

EVALUATION OF FOUR STATE IMPAIRED DRIVING ENFORCEMENT DEMONSTRATION PROGRAMS: GEORGIA, TENNESSEE, PENNSYLVANIA AND LOUISIANA

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ABSTRACT

This study provided a consolidated evaluation of four separate demonstration projects aimed at reducing impaired driving through well-publicized enforcement. Each of the four demonstration projects used different enforcement approaches in an effort to reduce impaired driving crashes in the State. Georgia experienced a significant decrease in drinking-and-driving fatal crashes (14 percent using a ratio measure in a time series analysis). The program in Georgia (2800 checkpoints) saved an estimated 60 lives in the first year. While Louisiana experienced a raw decrease in the ratio of drinking drivers to nondrinking drivers in fatal crashes, when the control parishes and the comparison States were taken into consideration, the apparent decrease was neutralized. Although Pennsylvania's selected counties showed relative decreases across a variety of measures when compared to control counties and surrounding States, these decreases were not significant. In Tennessee, the ratio measure showed a significant decrease (-10.6 percent) compared to surrounding States with an estimated 43 lives saved in the first year. In summary, it appears that if States use a sobriety checkpoint model that includes (a) a statewide effort, (b) numerous sobriety checkpoints conducted each weekend throughout the year, (c) intensive publicity about the enforcement, and (d) properly trained law enforcement officials, significant decreases in impaired driving fatalities can be realized.

Laws dealing with driving while intoxicated (DWI) or driving under the influence (DUI), and the enforcement of these laws in the United States, serve as both general and specific deterrents to driving while impaired by alcohol. The adoption of these laws and the publicized enforcement of them tend to deter some drivers from drinking and driving in the first place. This is because they do not want to break the law or do not want to get caught breaking it

(general deterrence). When drivers are detected and arrested for DUI or DWI during enforcement efforts, this serves as a specific deterrent to some of them because they do not want it to happen again. However, it is impossible for law enforcement to detect every impaired driver on the road. As a deterrent to impaired driving, it is important for enforcement strategies to increase the perceived risk of being caught.

During the past two decades, law enforcement in the United States has used sobriety checkpoints as a strategy to enforce impaired driving laws. At sobriety checkpoints, law enforcement officers stop all vehicles, or a systematic selection of vehicles, to evaluate drivers for signs of alcohol or other drug impairment. To minimize public concern about the activity and comply with court rulings, checkpoints typically are publicized in advance, and signs are posted at the approaches to the checkpoints warning drivers that a checkpoint is ahead. Law enforcement officers in uniform approach drivers and identify themselves, describe the purpose of the stop, and ask the driver questions designed to elicit a response that will permit the officer to observe the driver's general demeanor. Drivers who do not appear impaired are immediately waved on; however, those who show signs of impairment are usually detained in a safe holding area where they are investigated further and either arrested or released.

Research has indicated that sobriety checkpoints that are well publicized, conducted frequently, and have high visibility can serve as a general deterrent to impaired driving. Studies in the early 1980s found significant decreases in alcohol-related crashes associated with sobriety checkpoint programs (Epperlein, 1985; Lacey, Stewart, Marchetti, et al., 1986; Voas, Rhodenizer, & Lynn, 1985). Later studies confirmed that frequent, highly publicized checkpoint programs substantially reduced alcohol-related crashes by 10 to 20 percent (Levy, Shea, & Asch, 1988; Levy, Asch, & Shea, 1990; Wells, Preusser, & Williams, 1992). A summary of the U.S. literature, which examined nine studies through the early 1990s, concluded that the available evidence supports the hypothesis that checkpoints reduce impaired driving (Ross, 1992). A demonstration program in Tennessee ("Checkpoint Tennessee") showed a 20 percent reduction in alcohol-related fatal crashes extending at least 21 months after conclusion of the formal program (Lacey, Jones, & Smith, 1999). A review of the latest literature on the effectiveness of sobriety checkpoints and random breath testing found that sobriety checkpoints were effective in reducing alcohol-related fatalities and injuries (Peek-Asa, 1999). The Centers for Disease Control (CDC) conducted a systematic review of the evidence regarding interventions to reduce alcohol-impaired driving (Shults, Elder, Sleet, et al., 2001). Fifteen studies on the effectiveness of sobriety checkpoints were summarized and showed a median reduction of 20

percent in fatal and injury crashes associated with sobriety checkpoint programs.

