Preventing Driving Under the Influence (DUI): An Evaluation of Mandatory Use of Ignition Interlock Devices Among Those Convicted of a DUI*

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Abstract

Ignition interlock devices (IIDs) are individualized breathalyzers installed in private vehicles that prevent people who have been drinking from starting or operating a vehicle. We evaluate the effectiveness of IIDs in preventing subsequent drunk driving and alcohol related crashes among persons convicted of driving under the influence exploiting policy variation within the state of California. Specifically, we exploit quasi-experimental variation in IID installation trends created by the simultaneous imposition of a statewide IID mandate for a subset of offenders and the expiration of an earlier pilot program that mandated IID installation for a broader group. Specifically, in 54 of California's 58 counties, a 2019 state law induced a sizable increase in the likelihood that a DUI arrest is followed by an IID installation within two years. In the remaining four counties, the expiration of an earlier pilot program caused a large decrease in IID installation rates. We perform a difference-in-difference analysis of the relative pre-post-2019 changes in recidivism rates across these county groups to test for an effect of IID installation on recidivism. Mandatory IID installation requirements cause moderate reductions in subsequent DUI arrests, alcohol-related crashes, and alcohol-related crashes with injury. Increasing device take-up would enhance the effectiveness of such mandates.

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1 Introduction

In 2022, 13,524 people in the United States died in a car crash where a driver was under the influence of alcohol,[1] with the number of injuries resulting from drunk-driving crashes many multiples the number of fatalities.[2] The same year, 16,485 people were murdered.[3] Hence, the scale of loss and suffering resulting from drunk driving in the United States is comparable in magnitude to that resulting from the most serious violent crime.

State efforts to combat drunk driving and the associated social harms rely heavily on sanctions for those convicted of driving under the influence (DUI), tighter definitions of DUI through lower blood alcohol content (BAC) thresholds for establishing guilt, and swift and certain responses to drunk driving arrests and convictions. Although many of the sanctions might be characterized as creating a specific deterrent to DUI (Hansen, 2015), there are several sanctions that basically aim to address individual drivers by restricting driving privileges or employing technology to prevent driving while intoxicated.

One such sanction is the requirement that arrested and/or convicted drivers install an ignition interlock device (IID) in their vehicles. IIDs prevent people who have been drinking from starting or operating a vehicle. Currently, 31 states and the District of Columbia require all drivers convicted of a DUI to install an IID for a period prior to full restoration of driving privileges. The remaining states either require IIDs following repeat-offense convictions, or permit judges in DUI cases the discretion to impose IID orders post conviction.

In 2019, California implemented a state law (SB 1046) mandating a period of IID installation after conviction for a repeat DUI offense or a DUI offense involving injury (first time offenses without injury were notably excluded from the mandate). The implementation of this new law coincided with the expiration of a pilot program in four California counties that required IID installation following *any* DUI conviction. Hence, in 54 of California's 58 counties, the 2019 law stiffened IID requirements following a DUI conviction, while in the four remaining counties, the coincident of the

[1] See National Highway Traffic Safety Administration (NHTSA), accessed on August 11, 2024.

[2] For example, in California in 2022, total injuries in drunk-driving crashes are nearly 27 times the number fatalities recorded in the Statewide Integrated Traffic Records System (SWITRS) (see Table 1).

[3] See Federal Bureau of Investigation, Crime Data Explorer, accessed on August 11, 2024.

expiration of the pilot and the new legislation effectively relaxed the IID requirement for first-time offenses without injury. Consequently, there was a large relative change in IID installation rates within two years of arrest, with an absolute decline in installation rates for DUI arrests occurring within the four pilot counties and an absolute increase in installation rates for DUI arrests occurring in the remainder of the state.

We exploit this policy induced change in installation rates to estimate the effect of IID installation on various measures of DUI recidivism. In an analysis of administrative records maintained by the California Department of Motor Vehicles, we perform a difference-in-difference analysis comparing installation rates and recidivism outcomes in counties where the requirements were tightened relative to counties where the requirements were relaxed. Regarding outcomes, we estimate relative changes in subsequent DUI arrests, subsequent alcohol-involved crashes, and subsequent alcohol-involved crashes with injury. We also use two-stage least squares to generate structural estimates of the effect of installing an IID on recidivism for the three outcomes.

We document a large absolute increase in installation rates among arrests occurring in the 54 counties that were not part of the earlier pilot program and a large absolute decrease in installation rates in the four counties that were part of the earlier pilot, yielding a relative change in installation between these two county groups of roughly 25 percentage points. We find corresponding changes in recidivism, with statistically significant relative decreases in all three recidivism measures coinciding with the relative increase in installation rates for DUI arrests in counties newly subjected to IID requirements. The magnitude of the two-stage-least squares estimates suggests that installing an IID within two years of the initial arrest reduces the likelihood of a new DUI arrest by 17 percent, an alcohol-related crash by 25 percent, and a alcohol-related crash with injury by 17 percent.

