CRIMINAL DETERRENCE: EVIDENCE FROM AN INDIVIDUAL-LEVEL ANALYSIS OF 24/7 SOBRIETY

Beau Kilmer and Greg Midgette

Abstract

Decisionmakers continue to search for new ways to deter criminal behavior that do not rely on increasing the severity of punishment. This paper evaluates South Dakota's 24/7 Sobriety Program—a novel, large-scale intervention requiring those arrested for or convicted of an alcohol-related offense to abstain from alcohol and submit to alcohol tests multiple times daily. Those testing positive or missing a test receive a swift,certain, and moderate sanction; typically, a night or two in jail. To estimate the causal effect of the 24/7 program on the probability of rearrest or probation revocation for those arrested for a second or third driving under the influence (DUI) offense, we instrument an individual's 24/7 participation with program availability in the county of arrest. We estimate that the individual-level probability of rearrest or probation revocation is 13.7 percentage points (49 percent; $p = 0.002$) lower for 24/7 participants than non-participants 12 months after their DUI arrest. We detect substantive decreases at 24 and 36 months, but the precision of those estimates depends on model specification. These findings provide empricial support for applying "swift-certain-fair" sanctions to deter noncompliance in community supervision settings. This paper also provides policymakers with evidence for a new approach to reduce criminal activity among those whose alcohol use leads them to repeatedly threaten public health and safety. © *2020 by the Association for Public Policy Analysis and Management*

INTRODUCTION

The ability to deter rule violations is based on a combination of the certainty, celerity, and severity of the sanction for noncompliance (Beccaria, 1764; Bentham, 1789). With several studies finding that many individuals exhibit extraordinarily high discount rates (e.g., Ainslie & Haslam 1992; McClure et al. 2004; McClure & Bickel 2014), it is not surprising that recent reviews of the criminal deterrence literature minimize the importance of sanction severity (Chalfin & McCrary, 2017; National Research Council, 2014). However, there are practical and bureaucratic barriers to observing and quickly adjudicating every rule violation in most real-world settings. Indeed, the vast majority of offenses are neither detected by nor reported to authorities (e.g., Beitel, Sharp, & Glauz, 2000; Truman & Langton, 2015). This presents challenges for those tasked with supervising the millions of individuals subject to community corrections (Kaebel et al., 2016).

Journal of Policy Analysis and Management, Vol. 39, No. 3, 801–834 (2020)

Published by Wiley Periodicals, Inc. View this article online at wileyonlinelibrary.com/journal/pam
DOI:10.1002/pam.22217

These challenges are especially salient when it comes to addressing those who have been arrested and convicted for driving under the influence (DUI) of alcohol or other drugs. DUI is a common crime in the U.S.: There were approximately one million DUI arrests in 2018 (FBI, 2019) and the vast majority of the DUI events are never detected by law enforcement (Royal, 2003). Of special concern are those referred to as "hard core drinking drivers" (see e.g., NTSB, 2000; Simpson et al., 2004) who have previously been arrested for DUI, often with high levels of blood alcohol concentration, and likely meet clinical criteria for an alcohol use disorder. When describing the importance of focusing on this group, Simpson et al. (2004) argue, "Their persistent behavior brings public attention to the failures of the drinking driver control system, they continue their behavior despite threats and often despite repeated arrests and convictions, and they represent a substantial portion of drinking driver arrests and crashes" (p. 261).

In this paper, we evaluate a large-scale effort to reduce alcohol consumption and DUI which prioritizes the certainty and celerity of sanction for an offense while keeping severity low: South Dakota's 24/7 Sobriety Program (hereinafter, 24/7). Beginning in 2005, South Dakota's 24/7 pilot mandated as a condition of bond that those rearrested for DUI must abstain from alcohol and blow into a breathalyzer once in the morning and once at night every day. Those testing positive for alcohol were immediately jailed, typically for a day or two. The program expanded geographically as anecdotes about its success spread beyond the pilot counties and state legislation provided funding to interested counties in 2007. The program also expanded with respect to eligible offenses and alcohol detection technologies. From 2005 through February 2017, more than 30,000 unique South Dakotans participated in 24/7, accumulating more than five million days without a detected alcohol violation, missed test, or tampering event.

Our analysis is based on 16,513 individuals who were arrested for a second or third drunk driving offense in South Dakota from 2004 to 2011. We obtained the complete criminal history information for these individuals (including probation revocations) and determined whether they participated in 24/7 based on the program's administrative records. Since judges and other criminal justice officials in South Dakota had discretion about who could participate in 24/7, there are concerns about selection bias when comparing participants and non-participants; however, the direction of the bias is unclear. On one hand, some judges may have assigned people with more severe alcohol problems to 24/7, which could make it harder for the program to demonstrate an ability to reduce crime. On the other hand, some judges may have assigned individuals with less severe problems who would be more likely to successfully complete 24/7 and less likely to engage in future criminal activity regardless of participation.

To estimate the causal effect of 24/7 on the probability of being arrested or having probation revoked, we used the temporal variation in program availability in a county as an instrument for individual participation. The results show that 24/7 participation had a large effect on criminal behavior: We estimate that the probability a 24/7 participant was rearrested or had probation revoked 12 months after being arrested for DUI was 13.7 percentage points (49 percent; $p = 0.002$) lower than that of non-participants. We also detected reductions at 24 and 36 months—13.8 percentage points (35 percent; $p = 0.013$) and 11.7 percentage points (26 percent; $p = 0.009$), respectively. The 12-month findings are robust to a number of alternative assumptions and specifications. We also detect substantive effects at 24 and 36 months in the sensitivity analyses, though the precision depends on the model specification. In sum, these results provide evidence that it is possible to create a credible and effective deterrent threat on a large scale without increasing sanction severity.

BACKGROUND

In this section, we begin with a short summary of the research linking alcohol consumption and crime. We then highlight the research on public policies intended to reduce alcohol-involved crime, with a special focus on efforts intended to reduce alcohol-impaired driving. In the final section, we describe the inception and growth of 24/7 Sobriety in South Dakota and review the existing research on the program.

Alcohol and Crime

Some crimes are alcohol-involved by definition (e.g., public drunkenness, DUI). Other offenses are linked to alcohol because there is a belief by law enforcement officers, victims, and sometimes the alleged perpetrators that the crime was caused or intensified by alcohol consumption. The clinical literature overwhelmingly finds that alcohol intoxication impairs cognitive functioning, especially with respect to decisionmaking, problem solving, and risky behavior (e.g., Moselhy, Georgiou, & Kahn, 2001; Peterson et al., 1990). Furthermore, there is strong experimental evidence that acute alcohol intoxication can increase aggression in some users (Bushman & Cooper, 1990).

Alcohol use is common among people involved in the criminal justice system. For example, approximately one-third of those incarcerated in state prisons self-reported alcohol use at the time of their offense (Rand et al., 2010). Victims of violent crimes reported similar rates of alcohol involvement among their offenders (30 percent), with substantially higher rates reported among those assaulted by intimates (66 percent) and spouses (75 percent) (Greenfeld, 1998). Not surprisingly, very high rates of alcohol use disorders have been noted among drunk drivers (Brinkmann et al., 2002; Osilla et al., 2015). For example, among first-time DUI offenders in Los Angeles, Osilla et al. (2015) estimate that more than 90 percent met the diagnostic criteria for past-year alcohol abuse and about two-thirds met the criteria for alcohol dependence.

From a social perspective, the costs associated with alcohol-related crime are large but hard to precisely estimate, given questions about how much alcohol contributes to various crime categories. One study estimates that the crime-related costs of excessive drinking were more than $70 billion for 2006 (Bouchery et al., 2011). The study defined excessive drinking as one or more of the following: "Binge drinking ($\geq$4 drinks per occasion for a woman, and $\geq$5 drinks per occasion for a man); heavy drinking ($>$1 drink per day on average for a woman, and $>$2 drinks per day on average for a man); any alcohol consumption by youth aged $<$21 years; and any alcohol consumption by pregnant women" (Bouchery et al., 2011, p. 517). A more recent study focused only on the economic costs of alcohol-involved traffic crashes estimates that they exceeded $40 billion in 2010 (Blincoe et al., 2015).

Traditional Policy Responses to Reducing Alcohol-Involved Crime

A variety of public policies have been implemented to reduce alcohol-involved crime and other harms associated with alcohol consumption; this paper is largely focused on the former. Carpenter and Dobkin (2010) review the literature on how five alcohol regulatory policies (tax/price restrictions, age-based restrictions, spatial restrictions, temporal restrictions, and other regulations) affected Federal Bureau of Investigation (FBI) Index violent and property crimes—not DUI. They conclude that some of the correlations represent a causal effect and that this is "especially true for interventions that induce very large and stark changes in alcohol consumption (e.g., large price or availability changes), as well as for alcohol control policies that

effectively manipulate not only alcohol consumption but also potential and realized social interactions (e.g., mandatory closing hours and drinking ages)" (p. 323).

Those who have previously been arrested for alcohol-related DUI are of special interest because they are responsible for a disproportionate share of DUI-related fatalities, injuries, and costs (Dugosh, Festinger, & Marlowe, 2013). Those arrested for DUI are typically subject to community supervision as a condition of bail, and this supervision is often part of a sentence conditional upon conviction. Requirements of community supervision can be both affirmative and restrictive. A participant may face a portfolio of conditions that, for example, combine orders to meet regularly with a probation officer, attend self-help meetings, abstain and participate in drug or alcohol testing, avoid environments such as bars, and limit use of vehicles. The ultimate penalty for non-compliance is often some form of incarceration—either pre-trial detention for those who have not completed the adjudication process, the instantiation of jail terms for probationers, or the completion of previously determined terms of incarceration for parolees.

The typical practice in community supervision settings tends to be inconsistent with findings about risk perception observed research on the mechanisms of deterrence. Evidence from criminology suggests apprehension certainty is salient but severity is not (Chalfin & McCrary, 2017; Nagin, 2013). The effect of increasing certainty appears to be non-linear, with Klepper and Nagin (1989) finding a threshold effect of increasing certainty and Loughran et al. (2012) finding that higher-risk individuals exhibit larger behavioral response once that threshold (a perceived probability of arrest and clearance of roughly 0.3 to 0.4) is met. The role of celerity is less clear. Individuals evaluating the potential positive and negative consequences of DUI exhibit hyperbolic discounting (Loughran, Paternoster, & Weiss, 2012), which suggests that immediate rewards and sanctions have value.[1] Of note in the present context, the impact of even brief delay may erode that value precipitously compared to the near-immediacy of the desirable effects of substance use that an intervention intends to deter (Cook, 2011, 2016).

The process leading to the time behind bars for non-compliance typically introduces both delay and uncertainty. Nagin (2013) describes certainty as the product of the probabilities of apprehension, prosecution, conviction, and sanction. So, too, is celerity a function of these components, given that each link in the chain takes time. Violations are unlikely to be detected because of infrequent substance use testing, thereby introducing uncertainty. An officer must then refer the individual under supervision back to the court for a hearing, which introduces delay. Then at the hearing, the judge must decide whether to revoke conditional release, adding to the uncertainty. To compensate for the uncertainty and delay, sanctions are often severe when they are carried out. Revocation of release in the pre-trial context means detaining a (not yet convicted) person until the next available trial date, which may be weeks or even months away. For those on probation, revocation of release means imposing the required custodial sentence, which often involves a substantial amount of jail time: for example, 12 states required at least 30 days in jail for a repeat-DUI offense and an additional 32 states required at least a week in jail (Comoreanu, 2017).

For those who are convicted of DUI, jurisdictions often impose a variety of non-custodial sanctions—in addition to or instead of jail—intended to deter individuals from DUI. For example, there is sizeable literature suggesting that driver's license suspensions and restrictions can reduce repeat drunk driving (Rogers, 1997); however, it is not clear how much of this effect comes from incapacitation and how much comes from deterrence. To estimate the effect of increasing sanctions on the

[1] Alternatively, Loewenstein (1987) notes that costs associated with anticipation of receiving a sanction may yield negative discount rates.

probability of future drunk driving, Hansen (2015) uses a novel dataset of every DUI stop in Washington state from 1999 to 2007. Using a regression discontinuity design that exploits discrete thresholds around blood alcohol content (BAC) levels that determine standard (0.08) and aggravated (0.15) DUI offenses, he finds that the sanctions imposed at these thresholds are effective in reducing repeat drunk driving. Hansen acknowledges that the analysis cannot rule out that some of this effect may be attributable to incapacitation or rehabilitation, but a series of analyses suggest that the primary mechanism is deterrence via sanction severity.

An oft-cited meta-analysis of remedial programs targeting DUI offenders—including treatment, education, psychotherapy, counseling, and contact probation—finds that these programs lead to at least a 7 to 9 percent reduction in DUI recidivism and alcohol-related crashes (Wells-Parker et al., 1995). A more recent review notes "a dearth of high-quality evaluations of DUI interventions" in the peer-reviewed literature (Miller et al., 2015); however, the authors argue, "it is reasonable to conclude that evidence exists to suggest that multi-component programs (e.g., those that provide intensive supervision with treatment) are more effective than those which target only one aspect of the issue." One systematic review of DUI-treatment courts, which typically combine long bouts of treatment with strong judicial oversight for DUI offenders, suggests that they may reduce the risk of arrest for any type of offense by roughly 25 percent; however, the authors note that "the few available experimental evaluations of DWI drug courts do not uniformly support their effectiveness" (Mitchell et al., 2012).[2] That said, a report released on reducing drunk driving by the National Academies of Sciences, Engineering, and Medicine (NASEM, 2018) called for every state to implement DUI courts.

