Are Interlock Devices Helping Solve New Mexico's Drunk Driving Problem?

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Executive Summary

In recent years, New Mexico has frequently been cited as a model state for reforming drunk driving laws and policies. Particular attention has been paid to a 2005 law requiring ignition interlock devices for all convicted drunk drivers. That attention has derived primarily from research and presentations by Richard Roth of Impact DWI in Santa Fe. Interlock advocates have relied on Roth's results to credit that law with meaningful decreases in repeat offenses (recidivism) and alcohol-impaired fatalities.

We consider herein whether that credit is deserved. In particular, since the interlock policy has now been in effect for over five years, we seek to answer two questions: 1) Has Roth's research conclusively shown that New Mexico's interlock law has reduced drunk driving recidivism? 2) More importantly, has New Mexico's interlock law reduced drunk driving fatalities?

Roth's most optimistic result claims that the interlock law reduced long term recidivism by up to 18 percent, but there are too many statistical errors for us to have confidence in that claim. Even Roth himself equivocates somewhat, stating in one presentation that "it is not possible to conclude that differences in re-arrest rates after interlock removal are evidence of permanent behavior change ***caused*** by the period of interlocked driving."

Moreover, Roth and his research team acknowledge that interlocks have no lasting, long-term effect on driver behavior. They write that "after the interlock was removed, there was no significant difference in the recidivism rates" and "we found that the reduction in recidivism rate achieved during installation does not continue after the device is removed from the car."

Regarding the interlock law's effect on fatalities, 2008 data (primarily from Fatality Analysis Reporting System of the National Highway Traffic Safety Administration) suggest that New Mexico's interlock law had no discernable effect in reducing fatalities. Some interlock advocates have claimed that the interlocks mandate has reduced alcohol related fatalities by 35 percent. But association is not necessarily causation. That drunk driving fatalities in New Mexico generally decreased from 2005 to 2009[1] may be partially due each to a statewide emphasis on policy modifications and media campaigns, more responsible attitudes about driving drunk nationwide or just plain luck.

[1] Even as interlock use accelerated in 2009 there was an uptick in alcohol-impaired fatalities. That tends to counteract the advocates' association of reduced fatalities with increased interlock use. Association is not necessarily causation.

This study should give pause to policymakers seeking to replicate New Mexico's interlock law. Much less is known about interlocks' effectiveness than advocates would have us believe. We should be skeptical that interlocks are a panacea until we have robust evidence that they are effective.

Overview

New Mexico's history of driving while intoxicated (DWI) is one of the worst in the nation. For the last decade the state has tried to reduce drunk driving by implementing new rules.[2] One of the recent new rules (among many) was mandatory ignition interlocks for 1st time DWI offenders.[3] Two studies by Roth, Voas, Marques (2007-1 and 2007-2) (hereinafter referred to as Roth *et al*) estimate the impact of this new policy. Roth *et al* conclude that interlocks kept over 60 percent of drunk drivers from being re-arrested while *the device was on the car*, but interlocks were much less effective after the device was removed.

During this period, drunk driving deaths in New Mexico have dropped slightly faster than the nationwide average. Mothers Against Drunk Driving (MADD) has been using the Roth *et al* studies to make the claim that interlocks get most of the credit for the drop in fatalities. Some lawmakers have hopped on the bandwagon, too, touting interlocks as the high tech solution to New Mexico's and the nation's drunk driving problem. Examples:

> Starting with one state, New Mexico, in 2005, we now have12 states with statewide requirements for interlocks at .08 BAC… Results from New Mexico and Arizona are very strong with fatality reductions of 35 percent and 33 percent respectively.[4]
>
> New Mexico was the first state to attack the epidemic of drunk driving by implementing an aggressive ignition interlock penalty program for all offenders," Udall said in a news release. "The strategy has helped take drunk drivers off the roads and save lives. I believe enacting it nationwide would have the same positive and resounding effect.[5]

This study casts doubt on those claims. Not only do MADD and these lawmakers overstate the Roth *et al* findings, but there are flaws in the Roth *et al* economic and statistical reasoning that lead us to conclude that interlocks are much less effective than they assert. In brief:

1) **Interlock effects on Drivers**: Roth *et al* employed a promising method for isolating the interlock effects on recidivism. But their conclusion that there is a significant long-term recidivism reduction (roughly 18 percent of offenders who once installed interlocks) is doubtful. They made numerous statistical errors and declined to make the data available to us, so that we were unable to review and correct the errors.

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[2] Rules go beyond written laws and policies. They include anything that influences behavior including how vigorously laws and policies are enforced, how they are adjudicated, how they affect the behavior of prospective offenders, how they affect interest groups seeking government favors and so on.

[3] Offenders is used throughout the paper to mean offenders actually convicted of DWI.

[4] MADD – Press Release of 9/20/09 regarding MADD's third year of fighting for Elimination of Drunk Driving. See for the full article on MADD's Website at http://www.madd.org/media-center/press-releases/2009/madd-campaign.html.

[5] Senator Tom Udall – Commenting on Legislation he presented in the Senate for a Nationwide Ignition Interlock Lawin the Los Alamos Monitor. 12/28/09: http://www.lcni5.com/cgi-bin/c2.cgi?075+article+News+20091227143930075075004.

First and foremost, their most optimistic long term finding (39 percent reduction in recidivism for those who once had interlocks installed) results from a regression that is technically wrong. They treat offenders who have interlocks installed like they will behave exactly the same as those who have had their interlocks removed. This technique has no justification and the findings should be rejected.

Moreover, the remainder of their empirical work suffers from other errors, making us skeptical even of those results. The "garbage-in" error (significant amount of data are not measured correctly) and the "omission of crucial information" error (information that should have been included in their regression but wasn't) introduce inaccuracies in results and thereby reduce confidence in their conclusions.