2 Prior Research on the Effectiveness of IID Requirements

An IID is essentially an individualized breathalyzer installed in one's vehicle that prevents a car from starting unless the driver blows a test where the blood alcohol content (BAC) reading is below a set level (for example, below 0.02 grams per 100

ml). IIDs are typically calibrated to the individual, either through the machine being trained on the person's voice (e.g. humming while breathing into the machine) or through photos taken during testing to prevent using another person to start the car. IIDs also often require rolling retests after the car has been started, as well as periodic visits to the installer for calibration, downloads of breath test results, and verification that the device has not been tampered with.[4] Devices are widely used in the United States (Teoh et al., 2021) as well as other countries (Negussie, Geller and Teutsch, 2018) as a post-conviction sanction as well as an alternative to hard license suspension and a restricted license. The cost of installation, calibration, and maintenance is born by the sanctioned driver, though in the state we study installers are required to absorb the costs for people with very low income (with the ultimate incidence of these expenses presumably passed onto other drivers with incomes above the threshold).[5]

There are several systematic reviews of the literature on the impacts of IID on various measures of recidivism, including the review by Elder et al. (2011), Blais, Sergerie and Maurice (2013), and Willis, Lybrand and Bellamy (2004). These reviews arrive at the following conclusions:

- people are less likely to be arrested for a DUI while an IID is installed in their vehicle,
- people are less likely to be cited for driving on a suspended license when an IID is installed,
- alcohol-related crashes and subsequent injuries and fatalities go down in states with robust programs, and
- overall crashes may increase since IIDs are effectively a substitute for license suspension and likely lead to more driving.

[4] In California, compliance requires that the IID be calibrated by the installer every 60 days.

[5] In California, persons with household incomes below 400 percent of the poverty line can install an IID at reduced cost. For persons with incomes equal to 100 percent or less of the federal poverty line, they pay 10 percent of program costs and any costs associated with noncompliance. Persons with incomes 101 to 200 percent of the federal poverty line cover 25 percent of program costs as well as costs for noncompliance. This share increases to 50 percent for people with incomes 201 to 300 percent of the poverty line, and 90 percent for persons with incomes 310 to 400 percent of the poverty line. CalFresh recipients (the California Supplement Nutrition Assistance Program) qualify for a 50 percent subsidy to program costs. For basic information on the California IID SB 1046 program, see Statewide Ignition Interlock Device Pilot Program, accessed on October 1, 2024.

- Take-up/compliance with mandatory orders present key implementaiton challenges for effective use of IIDs.

Based largely on this research, a consensus report from the National Academies of Sciences on DUI policy in the United States recommends that IID installation be required for all DUI convictions (Negussie, Geller and Teutsch, 2018). Beyond the research included in these reviews, other researchers have studied factors that influence installation (for example, being able to avoid a hard suspension, alternative sanctions), the effects of monitoring of IID outcomes on compliance, and the predictive value of the data generated from IIDs. In this section, we provide a detailed and critical discussion of several of the key studies in this literature.

2.1 IIDs and recidivism while installed and after removal

Several researchers have studied the effects of IID installation on DUI recidivism. This research relies on a variety of methods including randomized control trials, observational analysis, analysis of drinking behavior while an IID is installed, and tests for heterogeneity in effectiveness associated with variation in how IID requirements are implemented (e.g, whether data are monitored, the severity of alternative sanctions).

The strongest evidence for a contemporaneous impact of IID installation on DUI recidivism is provided by Beck et al. (1999). The authors analyze the effects of IIDs on driving-while-intoxicated (DWI) recidivism among individuals with multiple DWIs in Maryland who are recommended for relicensing by a Medical Advisory Board (MAB) (the body tasked with reviewing such requests).

In conjunction with the Maryland Motor Vehicle Administration, the authors implemented a randomized control trial where roughly half of nearly 1,400 individuals were randomly assigned to a treatment group requiring an IID to be installed for one year, while the other half of the participants were randomly assigned to a business-as-usual control group (requirement to not drink at all if driving and to check in with a probation officer). In the first post-assignment year, 2.4 percent of treatment group members and 6.7 percent of control group members had another DWI violation, indicating that being assigned to treatment reduced the incidence of DWI's by 64 percent. Among those who did not recidivate in year one, 3.5 percent of the treatment group members recidivated in year 2 (when most IIDs had been removed among those who complied in year 1) with a comparable figure for remaining control groups

members of 2.6 percent. For the entire two-year period, 5.9 percent of the treatment group and 9.1 percent of the control group recidivated (with the difference significant at the five percent level of confidence).