The NASEM report also called for states to require those convicted for DUI to install an ignition interlock device (IID) on their vehicles if their BAC at the time of the arrest exceeded a state-determined threshold. With an IID, drivers must blow into a breathalyzer before starting the automobile, and it will not start if alcohol is detected (some devices also require the driver to blow after the trip has started). IIDs are effective in deterring impaired driving as long as they are installed, but installation rates are generally low for a few reasons, including lack of enforcement and monitoring to ensure compliance, as well as the fees and penalties that offenders have to pay before they are eligible for interlock-restricted driving privileges (Government Accountability Office, 2014). Further, the bulk of the evidence suggests that the IID effect quickly diminishes after the devices are removed from the vehicle, and there is very little evidence that these devices alone reduce alcohol consumption (Elder et al., 2011; Government Accountability Office, 2014; Voas, 2014; Willis, Lybrand, & Bellamy, 2004).

Two potentially important outliers exist in the IID literature. One study by Rauch et al. (2011) randomly assigned 1,927 drivers eligible for relicensure to either the two-year IID license restriction program or the "normal and customary sanctions afforded to multiple offenders." The study found that those assigned to the IID program still had a statistically significant reduction in the probability of an alcohol-impaired driving violation two years after the intervention. Another analysis of

[2] From Mitchell et al. (2012): "The systematic search identified 154 independent, eligible evaluations, 92 evaluations of adult drug courts, 34 of juvenile drug courts, and 28 of drunk-driving (DWI) drug courts. The findings most strongly support the effectiveness of adult drug courts, as even the most rigorous evaluations consistently find reductions in recidivism and these effects generally persist for at least three years. The magnitude of this effect is analogous to a drop in general and drug-related recidivism from 50% for non-participants to approximately 38% for participants. The evidence also suggests that DWI drug courts are effective in reducing recidivism and their effect on recidivism is very similar in magnitude to that of adult drug courts (i.e., a reduction in recidivism of approximately 12 percentage points); yet, some caution is warranted, as the few available experimental evaluations of DWI drug courts do not uniformly support their effectiveness."

a four-county IID pilot program by the California Department of Motor Vehicles (2016) yielded mixed results: "There was strong evidence of a reduction in DUI recidivism, across all offender levels, among those obtaining an IID-restricted license under provisions of this law. However, there is also strong evidence of a consistent increase in crashes, including fatal/injury crashes, among these same drivers" (p. xviii).

In the United States, there is substantial focus on preventing convicted drunk drivers from *driving* drunk, but there is considerably less focus on preventing convicted drunk drivers from *getting* drunk. In some jurisdictions, those arrested for or convicted of DUI can be ordered to abstain from alcohol as a condition of bond or probation, but this condition is rarely enforced (Heaton, Kilmer, and Nicosia, unpublished). 24/7 changed that in South Dakota.

Ordering Abstinence with Swift, Certain, and Fair Sanctions for Non-Compliance

While ordering individuals to abstain from substance use and monitoring compliance with biological testing is a common condition of supervision, there is a rigorous debate about the utility of requiring frequent testing with very modest sanctions for non-compliance (see e.g., Cullen et al., 2018; Duriez, Cullen, & Manchak, 2014; Kleiman, 2009; Kleiman, Kilmer, & Fisher, 2014). Kleiman (1997, 2016) used to refer to this approach as "coerced abstinence" but over time it was rebranded as "swift-certain-fair" (SCF).[3]

The SCF-type program that has received the most attention in recent years has been Hawaii's Opportunity Probation with Enforcement (HOPE) program. Probationers assigned to HOPE were ordered to abstain from illegal drugs and were subject to frequent and random drug testing with swift, certain, and short jail stays for noncompliance. A randomized controlled trial (RCT) of HOPE demonstrated this approach: 1) could reduce substance use and criminal activity among probationers; 2) had a larger benefit-cost ratio than traditional supervision; and 3) some of the benefits lasted well after program participation (Hawken et al., 2016; Hawken & Kleiman, 2009).

A multi-site demonstration field experiment (DFE) of HOPE on the mainland did not yield similar results on traditional criminal justice outcomes (Lattimore et al., 2016), triggering a high-profile discussion about the merits of HOPE and SCF in general (see e.g., Cook 2016; Hawken 2016; Nagin 2016). Examining RCT outcomes for more than 1,500 probationers in four sites, Lattimore et al. (2016) reported that:

> *"Recidivism results suggest that HOPE/SCF supervision was not associated with significant reductions in arrests over [probation as usual] PAU with the exception of a reduction in drug-related arrests in one site. There were significant—albeit conflicting—differences in time to revocation, with survival models suggesting shorter times to revocation in two sites and longer times to revocation in one site"* (p. 1104).

A subsequent cost analysis examining 24-month outcomes for a sample of the probationers in the DFE found that those assigned to HOPE incurred more incarceration and substance use treatment costs than those assigned to probation as usual (Cowell et al., 2018).[4]

[3] The SCF approach is often associated with efforts to reduce substance use in community supervision settings, but there are a growing number of examples of SCF principles being used to address other behaviors (Swift Certain Fair Resource Center, n.d.).

[4] Cowell et al. (2018) note: "Because no statistically significant positive differences were found in outcomes, HOPE participants spent more days in jail, and more HOPE participants were referred to residential treatment, the results from the current study show that the HOPE group incurred more criminal justice costs than did the PAU group, although for the 24-month analysis, the results were significant for only one of the four sites and overall. In subanalyses, however, in which the period was decreased to 6 and 12 months and the sample size was increased, the higher cost of HOPE DFE compared with that of PAU was statistically significant for more sites" (p.888).

In a response to this cost study, Hawken (2018) addressed a DFE outcome that had been largely omitted from most HOPE replication debates: use of illegal drugs. Hawken noted that:

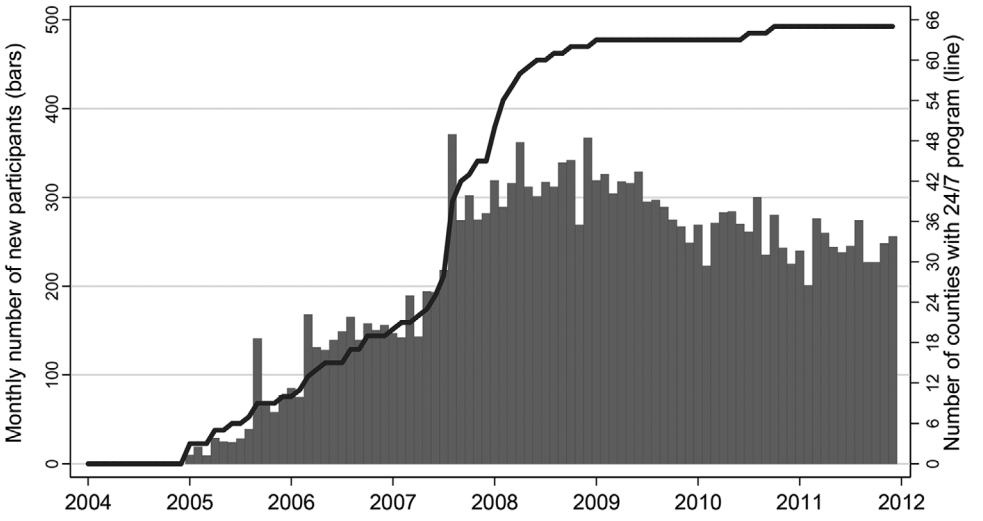
> *"A strength of the DFE was the inclusion of an independent drug test for research purposes, collected using an oral swab during 6-month and 12-month follow-up interviews—this ensured that everyone was tested the same way and at the same time. At the 6-month follow-up interview, HOPE participants were significantly less likely to test positive for an illicit drug than were members of the control group (16% vs. 30%). This trend was even more pronounced at the 12-month interview, with 13% of the HOPE group testing positive compared with 31% of the control group—again, a statistically significant contrast"* (p. 904).

Given that reducing substance use is an important goal of HOPE that could have implications for health, family relations, and possibly other outcomes excluded from the cost study, one should be skeptical of claims that the HOPE DFE produced null findings across all outcomes of interest (Humphreys & Kilmer, forthcoming).

At the same time HOPE was being developed and implemented in Hawaii, a similar program focused on reducing alcohol consumption and crime was independently developed in South Dakota. In 2003, South Dakota Governor Mike Rounds established a corrections working group focused on reducing the incarceration rate and the attendant cost of further prison construction (Rapid City Journal, 2003). Because alcohol-involved offenders accounted for a significant share of South Dakota's prison population, the state's new attorney general (AG), Larry Long, believed reducing alcohol consumption among individuals involved in the criminal justice system should be a priority. Long noted that many alcohol-involved offenders in South Dakota were ordered to abstain from alcohol as a condition of bond or community supervision, but this condition was not enforced (Long, 2009). Thus, he argued that the state should implement a pilot program that combined abstinence orders with twice-a-day breathalyzer tests of DUI arrestees; those testing positive for alcohol or skipping a test would be subject to a modest sanction: a night or two in jail. The program focused on alcohol consumption itself, rather than the DUI event that may result from excessive consumption, among the high-risk pool of repeat-arrestees.

For AG Long, the program was a pragmatic response to the cases he supervised. Impulsivity is especially common among individuals who drink and drive (Sloan, Eldred, & Xu, 2014) and severe penalties understandably require more due process and may be more costly for systems to impose; increasing severity may have the effect of reducing celerity and certainty. Also, within the clinical literature on alcohol treatment, individuals with alcohol use disorder have been found to be responsive to predictable, immediate consequences for behavior (e.g., receiving a small gift in exchange for passing a breathalyzer test; Petry et al. 2000). Based on the criminal history records used in this analysis, the median time between a second or third DUI arrest and final disposition of the case was 112 days, whereas the median time to enrollment in 24/7 for pre-trial participants was four days. Long's preference to prioritize certainty and celerity of sanction over severity was consistent with insights from the literature on offender decisionmaking and appeal to the decision heuristics defined in applications of behavioral economics to criminal justice (Pogarsky, Roche, & Pickett, 2018), but contrasted with the adjudication process in the state.

Long and his staff implemented the pilot—known as 24/7 Sobriety—in five South Dakota counties: the two most populous counties in the state (Pennington and Minnehaha) and three others (Bennett, McCook, and Tripp). There was a goal of getting counties with large and small populations, as well as at least one that did not have its own jail. The choice of the specific counties was heavily influenced by personal relationships that Long and his staff had with different judges and sheriffs (Mickelson, personal communication). The pilot, started in 2005, initially targeted those



Notes: We define 24/7 as operational in each county once the number of county residents in 24/7 for a given month equals or exceeds a quarter of the number of DUI arrests in the county, where the latter is defined as the county's moving monthly average during the previous year to address any seasonality.

Figure 1. Growth of 24/7 Sobriety in South Dakota.

arrested for a repeat DUI offense and was a condition of their bond. Specifically, judges in the pilot imposed two additional bond conditions on participants: (1) Defendants must abstain from alcohol, and (2) defendants must report to a test site once in the morning and once in the evening for alcohol tests. Long (2009) reported that "Defendants who tested positive were immediately incarcerated for violating the bond condition. Bench warrants were issued for defendants who failed to report to the test site on time. All defendants who violated a bond condition were incarcerated for 24 hours before making a court appearance, where the same conditions were reimposed."

More than 99 percent of scheduled alcohol tests were taken and passed, so state and county officials believed that the pilot program worked. Other counties soon began contacting the attorney general about participating, and by the end of 2006, residents of 25 counties were enrolled in 24/7 programs. A few counties also included testing for illegal drugs under 24/7. Further expansion was spurred by a state law passed in 2007 that appropriated $345,000 and set administrative rules for program operations, and by a second bill in 2008 that appropriated another $400,000 in state funding for program operations. Figure 1 shows how the program spread throughout the state.

Table 1 shows the distribution of offenses across counties in months prior to an operational 24/7 program and during 24/7. Counties with active 24/7 Sobriety programs have a relatively lower share of DUI and drug-related crimes, those which are most likely to be affected by 24/7, particularly during the period of the program's expansion across states as a DUI-focused intervention. As the program matured, arrestees were assigned to 24/7 for crimes other than DUI at higher rates.

The program also expanded with respect to eligible offenses and testing technologies. In 2006, the attorney general's office introduced the use of continuous alcohol monitoring ankle bracelets, which participants can wear for months at a time, even in the shower. Every 30 minutes, the device tests the participant for alcohol, and it can also determine whether someone has tampered with the device. Initially this

Table 1. Distribution of offenses by 24/7 program status, 2004 to 2011.