It is reasonable to conclude that requiring interlocks for all offenders may make a modest reduction in long term recidivism; but anecdotal evidence suggests it may be much less than the 18 percent reduction asserted by Roth *et al*.

2) **Interlock Effects on Innocent People Killed by Drunk Drivers**: Despite the media attention when it happens, the long-term effect of interlocks on innocent people killed by drunk drivers is small. It is in the average range of one person killed by a drunk driver every 1.2 years under Roth *et al*'s optimistic assertion to one person killed by a drunk driver every 4.5 years under our more modest assertion. This should not be surprising given the large number of drunk drivers relative to those who have had interlocks installed.

3) **Incremental Benefits versus Incremental Costs of Interlock**s: The long term incremental benefit of interlocks is the reduction in innocent people killed by drunk drivers solely due to the interlock-induced deterrence of drunk driving. As mentioned above, we estimate a range of benefits to average from about one less person killed every 1.2 years to about one less killed every 4.5 years. When we consider that most drunk drivers kill only themselves or their adult passengers who consented to getting in the car, we find the potential benefits to be quite small. New policy initiatives such as mandating increased use of interlocks entail additional costs, which should be compared to incremental benefits. It is beyond the scope of this study to try to estimate all of those additional costs. Our objective here is simply to note the kinds of costs that should be considered when evaluating policy alternatives.

What are the incremental costs associated with these estimated reductions in drunk driver caused fatalities? Are they justified based on the incremental benefits? First there are the obvious direct costs: enforcement, courts, monitoring, follow-up compliance and the like. Direct monitoring costs are estimated to be roughly $13.1 million per year alone. More difficult to assess are the indirect costs that often manifest themselves as "unintended consequences." These costs involve adjustments by interlock firms, lawyers, elected judges, bartenders, party hosts and entrepreneurial efforts to overcome interlocks to name just a few.

It is hard to improve on a deterrent that already exists, namely the drunk driver's personal risk of killing himself and his family and friends who are passengers. No wonder deterring drunk driving seems like such an intractable problem. If that deterrent is not working like we would hope, it is hard to believe that interlocks (or any action short of incarceration) will measurably improve the situation.

Introduction

As economists, we often are asked to give definitive answers about whether a public policy proposal will work or not. Examples: should we give tax breaks to high tech firms? Should we increase the gross receipts tax rate to close the budget shortfall? Do food stamps really help stamp out poverty? These questions have the property that each is asking for some degree of certainty, while being vague about the criteria by which to judge "work out or not."
Most such public policy questions cannot be simply answered, and that is so in the case of ignition interlocks. We do not argue here that ignition interlocks have no effect, or are indeed bad for society. Instead we use the economic way of thinking about interlocks policy, offering fresh insights into the problem.

Fundamentally, the economic way of thinking itself is simple: Incentives matter and tradeoffs exist. Once we attempt to isolate the incentives and tradeoffs, however, the problem becomes much more complex. We attempt to do some of that isolating below, and we think the reader will appreciate the complexity of the problem and gain a new understanding of the tradeoffs.
We begin by reviewing how economists think about deterring drunk driving (Becker, 1968).

Second, we summarize two important economic studies that estimate the effectiveness of policies designed to deter drunken driving (Benson *et al,* 1999 and Levitt *et al,* 2001). Third, we summarize and critique two interesting studies that attempt to isolate the effectiveness of New Mexico's interlock policy (Roth *et al,* 2007-1 and Roth *et al,* 2007-2). Finally, we estimate a plausible range of incremental interlock benefits based on our critique of those two studies and anecdotal evidence of crashes and arrests in 2008. Our study raises the issue of incremental costs as well, because we cannot think about tradeoffs without thinking about costs.

How Economists Think about Deterring Drunk Driving (Becker, 1968): Incentives Matter

How does someone who has been consuming alcohol decide whether or not to drive? Her decision to drive is based on perceived benefits enjoyed less perceived costs borne. Her benefits derive from convenience of saving time and forgoing the expense of calling a cab or reluctance to impose on a friend. Her costs derive from her perception of the risk of arrest, the likelihood of conviction and severity of punishment if convicted. And there is one additional perceived cost of deciding to drive drunk that is mostly independent of policy: It is the cost of a prospective crash. Note that most of this cost is internalized because it is borne by the driver (although the driver may consider, to some extent, the potential cost imposed on third parties).

Once we assess the *incentives* that affect the typical drunk driver's decision calculus, economists are in a position to assess the benefits of the mix of policies designed to deter drunk driving.
In order to round out economic analysis of the mix of policies, of course, we must also reconcile benefits with costs. What are the costs of enforcement, courts, punishment, imprisonment and the like? Optimally, the marginal benefit of each policy designed to deter drunk driving should equal its marginal cost.

Estimating the Benefits of Policies Designed to Deter Drunk Driving

Benson, Rasmussen, and Mast (1999) conducted an exhaustive empirical study estimating the effectiveness of enforcement and punishment policies across states. What are the robust results? What are the many unknowns?

The perceived likelihood of arrest and conviction seems to matter most, implying that lowering the standards for conviction would be an effective policy change in some states (such as New Mexico). The mix of other policies matter, too, at least in the aggregate. But they were unable to isolate the effectiveness of separate policies.

Benson *et al* (1999) do not assess the tradeoffs associated with each mix of enforcement and punishment policies. They emphasize only benefits.

New Mexico's recent DWI Czar Rachel O'Connor[6] independently agrees with Benson *et al,* indicating that interlocks probably accounted for only part of the effect of the new DWI policies. O'Connor noted that "a lot of things have contributed to reducing impaired driving." She characterized the 48 policy changes (legislative, program, and regulatory) as ""very complicated", remarking that "we're in the process of trying to do an evaluation, [but] NHTSA has said 'you've made so many changes at once that it is hard to sort out what effects what'." In addition to the increased use of ignition interlocks, New Mexico implemented stiffer penalties implemented over several years for DWI offenses and more DWI checkpoints combined with strong media messages about the new policies. According to O'Connor "While there is some attribution to [the reduction in fatalities] in terms of ignition with interlock, there were also a lot of different changes that impacted driving fatalities.", starting with... a huge public awareness campaign… 2005 forward, it is geared at what are the penalties in New Mexico... It is coupled with... high visibility law enforcement, and related to a lot of different things if you get convicted."