The legality of sobriety checkpoints has been challenged in U.S. courts. In 1990, the U.S. Supreme Court upheld the constitutionality of sobriety checkpoints in a case that challenged them under the Fourth Amendment to the U.S. Constitution, which protects against unreasonable searches and seizures (*Michigan v. Sitz*, 1990). The Court held that the interest in reducing the incidence of alcohol-impaired driving was sufficient to justify the brief intrusion occasioned by a properly conducted sobriety checkpoint. However, 10 States still report that sobriety checkpoints are illegal based on State law or have been ruled to violate the state constitution (Fell, Lacey and Voas, 2004).

Research examining different alcohol-impaired driving law enforcement strategies showed that the proportion of all crashes involving alcohol declined an average of 28 percent in four communities that used publicized sobriety checkpoints compared with a 17 percent decline in communities that used only publicized roving patrols (or saturation patrols). There were no differences in effectiveness for sobriety checkpoint programs with small staffing levels (3 to 5 officers per checkpoint) compared with high staffing levels (8 to 12 officers), or for checkpoints that stayed in one location versus those that moved around (Stuster & Blowers, 1995). Law enforcement and other officials have been skeptical of the cost benefit of sobriety checkpoints, but at least one study indicates that checkpoint programs can yield considerable cost savings (Miller, Galbraith, & Lawrence, 1998).

Saturation and roving patrols are additional strategies used by law enforcement to enforce impaired driving laws. These strategies essentially involve sending more officers than normal to patrol areas where alcohol-related crashes frequently occur and/or areas where there are a high number of arrests for DUI or DWI. Saturation patrols appear to be effective in reducing impaired driving if they are highly publicized (Stuster & Blowers, 1995). However, the research on this strategy is limited and is certainly not as extensive or convincing as that on sobriety checkpoints. Roving patrols are generally conducted in association with checkpoints to cut down on drivers circumventing checkpoint enforcement locations. Officers are usually dispatched to alternative routes to patrol for drinking drivers.

In an ongoing effort to reduce alcohol-related fatalities and injuries, and with this evidence at hand, NHTSA provided \$1,000,000 each for four statewide enforcement programs that target motorists who drive while impaired. These programs were initiated in 1999 in Georgia, Louisiana, Pennsylvania, and Tennessee. Although based on the same general concept of increased enforcement, coupled with a strong public information and education component, the States were encouraged to develop their own programs and procedures. Different histories of DUI enforcement, legislative climates, laws, and management structures

resulted in significant differences among the sites. All four States used earned media (such as press releases and project kickoff press conferences). Only Georgia used paid media, which was sponsored by a public/private partnership. All States conducted sobriety checkpoints to varying degrees, and most supplemented those with saturation patrols. Table 1 shows a brief summary of the key activities in each State.

Table 1 – Summary of State Program Elements

State	Program dates	Coverage & coordination	Type of media	Type of enforcement
GA	Planning 10/99 – 6/00 Implementation 7/00 – 9/01	Statewide Coordinated by the Highway safety Office	Earned media, shifted to paid media halfway through	2,837 sobriety checkpoints conducted
LA	Planning: 9/99–4/00 Implementation 7/00–4/01	Conducted in 16 parishes out of 64 in the State. Coordinated through the Louisiana State Police (LSP) and the LA Highway Safety Commission.	Earned media only Used advertising firm and public relations firm.	217 saturation patrols, then sobriety checkpoints after they became legal
PA	Planning: 9/99–6/00 Implementation 7/00–9/01, most enforcement began later in the timeframe	Targeted counties (14 of 67 counties) Coordinated by the PA DOT	Earned media using local coordinators. Community- based public education program	Sobriety Check- points (approx. 150) Reduced Staffing Check- points (RMC) (approx 150), roving patrols (480) and mobile awareness patrols (with trailer) (360)
TN	Implementation 11/00 – 1/02	Statewide Coordinated through THP	Earned media only	Sobriety checkpoints (535), enforcement roadblocks (270), roving and saturation patrols (270)