Non-experimental studies in several states tend to find lower recidivism rates while IIDs are installed. McCartt, Leaf and Farmer (2018) use time series modeling and descriptive analysis to study a series of policy changes in Washington state pertaining to IID installation requirements. The first major change in 2003 shifted responsibility from the courts to the department of licensing for issuing IID orders for those required under state law to install an IID (those with prior DUI convictions and first-time DUIs with BAC levels in excess of 0.15). In 2004, the legislature expanded the IID requirement to include all DUI convictions. In 2009, the policy was changed to permit the installation of an IID immediately following arrest to avoid an administrative preconviction license suspension. Finally, in 2011, state policy was altered to require four months of IID installation without any noncompliance as a prerequisite to license reinstatement. The study documents a large increase in IID installations and a shortening time period between arrest and the installation date with each subsequent reform. There is some evidence of avoidance of the requirement by pleading down to reckless driving convictions. The strongest evidence of an impact on recidivism is observed for first-time DUI convictions.

Marques et al. (2010) study the expansion of IID requirements in New Mexico, first for first time offenses with aggravating circumstances (in addition to people with priors), and ultimately a universal requirement for all DUI offenses. In addition to a statewide analysis, the authors separately study Santa Fe County, where local courts temporarily enforced electronic monitoring and home confinement for individuals who failed to install IIDs in their cars. Similarly to the results from prior studies, the authors find that IID installation suppresses DUI recidivism on the order of 68 percent relative to the recidivism rate for a non-experimental comparison group. Although compliance with the IID installation requirement hovered around 50 percent statewide, compliance in Santa Fe while the harsh alternative sanctions were in place reached nearly 75 percent.

There are several evaluations of past policy experimentation with IIDs in California. DeYoung, Tashima and Masten (2004) evaluate the implementation and effectiveness of early California legislation that guides the use of ignition interlock devices. Effective as of 1999, California judges were mandated to order ignition interlock

installations for all DUI offenders arrested or cited for driving on a DUI-suspended license (an offense with the abbreviation DWS-DUI). Judges also had discretion to order IIDs for other DUI offenders, although the evaluation by DeYoung (2002) suggests that they rarely used this option.[6] Beyond these provisions, people with a suspended license had the option to install an IID after serving half their suspension for earlier reinstatement of their driving privileges.

The authors test for effects of the intervention on subsequent DUIs and crashes using duration analysis. They find that people ordered to install an IID or restricted to driving only vehicles with IIDs due to a DUI-DWS were 24 percent less likely to experience a crash during a three-year observation period. There was no measurable effect on the likelihood of a subsequent DUI. In an analysis of individuals who actually install an IID, they find significant reductions in the likelihood of a subsequent DUI, but a higher risk of future crashes (likely reflective of more driving since the IID reinstates driving privileges). The authors find similar patterns for people who are repeat DUI offenders.

There are two evaluation studies of a four-county California pilot program that began in 2010. Assembly Bill 91 (Feuer, Chapter 217, Statute 2009) authorized a pilot program to be implemented from July 1, 2010, through January 1, 2016 requiring all persons convicted of a DUI in Alameda, Los Angeles, Sacramento and Tulare counties to temporarily install IIDs on all vehicles they own or operate. Chapman, Oulad and Masten (2015) assess whether the rate of DUI convictions (normalized by the number of licensed drivers) declines in pilot counties relative to other counties in the state. They test for differential impacts on the incidence of first-time DUIs per 100,000 licensed drivers, second-time DUIs, and third- or higher-time DUIs. Their DUI measures include actual DUI convictions, as well as convictions where the person pleads down to a reckless driving offense. The authors find a sizable increase in IID installations in pilot counties relative to non-pilot counties, but no evidence of a

[6] DeYoung (2002) studies aspects of the implementation of an early California law that required IID orders for people convicted of driving on a suspended license due to a DUI. The study is primarily concerned with how frequently judges order IIDs in cases where the law requires, how often people subjected to these orders actually have one installed, and perception among judges, district attorneys, and citizens subject to an order regarding the effectiveness of the devices. The main finding is that judges more often than not fail to order the installation of IIDs in cases where such a requirement is required by law. The reasons given are (1) many DUI-DWS cases involve someone who does not own a car, and (2) beliefs that the devices are not affordable for many. Among the small number of people interviewed about their personal experience with IIDs, none tried to circumvent the device and many thought that the devices permanently altered their behavior.

relative decline in the rates of DUI convictions.

The Department of Motor Vehicles (2016) presents a subsequent evaluation testing for specific deterrence for people with DUI convictions in the pilot counties. Specifically, the study tests the impact of this earlier pilot on recidivism outcomes and the incidence of crashes among people convicted of DUI offenses who were ordered to install an IID in the pilot counties. The authors use survival analysis to test whether the IID order reduces the likelihood of a subsequent DUI conviction and subsequent crashes during predetermined post-conviction observation periods. They find no evidence of an intent-to-treat effect on DUI recidivism for first-time, second-time, and third- or greater DUI offenders (using propensity score analysis to identify a non-experimental comparison group). There is some evidence of a higher incidence of crashes among people in pilot counties. In the analysis of people who comply with the IID order, they find much lower DUI recidivism (using both measures) but substantially higher crash rates. It is not clear what explains the higher crash rates, but the authors hypothesize that people with DUI convictions are at a relatively high risk of a crash independently of their drinking behavior. Given that the IID can be used to shorten the license suspension period, this suggests that IIDs may not be an effective substitute for a hard suspension in terms of traffic safety outcomes.