Danger of Drunk Driving

Levitt and Porter (2001) used an innovative technique to estimate the relative risk of sober versus drunken drivers. They did so by observing the proportion of two car crashes involving drunk-drunk, sober-drunk and sober-sober drivers. Then they make simplifying, plausible assumptions about driver behavior to derive the estimate using algebraic manipulation and maximum likelihood estimating technique.

Consistent with the Benson *et al* (1999) finding, Levitt and Porter conclude that the perceived likelihood of arrest has the most potential for deterring drunk driving, e.g. "a one standard-deviation increase in police per capita leads to a 3 to 6 percent-point" reduction in fatal crashes. They recommend "dedicated police patrols" and "rewards… to motorists who use cellular phones to help police identify reckless drivers."

Levitt and Porter improve policy evaluation of deterrent initiatives by citing a "value of life" study (Viscusi, 1992), enabling the analyst to focus on benefit-cost tradeoffs in dollars. We don't attempt to convert fatalities to a dollar "value of life" herein, emphasizing number of fatalities reduced instead. If the objective of the analyst is to maximize the number of lives saved for a given amount of resources, it makes no difference which way we quantify benefits. The important issue is getting the most benefit in terms of lives saved given the costs imposed by the effective mix of DWI rules.

[6] In a phone conversation with Kevin Rollins on 7/26/10. Recording available upon request.

Roth, Voas, Marques (2007-1 and 2007-2) empirical findings re effectiveness of interlocks

Roth *et al* conducted two studies (2007-1 and 2007-2) of effectiveness of interlocks in deterring drunk driving. Both studies employed Cox regressions (see explanation below in **statistical flaws** section) to estimate relative recidivism rates of convicted drunk drivers who installed interlocks to those who did not. Both studies tried to control for driver traits that affect recidivism, namely BAC, gender, and age.

The first study covered the period July 1999 through December 2002 (before the "mandatory" interlocks law) when ignition interlocks were an optional sanction for second and third DWI offenders. When the sanction was imposed offenders did not gain eligibility to drive legally, so most drivers (95%) who installed interlocks also had revoked driver's licenses. The findings of the study were:

1. Those offenders who installed interlocks were 66 percent less likely to recidivate than were the comparison group *while the interlocks were installed*.
2. Offenders reverted to the same recidivism rate as the comparison group *after the interlocks were removed*.
3. Offenders who installed interlocks were 22 percent less likely to recidivate than were the comparison group during the entire period of the study, including *while the interlocks were installed and after their removal*. We are particularly skeptical of this finding (as discussed below), so we do not include it in our assessment of benefits and costs.

The second study covered the period January 2003 through December 2005, the period when interlocks became "mandatory" for "aggravated"[7] first offenders. Recidivism rates for first offenders who installed interlocks were compared to those who did not. Unlike the interlock group in the first study, the law permitted first offenders who installed interlocks to regain their eligibility to drive their interlock-installed vehicles so most drivers who installed interlocks had their licenses reinstated with an interlock restriction. The findings of the study were:

1. Those first offenders who installed interlocks were 61 percent less likely to recidivate than were the comparison group *while their interlocks were installed*.
2. Offenders who had installed interlocks were 18 percent less likely to recidivate than were the comparison group *after their interlocks were removed*.
3. Offenders who installed interlocks were 39 percent less likely to recidivate than were the comparison group during the entire period of the study, including *while the interlocks were installed and after their removal*. Again, we are particularly skeptical of this finding (as discussed below), so do not include it in our assessment of benefits and costs.

Critique of Roth, Voas and Marquez (2007-1 and 2007-2) findings re effectiveness of interlocks

Based on their empirical work, Roth and his colleagues conclude that interlocks are quite effective in reducing recidivism. Mothers Against Drunk Driving (MADD) and influential lawmakers have picked up on their research and go even further, asserting that interlocks are effective in reducing both recidivism and fatalities.

[7] Aggravated offenders are those who have a BAC greater than 0.16.

But Roth and other researchers have yet to prove that interlocks reduce recidivism rates enough to justify their increased application to all drunk driving offenders. Despite the conclusions drawn by Roth, his colleagues and MADD, the studies performed contain significant economic and statistical flaws that reduce the validity of their findings. Roth himself (Roth 2010, slide 11) downplays the long term effectiveness of interlocks: "Unfortunately there are unmeasured correlates such as self-selection and judicial biases toward more severe sanctions for those most likely to recidivate. Therefore it is not possible to conclude that the differences in re-arrests rates after interlock removal are evidence of permanent behavior change ***caused*** by the period of interlocked driving" (emphasis in original).

Economic Flaws

How do attorneys, judges and those accused interact to decide that an interlock will be installed? The plea bargaining process itself may lead to interlock installation by those who are less apt to recidivate. In other words, there is probably some self selection bias that emerges in determining which offenders do and do not install interlocks. That self selection bias is likely[8] to build in a smaller rate of recidivism among those offenders who install interlocks compared to those who do not.

Roth *et al* (2007-1 and 2007-2) and Roth in his 2009 presentation to the Denver Interlock Institute (Roth, 2009) argue that interlocks, when actually installed, increase the differential recidivism between non-interlock and interlock groups. Assuming that their empirical estimate is somewhat close to the truth, is it correct to conclude (as Roth and his co-authors do) that the causation is only in one direction – lower recidivism in the interlock group? In other words, couldn't some of the differential be attributed to increasing recidivism in the non interlock group (with important spillover effects on other drunk drivers who have not yet been arrested)?

