

## ORIGINAL ARTICLE

## Longer term effects of New York State's law on drivers' handheld cell phone use

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**Objective:** To determine whether substantial short term declines in drivers' use of handheld cell phones, after a state ban, were sustained one year later.

**Design:** Drivers' daytime handheld cell phone use was observed in four New York communities and two Connecticut communities. Observations were conducted one month before the ban, shortly after, and 16 months after. Driver gender, estimated age, and vehicle type were recorded for phone users and a sample of motorists.

**Intervention:** Effective 1 November 2001, New York became the only state in the United States to ban drivers' handheld cell phone use. Connecticut is an adjacent state without such a law.

**Sample:** 50 033 drivers in New York, 28 307 drivers in Connecticut.

**Outcome measures:** Drivers' handheld cell phone use rates in New York and Connecticut and rates by driver characteristics.

**Results:** Overall use rates in Connecticut did not change. Overall use in New York declined from 2.3% pre-law to 1.1% shortly after ( $p < 0.05$ ). One year later, use was 2.1%, higher than immediately post-law ( $p < 0.05$ ) and not significantly different from pre-law. Initial declines in use followed by longer term increases were observed for males and females, drivers younger than 60, and car and van drivers; use patterns varied among the four communities. Publicity declined after the law's implementation. No targeted enforcement efforts were evident. Cell phone citations issued during the first 15 months represented 2% of all traffic citations.

**Conclusions:** Vigorous enforcement campaigns accompanied by publicity appear necessary to achieve longer term compliance with bans on drivers' cell phone use.

In December 2002, there were an estimated 142 million subscribers of cellular telephones (hereafter referred to as cell phones) in the United States. Americans talked more than 600 billion minutes on their cell phones in 2002.<sup>1</sup> In a 2002 national telephone survey, one in three drivers reported using a cell phone while driving during at least some trips, and one in four reported using a phone during at least half of all trips.<sup>2</sup> Based on observations of motorists at controlled intersections in 2002, an estimated 4% of drivers of passenger vehicles were talking on handheld cell phones at any given time during daylight hours, up from 3% in 2000.<sup>3</sup>

Experimental studies (using driving simulators, test tracks, or on-the-road driving in controlled settings) have found impairments in driving performance associated with cell phone use.<sup>4–8</sup> Some studies suggest that degradations in performance are similar for either handheld or hands-free devices.<sup>7–10</sup> Other studies have reported greater impairment with handheld phones.<sup>4–11–12</sup> Experimental studies use selected measures of driving performance in controlled settings and selected components of the phone task, and their applicability to driving in the real world is unknown. Epidemiological research has found increased crash risk associated with drivers' use of cell phones,<sup>13–14</sup> although the size of the estimated risk varies among studies. Redelmeier and Tibshirani examined cell phone billing records of Canadian drivers in property-damage crashes and found a fourfold increase in crash risk associated with drivers' phone use.<sup>15</sup> Epidemiological studies have not established the relative risks of hands-free and handheld devices.

Since January 2002, eight state legislatures in the United States have considered limitations on all types of cell phones, and 35 have considered limiting only handheld devices. Two states limit cell phone use by drivers holding provisional

licenses; six states ban cell phone use by school bus drivers.<sup>16</sup> However, New York is the only state to ban talking on a handheld cell phone while driving by all drivers of all vehicles. In New York, it is a traffic violation, punishable by a \$100 fine, for a driver to talk on a cell phone while the vehicle is moving, unless the driver is placing an emergency call.<sup>17</sup> The law does not apply to hands-free devices; nor does it prohibit manual dialing or using a handheld phone when the vehicle is stopped. The law, effective 1 November 2001, was implemented in three phases. During November 2001, law enforcement officers could issue verbal warnings. Citations could be issued from 1 December through 28 February 2002, but judges could waive fines for the first offense upon proof of purchase of a headset or speakerphone. Effective 1 March 2002, fines no longer were waived.

McCartt *et al* reported a substantial short term effect of the law on drivers' use of handheld cell phones.<sup>18</sup> Observed use declined significantly from 2.3% before the law to 1.1% in the first few months after the law. In Connecticut, an adjacent state with no such law, the use rate of 2.9% measured before New York's law did not change significantly. Although there was considerable unpaid publicity in New York when the law was enacted and implemented and when the fine-with-waiver period began, this rapidly dissipated. There was no statewide enforcement campaign targeting cell phone violations. Thus, results suggested that enacting legislation restricting drivers' use of handheld cell phones and the accompanying publicity had a strong effect on motorists'

**Abbreviations:** CI, confidence interval; SUV, sport utility vehicle

behavior, even in the absence of publicized intensive enforcement.