METHODS

REPORTED BEHAVIOR – Telephone surveys of a random sample of the driving population in the four States were conducted by Schulman, Ronca & Bucuvalas, Inc. (SRBI) under separate NHTSA sponsorship. The objective of the survey effort was to determine the extent to which the alcohol enforcement programs affect the awareness, attitudes, and drinking-and-driving behavior of motorists. Each survey was conducted among a random sample of 1,000 drivers. The same methodology was used in each State and in each wave of the survey. The

surveys were conducted in the jurisdictions within each State where the demonstration programs were implemented. For example, in Georgia the surveys were conducted statewide, whereas in Pennsylvania, surveys were conducted in the 14 counties participating in the demonstration program. Surveys were conducted in three waves. A baseline survey of drivers was conducted prior to the implementation of the programs. Midway through the program effort, a second survey was conducted between January 18 and February 11, 2001. The Tennessee program actually started later in 2000, so the midpoint survey was not quite midpoint. Approximately one year after the initial interviewing, a final wave of interviewing took place.

FATAL CRASH ANALYSIS – An analysis of each of the four State programs was conducted using the same basic technique (an interrupted time-series analysis of alcohol-related fatal crashes) so that test results would be comparable between evaluations (Box & Jenkins, 1976). This methodology has been used in other evaluations of alcohol safety laws (Voas, Tippetts, & Fell, 1999; 2000). For States implementing statewide programs, the analysis was on statewide data from the Fatality Analysis Reporting System (FARS) (NHTSA, 2002). For States implementing programs in selected areas of the State, the analysis focused on the targeted jurisdictions. This analysis provided a more valid basis to compare results among programs and obviated some of the methodological issues such as regression to the mean that before and after designs do not address as well. FARS data from the previous fifteen years (1987–2001) were aggregated into 90 bimonthly totals for each State. Fifteen years was used because it provides a more accurate long-term trend and reduces the variance in the crash data. The aggregation was done separately for three variables:

- Involved drivers measured or imputed to be alcohol-positive ($BAC \geq .01$).
- Involved drivers measured or imputed to be alcohol-negative ($BAC = .00$).
- Alcohol-related fatalities (fatalities in crashes where either a driver or a pedestrian or bicyclist was alcohol-positive ($BAC \geq .01$)).

The first two variables were combined into a ratio series comparing the number of drinking drivers in fatal crashes to the number of nondrinking drivers in fatal crashes. This allows us to examine how the number of alcohol-related drivers in fatal crashes changes in relation to those drivers in fatal crashes who were not drinking. Nondrinking drivers in fatal crashes are an indicator for the underlying general crash risk and changing driving exposure that fluctuates independent of alcohol involvement. The ratio allows us to normalize for exposure, that is, the potential for a fatal crash, which may fluctuate due to a host of non-alcohol-related factors such as miles driven, weather, road conditions,

changing population demographics (such as age), and safety devices in vehicles. The third variable (alcohol-related fatalities) was expressed in a ratio relative to annual vehicle miles traveled (VMT), to use a different control for exposure. This is the measure NHTSA uses to track progress in impaired driving rates. While this measure included alcohol-related pedestrians and bicyclists, these fatalities accounted for a small portion of the problem, especially when expressed as a rate per VMT.

One way to account for extraneous factors is to use the nonalcohol crashes/fatalities as a covariate or regressor within the model. Another approach is to account for the control group explicitly as part of the dependent measure, by combining the two figures into a single measure or rate, such as percentage of crashes that are alcohol-involved, or the odds-ratio of alcohol-to-sober fatalities. These are the two most commonly used arithmetic methods of control for extraneous general factors. Arithmetically, both the proportion and the odds-ratio are closely related. Both use the alcohol-counts (or treatment group) for the numerator, and both contain the sober counts (or, comparison group) in the denominator, but the proportion also adds the numerator into the denominator. It is not efficient to compare the treatment group with a control group that has been altered by adding the treatment group in there, too. At the individual case level, it is not done with other parametric analyses (except under very rare circumstances where such a nonorthogonal contrast is specified); in a simple independent sample t-test for two groups, we do not compare the treatment group versus both combined.