Voas et al. (2021) provide a novel analysis that differs from other studies of IIDs in that the authors focus directly on drinking behavior. While based on a very small sample, the analysis reveals some of the behavioral mechanisms that may be behind the suppression of DUI recidivism while devices are installed. The authors specifically study how drinking behavior changes with a DUI arrest, the installation of an IID, and the removal of the IID. Based on interviews with people with DUI arrests who are ordered to install an interlock device (the interviews were conducted as part of the Managing Heavy Drinking (MHD) study),[7] the authors measure average drinks per day, number of days drinking, number of days of heavy drinking, and the incidence of driving after drinking for a time period proceeding arrest, after arrest but prior to IID installation, while the IID is installed in one's vehicle, and following IID removal. On average, approximately three months elapse between arrest and IID installation, and IIDs are required for periods of six months to one year. The authors document sizable

[7]The MHD study is a longitudinal analysis of subjects in Erie County New York with data collected between 2015 and 2020. In addition to the empirical findings in this and other studies, MHD subjects have also been studies using qualitative interview techniques (Romosz et al., 2021).

reductions in drinking and drinking and driving following arrest that are sustained throughout the IID installation period. In addition, while there is some rebound in these outcomes towards the pre-arrest level following the removal of the IID, all of the drinking outcomes as well as the propensity to drink and drive remain below pre-arrest levels following the removal of the IID.

2.2 IIDs, crashes, and fatalities

A clear goal motivating the use of IIDs is to prevent dangerous driving that results in property damage, injuries, and deaths. Over the past decade, over 1,000 people in California a year die on average in alcohol-related traffic fatalities (see Table 1 below), equaling over half of the number of people who die annually by homicide in the state. If serious injuries are included, the number of Californians harmed in an alcohol-related crash is between 3,300 and 5,100. If we include all injuries, annual incidents typically fall in the 23,000 to 28,000 range. Alcohol-involved injuries and fatalities account for 10 to 12 percent of crashes causing injury or death in any given year.

Several researchers have investigated the extent to which the use of IIDs as an alternative sanction for DUI arrests and convictions impacts crash fatalities. Several have used aggregate data to test for changes in policy on overall fatality rates. These studies tend to find evidence that the expanded use of IIDs reduces fatalities. Other studies look at individual-level evidence to assess whether people who install IIDs after arrest or conviction for a DUI have a lower likelihood of a subsequent crash. The evidence from this research is mixed and suggests that people with DUI convictions may be riskier drivers overall.

An early example of research using aggregate data is provided by Voas, Tippetts and Fell (2000). The authors use state-level data to study the effects of laws that make driving a vehicle with a BAC above a given limit illegal per se as well as state laws that authorize administrative license suspensions following a DUI arrest. Although the study does not specifically explore the effects of IIDs, the methods are somewhat similar to later research exploiting interstate and over-time variation in policy choices made by states. The dependent variable studied is the ratio of alcohol-related crash fatalities to nonalcohol crash fatalities. They find significant effects of these policy levers on the dependent variable using data covering roughly the last twenty years of

Table 1: **Annual Number of Crashes, Alcohol Involved Crashes and the Consequent Fatalities and Injuries, Fatalities and Serious Injuries, and Fatalities in California, 2014 through 2023**

Year	All crashes	Alcohol-involved crashes	All fatalities and injuries	Alcohol-involved fatalities and injuries	Alcohol-involved fatalities and serious injuries	Alcohol-involved fatalities	Alcohol-involved fatalities from the FARS data
2014	165,624	16,078	234,030	23,230	3,344	940	876
2015	181,837	16,995	257,996	24,574	3,481	917	902
2016	198,899	18,436	283,874	26,850	3,819	982	1,114
2017	197,146	18,155	281,064	26,591	4,014	1,050	1,141
2018	195,449	18,433	278,658	26,971	4,436	956	1,116
2019	190,649	18,468	272,768	26,922	4,434	929	966
2020	147,148	16,281	207,870	23,275	4,259	1,034	1,180
2021	162,036	19,850	229,586	28,695	5,115	1,166	1,370
2022	161,289	19,371	228,048	27,825	4,819	1,036	1,479
2023	163,071	18,774	229,941	27,283	4,412	938	-

Source: Transportation Injury Mapping System (TIMS) summary of California statewide crash and injury data from the Statewide Integrated Traffic Records System (SWITRS). Summary statistics accessed on December 5, 2024. Fatalities from the Fatality Analysis Reporting System (FARS) files differ from those in the SWITRS data due to the imputation procedure used in FARS to impute whether alcohol was involved in fatalities where information pertaining to alcohol was not reported. FARS data are currently publicly available through 2022. The SWITRS data defines alcohol-involved incidents as those where the officer who filled out the crash report perceived that party fell under one of the following categories: had been drinking, under influence; had been drinking, not under influence, or had been drinking, impairment unknown. This may or may not be determined with the help of a BAC reading.

the twentieth century.

