756)
Precipitation inches, year	-0.020 (0.016)	-0.006 (0.011)	0.015*** (0.003)	0.023*** (0.004)
Heating degree days, 000's, year	-0.252 (0.301)	-0.229 (0.242)	0.052 (0.088)	0.087 (0.103)
Observations	441	441	441	441
R-squared	0.96	0.95	0.79	0.86

Notes:

- All fatality measures exclude fatalities involving alcohol or young drivers
- Regressions include state and year fixed effects
- Robust standard errors, clustered on state
- Omitted category: % of adults age 65+
- *** denotes significance at 1% level; ** at 5%; * at 10%

Table 10: Impact of hands-free laws: shift and linear trend

Dependent variable in column header:	(1) All fatalities	(2) Fatalities not involving alcohol or youth drivers	(3) Fatalities in **bad weather** not involving alcohol or youth drivers	(4) Fatalities in **wet road conditions** not involving alcohol or youth drivers
Mobile phones (%)	3.498	2.406	2.311**	2.653**
	(2.966)	(2.532)	(0.892)	(1.034)
Hands-free law	-1.208	-0.649	-0.154	-0.112
	(0.942)	(0.589)	(0.231)	(0.191)
Hands-free law * years in effect	0.145	-0.058	-0.085	-0.152*
	(0.240)	(0.165)	(0.090)	(0.084)
NY time trend	0.066	-0.072	0.011	0.033
	(0.126)	(0.083)	(0.033)	(0.038)
DC time trend	0.046	0.022	0.002	-0.041
	(0.123)	(0.088)	(0.034)	(0.034)
NJ time trend	0.069	-0.005	0.032	0.017
	(0.087)	(0.054)	(0.022)	(0.019)
VMT per capita, 000's	0.188	0.138	0.040	-0.027
	(0.233)	(0.185)	(0.063)	(0.045)
Unemployment rate (%)	-34.931***	-31.403***	-1.570	-5.536
	(12.600)	(8.594)	(3.750)	(3.371)
% adults 16-24	54.179*	13.211	-15.953	-4.424
	(30.493)	(26.661)	(9.884)	(8.756)
% adults 25-44	58.202	18.649	-13.587	-0.349
	(45.158)	(35.174)	(12.142)	(11.296)
% adults 45-64	86.268*	26.856	-13.092	5.758
	(45.374)	(34.973)	(12.563)	(10.888)
Precipitation inches, year	-0.020	-0.006	0.015***	0.023***
	(0.016)	(0.011)	(0.003)	(0.003)
Heating degree days, 000's, year	-0.262	-0.229	0.052	0.093
	(0.304)	(0.245)	(0.088)	(0.104)
Observations	441	441	441	441
R-squared	0.96	0.95	0.79	0.86

Notes:

- All fatality measures exclude fatalities involving alcohol or young drivers
- Regressions include state and year fixed effects

- Robust standard errors, clustered on state
- Omitted category: % of adults age 65+
- *** denotes significance at 1% level; ** at 5%; * at 10%

Table 11: Summary statistics for variables used in the month-level analysis

	Mean	Standard deviation
Mobile phone ownership (interpolated from annual levels)	.550	.120
VMT per capita, 000's	.874	.176
Unemployment rate	.047	.012
Precipitation inches	3.08	2.08
Heating degree days, 000's	.422	.412
Fatalities (per 1,000,000 pop.)	14.05	6.31
Fatalities not involving alcohol or young drivers (per 1,000,000 pop.)	8.25	3.97
Fatalities **in good weather** not involving alcohol or young drivers (per 1,000,000 pop.)	7.07	3.69
Fatalities **in bad weather** not involving alcohol or young drivers (per 1,000,000 pop.)	1.14	1.21
Fatalities **in dry road conditions** not involving alcohol or young drivers (per 1,000,000 pop.)	6.62	3.73
Fatalities **in wet road conditions** not involving alcohol or young drivers (per 1,000,000 pop.)	1.58	1.67
Hands-free law (dummy)	.019	.136
Hands-free law * months in effect	.341	3.13

N=4753
49 states (incl. DC, excluding AK and HI) over 97 months (Dec. 1997 – Dec. 2005)