In terms of consumption, the size of increases/decreases of an odds-ratio are simpler to interpret (e.g., 1.4 times as likely) than are the relative percentage of a percent or percentage of a proportion that can be confusing to the nonresearch public. For each of the four States, these two bimonthly measures, alcohol-to-sober drivers and alcohol-related fatalities per VMT, were aggregated separately for three or four groups:

- Treatment (TR)—those counties within the State that participated in the intervention (in Louisiana and Pennsylvania, a portion of the counties; the entire State for Georgia and Tennessee).
- Within-State comparisons (C1)—those other counties not participating (not applicable in Georgia or Tennessee).
- Neighboring States (C2)—selected nearby comparison States, pooled.
- The rest of the Nation (RoN), pooled.

Neighboring State comparisons were selected as follows:

- Tennessee: Missouri, Arkansas, South Carolina, North Carolina, Virginia, Alabama, Kentucky, and Mississippi (all States that surround Tennessee, except Georgia, which was also one of the intervention States).

- Georgia: Mississippi, Alabama, South Carolina, Florida (all States that surround Georgia, except Tennessee, which was also one of the intervention States).
- Louisiana: Mississippi, Alabama, Arkansas (all States that surround Louisiana, except Texas, which was one of the other intervention States funded by NHTSA at a later date).
- Pennsylvania: Ohio, West Virginia, Maryland, New Jersey (all States that surround Pennsylvania, except New York, which has a unique STOP DWI enforcement program equivalent to the intervention States).

Note that by coincidence of geography, Mississippi and Alabama comprised a substantial portion of the comparison pool for three of the four States. Some of these comparison States experienced a change in alcohol-legislation status near the time of the intervention period (but generally, for lowering the illegal blood alcohol concentration (BAC) limit to .08; most had already passed administrative license revocation). The only significant alcohol law changes in those comparison States took place at the very end of the post-intervention time series in the last few months of 2001. Because the .08 BAC changes in comparison States were only in place for 2 to 3 time points, they were far enough removed from the intervention points so as not to affect the comparison States' series and were unlikely to have had much potential for statistically corrupting the comparison series. The same holds true for the .08 law change in Georgia, one of our treatment (TR) States.

Interrupted time-series analyses were performed using ARIMA intervention models, for both dependent measures (drinking drivers to nondrinking drivers' ratio and alcohol-involved fatalities per VMT) for each State (TR). Additional time series using the same intervention dates were performed for the one or two comparison series available, as applicable (C1, C2). The time series representing the RoN were included as a regressor series in each analysis, to factor out general fluctuations or trends over time that would affect the Nation generally. The series were all analyzed using a natural log transform, to normalize the series' variances (also rendering the variance independent of the level); this transformation also permits straightforward interpretation of the binary intervention dummy variable as a percentage change (after de-transforming). Thus, 20 separate time-series analyses were performed: six each for Louisiana and Pennsylvania, and four each for Tennessee and Georgia.

RESULTS

TELEPHONE SURVEY RESULTS – When asked if respondents (all) had driven within 2 hours of drinking in the last 30 days, between 15 and 26 percent of the population reported that they had.

In three of the four States, this number increased at the midpoint. This might be related to the holiday season. Only Georgia experienced a notable decrease in the proportions of persons reporting driving after drinking at midpoint and at the final wave (See Figure 1).

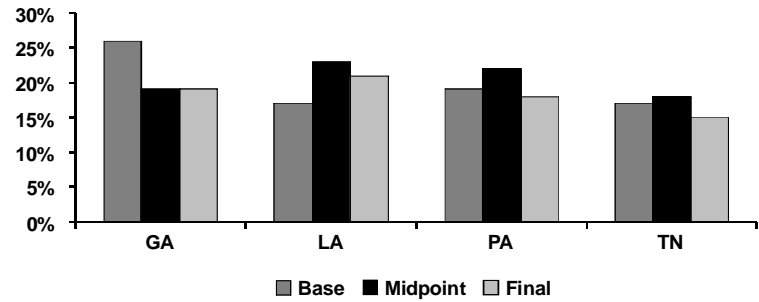


Figure 1 – Driven Within 2 Hours of Drinking (last 30 days)

Another measure is the percentage of people who reported driving when they thought they were over the legal BAC limit. Here, between 7 and 20 percent of the population admitted driving over the limit in the last 30 days. There were fairly substantial decreases in this reported behavior in Georgia and in Pennsylvania, and to a smaller degree, in Tennessee, at the project’s midpoint and at the final wave. In Louisiana, there was an increase from the beginning to midpoint, then a decrease from the midpoint to the final wave. Results may be somewhat affected by seasonal differences in drinking (See Figure 2).