	Pre-24/7		Post-24/7	
Offense	Number	Percentage	Number	Percentage
Assault	4,372	6.5	10,422	7.2
Other violent	6,173	9.1	15,312	10.5
Domestic violence	5,330	7.9	12,925	8.9
DUI 1st	19,309	28.6	38,211	26.3
Repeat DUI	7,912	11.7	15,546	10.7
Drug offense	12,229	18.1	25,582	17.6
Theft and burglary	6,144	9.1	15,862	10.9
Total Arrest Events	**67,471**		**145,565**	

information was relayed to a private company via a modem in the individual's home, but over time, more participants just came into the sheriff's office once or twice a week to upload the information from the bracelet to the private company.[5]

Decisions about whether someone was tested twice a day or wore a bracelet were ad hoc: In some rural areas, it was infeasible for someone to drive in twice a day, and in some cases judges would switch people to the bracelet if they did not perform well with the twice-daily testing. In other places, participants who preferred to wear the bracelet and could afford it (daily costs were $6 instead of $2 per day—$1 for each breathalyzer test) had the option if there were bracelets available. The attorney general purchased the bracelets and was in charge of distributing them throughout the state. To date, the majority of 24/7 participants in South Dakota are monitored via twice-daily breathalyzers; in 2015, 21 percent of participants wore the ankle bracelets. The state began piloting twice-daily testing via IID in October 2012 and had 350 IID participants by June 2015.

The duration of 24/7 participation is not fixed, and judges may keep participants in the program longer if they are struggling. It is not uncommon for someone to start on the program before his or her trial and continue post-conviction. In our analytic dataset, the median number of days for participants on the alcohol monitoring bracelet exceeds the median for twice-daily participants (180 versus 109),[6] but these figures tell us nothing about the relative efficacy. For example, those who know they are going to be on the program for a short period may be more willing to submit to twice-daily tests than those who know they will be on the program for an extended period (e.g., if they lost their license because of a DUI and would like a restricted permit to drive to work, which requires 24/7 participation).

Of all 24/7 participants, 53 percent make it through the program without a violation (i.e., a failed or unexcused missed test), 19 percent violate once, 11 percent

[5] Through September 2019, there were more than 11 million breathalyzer tests, and the pass rate was 99 percent (with no-shows in the denominator). Ankle bracelet participants accumulated approximately 11,000 violations over 2.2 million days of monitoring, equivalent to more than a 99 percent daily compliance rate.

[6] Participants' days monitored in the analytic dataset for this analysis are greater than figures published in prior analyses of South Dakota 24/7 Sobriety for a combination of reasons. First, we restricted observations to individuals who began the program at least six months prior to the data end date, which reduces bias toward zero in the statistics due to right-censoring. Second, we restricted observations to first-time enrollment, which removes potential erratic enrollee entries from multiple testing sites under separate participant identification numbers. Finally, first-time participation typically is longer in duration than subsequent spells, partly because of the higher risk of removal for those who have enrolled multiple times (due to higher rates of rearrest or poor program performance).

violate twice, and about 17 percent violate three or more times. Judges decide when someone is terminated from the program, and these individuals are generally returned to jail for violating their conditions of bond or probation. Violations are always supposed to be sanctioned with jail time (South Dakota Office of the Attorney General, n.d.), ranging from 12 to 72 hours behind bars (Midgette, 2014). Systematic data on repercussions for 24/7 violations do not exist, because violations are neither criminal nor civil offenses and do not appear in criminal records. However, state guidelines and trainings call for immediate incarceration for alcohol-positive participants and those who miss a test. Based on information gathered from field studies of eight sites that account for nearly 80 percent of participants, sites routinely hold violating participants for a short period in a cell after an in-person alcohol violation and on next contact after notifying participants of non-compliance by phone after remote alcohol monitoring violations and no-shows (Midgette, 2014). Some counties have also adopted standardized graduated sanctions for violations (e.g., a 12-hour hold for the first violation and 24 hours for the second).

Through February 2017, more than 30,000 unique South Dakotans had participated in 24/7 and accumulated more than five million days without a detected drinking event (authors' calculations). To be clear, five million days without a drinking violation does not imply five million days without *any* drinking because alcohol passes through the system relatively quickly and the testing media have limited sensitivity. If someone passes a breathalyzer test at 8 a.m., they can theoretically have a few drinks and still test negative for alcohol again that evening. Also, if individuals are wearing alcohol monitoring bracelets and only consuming small amounts of alcohol throughout the day, the detected level of drinking may not meet the threshold for a violation.[7] While these individuals are technically ordered to abstain from alcohol, the program's testing is largely focused on reducing heavy drinking and its associated outcomes.

As 24/7 was being implemented in South Dakota, there were other interventions being adopted throughout the state that also may have influenced alcohol-related driving outcomes. Long (2009) notes that in 2006, the state repealed its implied consent law, and around the same time the state revised the educational programming for those convicted of their first DUI. In addition, Long reports that there was increased DUI enforcement (e.g., sobriety checkpoints), an increase in media campaigns targeting impaired driving, and the implementation of a new program targeted at parents to help reduce underage drinking.

To help isolate the effect of 24/7, Kilmer et al. (2013) exploited county-level variation in implementation dates (see Figure 1) to assess the effect on five county-level outcomes: first time DUI arrests, repeat DUI arrests, domestic violence arrests, total traffic crashes, and traffic crashes involving men ages 18 to 40. Using county-level and year-month fixed effects, as well as several county-level control variables (at the month and annual levels), they found that, after 24/7 was operational at the county level, repeat DUI arrests dropped by 12 percent and domestic violence arrests dropped by 9 percent. There was no evidence that the program influenced the

[7] The science of remote alcohol testing is improving and there are multiple companies providing this type of supervision. For the remote device South Dakota was using during our study period, one study found that "Nearly 40% of participants drinking one beer did not have a positive [transdermal alcohol concentration] (TAC) reading. However, positive TAC readings were observed in more than 95 and in 100% of participants drinking 2 and 3 or more beers respectively. The probability of peak TAC detection was a positive function of the number of beers consumed and a negative function of the minimum TAC threshold for detection. Drinking was somewhat more likely to be detected in females than males drinking 2-5 beers, but not after 1 beer. Use of AMS standard criteria only reliably detected the consumption of 5 beers, and 45.9% of all occasions of drinking 1-3 beers were undetected using 0.02 g/dl as a threshold" (Roache et al., 2015, p.1120).

number of first-time DUI arrests or total traffic crashes. In the main specifications, 24/7 was defined as operational once the number of county residents in 24/7 for a given month equaled or exceeded 25 percent of the number of DUI arrests in the county, where the latter was defined as the county's moving monthly average during the previous year to address any seasonality. If one used a less conservative threshold of 10 percent, the reduction in the number of repeat DUI arrests at the county level changed from 12 percent to nearly 18 percent.

Subsequent research using a similar research design found that implementation of 24/7 was associated with a 4 percent reduction in all-cause adult mortality, concentrated among circulatory and injury-related deaths (Nicosia, Kilmer, & Heaton, 2016). Another aggregate-level analysis using the National Incident Based Reporting System and a triple-difference methodology for a limited set of counties provides additional support for the efficacy of 24/7 in South Dakota (Heaton, Kilmer, & Nicosia, unpublished).

Thus far, however, there have been no peer-reviewed studies of South Dakota's 24/7 Sobriety Program using individual-level data. An obvious concern with community-level analyses is that they limit the ability of researchers to identify the causal mechanisms driving the observed change (e.g., general versus specific deterrence). Furthermore, policymakers want to know the individual-level effect of 24/7 so that they can make comparisons with other well-studied programs targeted at the same population, such as DUI courts and IIDs (discussed previously).

A report by Loudenberg, Drubbe, and Leonardson (2013) sought to assess the effect of 24/7 on the time until next DUI arrest by comparing a sample of 24/7 participants from 2005 to 2010 with a matched sample of "non-program participants who were arrested in 2003, 2004, or 2005 and who did not participate in the 24/7 Sobriety Program for the DUI offense on the docket" (p. 25). The differing time periods for the survival analysis raise concerns that the risk of arrest could have been much different for the treatment and control groups (and there is no attempt to control for this). In addition, there are also important selection issues: Those in the treatment group were limited to participants who were in 24/7 for at least 30 days. The report also included a different analysis that compared any twice-daily breathalyzer participants with anyone who was arrested for a DUI offense. These did not appear to be matched samples (other than on DUI offense arrest) or controls for covariates that could influence the probability of rearrest.

Finally, the previously mentioned NASEM (2018) report on alcohol-impaired driving reviewed the evidence on 24/7, concluding that the program has been shown to be effective in some rural areas; however, the authors argue that an important limitation to the existing literature is the "reliance on aggregate analyses rather than individual-level data." This paper seeks to fill this gap.

DATA AND METHODS

This paper contributes to the deterrence literature by estimating the causal effect of 24/7 participation on criminal recidivism using individual-level data and program availability as an instrumental variable. *A priori*, we expect the deterrent effect of 24/7 participation on criminal behavior to diminish as the time after participation elapses.

Data

This analysis uses criminal record data from the South Dakota Attorney General's Office for 20,243 arrest events among 16,513 individuals arrested for a second or third offense for DUI (DUI-2 and DUI-3, respectively) in South Dakota between

2004 and April 2012. These data include both 24/7 participants and those who never participated in the program. Each county enters information about 24/7 participants and their testing results into the attorney general's statewide 24/7 database. We obtained participant-level data dating back to the original pilot in 2005.

Most the 20,243 observations in the sample are individuals who enter the data exactly once. Of 3,370 individuals who enter the data twice (i.e., once for their DUI-2 arrest and once for their DUI-3 arrest), 1,387 are enrolled in 24/7 in their second observation and the remaining 1,983 do not enter 24/7 for either observed arrest. Individuals who are assigned to 24/7 as a consequence of their DUI-2 arrest are excluded from the DUI-3 group. Omitted DUI-3 participants previously assigned to 24/7 for a DUI-2 offense are included in the sample only for that initial DUI-2 participation spell. We do not consider DUI-3 arrestees who had previously been on 24/7 since re-enrollment is necessarily a consequence of rearrest, thus creating an endogeneity problem. Further, prior exposure to the program may change risk perceptions and differentially affect the deterrent power we seek to measure (Bushway & Owens, 2013).

We do not have information about all crimes committed by DUI arrestees; thus, we rely on administrative information about arrests and probation revocations. Because alcohol influences a wide array of criminal behaviors, we consider any offense in the main estimates summarized in Table 4, not just those specifically associated with alcohol, such as DUI or public drunkenness. Since some of the individuals in the control and treatment groups are on probation, it is possible that their probation officer will seek to revoke probation for a violation instead of making a new arrest. While probation revocation is uncommon in our analytic sample, it is appropriate to incorporate that information into a dependent variable that serves as a proxy for criminal behavior. We consider analyses that exclude probation information in the sensitivity analysis.

To control for time-varying socioeconomic conditions in each county, we include the non-seasonally adjusted unemployment rate from the Bureau of Labor Statistics (n.d.). For each county-year, we also include per capita sworn law enforcement officers as a proxy for local-level changes in law enforcement (FBI, 2017) and per capita on- and off-premises alcohol outlets to help control for alcohol availability (United States Census, 2018).

Empirical Strategy

Judges, probation officers, and parole officers have discretion about who enters the 24/7 program. Some participants enter the program as a condition of bond, some enter as a condition of probation, some participate to obtain a restricted driver's license (which allows them to drive to work), and some participate in multiple settings (i.e., pre- and post-conviction). Days on the program can vary dramatically depending on the time between arrest and disposition, and participants sometimes will be ordered to stay on the program longer if they violate program rules. There is tremendous variation at which point in the process individuals are ordered to participate, especially in the earlier years.

The lack of uniformity creates challenges for evaluation. If one defines the treatment as beginning after the first test, then how does one define the control group that is not subject to 24/7 testing and sanctions and determine when to start measuring the time at risk of rearrest or a community corrections violation? To address this issue, we employ an approach that biases our results toward not finding an effect: If anyone is tested as part of the 24/7 program after an arrest for DUI-2 or DUI-3—even if he or she eventually drops out of the program after a couple of days—we consider that person treated. Those who are in the program for a short amount of

Table 2. Covariate summary statistics by program status at county level.

	24/7 Status	
	Before	Operational
County-level characteristics		
Sworn officers per capita	15.3	16.4
Package stores per capita	5.5	5.8
Bars per capita	2.2	1.7
Unemployment rate (percentage)	3.8	4.2
Arrestee-level characteristics		
DUI-2 arrestee (percentage)	67.2	68.2
DUI-3 arrestee (percentage)	32.8	31.8
Male (percentage)	80.0	76.6
Age (years)	34.2	33.8
Violent prior (percentage)	16.5	15.8
Drug prior (percentage)	4.8	7.5
Priors arrests	2.7	2.8
Days since previous DUI	1,214	1,205
Days from arrest to 24/7 entry	118.7*	46.1
Days in 24/7	222.6*	281.8
Sample size		
County-months	1,325	1,834
Cases	6,292	13,951

Notes: There were 217 participants in the sample assigned to 24/7 prior to the program reaching the 25 percent threshold we use to define a county as operational. *p<.05.

time would not be expected to yield benefits, so this should dilute the treatment effect. Additionally, if judges or probation officers ordered some DUI-3 arrestees to participate in 24/7 in lieu of jail, then the control cases that were remanded to jail would be incapacitated and not at risk for being arrested for a new crime. Thus, our approach is conservative against finding an effect.