Kaufman and Wiebe (2016) use state panel data and difference-in-difference analysis to estimate the effect of a universal IID requirement for all DUI convictions on fatal alcohol-related crash rates. From 2004 through 2013, 18 states enacted and implemented legislation mandating such universal requirements. Using publicly available data from the Fatal Analysis Reporting System (FARS), the authors estimate a simple OLS model where the key explanatory variables are state and year fixed effects and an indicator for the presence of an IID law. They find an approximate 15 percent reduction in deaths from alcohol-related crashes.

Teoh et al. (2021) use FARS data to study the effects of state laws that mandate universal IID installation for all DUIs, IID requirements for repeat offenders, or IID requirements for repeat offenders and those with a BAC reading of 0.15 or higher on fatal crashes. The study includes all states and the District of Columbia with the exception of California.[8] Their main estimation results involve a Poisson regression of the count of fatal alcohol-involved crashes on an indicator for the presence of a state law and a control variable measuring all other fatal crashes. They find a substantial and significant effect of state laws on alcohol-involved fatal crashes (roughly 26 percent fewer such fatalities in states with an IID law).

Regarding analysis focused on individual data for people with installed IIDs, we have already discussed the evaluations of previous policy changes in California that appear to show decreases in alcohol-related crashes but increases in the overall number of crashes for people ordered to install a device who comply with the order (DeYoung, Tashima and Masten, 2004; Department of Motor Vehicles, 2016).

2.3 Determinants of take-up, the effects of monitoring on compliance, and the predictive value of data collected from IIDs

Beyond research on the effects of IIDs on recidivism and crashes, there are several other questions that are explored in the literature that are largely related to policy implementation issues. The first among these concerns is compliance. Almost all studies find imperfect compliance, with many people ordered to install an IID either

[8]California is excluded since during the time period studied (2001 through 2019) California only had the AB 91 pilot universal IID program for four large counties.

claiming that they do not own a car or choosing not to install. Many of the studies reviewed above find compliance rates of roughly 50 percent. The New Mexico evaluation (Marques et al., 2010) finds very high compliance in Santa Fe County, where noncompliance with the installation order temporarily resulted in house arrest.

Although compliance rates are far below 100 percent, most of the research does find substantial increases in installation following legislation that expands IID requirements or that enables people to install an IID to avoid a hard suspension. For example, Mcknight and Tippetts (2020) study the effects of policy changes designed to increase the use of IIDs on overall installations, the number of IIDS in place, and, for one state, a measure of noncompliance. Using administrative data for Florida and West Virginia, the authors study the effects of several policy changes implemented within these states. First, Florida lowered the BAC limit for aggravated DUI (an offense for which first-time offenders are ordered to install an IID in order to reinstate their licenses) from 0.2 to 0.15. They study the effects of two policy changes in West Virginia: an earlier change in policy that removed DUI offenses from one's criminal record for first time offenders who successfully complete an IID-required period and a latter change in policy that permitted installation of an IID along with waiving an administrative hearing in exchange for avoidance of a hard license revocation. In Florida, the authors document a discrete increase in IID installations corresponding to the policy change. Similarly, in West Virginia, where the changes in policy effectively allowed people to opt for IID installations as an avenue to avoid a criminal history record or a suspension, the authors also document sizable increases in installation associated with the policy change.

There is at least one qualitative study (though based on a small number of participants) investigating why people do not install IIDs. Romosz et al. (2021) presents the results of qualitative interviews with six individuals in upstate NY who were ordered to install interlocks but chose not to. The interviews reveal that cost, stigma concerns, distrust of authorities, and the belief that the requirement would not be properly enforced are the primary reasons for not installing a device. Several of the participants indicated that they still drove their cars or the cars of others, but drove more carefully and took alternative routes where they believed that they would be less likely to be detected by police.

Surprisingly, most states do not monitor the data generated by IIDs and do not take into account lockouts and attempts to alter the device in the process of restoring

driving privileges. At least one study indicates that this information may be useful in achieving better outcomes for people who install IIDs. Zador et al. (2011) present the results of a randomized control trial in Maryland based on a sample of Maryland drivers very similar to those studied in Beck et al. (1999) who are ordered to install an IID as part of their journey toward full restoration of their driving privileges. Rather than studying the effects of an IID per se, this study investigates whether being assigned to a treatment groups involving both closer monitoring and modest graduated sanctions improve compliance with the IID restrictions.[9]

The authors find that individuals assigned to the graduated-sanctions treatment group had substantially fewer noncompliance issues (mostly initial failed breath tests), with the reduction on the order to 25 to 30 percent relative to control group levels. They also found (in terms of the results for variables included as controls) that violations decreased with time on the IID, suggestive of a learning process as time on the IID increased.