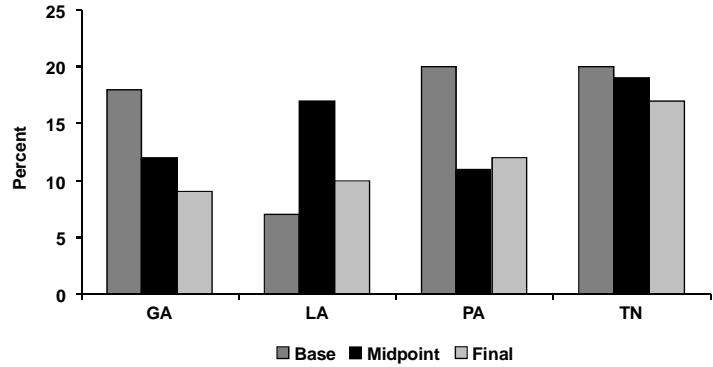


Figure 2 – Drove When Thought Were Over Legal Limit (last 30 days)

Another way to examine reported behavior changes is to see if respondents reported that they deliberately avoided driving in last 30 days because they had had too much to drink. The results here were somewhat encouraging; there was a large increase in Georgia at the midpoint, with moderate increases in Louisiana and Tennessee (Figure 3).

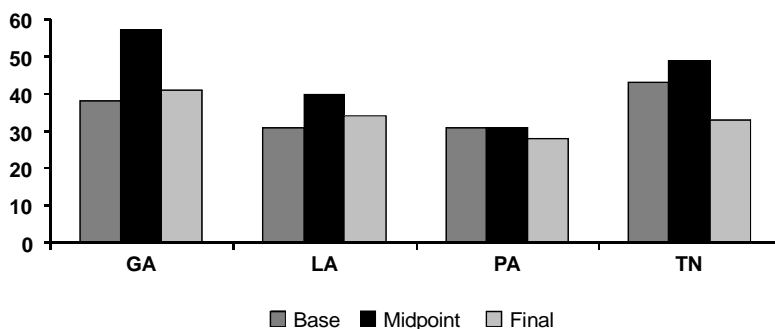


Figure 3 – Deliberately Avoided Driving Because of Too Much to Drink (last 30 days)

FARS ANALYSES – The results of these analyses are shown in the column of Table 2 labeled “change.” The results for each Treatment series and for each within-State comparison series are contrasted with the results for the comparison series for neighboring States to test whether the changes shown within these four States were significant relative to any change occurring in the nearby region. Those results, showing the net or cumulative differences, are shown in the column labeled “cuml effect” in Table 2.

Table 2 – FARS Analysis of Four States’ Alcohol Demonstration Grant Results

		Louisiana	change	prob.	se(b)	df	Cuml effect	Se (diff)	prob	df
Drivers	Ratio	intervention counties	-11.27%	.048	.0710	87	0.855%	.0860	.461	82
		within-State controls	-18.49%	.009	.0847	86	-7.349%	.0976	.218	81
		neighboring States	-12.02%	.010	.0486	85				
FATALS	VMT	intervention counties	-8.40%	.084	.0631	86	14.88%	.0844	.052	81
		neighboring States	-20.27%	.000	.0560	85				
		Pennsylvania	change	prob.	se(b)	df	Cuml effect	Se (diff)	1-tail prob	df
Drivers	Ratio	intervention counties	0.47%	.529	.0644	87	-8.595%	.0905	.162	83
		within-State controls	1.37%	.410	.0597	85	-7.773%	.0872	.178	81
		neighboring States	9.92%	.140	.0635	86				
FATALS	VMT	intervention counties	9.39%	.125	.0774	87	-1.57%	.0841	.426	82
		within-State controls	1.54%	.404	.0627	86	-8.64%	.0708	.103	81
		neighboring States	11.14%	.002	.0329	85				