Table 2 displays the descriptive statistics for counties before and after 24/7 implementation. Approximately three-quarters of the arrestees are men, and they average three prior offenses (median = 2). By most observable individual- and community-level measures that may be potential confounders, participants and their comparison group are similar in aggregate. The median time between the index DUI arrest and the prior DUI is 7.5 percent longer for the 24/7 group, although the mean is nearly identical.

We include criminal history characteristics to account for individual-specific differences in rearrest risk using separate indicators of prior arrest for violent crime, drugs, or illegal weapons; we also use an indicator for DUI-3 (versus DUI-2). After accounting for incapacitation, we expect that each of these attributes is positively related to probability of rearrest. Because the time since the previous DUI is correlated with future reoffending, we use decile buckets based on the complete analytic sample used in our main results for the time in days between an arrestee's prior DUI and the DUI serving as the starting point for measurement in the present analysis (e.g., days between DUI-1 and DUI-2 for DUI-2 arrestees). Generally, faster prior recidivism should be a predictor of faster rearrest in the present; however, the particular shape of this relationship is unknown. Thus, we flexibly account for the relationship between unobserved individual characteristics proxied by the count of days between prior DUI offenses and the outcome of interest by employing decile bucket dummies rather than prescribing a shape to the relationship.

Probit. We first estimate probit models to understand what factors predict rearrest or revocation within a 12-, 24-, and 36-month time frame among DUI-2 and DUI-3 arrestees combined. The probability of rearrest or probation revocation is estimated using equation (1):

$$P(A_{ict}) = \beta_0 + \beta_1 24/7_i + \beta_2 X_{ict} + \alpha_c + \alpha_t + \varepsilon_{ict}, \quad (1)$$

where the probability that an individual is rearrested or has probation revoked within a fixed time horizon (A_{ict}) is a function of 24/7 enrollment ($24/7_i$), vectors of individual- and county-level characteristics (X_{ict},), county fixed effects (α_c) that remove any time-invariant unobservable differences across counties, and time fixed effects (α_t) based on the month and year when each DUI arrest occurred (e.g., January 2004, February 2004). Standard errors are clustered at the county level to account for serial correlation; two-way clustering on both county and month produces similar results.

Seemingly Unrelated Bivariate Probit

Any adult repeat-DUI arrestee in a county with an active 24/7 program is eligible for enrollment, but not all such arrestees are enrolled. Over our sample set, the share of DUI-2 and DUI-3 arrestees in counties with 24/7 who entered the program increased from 27.7 percent in 2006 to 38.6 percent in 2011. The discretion that judges and probation officers have raises the issue of selection bias; however, the direction of the bias is unclear. In some cases, individuals who are most likely to recidivate could be ordered to the program because they need the most help; in other cases, judges and probation officers may select only those believed to have the best chance at success in the program. We also must consider the observed selection that occurs when DUI offenders choose to participate in 24/7 as a condition of receiving a restricted driver's license that allows them to drive only to work. An unknown share of DUI offenders choose to drive even though their driver's license is revoked.

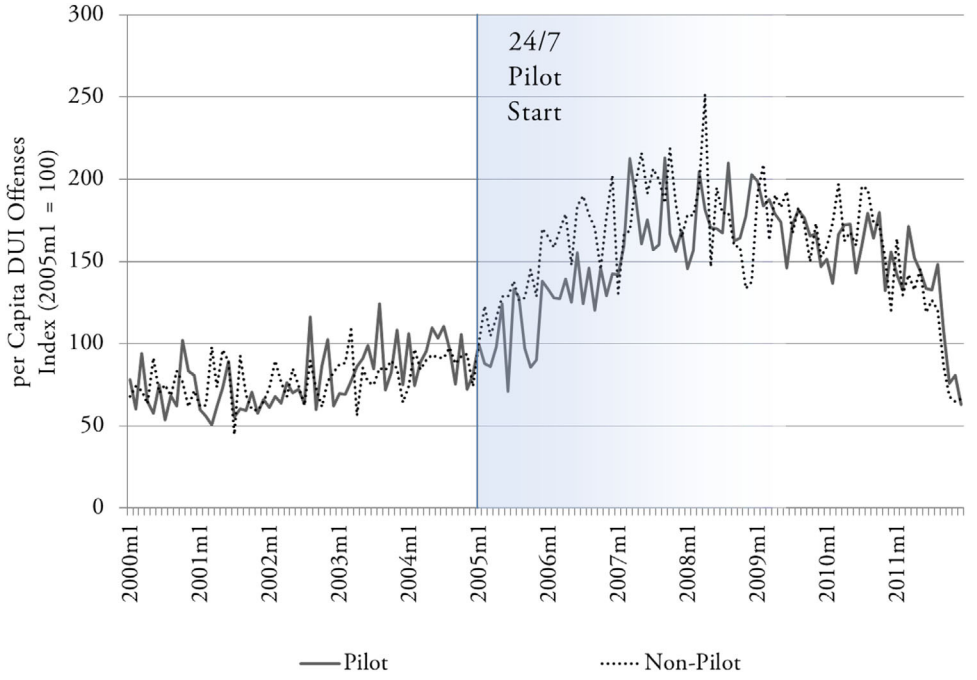
We attempt to account for this potential bias by using an instrumental variable (IV) bivariate probit approach. Ideally, the IV will predict enrollment in 24/7 without being correlated with the residual error. In this case, we exploit the variation in the timing of 24/7 implementation across counties. Specifically, we construct our instrument as an indicator for whether the county of the individual's arrest had an operational 24/7 program (which Kilmer et al., 2013, defined as the number of 24/7 participants in any given month equaling or exceeding one-quarter of the 12-month moving average count of DUI arrests in that county). We estimate the probability of rearrest or probation revocation ($P(A_{ict})^*$) by solving two equations simultaneously:

$$24/7_i^* = \gamma X_{ict} + \delta Z_{ct} + \alpha_c + \alpha_t + u_{ict} \quad (2)$$

$$P(A_{ict})^* = \beta_0 + \beta_1 24/7_i + \beta_2 X_{ict} + \alpha_c + \alpha_t + \varepsilon_{ict}, \quad (3)$$

where $24/7_i = 1$ if $24/7_i{}^* > 0$, and $24/7_i = 0$ if $24/7_i{}^* \leq 0$.

The model is identified using the instrument (Z_{ct}) in addition to the covariates—X_{ict}, α_c, and α_t—defined in equation (1). We simultaneously estimate the probability of rearrest or probation revocation $P(A_{ict})$ based on the endogenous variable program participation ($24/7_i{}^*$) and the other covariates. We assume that ε_{ict} and u_{ict} are distributed bivariate normal, such that $E[\varepsilon_{ict}] = E[u_{ict}] = 0$, $\text{var}[\varepsilon_{ict}] = \text{var}[u_{ict}] = 1$ (Greene, 2011). This county-level instrument allows us to isolate and evaluate individual-level variation across and within 24/7 assignment. Thus, our bivariate probit approach allows inference of the average treatment effect (ATE) among the repeat DUI offenders. We report ATE as percentage point changes associated with



Notes: This chart is based only on repeat-DUI arrests. The pilot included five counties; the non-pilot counties grew over time to include 60 of a possible 61, defining participation by county of residence.

Figure 2. Comparison of Pilot and Non-Pilot Counties Before and After 24/7 Implementation.
[Color figure can be viewed at wileyonlinelibrary.com]

24/7 participation compared with non-participation and percent changes comparing the percentage point marginal effect of 24/7 assignment to the comparison group of non-assignees; the estimate is evaluated across the sample observations at the observed values of their covariates. In practice, this is generally comparable to the local average treatment effect (LATE) obtained by two-stage least squares (Angrist & Pischke, 2009). We assess this assertion in this specific context as a robustness check.

Monotonicity is likely satisfied because the probability of assignment to 24/7 is always positively related to the program's availability in the county where an individual is arrested or resides. The independence assumption underlying our instrument requires that the timing of a county's implementation is not due to individuals' future recidivism risk and that the expansion of 24/7 over time is not related to rearrest or revocation rates within counties. The use of program availability in the county of arrest as an instrument is fundamentally similar to judge fixed effects, assuming monotonicity of assignment within judges (or in this case counties) to estimate LATE (Imbens & Angrist, 1994).

To help test this assumption, we compare repeat-DUI arrest rates in pilot and non-pilot counties in the pre-program period. Figure 2 shows the indexed rate of repeat-DUI per capita for the five-year periods before and after the 2005 rollout of the program. Because the program began as a pilot but was adopted throughout the state over time, implementation is represented as a gradient box, where the darker shaded period beginning in January 2005 indicates the program running strictly in the pilot counties. There is a notable visual gap between the pilot and non-pilot counties in

the period when the program was concentrated in the few pilot counties, echoing the findings of county-level effectiveness in Kilmer et al. (2013). This gap dissipates over time, likely because other counties began utilizing the program. There is no evident difference between DUI rates in the period preceding implementation, thus we do not believe selection is a threat at the county level.

RESULTS

Main Results

Figure 3 plots the survival curves for 24/7 participants and those in the control group. These unadjusted data suggest that the time until next arrest or probation revocation is longer for 24/7 participants than for non-participants ($\chi^2 = 90.98$, $p < 0.001$).

Table 3 presents the results of probit models that examine the probability of being arrested for a new offense or having probation revoked after 12, 24, and 36 months. Model 1 includes county-level controls but no criminal history information; Model 2 adds decile buckets for the number of days between the preceding and index DUI arrest; and Model 3 adds indicator variables for the number of priors in each criminal history, collapsing all larger quantities at 10 due to sparseness, and for prior violent crime and drug arrests. The coefficient on the 24/7 participation variable is negative and statistically significant in all models, suggesting that the program is associated with a reduction in the probability of being arrested or having probation revoked. In the base specification (Model 1), these 24/7 participants were 10.7 percentage points ($p < 0.01$) less likely than non-participants to be arrested or have their probation revoked 12 months after DUI arrest. We also detected reductions at 24 and 36 months—8.9 percentage points ($p < 0.01$) and 7.3 percentage points ($p < 0.01$), respectively. The absolute value of the marginal effects in the full specification (Model 3) is slightly smaller for all three time periods but still statistically significant at the 0.01 level.

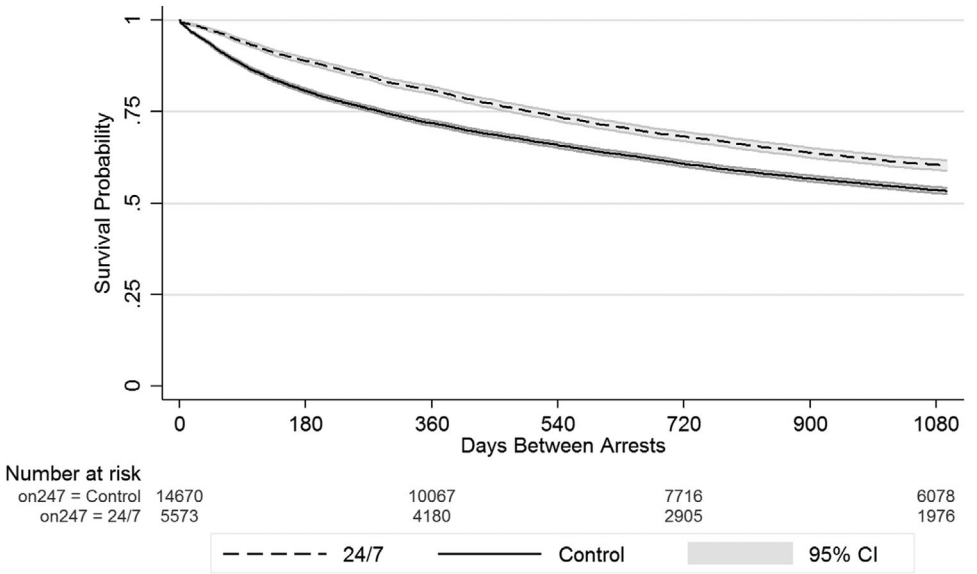


Figure 3. Kaplan-Meier Survival Functions.

Table 3. Marginal effect estimates of 24/7 on recidivism—probit models.

	(1)	(2)	(3)	N
12 months	−0.107	−0.105	−0.097	19,114
	(0.012)	(0.012)	(0.012)	
24 months	−0.089	−0.087	−0.079	16,882
	(0.011)	(0.010)	(0.010)	
36 months	−0.073	−0.072	−0.067	14,513
	(0.014)	(0.012)	(0.012)	
Controls				
County and month fixed effects	Yes	Yes	Yes	
Age and gender	Yes	Yes	Yes	
County-level controls	Yes	Yes	Yes	
Previous DUI interval	No	Yes	Yes	
Detailed criminal history	No	No	Yes	

Notes: Standard errors are in parentheses and clustered at the county level. The control variables for all models include gender, age, categorical indicators for number of prior arrests (top-coded at 10), an indicator for DUI-3 (versus DUI-2), indicators for prior arrest for violent or drug-related crimes, indicators for the observation's decile rank for days between prior DUI offenses, and county-level police per capita, bars per capita, liquor stores per capita, unemployment, and county and month fixed effects.

Table 4. Marginal effect estimates of 24/7 on recidivism—bivariate probit models.