There is a sound basis for thinking that at least some of the differential is caused by an increase in drunk driving by others, because the interlock law reduces prospective punishment if convicted.[9] For example, offenders with interlocks can now continue to drive their car, as opposed to losing their driving rights completely as was the norm in the past. Some may view interlocks as a less severe sentence. Economists are convinced (Becker, 1968) that lower expected punishment leads to more of the activity we would like to deter (since net prospective benefits of drunk driving thereby increase).

How much might this flaw matter? Unfortunately, we don't know the magnitude of the effect on all drivers of reduced consequences for convicted drunk drivers; we only claim to know the direction of the effect: more drivers will be driving drunk. The point is that incentives matter and the researcher must be careful about drawing conclusions without thinking through the likely reactions of *all drivers* to a change in policy.

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[8] 2000 to 2002 data on recidivism showed that first time offenders are 21 percent less likely to recidivate than prior repeat offenders (source: DWI Resource Center and author's calculation). That 21 percent may be more receptive to installing interlocks since they don't intend to reoffend anyway.

[9] It may be argued that people (and particularly drunk drivers) are numerically challenged when it comes to assessing prospective likelihood and severity of punishment. We have no quarrel with such an argument over the short term. But, it seems to us, that such an argument may miss important learning adjustments to changed rules over the longer haul. Moreover, with all the media and advertising attention given drunk driving, numerically challenged drunks may actually overestimate the likelihood of a bad outcome to drinking and driving.

Statistical Flaws

Empirical tests of human interaction are difficult and particularly so in the case of isolating estimates of the effectiveness of each of the many policies designed to deter drunk driving (to wit, the Benson *et al* 1999 study discussed above). Being acutely aware of this empirical challenge, we think the method Roth and his colleagues chose to isolate the effectiveness of interlocks was innovative. Their innovation was to use Cox regressions to compare recidivism for those who installed interlocks and then had them removed to a control group that never had an interlock installed.

Cox regressions are often called survival analysis; and they are used predominantly in health care to compare the risks of two groups as, for example, in comparing the risks of a group that received medicine and a control group that did not. Interlocks are the analogue of medicine; they reduce the risk of recidivism among interlocked group in comparison to the non-interlocked group. Their Cox regressions aim to quantify how much interlocks reduce the risk of recidivism.

Unfortunately their errors are too numerous for us to have confidence in the conclusions they draw from their estimates. Correcting their errors might lead to more accurate and robust results; although it is unlikely to improve their case for increased use of interlocks. Here are the flaws that need to be corrected:

Their **first flaw** is in the regression[10] they emphasize in touting their conclusions. There is no reason to think that an offender with the interlock installed will behave just like an offender with the interlock removed.[11] But that is exactly the implied assumption when they combine the period when the interlock is installed with the period after it is removed. As a result the regression suffers from misspecification error (two variables treated as the same variable).[12] It is not possible to tease out the separate effects for the "interlock-on" offender and the "interlock-removed" offender in the same regression; and treating them as if they were the same is numerically meaningless.[13] For this reason, we ignore it in our discussion of benefits and costs below.

The **second flaw** is that all of their regressions suffer somewhat from what might be thought of as the "garbage-in" problem,[14] meaning that some of the data are not accurately measured. The first "garbage-in" error occurs because the interlock group data for the regressions are *averaged* with respect to "index dates" specifying time of installation, duration of installation and duration of removal. To wit, one-half of the interlock group in both studies had interlocks installed before the index date for including them in the regression and one-half still had interlocks installed after the index date for their removal. That means a significant proportion of those measured as being

[10] The third regression in each of their 2007 studies.

[11] Unfortunately it is not possible to put both interlock-installed and interlock removed variables into the same regression equation, since one is a perfect linear combination of the other (when interlock installed equals one, interlock removed equals zero and vice versa). Statistical programs cannot distinguish between the two variables since they contain exactly the same information, and the program will not run.

[12] The two regressions in each study in which the "interlock on" and "interlock off" periods were separate seemed to us to be on the right track.

[13] While reviewing academic work it is always good to keep in mind the possibility of "confirmation bias." Roth and his colleagues are interlock advocates; and they are unable to provide convincing empirical evidence of interlock effectiveness over the long term *unless they employ this flawed specification*. Of course, we, too, have a well known confirmation bias: We tend always to be skeptical about the putative benefits of new government programs.

[14] This is known as the "errors in variables" problem in econometrics.

interlocked or interlocked-removed were actually in the other group. Measurement errors like this result in inaccurate estimates. In the case of the estimated 66 percent reduction in recidivism for interlocked offenders, for example, we know that the 66 percent estimate is likely to be off the mark compared to an estimate for which the data did not suffer from the "garbage-in" problem. We do not, however, know how far off or in what direction the inaccuracy manifests itself.

A second "garbage-in problem" arises because they treat age and BAC as bifurcated variables. Doing so loses necessary information. Why not be accurate and use the specific age and the specific BAC? There is no reason known to us that the additional specificity for those two variables would not satisfy the assumptions needed for Cox regressions.

The **third flaw** is that they omitted crucial information[15] regarding the locale of offenders. Roth *et al* did not account for important differences in locale for offenders in each group. Specifically, there is wide variation in recidivism in locales across New Mexico, ranging from a 19.2 percent re-arrest rate to a 9.3 percent re-arrest rate.[16] This error could be corrected by including a locale variable for each offender as measured by the overall recidivism rate observed in the recent past for that locale. Roth was apparently unaware of locale data and indicated that he thought its inclusion would be a good modification in the future.[17] Like the "garbage-in" problem, the problem of "omitted crucial information" problem results in inaccurate estimates.