There is also evidence from several studies that data generated by IIDs are predictive of future DUI events. Marques et al. (2001) use IID recorder data for roughly 2,500 individuals in Alberta, Canada to assess whether data collected while an interlock is in place can predict future DUI recidivism. They find that in addition to prior DUIs, IID warnings (BAC readings between 0.02 and 0.04) and IID lockouts for higher BAC readings are predictive of future violations. Marques et al. (2010) finds similar results in their analysis of data from New Mexico. Hence, better use of this information, in addition to improving compliance, can lead to more accurate assessments of future risk that could be used to individualize responses to a DUI offense.[10]

[9]The principal measures of compliance/noncompliance used are failed breath tests, efforts to bypass the IID, refusals to retest, and retest failures. The treatment involved the following graduate sanctions regime: closer monitoring of noncompliance incidents, with noncompliance resulting in warning letters, increases in the required frequency fo device calibration visits, referral to the Maryland Medical Advisory Board for evaluations of one's alcohol problems, potential lengthening of the required IID installation period, and possible revocation of driving privileges. Monitoring of the control group appeared to be nonexistent, with little consequences for failures to comply and the removal of the IID and full restoration of driving privileges once the period expired.

[10]While most people convicted of a DUI do not re-offend, a sizable minority do and being able to discriminate between high and low-risk cases would be helpful in fashioning diversion programs and identifying persons that require closer monitoring. Rauch et al. (2010) study the relative risks of subsequent drinking and driving violations among licensed drivers in Maryland after stratifying drivers by the number of prior violations. Studying the period from 1999 through 2004, the authors find a violations rate per 1,000 licensed drivers for those with one prior violation (24.3 per 1,000) that is over seven times the rate among drivers with no prior violations (3.4 per 1,000). While the

3 Policy Variation in IID Mandates Used in this Study

Our primary empirical strategy exploits the coincident implementation at the beginning of calendar year 2019 of a statewide IID mandate for a limited set of DUI convictions and the expiration of a pilot program mandating IID installation for all DUI convictions in four California counties. We document a large relative increase in IID installation rates during the two-year period following arrest for counties that were not part of the earlier pilot relative to counties that were.

In this section, we discuss the details of key policy changes that are central to our identification strategy. We also document the likely effects of the pandemic on DUI case processing, convictions, and subsequent IID installations. We defer the methodological discussion to the next section, as our estimation strategy is tailored to and driven by the implementation details of the legislation that we document here.

3.1 IID Policy History

Before 2019, California made limited use of IIDs with some targeted mandates enforced at the discretion of judges and geographically limited pilot efforts to enforce universal mandates. Beginning in 1993, California required mandatory IIDs for repeat offenders[11] and since 1999 for people arrested for driving on a suspended license where the suspension was caused by a DUI violation.[12] Moreover, judges have for decades had the discretion to mandate IID installation upon conviction based on the particulars of a given case. DeYoung (2002) finds uneven enforcement of these early mandates and a generally low application of discretionary IID requirements.

California's first exploration of a more binding mandate was implemented in 2010. California Assembly Bill (AB) 91 creating a four-county pilot program to be imple-

re-offending rates for people with one prior is lower than those for drivers two or more priors, the findings suggest that people with one prior are at much higher risk of subsequent violation relative to drivers with no priors. Note, research from other states also indicates that people with one prior are at elevated risk of adverse subsequent driving outcomes. In a literature review of what is known about alcohol-impaired driving prepared by the National Highway Traffic Safety Administration, Jones (2000) presents estimates of average traffic crashes for the period 1985 through 1991 for California drivers by the number of prior DUI convictions. The incidence of crashes among people with one prior DUI is roughly 1.75 times the rate for people with no priors.

[11] See Assembly Bill 2851 (Friedman, Chapter 694, Statute 1992).

[12] See Assembly Bill 762 (Torlakson, Chapter 756, Statute 1998).

mented from July 1, 2010 through the end of calendar year 2016. The pilot required mandatory IID installations for all individuals convicted of a DUI offense with or without injury in all vehicles they own and operate. The mandated IID installation periods ranged from five months for a first offense to four years for persons with three or more priors, did not depend on the discretion of the judge, and was a pre-condition for eventual restoration of driving privileges. The expiration date of the pilot program was eventually extended through the end of 2018.[13] The pilot was in effect in Alameda, Los Angeles, Sacramento, and Tulare counties. Collectively, these four counties account for roughly one-third of the state's population and roughly 26 percent of state DUI arrests.