	(1)	(2)	(3)	N
12 months	−0.149	−0.134	−0.137	19,119
	(0.053)	(0.051)	(0.044)	
24 months	−0.158	−0.134	−0.138	16,886
	(0.069)	(0.065)	(0.056)	
36 months	−0.133	−0.109	−0.117	14,518
	(0.056)	(0.051)	(0.045)	
Minimum F-statistic on instrument under 2SLS	42.52	42.70	43.10	
Controls				
County and month fixed effects	Yes	Yes	Yes	
Age and gender	Yes	Yes	Yes	
County-level controls	Yes	Yes	Yes	
Previous DUI interval	No	Yes	Yes	
Detailed criminal history	No	No	Yes	

Notes: Standard errors are in parentheses and clustered at the county level. An indicator for an operational 24/7 program in county and month of arrest event is used to instrument 24/7 enrollment. The control variables for all models include gender, age, categorical indicators for number of prior arrests (top-coded at 10), an indicator for DUI-3 (versus DUI-2), indicators for prior arrest for violent or drug-related crimes, indicators for the observation's decile rank for days between prior DUI offenses, and county-level police per capita, bars per capita, liquor stores per capita, unemployment, and county and month fixed effects.

To address the concerns with selection, we estimate IV bivariate probit models, where our binary instrument equals one if the 24/7 program was operational in that county at the time of the initial arrest. The F-test statistic associated with the coefficient on the active 24/7 program indicators are consistently large, and the estimated coefficient itself is positive and significant ($p < 0.001$), indicating that the presence of an operational 24/7 program in a county is a strong predictor of assignment to the program. The structure of Table 4 mimics Table 3, except we are now focused on the IV models. The full model in Table 4 (Model 3), our preferred specification, indicates that the probability a 24/7 participant was rearrested or had

probation revoked 12 months after being arrested for driving under the influence was 13.7 percentage points (49 percent; $p = 0.002$) less than that of non-participants.

We also detected reductions at 24 and 36 months—13.8 percentage points (35 percent; $p = 0.013$) and 11.7 percentage points (26 percent; $p = 0.009$), respectively. Although the IV coefficients are between 29 percent and 43 percent larger than those generated from the probit models, the difference in estimates is not statistically significant based on a z-test of coefficient equality. Using a z-test with pooled variance, the obtained z-statistic is between 0.877 and 1.074. The notion of policy exogeneity is corroborated by the consistent finding that the residuals of the instrument and policy estimations are uncorrelated for the bivariate probit models (as denoted by the model ρ-statistic), indicating the instrument and policy equations are independent (Wooldridge, 2010).

Among model covariates, the coefficient estimates are largely consistent with our assumptions of rearrest risk. Across specifications, age is negatively related to rearrest probability, while prior drug convictions, specifically, and the total count of prior convictions are positively related to rearrest risk. The length of time between prior DUI arrests is negatively related to rearrest. Notably, DUI-3 participants were somewhat less likely to reoffend (p-value < 0.10), while bars per capita were negatively related to rearrest after controlling for the density of other alcohol outlets in the county. We find no relationship between rearrest and gender, sworn officers per capita, prevailing unemployment rates, or violent prior convictions.

Robustness Tests

Table 5 displays the results of various robustness tests. For comparison purposes, panel A includes the baseline IV bivariate probit results from Table 4. The first alternative scenario (panel B) limits the analytic sample to those with 36-month follow-up data, allowing us to examine a consistent sample of arrestees over all three time periods. Because ending the sample at April 2009 instead of October 2011 reduces the sample size for the 12-month analyses by about 24 percent, we would expect the results to become less precise; however, this was neither the case for the 12-month nor 24-month runs.

Panel C limits the 24/7 treatment group to only those who were in the program for two years or less. The motivation for this scenario was to assess whether our baseline results were being driven by those who participated for more than two years. The results are more precise, and the absolute value of point estimates is larger, suggesting that long-term participants do not drive the results.

Panels D and E explore what happens when we add new variables to the model. In panel D, we control for whether the individual was convicted for the DUI arrest that got them into the program; in panel E, we control for the number of days the person was incarcerated for convictions on the DUI arrest (days sentenced minus days suspended + 1, then log-transformed to account for skewness). Neither of these additions makes a substantive difference.

Our baseline runs focus on whether 24/7 influences the probability of being arrested for a new offense after a DUI arrest or having probation revoked. Panel F explores the effect of excluding the probation information and focusing only on time of arrest. The absolute values of the effects sizes become somewhat smaller across the three time periods, but all remain statistically significant. This is not surprising, because ignoring probation revocations biases our results toward not finding an effect of 24/7 on criminal behavior.

Panel G shows results from models omitting controls based on past criminal history information except days between prior DUI arrests. This should be insightful to researchers who have access to Department of Motor Vehicle records but not

Table 5. Robustness tests.

	12 months	24 months	36 months
Panel A: Baseline (from Table 4)	−0.137	−0.138	−0.117
	(0.044)	(0.056)	(0.045)
Panel B: Limit to those observed ≥ 36 months	−0.151	−0.142	−0.117
	(0.046)	(0.058)	(0.045)
Panel C: Limit to those on 24/7 < 2 years	−0.210	−0.240	−0.222
	(0.065)	(0.063)	(0.056)
Panel D: Include control for convictions	−0.137	−0.138	−0.117
	(0.043)	(0.058)	(0.046)
Panel E: Include control for time served	−0.137	−0.132	−0.124
	(0.037)	(0.053)	(0.038)
Panel F: Exclude probation revocation	−0.129	−0.122	−0.095
	(0.041)	(0.054)	(0.040)
Panel G: Limited criminal histories	−0.134	−0.134	−0.109
	(0.051)	(0.065)	(0.051)
Panel H: Repeat capture	−0.201	−0.234	−0.215
	(0.071)	(0.070)	(0.060)
Panel I: Two-stage least squares	−0.127	−0.096	−0.074
	(0.050)	(0.067)	(0.069)
Panel J: 24/7 operational at 10 percent DUI enrollment	−0.148	−0.145	−0.128
	(0.043)	(0.054)	(0.045)
Panel K: 24/7 operational at 40 percent DUI enrollment	−0.134	−0.126	−0.103
	(0.044)	(0.052)	(0.040)
Panel L: Exclude police per capita from covariates	−0.139	−0.138	−0.118
	(0.044)	(0.056)	(0.045)

Notes: The first row of each panel displays the average marginal effect of 24/7 on rearrest/revocation from bivariate probit models where control variables for panels A through F, H, and I include gender, age, categorical indicators for number of prior arrests (top-coded at 10), an indicator for DUI-3 (versus DUI-2), indicators for prior arrest for violent or drug-related crimes, indicators for the observation's decile rank for days between prior DUI offenses, and county-level police per capita, liquor stores per capita, bars per capita, unemployment, and county and month fixed effects. Panel G excludes categorical indicators for number of prior arrests (top-coded at 10) and indicators of prior arrests for violent or drug-related crimes. The second row displays standard errors clustered at the county level. Samples sizes for all models are n = 19,119 at 12 months; n = 16,886 at 24 months; at n = 14,518 for 36 months, with the following exceptions: Panel B n = 14,518 at 12, 24, and 36 months; and Panel C n = 18,737 at 12 months; n = 16,584 at 24 months; and n = 14,320 at 36 months.

criminal history information. The results remain virtually unchanged, suggesting that it is not critical to include criminal history data when the time since the previous DUI is available; however, if researchers rely on Department of Motor Vehicle data only, they cannot examine whether 24/7 or another intervention influenced crimes not related to driving.

Panel H includes an additional indicator for repeat individual-level occurrences (i.e., persons captured in the data for both DUI-2 and DUI-3). Individuals may enter the dataset twice if they are arrested for DUI-2 and DUI-3 between 2004 and 2012 and are not assigned to 24/7 twice, because we include only first-time 24/7 participants. The latter condition is rare, given that 24/7 becomes and remains ubiquitous for repeat-DUI arrestees in nearly all counties where its enrollment rate exceeds our 25 percent threshold. Repeat entrance is strongly positively related to rearrest for the three time periods, suggesting these individuals are at higher risk of recidivism. After accounting for repeat entrants, the estimated reduction in rearrest due to 24/7 is in the range of 21 to 23 percentage points for the three time periods, all with $p < 0.01$.

Our preferred specifications use seemingly unrelated bivariate probit models to address selection issues related to 24/7 participation. A common and consistent alternative is to use two-stage least squares, in this case estimating a linear probability model (LPM) in each stage. Point estimates from this method are typically similar to those produced under bivariate probit (Angrist & Pischke, 2009), with generally larger confidence intervals. In this specific case of LPM in both stages, the standard errors on our LATE estimates are robust to heteroscedasticity and are less precise, at least in part due to the non-linear nature of the outcome, policy, and instrument variables. This does not mean they are falsely imprecise. Rather, this sensitivity analysis is an important and relatively conservative check on our main results. Panel I of Table 5 displays the results for this alternative approach. While the absolute values of the marginal effects get slightly smaller for the 12-month run (-0.127 versus -0.137), it remains statistically significant ($p = 0.010$). Though the direction and magnitude of the effects at 24 and 36 months suggest a substantive effect, they are much less precise. For this reason, we interpret our main results for 24 and 36 months with caution.

In panels J and K, we test the sensitivity of our results to alternative definitions of when 24/7 is considered operational in a county. Recall that our main specification considers 24/7 operational when the number of 24/7 participants in any given month equals or exceeds 25 percent of the 12-month moving average count of DUI arrests in that county. When we use a less conservative 10 percent threshold, the effect becomes somewhat larger. With the more conservative 40 percent threshold, the effect is smaller and less precise but remains similar.

Finally, we consider the possibility that the number of police per capita is decided endogenously. We chose to include this measure as a proxy for crime detection risk. Ostensibly, the probability of getting caught for a crime is positively related to how heavily a county is policed, but by that logic it is possible that more arrests lead to higher rates of both arrest and 24/7 enrollment. In that sense, there could be an endogeneity problem, but we believe this to be unlikely. We exclude police per capita to estimate the program's impact in panel L. The results do not differ notably from our preferred specification.

Subgroup Analyses

This section presents the results of various subgroup analyses intended to help us get a better understanding of the causal mechanisms driving our main results. Table 6 presents results from our preferred IV specifications separately for DUI-2 and DUI-3 arrestees. On average, we would expect DUI-3 arrestees to have more problems with alcohol than DUI-2 arrestees. Thus, if 24/7 is significantly reducing alcohol consumption for participants, we would expect to see less drinking and, thus, less criminal activity from DUI-3 arrestees compared with those who had been arrested for DUI only twice.

At 12 months after DUI arrest, the coefficient on 24/7 is negative and statistically significant ($p < 0.05$) for both DUI-2 and DUI-3 arrestees. The coefficients remain negative for DUI-2 at 24 and 36 months, but they become less precise and no longer statistically significant. The story is much different for DUI-3 arrestees, for which the coefficient remains negative and statistically significant at 24 and 36 months. These results are consistent with the hypothesis that 24/7 reduces alcohol consumption and has a larger effect on the criminal behavior of those with more-serious alcohol problems.

The fact that the coefficients indicate a longer-lasting effect of 24/7 for DUI-3 arrestees may be due to their longer participation: The median time on the program is more than 40 percent longer for these individuals compared with DUI-2 arrestees;

Table 6. Results by DUI level.

	(1)		(2)	
	DUI-2		DUI-3	
	Marginal Effect	N	Marginal Effect	N
12 months	−0.082	12,929	−0.163	6,190
	(0.037)		(0.076)	
24 months	−0.068	11,437	−0.163	5,449
	(0.060)		(0.065)	
36 months	−0.064	9,871	−0.168	4,647
	(0.053)		(0.061)	

Notes: This table displays the average marginal effect of 24/7 on rearrest/revocation from bivariate probit models that include controls for gender, age, categorical indicators for number of prior arrests (top-coded at 10), indicators for prior arrest for violent or drug-related crimes, indicators for the observation's decile rank for days between prior DUI offenses, and county-level police per capita, liquor stores per capita, bars per capita, unemployment, and county and month fixed effects. Standard errors are in parentheses and clustered at the county level.

Table 7. Results by arrest category.

	(1)		(2)		(3)	
	DUI		Violent Crime		Property Crime	
	Marginal Effect	N	Marginal Effect	N	Marginal Effect	N
12 months	−0.073	19,119	−0.026	19,119	−0.015	19,119
	(0.017)		(0.016)		(0.013)	
24 months	−0.064	16,886	−0.019	16,886	−0.009	16,886
	(0.032)		(0.013)		(0.013)	
36 months	−0.018	14,518	−0.017	14,518	0.007	14,518
	(0.031)		(0.014)		(0.017)	

Notes: This table displays the average marginal effect of 24/7 on rearrest/revocation from bivariate probit models that include controls for gender, age, categorical indicators for number of prior arrests (top-coded at 10), indicators for prior arrest for violent or drug-related crimes, indicators for the observation's decile rank for days between prior DUI offenses, and county-level police per capita, liquor stores per capita, bars per capita, unemployment, and county and month fixed effects. Standard errors are in parentheses and clustered at the county level.

however, this is not necessarily the reason for the stronger effects. Participation length may be extended for those who are not doing well in the program (and shortened for those doing well) based on testing violations but who have not been arrested and thus remain under monitoring rather than being incarcerated. That said, our main results, robustness checks, and this analysis are consistent with the contention that the effect of 24/7 on criminal activity extends beyond the time the individuals are in the program—especially for DUI-3 arrestees.