Table 12: Impact of hands-free laws, monthly, one-time shift only

	(1)	(2)	(3)	(4)
	All fatalities	Fatalities not involving alcohol or youth drivers	Fatalities in **bad weather** not involving alcohol or youth drivers	Fatalities in **wet road conditions** not involving alcohol or youth drivers
Mobile phones (%)	1.811	3.269	2.315***	3.871***
	(2.483)	(2.109)	(0.821)	(0.977)
Hands-free law	-0.709	-0.753***	-0.213***	-0.268**
	(0.483)	(0.244)	(0.066)	(0.117)
VMT per capita, 000's	8.784*	4.964	-1.355*	-2.273*
	(4.973)	(3.363)	(0.759)	(1.136)
Unemployment rate (%)	-47.171***	-31.510***	-4.032	0.152
	(15.696)	(8.439)	(3.297)	(3.269)
Precipitation inches, month	-0.144***	-0.074**	0.140***	0.182***
	(0.039)	(0.028)	(0.015)	(0.022)
Heating degree days, 000's, month	-2.962***	-1.266**	1.196***	2.291***
	(0.883)	(0.550)	(0.197)	(0.290)
Observations	4753	4753	4753	4753
R-squared	0.74	0.64	0.39	0.47

Notes:

- All fatality measures exclude fatalities involving alcohol or young drivers
- Regressions include state and year*month fixed effects
- Robust standard errors, clustered on state
- *** denotes significance at 1% level; ** at 5%; * at 10%

Table 13: Impact of hands-free laws, monthly, shift and linear trend

	(1)	(2)	(3)	(4)
	All fatalities	Fatalities not involving alcohol or youth drivers	Fatalities in **bad weather** not involving alcohol or youth drivers	Fatalities in **wet road conditions** not involving alcohol or youth drivers
Mobile phones (%)	2.153	3.418	2.422***	4.093***
	(2.642)	(2.211)	(0.848)	(1.005)
Hands-free law	-0.867	-0.390	-0.195	-0.245
	(0.910)	(0.489)	(0.153)	(0.203)
Hands-free law * months in effect	-0.019	-0.022**	-0.002	-0.010
	(0.013)	(0.009)	(0.009)	(0.011)
VMT per capita, 000's	8.796*	4.960	-1.356*	-2.271*
	(4.984)	(3.365)	(0.759)	(1.135)
Unemployment rate (%)	-46.382***	-31.612***	-3.667	0.687
	(16.205)	(8.686)	(3.367)	(3.345)
Precipitation inches, month	-0.144***	-0.074**	0.140***	0.182***
	(0.039)	(0.028)	(0.015)	(0.022)
Heating degree days, 000's, month	-2.968***	-1.264**	1.193***	2.288***
	(0.881)	(0.550)	(0.197)	(0.290)
NY time trend	0.016	0.001	-0.001	0.005
	(0.017)	(0.010)	(0.004)	(0.005)
CT time trend	-0.001	-0.005**	-0.004***	-0.005***
	(0.004)	(0.003)	(0.001)	(0.001)
DC time trend	0.003	-0.004	0.001	-0.001
	(0.009)	(0.005)	(0.001)	(0.002)
NJ time trend	0.008	0.002	0.003	0.004**
	(0.010)	(0.006)	(0.002)	(0.002)
Observations	4753	4753	4753	4753
R-squared	0.74	0.64	0.39	0.47

Notes:

- All fatality measures exclude fatalities involving alcohol or young drivers
- Regressions include state and year*month fixed effects
- Robust standard errors, clustered on state
- *** denotes significance at 1% level; ** at 5%; * at 10%