		Tennessee	change	prob.	se(b)	df	Cuml effect	Se (diff)	1-tail prob	df
Drivers	Ratio	intervention counties	-18.31%	.001	.0575	83	-10.569%	.0607	.035	76
		neighboring States	-8.65%	.000	.0196	83				
Fatalis	VMT	intervention counties	-2.95%	.268	.0483	84	0.62%	.0534	.454	80
		neighboring States	-3.55%	.116	.0228	86				
		Georgia	change	prob.	se(b)	df	Cuml effect	se(diff)	1-tail prob	Df
Drivers	Ratio	intervention counties	-6.57%	.092	.0507	85	-13.97%	.0576	.005	80-5
		neighboring States	8.61%	.003	.0274	85				
Fatalis	VMT	intervention counties	-4.88%	.131	.0443	80	-4.64%	.0508	.177	70-10
		neighboring States	-0.25%	.920	.0249	80				

Numbers in **bold** indicate significant changes ($p < .05$).

Within the intervention counties, only one State (Louisiana) showed a significant raw decrease (-11.27 percent)—which was coupled with an even larger decrease in the within-State control counties (-18.49 percent). This raw decrease in Louisiana was seen only for the ratio series; the alcohol-involved fatalities and VMT series was marginal but not significant at the .05 level. However, looking at raw decreases does not tell the whole story. We must look at each State relative to other counties and neighboring States in its region because other factors such as gradual shifts in public drinking-and-driving acceptance could influence driving behavior.

In Louisiana, the comparison series for the neighboring States showed an equally large decrease, so that when the Louisiana parishes' decrease is expressed as a net change relative to the comparison group, the apparent decrease was neutralized, and no longer significant. In fact, for the alcohol-involved fatalities and VMT series, the intervention counties ended up showing a nearly significant increase relative to the comparison States.

Georgia's raw 6.6 percent decrease was not significant by itself, but because the comparison States actually increased (8.6 percent), Georgia's net decrease of 14 percent becomes significant when expressed relative to the comparison States. Again, this decrease was found for the ratio series only, and not for the alcohol-involved fatalities and VMT series, although there was still a relative 4.6 percent decrease

in that series (but it was not statistically significant). The Georgia intervention was estimated to save 60 lives per year in reductions in drinking-driver fatal crashes.¹

Tennessee’s raw decrease of 18.3 percent in the driver ratio series was reduced to –10.6 percent when the neighboring States were taken into account and was significant ($p<.035$). Conversely, Tennessee’s change of –3 percent in the alcohol-involved fatalities and VMT measure actually is neutralized relative to the comparison States’ similar decrease. Using the same assumptions that we used for Georgia, but applied to the 10.6% decrease in Tennessee, the intervention saved an estimated 43 lives per year in Tennessee. Although there were decreases in each of the four cumulative measures in Pennsylvania, none was statistically significant.

Figures 4 through 7 show the time series observed and fitted driver ratio rates (drinking drivers over nondrinking drivers in fatal crashes) for Georgia and Tennessee (the only two States with significant decreases) and their surrounding comparison States.

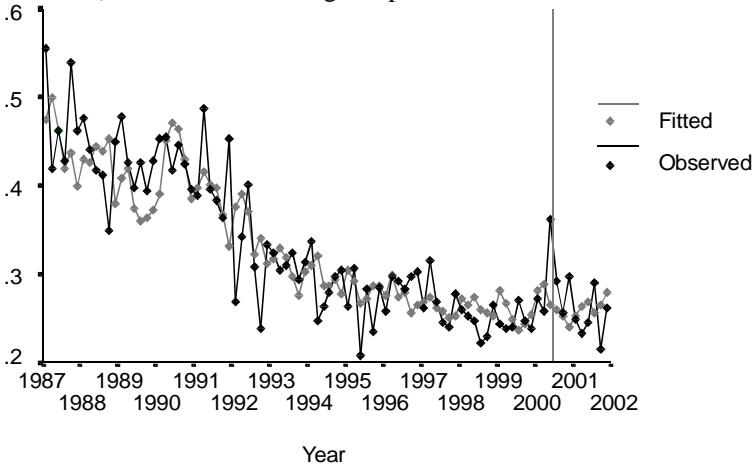


Figure 4 – Observed and Fitted Bimonthly Ratios of Drinking Drivers to Nondrinking Drivers in Fatal Crashes for Georgia