The "garbage-in" problems with the regressions, as discussed above, introduce a **fourth flaw**. Because of those problems their Cox regressions do not satisfy a critical assumption necessary to make them valid. That assumption is known as the "proportionality assumption." In the context of the Roth *et al* regressions it must be assumed that the levels of BAC, gender, age, and presence of interlocks produce proportionate changes in the relative risk of recidivism of the two groups *independent of time*. This seems implausible because of the way they mis-measured some of the data. First, as discussed above, some data points are known to be assigned to the wrong group. Second, (also as discussed above) there is little effort to accurately track similar cohorts[18] in the interlock and comparison groups with respect to time.

With literature that's garnered as much attention as the work of Roth *et al*, it's only appropriate that the researchers subject their empirical work to peer review. Their data set was funded and work encouraged by the New Mexico Department of Transportation. We have requested that the raw data be made available to us[19] for the purposes of replicating and/or improving the econometric tests; and we would like to see the data placed in the public domain.

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[15] This is known as "omitted variable bias" in econometrics. It often occurs because the researcher does not have access to the information. Roth *el al* 2007-2 explicitly mention this problem on page 352.

[16] Source: DWI Resource Center for those arrested for DWI the first time in years 2000 to 2002.

[17] Personal communication of 1/4/11.

[18] Roth *et al* (2007-1) gave a bit more attention to this problem in their first study by assigning cohorts in the comparison group the same proportion of traits for offenders with an interlocks. However, they were unable to do so by matching each individual offender with comparison group cohorts during the exact periods when that offender's interlock was installed and after it was removed. We would like to see them redo by correcting this with a few lines of code (while correcting the other flaws exposited above).

[19] Email exchange between Kevin Rollins and Richard Roth of June 17, 2010. Roth writes, "I am reluctant to just give away my data base because of all the work that has gone into it and that I would not be able to refuse other requests. Perhaps you can make a list of your questions and queries that you would like for me to run and then come look over my shoulder while we try to get answers to your questions." The data that have been provided to the Roth

Benefits and Costs of New Mexico's Existing Interlocks Law

What are the benefits and costs of New Mexico's existing interlock law? We answer the question by comparing pre-interlock law to existing interlock law, *assuming no other policy changes in enforcement, punishment and the like*. As noted above, lots of rules modifications have actually been made; but we can still think of interlocks rules in isolation from other rules. In fact, we have enough anecdotal data to make order-of-magnitude estimates of the range of benefits and costs. Additional deterrence of drunk drivers results in fewer fatalities. Fewer fatalities embody the benefit. Therefore we need some idea of the likelihood that a typical drunk driver will cause a fatal crash. Armed with that information, we can see how deterring more drunk drivers reduces drunk driver caused fatalities. Assessing that likelihood is step one in estimating the number of fatalities reduced.

Step one: Estimate the range of likelihoods of a typical drunk driver causing a fatality

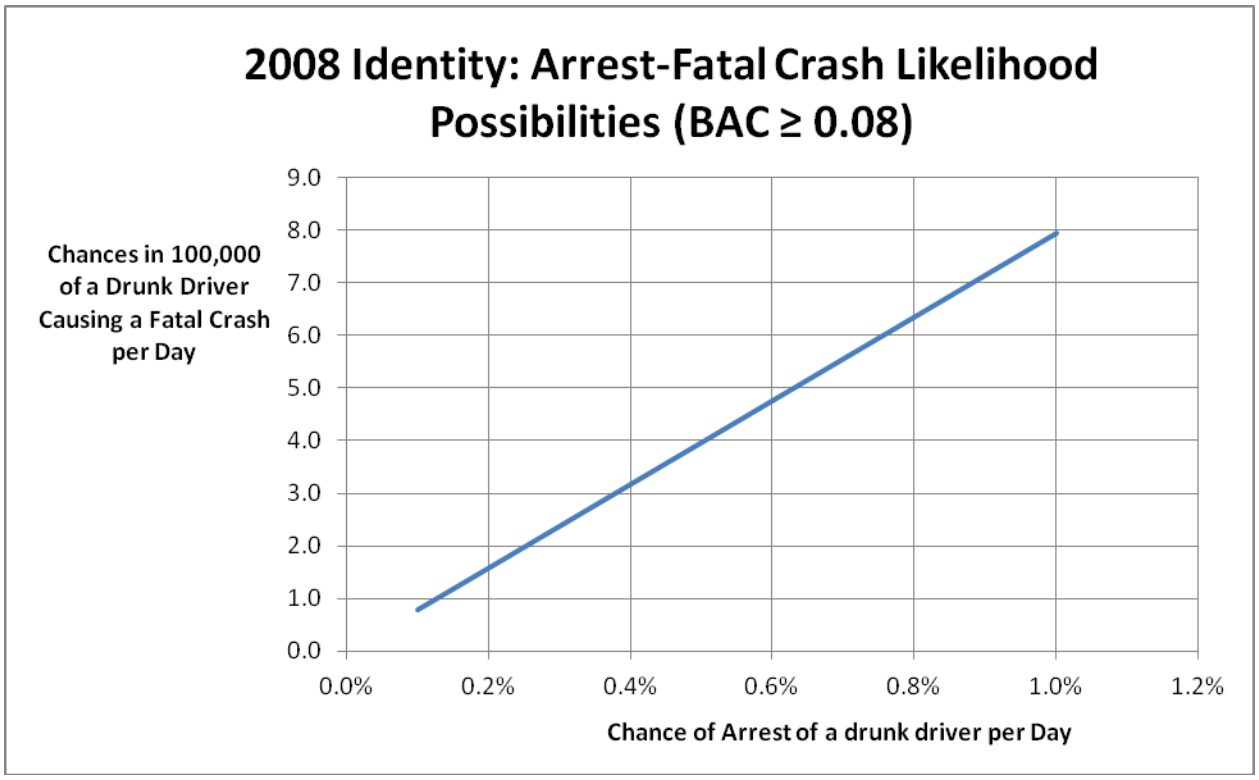
What is the daily[20] likelihood of fatality caused by the typical drunk driver? We need an estimate, or more practically a range of estimates, in answer to this question. We derive a plausible range of estimates using 2008 data for arrested drunk drivers and drunk driver caused fatalities, all with BAC $\geq$ 0.08. It turns out that the likelihood of a drunk driver caused fatality is equal to the probability of arrest times the ratio of observed fatalities to observed arrests (see Appendix A for the algebraic justification for this claim). The ratio of observed fatalities to observed arrests for 2008 is 0.0065. The table and graph below present the range of plausible likelihoods of drunk driver caused fatalities for each of the range of plausible probabilities of arrest.