However, experience with other highway safety laws indicates that publicized vigorous enforcement is needed to sustain initial compliance with a new law. Thus, an important question is whether the observed short term decline in New York drivers' handheld cell phone use has been sustained. To answer this question, drivers' handheld cell phone use was observed in March 2003, one year after the law took full effect.

## METHODS

### Collecting observation data

A detailed account of the method for conducting observations, summarized here, is provided in McCartt *et al.*<sup>18</sup> Daytime observations of drivers were conducted at controlled intersections in four small to medium sized upstate communities in New York State (Albany County, Cities of Binghamton and Kingston, Village of Spring Valley) and in two communities in central Connecticut (Town of Hamden, City of Hartford). Areas considered for observation in New York excluded the downstate counties of Nassau, Suffolk, and Westchester due to pre-existing local bans on cell phone use while driving; New York City because of its unusual traffic patterns, major congestion, and the difficulty of finding suitable observation sites; and the western and northernmost counties due to inclement winter weather conditions.

Observations were conducted at controlled intersections on geographically dispersed, heavily traveled roads. Limited access highways were excluded. Observations were conducted on a Thursday or Friday in seven observation periods throughout the day. Approaching vehicles in the closest two lanes were observed by a person positioned at the roadside at or near the intersection. Emergency vehicles, tractor-trailer trucks, and buses were excluded. In accordance with the law, cell phone use was recorded as "yes" only if the driver was holding the telephone to the ear while the vehicle was moving.

Pre-law observations were conducted about one month before the warning period began on 1 November 2001; short term compliance was measured by observations conducted immediately after the fine-with-waiver phase took effect on 1 December and immediately after the fine-without-waiver phase took effect on 1 March 2002. Longer term compliance was measured in observations conducted during the first two weeks of March 2003.

Thirty five minutes of each observation period focused on enumerating cell phone use. A handheld counter recorded drivers not using a handheld cell phone. For drivers using a cell phone, the counter was not clicked and the following information was recorded: estimated age category (younger than 25, ages 25–59, ages 60 and older), gender, and vehicle type (car, pickup truck, sport utility vehicle (SUV), van or minivan, large single unit truck with more than four tires). During five minutes before and five minutes after the 35 minute cell phone observations, the age category, gender, and vehicle type were recorded for a sample of drivers in passing traffic.

### Analyses

Estimates were derived of the proportion of drivers in qualifying vehicles who were using handheld cell phones. For each community and for the communities combined in each state, cell phone use rates for the December 2001 and March 2002 surveys were not significantly different; thus, data for these surveys were combined to measure short term compliance. Use rates were compared between the pre-law

and short term post-law surveys, the short term and follow up post-law surveys, and the pre-law and follow up surveys.

Assuming that patterns of handheld cell phone use in New York would have followed the trends observed in Connecticut, absent New York's law, logistic regression models made a direct statistical comparison between the changes observed in cell phone use rates in New York relative to the changes in Connecticut. The models approximated a linear relationship between the logarithm of the odds of cell phone use and variables representing the state (New York/Connecticut), time period (after/before), and the interaction of state and time period. The estimated percentage change in use rates in New York relative to those in Connecticut (based on the ratio of after/before odds ratios) was a function of the model coefficient for the interaction variable. An Appendix provides additional detail on the logistic models.

To derive the proportions of observed cell phone use for the recorded driver characteristics, the percentage distributions of driver characteristics observed during the 10 minute observations of passing traffic were applied to the total vehicles counted during the 35 minute cell phone observation periods. The formulae for these calculations and associated 95% confidence intervals (CIs) are provided in McCartt *et al.*<sup>18</sup>

## RESULTS

Table 1 provides observed rates of handheld cell phone use in New York and Connecticut during each of the three observation periods. For the New York communities combined, the pre-law handheld cell phone use rate of 2.3% declined significantly to 1.1% immediately after the law took effect. Use then rose during the following year to 2.1% in March 2003, a level significantly higher than the short term compliance rate and not significantly different than the pre-law rate. During the same time periods, the rate for the Connecticut communities combined was 2.9% before the New York law, 2.9% immediately after the law, and 3.3% in March 2003. The pre-law use rate for Connecticut was higher than that for New York, but the difference was much larger immediately after the law when the New York use rates declined, and was still somewhat larger one year later when the increase in New York use rates was only partially offset by a rise in the Connecticut use rate.