¹ This estimate assumes (a) an attribution rate of 0.877 fatalities per each alcohol-positive driver in a fatal crash (average in FARS), (b) that 75 percent of all alcohol-positive drivers are at BACs of .10+ (average in FARS), and (c) that the decrease is relative to the changes occurring in Georgia’s comparison States (AL, FL, MS, SC).

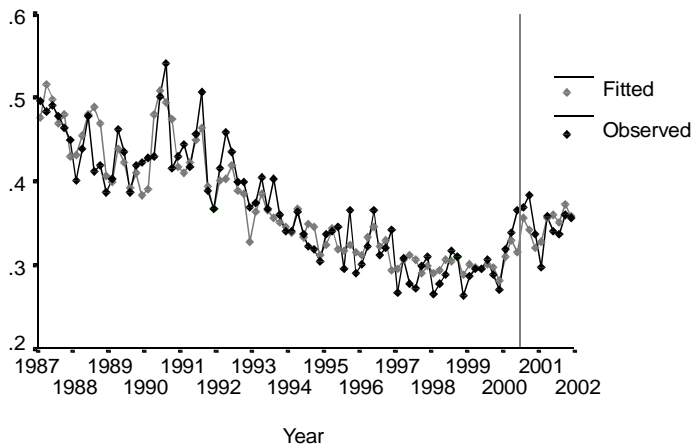


Figure 5 – Observed and Fitted Bimonthly Ratios of Drinking Drivers to Nondrinking Drivers in Fatal Crashes for Georgia Comparison States

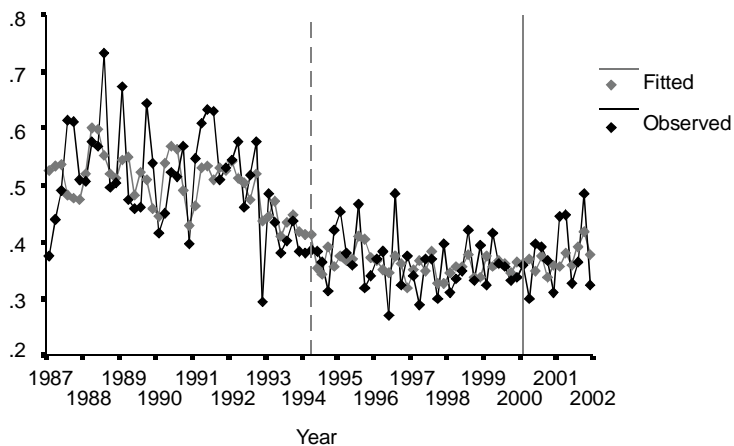


Figure 6. Observed and Fitted Bimonthly Ratios of Drinking Drivers to Nondrinking Drivers in Fatal Crashes in Tennessee

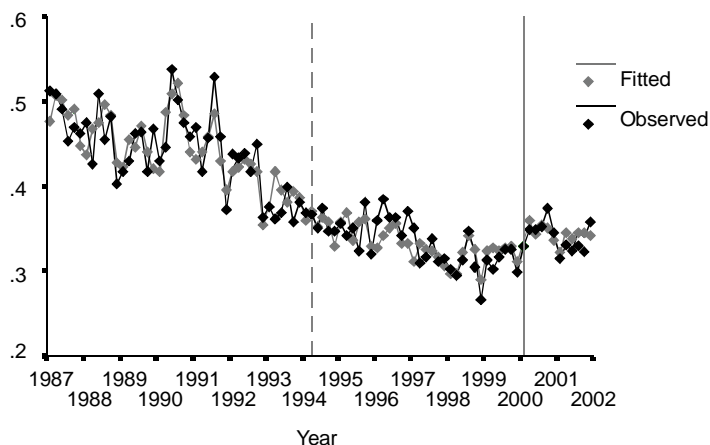


Figure 7. Observed and Fitted Bimonthly Ratios of Drinking Drivers to Nondrinking Drivers in Fatal Crashes in the Tennessee Control States