Table 1: Plausible values of probability of arrest and drunk driver caused fatalities in 2008

Probability of Arrest	**0.1%**	0.2%	0.3%	0.4%	**0.5%**	0.6%	0.7%	0.8%	0.9%	**1.0%**
Chances in 100,000 of Causing a Fatal Crash	**0.7**	1.3	2.0	2.6	**3.3**	3.9	4.6	5.2	5.9	**6.5**

team should be accessible to other researchers. We recommend moving some of the funding from the DWI czar's budget to UNM for maintenance and serving of this dataset.

[20] We use "daily" as our unit of time for our macro assessment. If we had the data, it would be better to reduce the unit of time, the geographic area (perhaps by county rather than statewide) and so forth.



Step two: Estimate the range of reduced drunk-driver-caused fatalities based on number of drunk drivers deterred by interlocks

Step two consists of two parts, each with a different assumption.

1. The first is based on Roth *et al's* problematic long term estimate[21] of the proportion of drunk drivers deterred, namely 18 percent (Roth *et al*, 2007-2, table VI, p. 350) who don't recidivate due to interlocks. This assumption provides us with an optimistic view of fatalities reduced by interlocks.
2. The second is based on what we think a more modest proportion of formerly interlocked offenders deterred might be. Because of the economic and statistical flaws discussed above, Roth *et al*'s estimate of an 18 percent reduction in drunk drivers deterred is like a guess. We certainly expect that interlocks may have some deterrent effect over the long term, but we would be surprised if it is 18 percent or higher.[22] Moreover, the long term effect of interlocks goes well beyond the 2 to 3 year study periods that estimate their effectiveness; and that makes us even more skeptical about the 18 percent estimate. Over longer periods offenders are likely to find ways around interlocks, just like market participants find ways around taxes and regulations over time. Couple that with the likely self selection bias discussed above and we are led to use a competing intuitive guess of 5

[21] Again, we don't use conclusions from their 3rd regression because it is invalid, as explained in first flaw discussion above.

[22] If they would like to prove that their estimate is more realistic than what we think, then they should redo their empirical work by correcting the flaws we enumerate above (or they can provide us with the data we asked for and we will redo it for them).

percent reduction in recidivism as a more modest long term effect for formerly interlocked offenders.[23] While we make no claims as to its precision, 5 percent assumption provides us with a contrasting view of fatalities reduced by interlocks.

***Step three: Estimate the range of benefits (reduction in fatalities caused by drunk drivers) using two perspectives, the* "personal responsibility perspective" *and the* "paternalistic perspective."**

We lay out two perspectives in our attempt at quantifying the benefits. The first perspective may be referred to as the "**personal responsibility perspective**." This is our preferred perspective, since it is our observation that more desirable outcomes generally emerge whenever individuals are responsible for their own actions .[24] Moreover, since drunk drivers are disproportionally killed in crashes,[25] it is *for the most part they who have to bear the costs of their irresponsible behavior, personally and individually.* No wonder deterring drunk driving seems like such an intractable problem.

The second perspective may be referred to as the "**paternalistic perspective;**" it views government as being able to coerce individuals into modifying their behavior in the way those holding the perspective see fit. The difference between these two perspectives will become clear by reference to table 2, which summarizes the relevant fatality statistics for 2008.

The personal responsibility perspective focuses on the 3rd column – *harm done to others by the drunk driver*. In 2008 that harm consisted of 8 fatalities plus collateral damage as indicated. The paternalistic perspective focuses on all 3 columns, adding in fatalities of the often drunk passengers of drunk drivers as well as the drunk drivers themselves.[26]

[23] Roth's most recent graphs of recidivism for 1st , 2nd , and 3rd convictions (Roth 2010, slide 8) from one or two years after interlock installation until five years after interlock installation indicate reduced recidivism on the order of 7 to 7.5 percent (author's calculation). The graph for offenders with 4 or more convictions from 2 to 5 years after interlock installation (interlocks are generally on for more than one year) actually shows an *11 percent increase* in recidivism, although the number of those offenders is less than one percent of total offenders.

[24] See, for example, James Gwartney, Joshua Hall and Robert Lawson plus other contributors, *Economic Freedom of the World 2010 Annual Report,* Fraser Institute, http://www.freetheworld.com/index.htm .

[25] National Highway Traffic Safety Administration, Fatality Analysis Reporting System, 2008 data and author's calculation. See Table 2.

[26] We treated some drivers as drunk even though their BAC test result was refused, *as long as the vehicle contained drunk passengers.*

Table 2: 2008 New Mexico Fatalities		
Drivers with BAC ≥ 0.08 Killed	Passengers of Drunk Drivers Killed	Pedestrians or People in Other Cars Killed by Drunk Drivers
59	21	8[27]
		Plus 8 severe injuries plus property damage

Benefit Scenarios Based on the Three Steps

Table 3 presents the range of possible deterrent benefits of interlocks based on the foregoing three steps: The second row of table 3 presents three alternative estimates of the probability of a drunk driver caused fatality.

Assume that Roth and colleagues' most optimistic estimate is accurate, namely that the one-time installation of an interlock, including the period after its removal, produces long term reduction in drunk driving by 18 percent within that group. We assume that roughly 4,000 offenders per day have or once had an interlock installed (out of the 13,200 drunks in 2008 as basis for analysis) are affected.[28] Since 18 percent of them are now deterred from driving drunk, the daily number of drunk drivers is reduced by 720. If we also assume that these 720 deterred drunk drivers are as dangerous as the typical drunk driver, we calculate that the ***range of possible reductions in the yearly number of fatal crashes*** due to the interlock policy is as shown in the bottom two rows of table 3.