The logistic regression modeling estimated that, when compared with pre-law survey results and corresponding use rates in Connecticut, the short term post-law use rate in New York was approximately 53% (95% CI 40% to 63%) lower than would have been expected absent the law; this decline was significant ( $p < 0.0001$ ). The logistic model estimated that the March 2003 use rate in New York compared to the pre-law rate was 21% lower than would have been expected (relative to Connecticut). This estimated reduction is much smaller than immediately after the law and was not statistically significant.

Patterns of handheld cell phone use rates over time were not uniform among the four New York areas (table 1). In one community (Binghamton) the significant short term decline in use was sustained in the follow up survey. This community accounted for most of the estimated long term reduction in handheld cell phone use in New York. In two communities (Albany and Kingston), the significant short term declines were followed by returns to pre-law use rates. The use rate for the fourth community (Spring Valley) was significantly higher than rates for the other communities in surveys both before and after the law, and the short term post-law decline in the cell phone use rate was smaller than declines in the other communities and not significant.

**Table 1** Percentage using handheld cell phones in New York and Connecticut cities before and after effective date of New York State cell phone law

	Percentage cell phone use: %(No)			Percentage point differences in use rates (95% CI)		
	Pre-law	Post-law Short term (December 2001/March 2002)	Follow up (March 2003)	Short term v pre-law observations	Follow up v short term observations	Follow up v pre-law observations
Connecticut (two cities combined)	2.9 (7110)	2.9 (14205)	3.3 (6992)	-0.1 (-0.4 to 0.5)	0.4 (-0.1 to 0.9)	0.4 (-0.2 to 1.0)
New York (four cities combined)	2.3 (11768)	1.1 (25694)	2.1 (12571)	-1.2* (-1.5 to -0.9)	1.0* (0.7 to 1.2)	-0.2 (-0.6 to 0.2)
Albany	2.2 (3537)	0.8 (8328)	2.1 (3917)	-1.5* (-2.0 to -1.0)	1.3* (0.8 to 1.8)	-0.2 (-0.8 to 0.5)
Binghamton	2.2 (3327)	0.8 (6198)	0.7 (2872)	-1.4* (-2.0 to -0.9)	-0.1 (-0.5 to 0.3)	-1.6* (-2.1 to -1.0)
Kingston	1.9 (2805)	1.1 (6329)	2.1 (2901)	-0.8* (-1.4 to -0.3)	1.0* (0.4 to 1.6)	0.2 (-0.5 to 0.9)
Spring Valley	3.0 (2099)	2.3 (4839)	3.5 (2881)	-0.7 (-1.6 to 0.1)	1.2* (0.4 to 2.0)	0.5 (-0.5 to 1.5)

\*Statistically significant ( $p < 0.05$ ) based on test of difference in proportions.

### Cell phone use rates by driver gender, age, and vehicle type

In New York, use rates by driver characteristics were calculated for the pre-law survey, the December 2001 and March 2002 surveys combined, and the March 2003 survey (table 2). Differences were judged significant if the 95% CIs of the estimated use rates did not overlap. Across all surveys in New York, cell phone use rates were similar for males and females. Use rates were higher for drivers younger than 25 than for drivers ages 25–59, but the differences were not significant. Use among drivers ages 60 and older was negligible across all surveys. With regard to vehicle type, drivers of cars had the lowest use rates, but only the difference between drivers of cars and drivers of SUVs was significant in all surveys.

In New York, the pattern in handheld cell phone use observed for all drivers—an initial significant post-law decline followed by a return to near pre-law use rates one year later—was observed for men and women, for estimated ages younger than 25 and ages 25–59, and for drivers of cars and vans (table 2).

In Connecticut, estimated handheld cell phone use rates for the driver subsets were examined with data from all surveys combined. Differences were not significant between male and female drivers or between drivers of estimated ages younger than 25 and ages 25–59; cell phone use among the

oldest drivers was very low. Cell phone use was lowest among drivers of cars, and the difference between drivers of cars and drivers of SUVs was significant.

### DISCUSSION

Initial substantial declines in drivers' use of handheld cell phones, measured soon after New York's ban took effect,<sup>18</sup> were not sustained. There was a significant decrease in use during the first few months of the law, but most of this dissipated during the subsequent year. Relative to the change in use rates in Connecticut over the same time period, the March 2003 use rate in New York relative to the pre-law rate was 21% lower than the expected use rate, but this difference was not statistically significant. The overall trend in compliance in New York occurred for both males and females, drivers younger than 60, and drivers of cars and vans.