Table 14: Impact of hands-free laws, monthly, non-linear effects

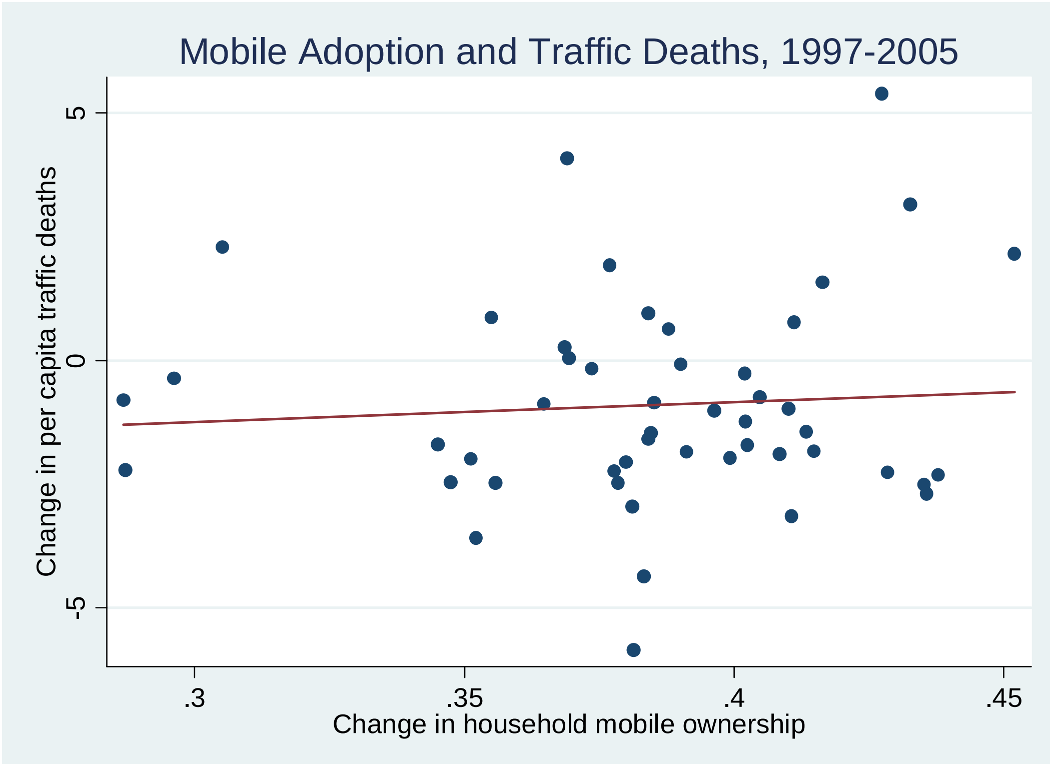
	(1) All fatalities	(2) Fatalities not involving alcohol or youth drivers	(3) Fatalities in **bad weather** not involving alcohol or youth drivers	(4) Fatalities in **wet road conditions** not involving alcohol or youth drivers
Mobile phones (%)	2.027 (2.642)	3.299 (2.231)	2.366*** (0.850)	4.021*** (1.013)
Hands-free law in effect 1-6 mos. (NY, NJ, DC, CT)	-1.487 (1.209)	-0.765 (0.734)	-0.310* (0.176)	-0.330 (0.227)
Hands-free law in effect 7-12 mos (NY, NJ, DC)	-1.136* (0.634)	-0.709 (0.486)	-0.169 (0.110)	-0.299** (0.139)
Hands-free law in effect 13-18 mos (NY, NJ, DC)	-0.863 (0.906)	-0.469 (0.482)	0.004 (0.124)	-0.099 (0.185)
Hands-free law in effect 19-24 mos (NY only)	-2.139** (0.855)	-1.475*** (0.532)	-0.060 (0.109)	-0.208 (0.129)
Hands-free law in effect 25-36 mos (NY only)	-1.263 (1.037)	-1.253* (0.672)	-0.304** (0.140)	-0.495*** (0.171)
Hands-free law in effect 37+ mos (NY only)	-2.165* (1.267)	-1.609* (0.828)	-0.429** (0.178)	-0.747*** (0.208)
Hands-free law passed, not yet in effect	-0.782** (0.305)	-0.477 (0.299)	0.162 (0.154)	0.298** (0.135)
VMT per capita, 000's	8.782* (4.988)	4.949 (3.367)	-1.354* (0.759)	-2.269* (1.135)
Unemployment rate (%)	-46.163*** (16.243)	-31.451*** (8.689)	-3.718 (3.363)	0.563 (3.344)
Precipitation inches, month	-0.145*** (0.039)	-0.074** (0.028)	0.140*** (0.015)	0.183*** (0.022)
Heating degree days, 000's, month	-2.982*** (0.881)	-1.272** (0.550)	1.195*** (0.197)	2.292*** (0.290)
NY time trend	0.019 (0.018)	0.004 (0.011)	0.000 (0.003)	0.004 (0.003)
CT time trend	0.001 (0.004)	-0.004 (0.003)	-0.004*** (0.001)	-0.006*** (0.001)
DC time trend	0.006 (0.009)	-0.003 (0.006)	-0.000 (0.002)	-0.002 (0.002)
NJ time trend	0.010 (0.009)	0.003 (0.006)	0.002 (0.002)	0.003 (0.002)

Observations	4753	4753	4753	4753
R-squared	0.74	0.64	0.39	0.47

Notes:

- All fatality measures exclude fatalities involving alcohol or young drivers
- Regressions include state and year*month fixed effects
- Robust standard errors, clustered on state
- *** denotes significance at 1% level; ** at 5%; * at 10%

Figure 1


Mobile Adoption and Traffic Deaths, 1997-2005
Change in per capita traffic deaths
5
0
-5
.3
.35
.4
.45
Change in household mobile ownership

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