California Senate Bill (SB) 1046 introduced the first statewide mandate pertaining to IID installations, with an implementation date of January 1, 2019. The legislation has two key provisions. First, SB 1046 mandates IID installation for specific time periods as a precondition to restoration of driving privileges for certain DUI offense convictions. Second, the law creates the option for those arrested for a DUI to install an IID pre-conviction to avoid the pre-conviction administrative per se (APS) license suspension that follows a DUI arrest in the state.[14]

Regarding the first channel, people convicted for a DUI involving an injury, as well as individuals convicted of a DUI with prior alcohol related convictions (e.g., prior DUI, reckless driving involving alcohol) are required to install an IID for specified periods of time as a precondition to full restoration of driving privileges.[15] This

[13]The extension of the AB 91 pilot was authorized under the authorizing legislation (i.e., SB 1046) creating the 2019 IID mandate.

[14]When a person is arrested for a DUI in California, the arresting officer confiscates the persons driver's license and mails it to the DMV. The officer provides the driver with a temporary driver's license valid for thirty days following the arrest, after which the person's driving privileges are suspended for either four months (if a first offense) or a year (if the person has a prior alcohol-related driving convictions). The person has 10 days from the date of arrest to schedule a DMV hearing if they believe the arrest was unjustified. If they fail to schedule a hearing, the APS suspension goes into effect with the expiration of the temporary driver's license. Note, the APS suspension occurs whether or not the person is ultimately convicted. In addition, a subsequent conviction will often entail an additional revocation, often after driving privileges have been restored following an APS suspension. That being said, APS suspensions and post-conviction suspensions are served concurrently rather than consecutively and convicted persons typically receive credit for suspension time served prior to conviction.

[15]For people with prior alcohol related convictions convicted for a DUI without injury (CVC §23152), the required IID installation period is one year for persons with one prior, two years for persons with two priors, and three years for persons with three or more priors. For thosr with priors, conviction for a DUI with injury or vehicular manslaughter (CVC §23153 or PC §191.5(b)) adds an additional year to the IID requirement schedule. First offense convictions involving injury carry a

requirement is tied to the date of arrest and applies to all arrests on January 1, 2019 or later. Note that this requirement is not operative until the person is convicted; and even when mandated a person can choose not to install an IID. However, those who are required to but do not install an IID will be unable to obtain a license until they have met the installation time requirement, even after any license suspension period associated with the conviction has expired.[16]

Regarding the second channel, prior to the 2019 policy change one could not avoid the pre-conviction automatic license suspension that followed soon after an arrest. People arrested for a first offense were and are still eligible to apply for a restricted driver's license without an IID once they have served thirty days of the APS suspension, paid a fine, and obtained and provided in person at a DMV office proof of insurance. This restricted license allows driving only to and from work, as well as to DUI programming. For persons with priors, eligibility for a restricted license prior to the passage of SB 1046 required serving 90 days of the APS suspension (for a second offense) or six months (for a third offense) as well as other requirements.

SB 1046 created a second restricted license option during the APS revocation period that required the person to drive only an IID-equipped vehicle. This second option does not limit where or when the person can drive and can be exercised immediately, hence avoiding the first thirty days of the pre-conviction suspension period for a first offense, and the lengthier required suspensions for persons with priors. For those with prior DUIs, the IID-restricted license is the only currently available option to avoid a pre-conviction suspension.

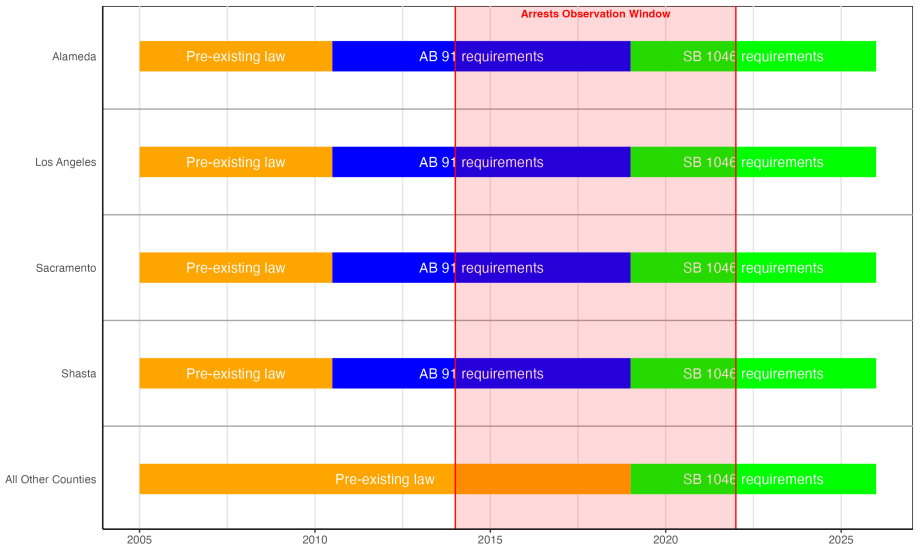
Figure 1 visibly summarizes the timing of the state's policy history and highlights the specific variations that we use to identify the effects of IID's on DUI recidivism. The figure presents separate timelines for the four counties subjected to the 2010 pilot (Alameda, Los Angeles, Sacramento, and Tulare) and a timeline for the remaining 54 counties in the state. All counties operate under pre-2010 policy through July 2010. The four pilot counties are then subjected to a strict and automatic IID mandate through the end of 2018, while the remaining 54 counties still operate under the older constellation of IID policies.