Both the short term and longer term results of New York's ban on handheld cell phone use are consistent with experience with other highway safety laws. Studies in the United States and elsewhere have found that publicity and enforcement are critical factors in the extent of drivers' compliance with a law. When seatbelt use laws first were enacted, there were initial increases in belt use, especially when the law was accompanied by publicity. Increases occurred even in the absence of vigorous enforcement, but

**Table 2** Estimated cell phone use rates in New York and Connecticut by gender, age, and vehicle type percentage (95% CI)

	New York			
	Pre-law	Post-law December 2001/ March 2002	March 2003	Connecticut: all waves combined
Gender				
Male	2.4 (2.0 to 2.8)	1.2 (1.0 to 1.4)	2.1 (1.8 to 2.5)	2.8 (2.6 to 3.1)
Female	2.1 (1.6 to 2.6)	1.0 (0.8 to 1.2)	2.0 (1.6 to 2.4)	3.1 (2.6 to 3.4)
Estimated age (years)				
<25	3.2 (2.0 to 4.4)	1.4 (1.0 to 1.8)	2.9 (2.2 to 3.6)	3.8 (2.8 to 4.6)
25–59	2.4 (2.0 to 2.8)	1.1 (0.9 to 1.3)	2.1 (1.8 to 2.4)	3.2 (2.8 to 3.4)
60+	0.2 (0.0 to 0.4)	0.3 (0.0 to 0.6)	0.1 (0.0 to 0.2)	0.5 (0.2 to 0.8)
Vehicle type				
Car	1.7 (1.4 to 2.0)	0.7 (0.6 to 0.8)	1.6 (1.4 to 1.9)	2.6 (2.3 to 2.9)
Pickup truck	2.1 (1.1 to 3.1)	1.4 (0.9 to 1.9)	3.5 (2.0 to 5.1)	2.8 (1.8 to 3.8)
SUV	4.8 (3.3 to 6.3)	2.4 (1.8 to 3.0)	2.7 (2.0 to 3.4)	4.5 (3.3 to 5.3)
Van/minivan	3.2 (2.1 to 4.3)	1.2 (0.8 to 1.6)	3.0 (2.1 to 3.9)	3.7 (2.6 to 4.6)
Single unit truck	4.7 (1.2 to 8.2)	2.1 (0.6 to 3.6)	1.9 (0.9 to 3.0)	2.4 (0.6 to 3.4)
Total 35 minute observations	11768	25694	12571	28307
Total 10 minute traffic flow observations	949	2070	909	2348

compliance declined a few months after the law became effective.<sup>19</sup> Numerous studies have shown that vigorous, well publicized enforcement campaigns are required to achieve increased seatbelt use.<sup>20-21</sup> In 1967, the United Kingdom adopted new legislation aimed at alcohol impaired driving that at the time was controversial and as a result generated a lot of publicity. Although the law initially was effective in reducing highway deaths and injuries, these effects dissipated within a few years. According to a study of the law's effects, this occurred because drivers discovered that their actual risk of being charged and punished was negligible.<sup>22</sup>

In New York, a spate of unpaid publicity surrounded the passage and initial implementation of the cell phone ban. However, this publicity dissipated soon thereafter. There was no statewide intensive enforcement campaign targeting cell phone use violations. According to the New York State Department of Motor Vehicles, approximately 100 250 cell phone tickets were issued during the first 15 months of the law (December 2001 to February 2003). The number of cell phone citations (79 876) issued during 2002 represented about 2% of all traffic citations during this period and about 4% of all moving and seatbelt violations. Monthly citation totals increased from 2380 in December 2001 to 6226 in April 2002, and then averaged about 7800. These numbers indicate that enforcement has occurred at a steady level since the law was enacted. Although data on the incidence of news stories were not collected for this study, it seems that this enforcement has not been sufficient to attract the attention of the media. Thus it seems unlikely that the public is aware of the very real possibility of tickets for violating the law.

There were differences among the New York communities in the patterns in handheld cell phone use over time. As levels of enforcement could be related to use rates in these communities, information was obtained on the number of cell phone citations issued December 2001 to February 2003 and the number of licensed drivers in 2001 in the counties in which these areas were located. The number of citations per 1000 licensed drivers was 3.1 in Binghamton (Broome County), where a significant initial post-law decline in cell phone use was sustained in the follow up survey. The rates were 4.1 and 6.7 in Albany County and Kingston (Ulster County), respectively, where significant initial declines were not sustained. The highest citation rate, 7.9, occurred in Spring Valley (Rockland County), where a non-significant initial decline occurred, followed by a significant increase. As the post-law use rates increased, so did the citation rates, and therefore the citation rates do not appear to suggest a differential deterrent effect from enforcement *per se*. Publicity levels in these communities were not measured, and cell phone subscription levels for these communities could not be obtained. Thus, the reasons for the variation among communities are unclear.