[27] Includes two minor passengers of drunk drivers also included in the second column.

[28] The medium probability of fatality in table 3 gives us a little over 7200 drunk drivers per day on average. If 50 percent of them once had interlocks installed, we get 3600 formerly interlocked drunk drivers per day. Rounding up to the nearest 1,000 we get the estimate of 4000.

Table 3: Interlock Benefits Based on 2008 as Representative Year (Roth *el al* reduction of 18 percent recidivism among drivers who are or have been interlocked)			
	Small probability of fatality	Medium probability of fatality	MADD estimate of probability
Probability of Fatal Accident[29] (chances per 100,000)	0.7	3.3	6.5[30]
Fatalities Reduced per year - Personal Responsibility Perspective	0.16	0.80	1.59
Fatalities Reduced per year- Paternalistic Perspective	1.71	8.56	17.12

Note the small effect on external fatalities per year for all three probabilities of a drunk driver caused fatality. The cell for "medium probability of fatality" and "fatalities reduced per year – personal responsibility perspective" reduces external fatalities by 0.80 per year on average. Only when we count primarily for the drunk drivers who kill themselves and their passengers do we get estimated lives saved by interlocks that are significantly greater than one per year (8.56 per year on average). In that case all drunk-driver caused fatalities[31] (paternalistic perspective) are reduced by 10 percent, a far cry from MADD's assertion of 35 percent.

Contrast table 3 with table 4 where we enter with the more modest estimate of a five percent long-term reduction in recidivism by those who have or have had interlocks installed. Except for that change, all other entries in the cells of table 4 have the same logic as for table 3.

[29] Recall that these alternative probabilities are each related to a probability of arrest from table 1.

[30] This comes from Zador, *et al,*1997. They estimate two probabilities of arrest: 1/88 and 1/772. For some reason the 1 in 88 (which has morphed into 1 in 87 and is nine times greater that their other estimate) has become accepted as fact in media reports on probability of arrest. This is despite many caveats the authors apply to both estimates. It is included here for information only. We think the probability and associated fatality reductions are far too high.

[31] Roth (2010, note for slide 11) himself mentions that reduced recidivism does not necessarily translate to reduced fatalities: "Unfortunately there are unmeasured correlates such as self-selection and judicial biases toward more severe sanctions for those most likely to recidivate. Therefore it is not possible to conclude that the differences in re-arrests rates after interlock removal are evidence of permanent behavior change ***caused*** by the period of interlocked driving."

Table 4: Interlock Benefits Based on 2008 as Representative Year (Our assumed reduction of five percent recidivism among drivers who are or have been interlocked)			
	Small probability of fatality	Medium probability of fatality	MADD estimate of probabiliy
Probability of Fatal Accident (chances per 100,000)	0.7	3.3	6.5
Fatalities Reduced per year - Personal Responsibility Perspective	0.04	0.22	0.44
Fatalities Reduced per year- Paternalistic Perspective	0.48	2.38	4.75

Note that this more modest reduction in recidivism translates to roughly1/5 person killed by a drunk driver per year. In other words, on average one less person is killed every four and one-half years. Note also that this reduction in recidivism translates to a 2.8 percent per year reduction in all drunk-driver caused fatalities (paternalistic perspective) on average, an even farther cry from MADD's 35 percent assertion.

Costs:

New policy initiatives such as interlocks entail incremental costs. What are the incremental costs associated with these estimated reductions in drunk driver caused fatalities? Are they justified based on the incremental benefits? It is beyond the scope of this study to try to estimate those costs. Our objective here is simply to note the kinds of incremental costs that should be considered when evaluating policy alternatives.

1. There are the obvious direct costs to the government: enforcement, courts, monitoring, compliance follow up and the like. Here is one concrete example of a direct cost: For the interlocks mandate to actually motivate compliance to ensure that the law is being followed, the interlock must not only be installed, but checked at regular intervals to see if it is working properly. The American Probation and Parole Association estimates conservatively that it would cost at least $1,095 per year per interlocked offender[32], a cost that motivated APPA to repudiate the idea of an unfunded federal interlock mandate for all states. If 12,000 offenders are interlocked per year in New Mexico (Roth, 2010), then the yearly cost of checking interlocks is nearly $13.1 million.
2. What kind of secondary costs, often referred to as "unintended consequences," may arise from mandatory long term interlocks? Those who have interlocks installed will discover clever ways to avoid them. Potential drunk drivers may discover that the consequences for drunk driving conviction have been reduced (no jail or license revocation), thereby increasing the likelihood that they will decide to drive while drunk. Interlocks allow offenders to go on with their normal routine. The government even provides financial assistance to interlocked offenders who qualify as being poor. Will bartenders be as watchful about one more drink, knowing that a patron has an interlock installed? How

[32] Letter from American Probation and Parole Association to congressional representatives dated July 20, 2009. $3 per interlocked offender per day times 365 days per year equals $1,095 per year.

about the police, will they be as diligent knowing that the high tech police device is replacing them? How about lobbying by interlock firms – will they deflect attention from more efficient ways to reduce fatalities? And how will this affect the defense bar – will the alternative of interlocks make it even more difficult for offenders to suffer any real consequences for their behavior. Don't forget that in New Mexico judges are elected, and that raises the question: What favor-seeking interest groups will seek to influence those judges?

Conclusion and recommendations

In this paper, we employed economic reasoning to offer new insights into ignition interlock policy. We were most interested in learning 1) whether interlocks reduce drunk driving recidivism 2) whether interlocks reduce drunk driving fatalities.