While Tables 3 through 6 focused on the probability of any type of arrest or probation violation, Table 7 looks at the effect of 24/7 on various types of arrests: DUI, violent crime, and property crime based on the Federal Bureau of Investigation definitions and four-digit National Crime Information Center (NCIC) Uniform Offense Classification Codes. Looking exclusively at the next recorded arrest to avoid confounding due to competing risks, of the 8,430 arrests observed in the three-year sample, 4,726 (56.1 percent) were for a subsequent DUI, 526 (6.2 percent) were for a violent crime, and 559 (6.6 percent) were for a property crime (theft, burglary,

larceny, destruction of property, vandalism, and arson). If 24/7 is reducing criminal activity via a reduction in alcohol consumption, we would expect to see larger effects for crimes with a stronger connection to alcohol. Thus, we hypothesized that the effect would be largest for DUI, and this is what we observe. The marginal effect sizes are smaller in absolute magnitude for violent arrests, but all are negative and the −2.6 percentage point (9.5 percent) effect at 12 months has a p-value < 0.10. The effects for property crime are very imprecise and much closer to zero.

CONCLUSION

While there is strong agreement that we should not depend on increasing severity to produce criminal deterrence (Chalfin & McCrary, 2017), there is less agreement about how to best incorporate certainty and celerity, especially in the case of community corrections. South Dakota's 24/7 Sobriety Program offers a new approach by requiring that alcohol-involved offenders abstain from alcohol, be tested for alcohol multiple times per day, and be rapidly sanctioned after testing positive or missing a test. The sanction is typically a night or two in jail.

Using variation in the timing of 24/7's implementation across counties in an instrumental variable bivariate probit model, we find strong evidence that 24/7 participation reduced criminal activity at 12 months after the initial arrest, and perhaps longer. These findings provide support for "swift-certain-fair" approaches to applying sanctions in community supervision. They also provide policymakers with evidence for a new approach to reduce criminal activity among those whose alcohol use leads them to repeatedly threaten public health and safety.

While the sign on the estimated effect is not surprising given previous community-level analyses of the program, the magnitude of the individual-level effect and the fact there appears to be a residual effect beyond the period of participation for some individuals is particularly noteworthy. South Dakota's 24/7 program is also attractive from a public budgeting perspective because its fiscal costs are covered by participant fees (Midgette, 2014), and it does not require treatment participation or any other formal programming. Counties with few participants may run short-term fiscal deficits when per-participant revenue fails to cover fixed costs; however, the state also produces revenue for each participant enrolled. The program maintains an official mechanism by which it reimburses counties that demonstrate operating losses or need for infrastructure improvement related to the 24/7 program. As a substance use intervention with relatively low cost and low administrative burden, the program is notable.

Since 24/7 was widely adopted in South Dakota and county-level analyses have been published, we can work backward from these figures to assess the plausibility of our individual-level results. Kilmer et al. (2013) conservatively estimated that county-level adoption of 24/7 was associated with a 12 percent reduction in repeat DUI arrests at the county level (they found no effect on first-time DUI arrests). But, as noted earlier in the text, only about one-third of DUI-2 and DUI-3 arrestees in counties with operational 24/7 programs end up participating in 24/7 (27.7 percent participated in 2006, and this increased to 38.6 percent in 2011). Considering that the bulk of 24/7 in its early existence was DUI-2 and DUI-3 arrestees, if we assumed that tripling the participation rate to 100 percent would triple the effect size at the county level, we would conclude that full participation by DUI-2 and DUI-3 arrestees would be associated with a 36 percent reduction in repeat DUI arrests. This conservative 36 percent reduction is in the same ballpark as the reductions in DUI arrests at 12 and 24 months reported in Table 7, which correspond to 34 to 57 percent reductions, respectively; this suggests that our causal estimates are in line with other analyses using different data and methods. This also suggests that much

of the county-level association estimated by Kilmer et al. (2013) is through specific deterrence.

The current analysis cannot distinguish the extent to which the specific deterrent effect is coming through certainty of sanction versus celerity. We also cannot rule out the possibility of some general deterrent effect. Given our research design, if there is spillover effect of 24/7 onto non-participants, it would moderate the effect size we see in models that do not account for program availability and, implicitly, would-be drunk drivers' knowledge of the program's use in their county. Our instrument explicitly accounts for program availability. Given the marginally larger effects estimated by the instrumental variable model over the probit estimates, this is a possibility. Alternatively, the difference in effect sizes may be capturing unobserved participant characteristics that are associated with higher rearrest risk. These possibilities are not mutually exclusive.

Further, if 24/7 participants spent more time behind bars than the controls did, some of the observed effect could be coming from incapacitation rather than deterrence. We attempt to address this with a sensitivity analysis that controls number of days the person was incarcerated for conviction on the DUI arrest, and the results remain unchanged; however, we are unable to specifically control for time spent behind bars before conviction. Because approximately half of 24/7 participants violated the program at least once, we know that these individuals typically spent a night or two behind bars while on the program. It is very unlikely that this incapacitation is driving our results, especially because our outcome windows range from 12 to 36 months, but we cannot rule out that it may have had an effect.

Another limitation to this analysis is that we do not have detailed information about how 24/7 was implemented in each county, nor do we know whether officials tasked with the decision to assign candidates to 24/7 are truly monotonic as assumed. Recent research suggests this common assumption may be tenuous (Frandsen, Lefgren, & Leslie, 2019). Similarly, we do not have information about whether counties changed how they addressed those who did not participate in 24/7 during our study period (e.g., were counties more likely to assign these individuals to treatment for a substance use disorder). If some locations reduced certainty or celerity of sanction over time or improved how they addressed DUI offenders not in 24/7, our results may be conservative.

It is important to stress that 24/7 does not require participants to enter treatment or engage in other services. This seems to be largely a deterrent effect, although one mechanism through which that deterrence might work is giving participants a reason to seek treatment on their own, whether paid professional treatment or self-help (e.g., Alcoholics Anonymous). Without stronger understanding of the mechanism resulting in reduced alcohol consumption and crime through 24/7, we cannot assess the degree to which the program is a cost-effective substitute for substance use treatment or specialty courts for some participants. However, a less risky utilization of the policy is as a risk assessment tool; when capacity constraints exist for more rigorous substance use interventions, 24/7 test results can provide empirical evidence for prioritizing risk. A participant who repeatedly violates 24/7 provides quick and clear evidence that additional services are necessary. This is what Hawken (2010) refers to as "behavioral triage."

Although we cannot make direct comparisons, one review of DUI-treatment courts—which are much more resource-intensive than 24/7 and are spreading throughout the country—suggests they may reduce the risk of arrest for any type of offense by roughly 25 percent (Mitchell et al., 2012).[8] Our 24/7 results for DUI-3

[8] Note that some proponents of DUI courts like to focus on what they call "top courts" that have a better recidivism rate. A one-page factsheet from the National Center for DWI Courts (2016) notes that

arrestees are in the same ballpark (12 months equals 52 percent; 24 months equals 38 percent; 36 months equals 33 percent). It would be extremely informative to randomly assign those convicted of repeat DUI to 24/7 or to another intervention (e.g., DUI court or IID) and compare the costs and benefits of these approaches (including effects on outcomes unrelated to driving). Additional insights about the long-term and dose-response effects of 24/7 could be obtained by randomly assigning 24/7 participants to different lengths of time on the program.

Another area ripe for experimental evaluation is to determine which type of monitoring regime is most cost-effective: twice-daily breathalyzer tests, alcohol monitoring bracelets, remote breath devices, kiosks with facial recognition software, or a different technology. Because those traveling to the testing facility could receive positive reinforcement from the testers for passed tests (e.g., "Nice job; see you tomorrow"), it would also be insightful to vary these interactions in future research to see how much they matter.

The findings from the HOPE DFE do raise questions about whether 24/7 would have similar effects on criminal justice outcomes outside of South Dakota. The program has now been implemented in several jurisdictions throughout the country, with large state programs starting in North Dakota in 2008 and Montana in 2010. Non-peer reviewed studies of the programs in these two states are promising (Kubas, Kayabas, & Vachal, 2015; Midgette et al., under review; Midgette & Kilmer, 2015), but there is a real need for experimental research to be conducted in urban areas outside of the Great Plains, and applications of certain sanctions to misconduct, or certain positive reinforcement for desired conduct, may be applicable to areas of criminal justice beyond drug and alcohol-related crime.[9]

While our results on criminal recidivism are striking, their importance extends beyond how we address DUI offenders. The growing bipartisan support for reducing reliance on long prison sentences to address nonviolent crime suggests there will be more reliance on probation and other forms of community supervision. Advances in technology will continue to make it easier and cheaper to monitor and detect violations (e.g., substance use, curfews, other place-based restrictions, interactions

"Top DWI courts reduce recidivism by 60%," and this statement is sourced with the same Mitchell et al. (2012) meta-analysis; however, the word "top" does not appear in the Mitchell et al. article. Mitchell et al. noted that three of the four experimental evaluations of DWI courts yielded positive results, and excluding the other experimental study raises the mean odds-ratio from 1.27 (confidence interval: 0.87 to 1.85) to 1.58 (confidence interval: 0.99 to 2.54). They conclude by "characteriz[ing] the evidence as cautiously supporting the effectiveness of DWI drug courts, because while quasi-experimental evaluations find strong and consistent indications that these programs reduce general and drug related recidivism, randomized experimental evaluations find a small, non-statistically significant reduction in recidivism. Yet, the findings from experimental evaluations of DWI drug courts are ambiguous in that the majority of these evaluations find positive effects but a single, influential evaluation with negative findings heavily influences the mean effect. Clearly, only additional evaluations using experimental methods can definitively resolve the remaining ambiguity surrounding the effectiveness of DWI drug courts."

[9] In 2014, the Mayor's Office for Policing and Crime in London, England implemented the Alcohol Abstinence Monitoring Requirement (AAMR) Pilot. The AAMR was influenced by South Dakota's 24/7, but differed in important ways: it exclusively used alcohol monitoring bracelets and did not use incarceration as a sanction (Bainbridge, 2019). Typically, the first violation was met with a warning and the second led to the initiation of a breach process where violators had to wait up to 25 days to reappear in front of a judge or magistrate who "selected a punishment from a range of modest options" (Bainbridge, 2019, p.1702). Despite the important differences from 24/7, the early data from the AAMR pilot were encouraging. Finlay and Humphreys (2017) noted that of the 111 individuals assigned to the AAMR pilot, "92% of offenders complied fully with alcohol abstinence; only nine people breached, four of whom ultimately completed their sentence" (52). While there was no formal comparison group for the analysis, the numbers are encouraging, and the pilot was expanded in London. In fall 2019, it was announced that all judges throughout England and Wales could require those convicted of alcohol-involved offenses to abstain from alcohol and wear the bracelets (Drury & Doyle, 2019).

with others under community supervision, and possibly even firearm usage . Ultimately, the ability to deter violations depends on how this information is used. To this end, deterring violations with SCF sanctions may be a promising approach for these opportunities.

BEAU KILMER is Director of the RAND Drug Policy Research Center, 1776 Main Street, Santa Monica, CA 90407–2138 (e-mail: kilmer@rand.org).

GREG MIDGETTE is an Assistant Professor in the Department of Criminology and Criminal Justice at the University of Maryland, 2220 Samuel J. LeFrak Hall, 7251 Preinkert Drive, College Park, MD 20742 (e-mail: gem@umd.edu).

ACKNOWLEDGMENTS

This research was supported by the National Institute on Alcohol Abuse and Alcoholism (R01AA020074 and R01AA024296). We thank Jonathan Caulkins, Paul Heaton, Keith Humphreys, Nancy Nicosia, Steve Raphael, Peter Reuter, and Rosanna Smart, as well as presentation attendees at the Alcohol Research Group, American Society of Health Economists, Association for Public Policy Analysis and Management, International Society for the Study of Drug Policy, and National Bureau of Economic Research Summer Institute, for valuable comments and suggestions. The manuscript was also improved by the feedback we received from the editors and three anonymous reviewers. We would also like to thank the Office of the South Dakota Attorney General and the alcohol testing companies (Alcohol Monitoring Systems and Intoximeters) for graciously providing data for this analysis. The views presented here represent only those of the authors.

REFERENCES

Ainslie, G., & Haslam, N. (1992). Hyperbolic discounting. In G.Loewenstein & J.Elster (Eds.), Choice over time (pp.57–92). Russell Sage Foundation.

Angrist, J., & Pischke, J.-S. (2009). Mostly harmless econometrics: An empiricist's companion. Princeton, NJ: Princeton University Press.

Bainbridge, L. (2019). Transferring 24/7 sobriety from South Dakota to South London: The case of MOPAC's Alcohol Abstinence Monitoring Requirement Pilot. Addiction, 114, 1696–1705.

Beccaria, C. marchese di. (1764). On crimes and punishments (1st ed.). Hackett Pub. Co.