This research did not address the effect of New York's handheld cell phone ban on motor vehicle crashes and associated injuries. The study indicates that compliance with handheld cell phone use bans will be a challenge over the long term for communities and states that enact such laws, limiting the ability of such laws to mitigate any increased crash risk associated with handheld cell phones. Without substantial and highly publicized enforcement efforts, compliance is likely to be quite low. As a number of studies have found deleterious effects on driving performance of hands-free as well as handheld devices,<sup>7-11</sup> any potential crash effects of New York's law may have been diluted if drivers substituted hands-free phones for handheld phones rather than not using phones at all. It was not possible to examine the extent to which New York drivers complied with the law by using a hands-free device, or whether some of these drivers' phone use may actually have increased after

## Key points

- Drivers' use of handheld cell phones declined substantially in the first few months after New York's ban on such use, based on observations conducted in four upstate communities.
- Most of this initial decrease dissipated during the subsequent year.
- Initial publicity about the law declined, and there was no publicized targeted enforcement campaign.
- Publicized enforcement campaigns appear necessary to achieve longer term compliance with bans on drivers' cell phone use.

the law. Such information and a better understanding of the relative crash risks associated with hands-free and handheld phones are necessary to establish the effectiveness of bans on handheld cell phone use in reducing motor vehicle crash risk.

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## APPENDIX

### Logistic regression models to compare changes in observed handheld cell phone use rates in New York relative to Connecticut

Two dummy variables were created. The dummy variable NY took the value 1 if the observation was made in New York and 0 if the observation was made in Connecticut. The dummy variable After took the value 1 if the observation occurred in the later time period and 0 if it occurred in the earlier time period.

Using these two variables, the following logistic regression model was fitted:

$$\log(O_{ij}) = b_0 + b_1(NY_i) + b_2(After_j) + b_3(NY_i)(After_j),$$

where  $O_{ij}$  is the odds of cell phone use when  $NY = i$  and  $after = j$ , where  $i$  and  $j$  can each be either 0 or 1.

For example, if we are looking at the two time periods of December 2001/March 2002 and September 2001 in New York, then the log odds for cell phone use in New York in December 2001/March 2002 is:

$$\begin{aligned}\log(O_{11}) &= b_0 + b_1(NY_1) + b_2(After_1) + b_3(NY_1)(After_1) \\ &= b_0 + b_1(1) + b_2(1) + b_3(1)(1) \\ &= b_0 + b_1 + b_2 + b_3\end{aligned}$$

The log odds for September is:

$$\begin{aligned}\log(O_{10}) &= b_0 + b_1(NY_1) + b_2(After_0) + b_3(NY_1)(After_0) \\ &= b_0 + b_1(1) + b_2(0) + b_3(1)(0) \\ &= b_0 + b_1\end{aligned}$$

For New York, the log odds ratio for the odds of cell phone use in December 2001/March 2002 and the odds in



September 2001 would be  $\log(O_{11}/O_{10})$ . For Connecticut, it would be  $\log(O_{01}/O_{00})$ .

To derive the ratio of the odds ratio for New York and Connecticut, find  $\log((O_{11}/O_{10})/(O_{01}/O_{00}))$  as follows:

$$\begin{aligned}\log((O_{11}/O_{10})/(O_{01}/O_{00})) &= \log((O_{11}O_{00})/(O_{10}O_{01})) \\ &= \log(O_{11}) + \log(O_{00}) - \log(O_{10}) - \log(O_{01}) \\ &= (b_0 + b_1 + b_2 + b_3) + (b_0) - (b_0 + b_1) - (b_0 + b_2) \\ &= b_3\end{aligned}$$

Thus,  $(O_{11}/O_{10})/(O_{01}/O_{00}) = \exp(b_3)$ . The odds ratio needed is equal to the exponentiated parameter estimate of the interaction term  $((NY_i) * (After_j))$ . The percentage change in use rates in New York relative to that in Connecticut is then estimated as  $100 * (1 - \exp(b_3))$ .

For example, for the two time periods of December 2001/ March 2002 and September 2001, the estimate for the interaction term is negative and significant ( $-0.7466$ ,  $p < 0.0001$ ), indicating that the change in use rates in New York (from 2.3% to 1.1%) was significantly greater than that in Connecticut (from 2.9% to 2.9%). The exponentiated interaction term (0.474, 95% CI 0.373 to 0.602) yields the point estimate of the odds ratio such that the relative decrease in cell phone use in New York is approximately 53% (95% CI 40% to 63%).

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