DISCUSSION

FARS ANALYSES – It appears from the FARS analyses that Georgia showed a statistically significant bottom-line decrease (14 percent in the ratio of drinking drivers to nondrinking drivers) accompanied by a nonsignificant 5 percent decrease in alcohol-related fatalities per 100 million VMT. Although Pennsylvania showed decreases in all four measures when compared to neighboring States—ratio: -8.6 percent (intervention counties); -7.8 percent (control counties); VMT: -1.6 percent (intervention); -8.6 percent (control)—none were statistically significant. Louisiana experienced one nonsignificant decrease in their four measures (-7.4 percent), and Tennessee experienced a relative significant decrease in one measure (-10.6 percent in the driver ratio; $p < .035$) and virtually no change in the other measure (+0.62 percent in alcohol-related fatalities per 100 million VMT). These findings are consistent with past research considering that Georgia followed very closely the “Checkpoint Tennessee” model of 1994–1995, with even more sobriety checkpoints per capita than Tennessee used back then and using paid advertising to increase awareness. Pennsylvania came close to experiencing an effect, but because the program was not conducted statewide, it may have hindered its effectiveness. Louisiana also only used certain parishes for their effort and was hindered by the prohibition of sobriety checkpoints until halfway through the implementation phase. Tennessee had a relatively large number of sobriety and enforcement checkpoints (~800) compared to Louisiana (a small number) and Pennsylvania (~300), but not nearly as many as Georgia (~2,800).

CONCLUSIONS

Each of the four alcohol demonstration projects used different approaches to reach the goal of reducing impaired driving crashes via highly publicized enforcement. Georgia experienced a significant decrease in drinking-and-driving fatal crashes (14 percent using the ratio measure). Louisiana was hindered in their program in that only certain parishes were selected for the increased enforcement and publicity, sobriety checkpoints were not conducted until partway through the implementation phase, and not many checkpoints were conducted. Consequently, while Louisiana experienced a raw decrease in the ratio of drinking drivers to nondrinking drivers in fatal crashes, when the control parishes and the comparison States are taken into consideration, the apparent decrease was neutralized. Although Pennsylvania showed decreases in all four measures when compared to surrounding States, these decreases were not significant. Tennessee experienced a significant decrease in one measure and no change in the other measure relative to the changes found in their surrounding States. Tennessee did not conduct nearly as many sobriety checkpoints as Georgia and did not use paid advertising. However, it is possible that their 800 checkpoints were publicized just enough to result in a 10.6 percent decrease in the ratio series.

Regarding public awareness of the programs, the telephone survey results did show some positive indications. Awareness of the programs did not appear to be high, but some reported drinking-and-driving behaviors did decrease. Past research indicates that the public tends to overestimate grossly the actual risk of arrest even in the absence of special well-publicized enforcement efforts. So it may not be reasonable to expect to raise that already high perception to an even higher level.

States that did not conduct a full statewide program (Louisiana and Pennsylvania) may have had a more difficult challenge in changing driver behavior than the ones which could provide a more consistent picture of enforcement activity (Georgia and Tennessee). Additionally, checkpoints are more uniquely identified by the public as impaired driving enforcement and are more visible than other types of DUI enforcement activity. Only Georgia indicated some consistent change in reported drinking-and-driving behavior. It appears that Georgia also did more of everything compared to the other three States: more checkpoints, more publicity, more paid advertising, and more police equipment. In totality, this may have resulted in a greater effect. In summary, it appears that States can expect significant decreases in impaired driving if they use a sobriety checkpoint model that includes the following:

- A statewide effort.
- Numerous checkpoints conducted throughout the year.

- Intensive publicity about the enforcement (especially paid advertising in Georgia's case)
- Trained and equipped law enforcement officials, as Georgia did (and to a smaller extent, Tennessee did, too).

Highly publicized sobriety checkpoints conducted weekly throughout a State appear to be one of the most effective strategies for immediately reducing impaired driving fatalities.

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