Beitel, G. A., Sharp, M. C., & Glauz, W. D. (2000). Probability of arrest while driving under the influence of alcohol. Injury Prevention, 6, 158–161.

Bentham, J. (1789). An introduction to the principles of morals and legislation. J. H.Burns & H. L. A.Hart (Eds.). London: Athlone Publisher.

Blincoe, L., Miller, T., Zaloshnja, E., & Lawrence, B. (2015). The economic and societal impact of motor vehicle crashes, 2010. (Revised). Washington, DC: National Highway Traffic Safety Administration (NHTSA).

Bouchery, E. E., Harwood, H. J., Sacks, J. J., Simon, C. J., & Brewer, R. D. (2011). Economic costs of excessive alcohol consumption in the US, 2006. American Journal of Preventive Medicine, 41, 516–524.

Brinkmann, B., Beike, J., Köhler, H., Heinecke, A., & Bajanowski, T. (2002). Incidence of alcohol dependence among drunken drivers. Drug and Alcohol Dependence, 66, 7–10.

Bureau of Labor Statistics. (n.d.). Local Area Unemployment Statistics. Washington, DC: Bureau of Labor Statistics. Available at https://www.bls.gov/lau/.

Bushman, B. J., & Cooper, H. M. (1990). Effects of alcohol on human aggression: An integrative research review. Psychological Bulletin, 107, 341.

Bushway, S. D., & Owens, E. G. (2013). Framing punishment: Incarceration, recommended sentences, and recidivism. The Journal of Law and Economics, 56, 301–331.

Carpenter, C., & Dobkin, C. (2010). Alcohol regulation and crime. In Controlling crime: Strategies and tradeoffs (pp.291–329). Chicago, IL: University of Chicago Press.

Chalfin, A., & McCrary, J. (2017). Criminal deterrence: A review of the literature. Journal of Economic Literature, 55, 5–48.

Comoreanu, A. (2017). Strictest and most lenient states on DUI. WalletHub. Retrieved January 15, 2020, from https://wallethub.com/edu/dui-penalties-by-state/13549/#detailed-findings.

Cook, P. J. (2011). Paying the tab: The costs and benefits of alcohol control. Princeton, NJ: Princeton University Press.

Cook, P. J. (2016). Behavioral science critique of HOPE. Criminology & Public Policy, 15, 1155–1161.

Cowell, A. J., Barnosky, A., Lattimore, P. K., Cartwright, J. K., & DeMichele, M. (2018). Economic evaluation of the HOPE demonstration field experiment. Criminology & Public Policy, 17, 875–899.

Cullen, F. T., Pratt, T. C., Turanovic, J. J., & Butler, L. (2018). When bad news arrives: Project HOPE in a post-factual world. Journal of Contemporary Criminal Justice, 34, 13–34.

Drury, I., & Doyle, J. (2019, September 30). Drink-drivers and other criminals convicted of alcohol-related violence will be tagged with "sobriety bracelets" that measure their booze consumption. Daily Mail. Retrieved October 4, 2019, from https://www.dailymail.co.uk/news/article-7522641/People-convicted-alcohol-related-crime-tagged-sobriety-bracelets.html.

Dugosh, K. L., Festinger, D. S., & Marlowe, D. B. (2013). Moving beyond BAC in DUI. Criminology & Public Policy, 12, 181–193.

Duriez, S. A., Cullen, F. T., & Manchak, S. M. (2014). Is Project HOPE creating a false sense of hope? A case study in correctional popularity. Federal Probation, 78, 57–62.

Elder, R. W., Voas, R., Beirness, D., Shults, R. A., Sleet, D. A., Nichols, J. L., Compton, R., & Task Force on Community Preventive Services. (2011). Effectiveness of ignition interlocks for preventing alcohol-impaired driving and alcohol-related crashes: A Community Guide systematic review. American Journal of Preventive Medicine, 40, 362–376.

Federal Bureau of Investigation (FBI). United States Department of Justice. (2017). Uniform crime reporting program data: Police employee (LEOKA) data, 2015. Inter-University Consortium for Political and Social Research [distributor].

Federal Bureau of Investigation (FBI). United States Department of Justice. (2019). Crime in the United States, 2018. Retrieved August 9, 2019, from https://ucr.fbi.gov/crime-in-the-u.s/2018/crime-in-the-u.s.-2018/topic-pages/tables/table-29.

Finlay, I. G., & Humphreys, K. (2017). Mandatory sobriety programmes for alcohol-involved criminal offenders. Journal of the Royal Society of Medicine, 110, 52–53.

Frandsen, B. R., Lefgren, L. J., & Leslie, E. C. (2019). Judging judge fixed effects. NBER Working Paper Series, No. 25528. Cambridge, MA: National Bureau of Economic Research.

Government Accountability Office. (2014). Traffic safety: Alcohol ignition interlocks are effective while installed; less is known about how to increase installation rates (GAO-14-559). Washington, DC: Government Accountability Office.

Greene, W. H. (2011). Econometric analysis. Upper Saddle River, NJ: Pearson/Prentice Hall.

Greenfeld, L. A. (1998). Alcohol and crime: An analysis of national data on the prevalence of alcohol involvement in crime. National Criminal Justice (NCJ) Reference No. 168632. Washington, DC: Bureau of Justice Statistics. Retrieved June 5, 2019, from https://www.bjs.gov/content/pub/pdf/ac.pdf.

Hansen, B. (2015). Punishment and deterrence: Evidence from drunk driving. American Economic Review, 105, 1581–1617.

Hawken, A. (2010). Behavioral triage: A new model for identifying and treating substance-abusing offenders. Journal of Drug Policy Analysis, 3.

Hawken, A. (2016). All implementation is local. Criminology & Public Policy, 15, 1229–1239.

Hawken, A. (2018). Economic implications of HOPE from the demonstration field experiment. Criminology & Public Policy, 17, 901–906.

Hawken, A., & Kleiman, M. (2009). Managing drug involved probationers with swift and certain sanctions: Evaluating Hawaii's HOPE: Executive summary. Washington, DC: National Criminal Justice Reference Services.

Hawken, A., Kulick, J., Smith, K., Mei, J., Zhang, Y., Jarman, S., Yu, T., Carson, C., & Vial, T. (2016). HOPE II: A follow-up to Hawaii's HOPE evaluation. U.S. Department of Justice Grant Report. Retrieved June 2, 2019, from https://www.ncjrs.gov/pdffiles1/nij/grants/249912.pdf.

Heaton, P., Kilmer, B. G., & Nicosia, N. (n.d.). Deterring crime in community supervision: Evidence from 24/7 Sobriety. Working Paper.

Humphreys, K., & Kilmer, B. (Forthcoming). Still HOPEful: Discussion of a "failed" replication of the swift, certain, and fair approach to reducing substance use among individuals under criminal justice supervision. Addiction.

Imbens, G. W., & Angrist, J. D. (1994). Identification and estimation of local average treatment effects. Econometrica, 62, 467–475.

Kaebel, D., Glaze, L., Tsoutis, A., & Minton, T. D. (2016). Correctional population in the United States. National Criminal Justice (NCJ) Reference No. 249513. Washington, DC: Bureau of Justice Statistics.

Kilmer, B., Nicosia, N., Heaton, P., & Midgette, G. (2013). Efficacy of frequent monitoring with swift, certain, and modest sanctions for violations: Insights from South Dakota's 24/7 Sobriety Project. American Journal of Public Health, 103, e37–e43.

Kleiman, M. A. (2009). When brute force fails: How to have less crime and less punishment. Princeton, NJ: Princeton University Press.

Kleiman, M. A. R. (1997). Coerced abstinence: A neo-paternalistic drug policy initiative. In L. M.Mead (Ed.), The new paternalism: Supervisory approaches to poverty (pp. 182–219). Washington, DC: Brookings Institution Press.

Kleiman, M. A. R. (2016). Swift–Certain–Fair: What do we know now, and what do we need to know? Criminology & Public Policy, 15, 1185–1193.

Kleiman, M. A. R., Kilmer, B., & Fisher, D. T. (2014). Theory and evidence on the Swift-Certain -Fair approach to enforcing conditions of community supervision. Federal Probation, 78, 71–74.

Klepper, S., & Nagin, D. (1989). Tax compliance and perceptions of the risks of detection and criminal prosecution. Law & Society Review, 23, 209–240.

Kubas, A., Kayabas, P., & Vachal, K. (2015). Assessment of the 24/7 Sobriety Program in North Dakota: Participant behavior during enrollment. Fargo, ND: Upper Great Plains Transportation Institute at North Dakota State University.

Lattimore, P. K., MacKenzie, D. L., Zajac, G., Dawes, D., Arsenault, E., & Tueller, S. (2016). Outcome findings from the HOPE Demonstration Field Experiment. Criminology & Public Policy, 15, 1103–1141.

Loeffler, C. E. (2014). Detecting gunshots using wearable accelerometers. PloS One, 9, e106664.

Loewenstein, G. (1987). Anticipation and the valuation of delayed consumption. The Economic Journal, 97, 666–684.

Long, L. (2009). The 24/7 Sobriety Project. Public Law, 17, 2.

Loudenberg, R., Drubbe, G., & Leonardson, G. (2013). South Dakota 24/7 Sobriety Program. Evaluation findings report. Salem and Sioux Falls, SD: Mountain Plains Evaluation, LLC.

Loughran, T. A., Paternoster, R., & Weiss, D. (2012). Hyperbolic time discounting, offender time preferences and deterrence. Journal of Quantitative Criminology, 28, 607–628.

Loughran, T. A., Pogarsky, G., Piquero, A. R., & Paternoster, R. (2012). Re-examining the functional form of the certainty effect in deterrence theory. Justice Quarterly, 29, 712–741.

McClure, S. M., & Bickel, W. K. (2014). A dual-systems perspective on addiction: Contributions from neuroimaging and cognitive training. Annals of the New York Academy of Sciences, 1327, 62–78.

McClure, S. M., Laibson, D. I., Loewenstein, G., & Cohen, J. D. (2004). Separate neural systems value immediate and delayed monetary rewards. Science, 306, 503.

Midgette, G. (2014). Monitoring with swift, certain, and moderate sanctions to reduce alcohol-related crime: The South Dakota 24/7 Sobriety Program. Santa Monica (CA): RAND RGSD-339. Retrieved March 18, 2020, from https://www.rand.org/pubs/rgs_dissertations/RGSD339.html.

Midgette, G., & Kilmer, B. (2015). The effect of Montana's 24/7 Sobriety program on DUI re-arrest. Santa Monica (CA): RAND WR-1083-MHP. Retrieved March 18, 2020, from https://www.rand.org/pubs/working_papers/WR1083.html.

Midgette, G., Kilmer, B. G., Nicosia, N., & Heaton, P. (Under Review). A natural experiment to test the effect of sanction certainty and celerity on substance-impaired driving: North Dakota's 24/7 Sobriety Program.

Miller, p. G., Curtis, A., Sønderlund, A., Day, A., & Droste, N. (2015). Effectiveness of interventions for convicted DUI offenders in reducing recidivism: A systematic review of the peer-reviewed scientific literature. The American Journal of Drug and Alcohol Abuse, 41, 16–29.

Mitchell, O., Wilson, D. B., Eggers, A., MacKenzie, D. L., & Mitchell, O. (2012). Drug courts' effects on criminal offending for juveniles and adults. Campbell Systematic Reviews, 4.

Moselhy, H. F., Georgiou, G., & Kahn, A. (2001). Frontal lobe changes in alcoholism: A review of the literature. Alcohol and Alcoholism, 36, 357–368.

Nagin, D. S. (2013). Deterrence in the twenty-first century. Crime and Justice, 42, 199–263.

Nagin, D. S. (2016). Project HOPE. Criminology & Public Policy, 15, 1005–1007.

National Academies of Sciences, Engineering, and Medicine. (2018). Getting to zero alcohol-impaired driving fatalities: A comprehensive approach to a persistent problem. Washington, DC: The National Academies Press.

National Research Council. (2014). The growth of incarceration in the United States: Exploring causes and consequences. J.Travis, B.Western, & F.Redburn (Eds.). Washington, DC: The National Academies Press.

Nicosia, N., Kilmer, B., & Heaton, P. (2016). Can a criminal justice alcohol abstention programme with swift, certain, and modest sanctions (24/7 Sobriety) reduce population mortality? A retrospective observational study. The Lancet Psychiatry, 3, 226–232.

NTSB. (2000). Actions to reduce fatalities, injuries, and crashes involving the hard core drinking driver [Safety Report]. Washington, DC: National Transportation Safety Board.

Osilla, K. C., Paddock, S. M., Leininger, T. J., D'Amico, E. J., Ewing, B. A., & Watkins, K. E. (2015). A pilot study comparing in-person and web-based motivational interviewing among adults with a first-time DUI offense. Addiction Science & Clinical Practice, 10, 18.

Peterson, J. B., Rothfleisch, J., Zelazo, P. D., & Pihl, R. O. (1990). Acute alcohol intoxication and cognitive functioning. Journal of Studies on Alcohol, 51, 114–122.

Petry, N. M., Martin, B., Cooney, J. L., & Kranzler, H. R. (2000). Give them prizes and they will come: Contingency management for treatment of alcohol dependence. Journal of Consulting and Clinical Psychology, 68, 250.