Upon reviewing the two Roth *et al* studies, we find that the claim that interlocks result in a 60-65 percent reduction in repeat offenses is highly inflated and misunderstood in the public policy arena. The studies contain significant economic and statistical flaws that reduce the validity of the findings. The claim that interlocks result in a 60-65 percent reduction in repeat offenses refers to when interlocks are installed on the car – typically only a 6-month period. Roth *et al* admit that "after the interlock was removed, there was no significant difference in the recidivism rates" and "we found that the reduction in recidivism rate achieved during installation does not continue after the device is removed from the car." Study author Paul Marques has elaborated, "The alcohol interlock was never designed to address the offender's drinking issue, and this is clearly evidenced by research that demonstrates that once an alcohol interlock device is removed from an offender's vehicle, the offender may continue to drink and drive – i.e., the drinking issue remains."[33]

Roth *et al* also refer to an 18 percent recidivism reduction once the interlock is removed. While we don't have the data to make competing empirical estimates, observed recidivism reality suggests that the reduction is much less than 18 percent over the long term. For example, Roth's post interlock empirical estimate is that recidivism was reduced by less than four percent (Roth *et al*, 2007-1, table II, page 23). Even where he shows anecdotal evidence of reduced recidivism on the order of 15 percent for post interlocked first offenders (Roth *et al*, 2007-2, figure 4, page 350), the process by which interlocks are assigned may be subject to self-selection bias. In other words, those who get interlocks may be less inclined to recidivate in the first place.

This brings us to the more important question: Do the benefits (in terms of reduced fatalities) justify the costs of interlocks?

Mothers Against Drunk Driving (MADD) says that requiring interlocks for all offenders results in a 35 percent reduction in drunk driving fatalities, but this claim is unsubstantiated. New Mexico's DWI Czar outlined 48 "very complicated" drunk driving policy changes (legislative, program, and regulatory) implemented since 2005. She noted that "a lot of things have contributed to reducing impaired driving" and indicated that interlocks probably accounted for only part of the effect of the new DWI policies. New Mexico did see fatalities decline, but DWI fatalities went up in 2009 even as interlock use was accelerating.

[33] http://tirf.ca/publications/PDF_publications/9th_Annual_Interlock_Symposium_final.pdf

Roth *et al* acknowledge this limitation of interlocks, saying "it is not clear whether the reduction in DWI recidivism is associated with a reduction in alcohol-related crashes." We found neither research nor statistics showing that interlocks contribute to a significant decrease in fatalities. Based on our analysis, our best prediction of the long term interlock induced reduction in fatalities is on the order of one fatality every four and one-half years.

Given these unproven/small reductions in recidivism and fatalities, we asked whether the benefits of such a policy outweigh the costs. Some direct costs to government of the interlock mandate may be reasonably estimated. Monitoring costs are a major component of direct costs. In New Mexico they amount to $13.1 million per year based on the number of interlocked offenders in 2010. Nationally, such a mandate is estimated to cost over $432 million annually. And that does not consider other direct costs and indirect costs (the unintended consequences).

Based on the Roth *et al* studies and our research, we cannot yet recommend that states adopt laws mandating interlocks for all offenders. New Mexico's interlock law has not been linked to a reduction in fatalities. New policies, especially those that involve high costs, should aim to substantially reduce fatalities.

Appendix A: Algebraic proof that the likelihood of a drunk driver caused fatality is equal to the probability of arrest times the ratio of observed fatalities to observed arrests with application to 2008 observations.

Let A be the number of drunk driver arrests per day. We can observe A. A = 36.2 arrests per day in 2008.
Let α be the probability of arrest per day per typical drunk driver per day. We cannot observe α.
If D is the number of drunk drivers (BAC ≥ 0.08) per day, then
Equation 1: $D = A/\alpha$.
We cannot observe D, but we can calculate it if we know α.
Let F be the number of drunk driver (BAC ≥ 0.08) fatalities per day. We can observe F.
F = 0.288 drunk driver fatalities per day in 2008.
Let π be the probability of fatality for a drunk driver per day. We cannot observe π.
Notice that
Equation 2: $F = D\pi = A\pi\alpha/\alpha$ and therefore $\pi/\alpha = F/A$.
The ratio of the number of fatalities per day to the number of arrests per day is equal to the ratio of the probability of arrest per drunk driver per day to the probability of fatality per drunk driver per day. We can observe F/A, so we have a good estimate of the *ratio* of probabilities π/α.
Numerical Example using 2008 data for BAC ≥ 0.08:
F = number of fatalities (BAC ≥ 0.08) per day = 0.288.
A = number of drunk driver arrests (BAC ≥ 0.08) per day = 36.2.
α = 0.002 is our crude estimate of the probability of arrest (BAC ≥ 0.08) taken from various studies, Roth (2009) assertion in his PPT presentation (one chance in 500). There are no truly robust estimates that we could find; they range from one in 200 to one in 2,000.
Solving Equation 2) for the probability of fatality for a drunk driver (BAC ≥ 0.08) per day: π = 0.000016 or 1.6 chances per 100,000 drunk (BAC > 0.08) drivers. From the Levitt and Porter (2002) study the probability of fatality for a sober driver would then be roughly 0.11 chances in one hundred thousand (1/15 that of a drunk driver).

Solving Equation 1for the number of legally drunk drivers per day in 2008: D = 18,088. Also, the assertion that 10 percent of licensed drivers have at least one DWI (by Steven Flint in his conversation with Kevin) seems consistent with our estimate that 1.3% of licensed drivers drive drunk (BAC > 0.08) daily, since many drunk drivers are habitual as evidenced by approximately 44 percent of DWI arrestees have had one or more[34] prior arrest.
In doing a benefit-cost study we test the sensitivity of our benefit and cost estimates to our somewhat crude estimates of α and *thereby* π by a simple equation, using 2008 data:
Equation 3: $\pi = \alpha F/A = 0.0065\,\alpha$

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[34] Estimate from period 2003 to 2005 DWI arrests of drivers with history going back as far as July 1984.

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