Pogarsky, G., Roche, S. P., & Pickett, J. T. (2018). Offender decision-making in criminology: Contributions from behavioral economics. Annual Review of Criminology, 1, 379–400.

Rand, M. R., Sabol, W. J., Sinclair, M., & Snyder, H. (2010). Alcohol and crime: Data from 2002 to 2008. National Criminal Justice (NCJ) Reference No. 231685. Washington, DC: Bureau of Justice Statistics. Retrieved June 2, 2019, from https://www.ncjrs.gov/App/Publications/abstract.aspx?ID=253763.

Rapid City Journal. (2003, July 27). Governor's corrections group meets. Retrieved June 2, 2019, from http://rapidcityjournal.com/news/local/governor-s-corrections-group-meets/article_8966cd51-fbd2-573e-ade4-420be97e9c6a.html.

Roache, J. D., Karns, T. E., Hill-Kapturczak, N., Mullen, J., Liang, Y., Lamb, R. J., & Dougherty, D. M. (2015). Using transdermal alcohol monitoring to detect low-level drinking. Alcoholism: Clinical and Experimental Research, 39, 1120–1127.

Rogers, P. N. (1997). Specific deterrent impact of California's 0.08% blood alcohol concentration limit and administrative per se license suspension laws. Retrieved June 2, 2019, from http://dmv.ca.gov/portal/wcm/connect/6b69e326-009d-4f1b-8103-879ec4b30426/S5-158.pdf?MOD=AJPERES&CONVERT_TO=url.

Royal, D. (2003). National Survey of Drinking and Driving Attitudes and Behavior: 2001 [Volume I: Summary Report]. Washington, DC: National Highway Traffic Safety Administration.

Simpson, H. M., Beirness, D. J., Robertson, R. D., Mayhew, D. R., & Hedlund, J. H. (2004). Hard core drinking drivers. Traffic Injury Prevention, 5, 261–269.

Sloan, F. A., Eldred, L. M., & Xu, Y. (2014). The behavioral economics of drunk driving. Journal of Health Economics, 35, 64–81.

South Dakota Office of the Attorney General. (n.d.). Example Protocol—Brookings County Policies and Procedures. Retrieved January 15, 2020, from https://atg.sd.gov/docs/Protocol.pdf.

Swift Certain Fair Resource Center. (n.d.). Programs Swift Certain Fair Resource Center. Retrieved January 15, 2020, from https://scfcenter.org/programs/.

Truman, J., & Langton, L. (2015). Criminal victimization, 2014. Washington, DC: Bureau of Justice Statistics. Retrieved June 2, 2019, from http://www.bjs.gov/content/pub/pdf/cv14.pdf.

United States Census. (2018). County business patterns. Washington, DC: Department of Commerce. Retrieved May 3, 2019, from https://www.census.gov/programs-surveys/cbp.html.

Voas, R. B. (2014). Enhancing the use of vehicle alcohol interlocks with emerging technology. Alcohol Research: Current Reviews, 36, 81.

Wells-Parker, E., Bangert-Drowns, R., McMillen, R., & Williams, M. (1995). Final results from a meta-analysis of remedial interventions with drink/drive offenders. Addiction, 90, 907–926.

Willis, C., Lybrand, S., & Bellamy, N. (2004). Alcohol ignition interlock programmes for reducing drink driving recidivism. Cochrane Database of Systematic Reviews, 4.

Wooldridge, J. M. (2010). Econometric analysis of cross section and panel data. Cambridge, MA: MIT press.

APPENDIX A: COMPARING DUI ARREST COUNTS

The data used in this analysis are drawn from the South Dakota Attorney General's Office Criminal History Database. The data include detailed information for all arrestees charged with DUI covering the period of analysis, 2004 to 2012. These data include arrest date, arresting agency, charges, incarceration time served and suspended, and final disposition for all publicly disclosed cases involving each DUI arrestee—that is, their complete criminal history in South Dakota. To verify that the data we received are a census of DUI arrests, we compare the annual count of DUI arrest events to DUI totals published by the South Dakota Department of Public Safety (DPS) (2017). The data are largely similar but not identical from year to year (see Table A1). The net difference may be due to expungement prior to the attorney general's report, the timing of data recording for the DPS report and criminal history data, or other reasons. In total, the criminal history database reports 2,568 (2.8 percent) more arrests than the DPS report does.

Table A1. Comparing DUI arrest counts by source.

Fiscal Year	Criminal History Database	Department of Public Safety Report
2004	10,582	9,049
2005	11,792	10,174
2006	12,179	11,282
2007	11,284	11,756
2008	10,395	11,029
2009	9,635	10,147
2010	9,183	9,246
2011	9,416	8,744
2012	8,723	9,194

APPENDIX B: TABLE 4 BIVARIATE PROBIT ESTIMATES WITH COVARIATES

Table B1. Model estimates for bivariate probit model 3.

	12 months	24 months	36 months
Equation 1 (Rearrest)			
24/7	−0.471**	−0.407*	−0.334**
	(0.151)	(0.167)	(0.130)
DUI-3 participant	−0.132	−0.124	−0.131*
	(0.074)	(0.065)	(0.061)
Male	0.0329	0.0279	0.0176
	(0.019)	(0.017)	(0.019)
Age	−0.0109***	−0.0136***	−0.0150***
	(0.001)	(0.001)	(0.001)
Police per Capita	−0.00947	−0.00146	0.00448
	(0.009)	(0.008)	(0.011)
Bars per Capita	−0.0311	−0.0392**	−0.0434**
	(0.016)	(0.014)	(0.016)
Retail Alcohol Outlets per Capita	0.0660*	0.0254	0.0114
	(0.027)	(0.024)	(0.020)
Unemployment Rate	−0.0094	−0.0105	−0.0425
	(0.025)	(0.022)	(0.024)
Prior Offenses			
(1 is omitted)	0.0329	0.0279	0.0176
	(0.019)	(0.017)	(0.019)
2	0.218***	0.229***	0.266***
	(0.027)	(0.025)	(0.037)
3	0.402***	0.432***	0.447***
	(0.044)	(0.031)	(0.030)
4	0.527***	0.646***	0.674***
	(0.032)	(0.039)	(0.042)
5	0.553***	0.661***	0.716***
	(0.036)	(0.064)	(0.068)
6	0.672***	0.818***	0.854***
	(0.056)	(0.041)	(0.064)
7	0.797***	1.025***	1.118***
	(0.056)	(0.061)	(0.067)
8	0.853***	0.942***	1.037***
	(0.087)	(0.072)	(0.066)
9	0.807***	1.006***	1.150***
	(0.084)	(0.076)	(0.066)
10 or more	1.174***	1.341***	1.429***
	(0.083)	(0.056)	(0.037)
Prior Violent Crime Arrest	0.042	0.035	0.036
	(0.030)	(0.025)	(0.024)
Prior Drug Crime Arrest	0.129***	0.0996*	0.090
	(0.032)	(0.040)	(0.048)

Table B1. Continued.

	12 months	24 months	36 months
Days to DUI-2 (10th decile is omitted)			
1st decile	0.647***	0.618***	0.587***
	(0.075)	(0.073)	(0.070)
2nd	0.575***	0.609***	0.647***
	(0.079)	(0.112)	(0.064)
3rd	0.308***	0.339***	0.413***
	(0.084)	(0.077)	(0.068)
4th	0.339***	0.333**	0.377***
	(0.092)	(0.108)	(0.095)
5th	0.410***	0.378***	0.418***
	(0.074)	(0.064)	(0.064)
6th	0.231***	0.223***	0.165**
	(0.055)	(0.050)	(0.055)
7th	0.237***	0.262**	0.246**
	(0.066)	(0.097)	(0.081)
8th	0.129	0.14	0.156*
	(0.097)	(0.095)	(0.062)
9th	0.0246	0.0703	0.0831
	(0.082)	(0.070)	(0.075)
Days to DUI-2 (10th decile is omitted)			
1st decile	0.641***	0.641***	0.641***
	(0.073)	(0.052)	(0.051)
2nd	0.536***	0.552***	0.572***
	(0.057)	(0.062)	(0.068)
3rd	0.420***	0.421***	0.446***
	(0.044)	(0.048)	(0.052)
4th	0.314***	0.326***	0.276***
	(0.049)	(0.053)	(0.055)
5th	0.230***	0.325***	0.251***
	(0.067)	(0.071)	(0.050)
6th	0.189***	0.226***	0.269***
	(0.040)	(0.036)	(0.050)
7th	0.212***	0.274***	0.258***
	(0.052)	(0.042)	(0.047)
8th	0.123*	0.118*	0.132**
	(0.050)	(0.053)	(0.049)
9th	0.0288	0.0556	0.0208
	(0.056)	(0.065)	(0.058)
Equation 2 (24/7 Assignment)			
Program Available			
(25% Threshold)	0.903***	0.891***	0.847***
	(0.118)	(0.129)	(0.129)
DUI-3 participant	0.350***	0.381***	0.408***
	(0.083)	(0.093)	(0.096)
Male	−0.0987*	−0.110*	−0.0923*
	(0.042)	(0.046)	(0.046)
Age	−0.00054	−0.00194	−0.00173
	(0.001)	(0.001)	(0.001)

Table B1. Continued.

	12 months	24 months	36 months
Police per Capita	0.0304	0.0408	0.0636
	(0.020)	(0.029)	(0.038)
Bars per Capita	−0.00535	−0.00798	−0.0388
	(0.029)	(0.034)	(0.040)
Retail Alcohol Outlets per Capita	−0.0165	−0.00299	−0.00985
	(0.067)	(0.068)	(0.084)
Unemployment Rate	0.027	0.0111	−0.0601
	(0.038)	(0.041)	(0.049)
Prior Offenses (1 is omitted)			
2	0.032	0.0331	0.0552
	(0.027)	(0.029)	(0.033)
3	0.0362	0.0646	0.037
	(0.029)	(0.035)	(0.031)
4	0.0293	0.0476	0.102*
	(0.032)	(0.037)	(0.043)
5	−0.0567	−0.0481	−0.0111
	(0.035)	(0.037)	(0.046)
6	−0.0188	0.00439	0.0172
	(0.043)	(0.046)	(0.054)
7	−0.131*	−0.0972	−0.0703
	(0.061)	(0.075)	(0.095)
8	−0.139	−0.0906	−0.061
	(0.072)	(0.070)	(0.090)
9	−0.166*	−0.153	−0.103
	(0.070)	(0.092)	(0.067)
10 or more	−0.190***	−0.140**	−0.0991
	(0.049)	(0.052)	(0.073)
Prior Violent Crime Arrest	−0.0176	−0.00779	−0.0201
	(0.028)	(0.026)	(0.031)
Prior Drug Crime Arrest	−0.0788**	−0.0606*	−0.0358
	(0.029)	(0.028)	(0.038)
Days to DUI-2 (10^{th} decile is omitted)			
1st decile	−0.133	−0.108	−0.066
	(0.102)	(0.103)	(0.111)
2nd	−0.101	−0.0687	0.0306
	(0.095)	(0.089)	(0.090)
3rd	−0.154	−0.11	−0.0391
	(0.090)	(0.085)	(0.083)
4th	−0.0958	−0.0512	0.0695
	(0.119)	(0.129)	(0.174)
5th	0.0202	0.081	0.267*
	(0.117)	(0.116)	(0.126)
6th	0.00352	0.132*	0.279***
	(0.070)	(0.067)	(0.079)
7th	0.0941	0.241*	0.357***
	(0.096)	(0.096)	(0.108)
8th	0.184**	0.190**	0.173*
	(0.069)	(0.060)	(0.069)
9th	0.180*	0.143	0.146*
	(0.078)	(0.076)	(0.074)

Table B1. Continued.

	12 months	24 months	36 months
Days to DUI-2 (10^{th} decile is omitted)			
1^{st} decile	0.0443	0.0654	0.137
	(0.111)	(0.112)	(0.124)
2^{nd}	0.0105	0.0444	0.142
	(0.091)	(0.098)	(0.117)
3^{rd}	0.0923	0.107	0.177
	(0.093)	(0.104)	(0.116)
4^{th}	0.0578	0.0849	0.202*
	(0.076)	(0.075)	(0.086)
5^{th}	0.0673	0.0875	0.193*
	(0.076)	(0.073)	(0.087)
6^{th}	0.0386	0.0654	0.126
	(0.058)	(0.059)	(0.083)
7^{th}	0.0454	0.0816	0.105
	(0.058)	(0.058)	(0.062)
8^{th}	0.124*	0.109	0.134*
	(0.058)	(0.057)	(0.060)
9^{th}	0.0428	0.03	0.0385
	(0.043)	(0.049)	(0.048)

Notes: Standard errors are in parentheses and clustered at the county level. An indicator for an operational 24/7 program in county and month of arrest event is used to instrument 24/7 enrollment. The control variables for all models include gender, age, categorical indicators for number of prior arrests (top-coded at 10), an indicator for DUI-3 (versus DUI-2), indicators for prior arrest for violent or drug-related crimes, indicators for the observation's decile rank for days between prior DUI offenses, and county-level police per capita, bars per capita, liquor stores per capita, unemployment, and county and month fixed effects. *p<.05; **p<.01; ***p<.001.