Starting in 2019, all counties are brought under the same policy mandate that

mandatory IID installation period of one year.

[16]People can apply for an exception to the requirement, typically based on not owning a car or being an out-of-state resident.

Figure 1: **Summary of California Policy Variation**



applies mandatory IID requirements to a subset of the DUI convictions with mandatory IID requirements under the earlier pilot. The coincidence of the expiration of the pilot and the implementation of SB 1046 relaxed IID requirements in the early pilot counties (for first-time convictions in particular) while tightening requirements in the remainder of the state. The figure also displays the time period that captures the DUI arrests that we use for our analysis. As is clear, the period we can observe in the administrative data extract provided to us spans the 2019 policy changes.

Comparisons of IID installation rates before and after 2019 reveal relatively small increases in installation rates. Table 2 presents the percent of arrests in which an IID is installed within two years of arrest for 2014 through 2021 for all DUI arrests and by the number of previous convictions. There are modest increases in the percentage of arrests where an IID is installed within two years. In the pre-period, the percentage is trending downward from 13.2 percent in 2014 to 11.4 percent in 2018. The installation rate jumps in 2019 to 16.9 percent, then declines to levels slightly above what we observe in the pre-period. We observe greater increases among arrests where the driver in question has previous DUI convictions. However, for all groups, the increases are modest.

The relatively small increases in installation rates are driven by several factors. First, the expiration of the earlier pilot program caused net declines in installation rates in the four affected California counties (a key aspect of our identification strat-

Table 2: **Percent of DUI Arrests where an IID is Installed Within Two Years: All Arrests and Arrests with No, One, Two, or Three or More Prior Alcohol Related Convictions**

Year	All arrests	No priors	One prior	Two priors	Three plus priors
2014	13.2	11.9	18.4	14.9	7.5
2015	12.5	11.1	17.6	14.3	7.8
2016	12.1	10.6	17.5	15.5	7.4
2017	12.0	10.5	17.2	15.0	8.0
2018	11.4	9.7	17.1	14.0	7.2
2019	16.9	15.5	23.0	18.9	9.8
2020	13.6	11.7	19.8	16.6	9.5
2021	14.5	12.4	21.6	18.1	9.4

Authors' tabulations from the Drivers Master File Records.

egy), a pattern that we will document shortly.

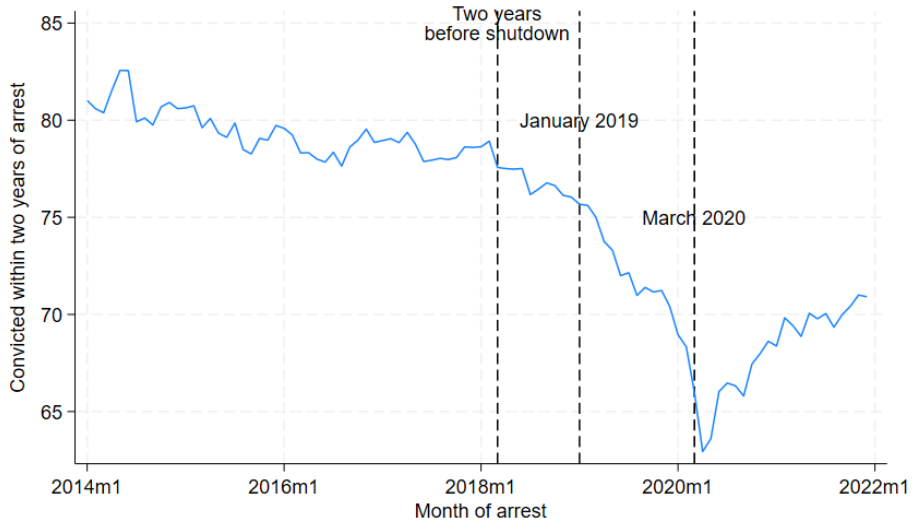
Second, interruptions in case processing caused by the pandemic reduced conviction rates, thus reducing the percent of arrests resulting in an IID mandate. Figure 2 displays the percent of DUI arrests that result in a conviction within two years by month of arrest.[17] Note, since the figure depicts the percentage of DUI arrests where conviction occurs within two years, conviction rates for arrests occurring prior to March 2018 (24 months before the stay-at-home order) have post-arrest observation windows that are entirely pre-pandemic while later arrests have observations windows that extend beyond the pandemic start date. While conviction rates exhibit a slight downward trend prior to March 2018, we observe steeper declines in conviction rates thereafter. Two-year conviction rates hit a low in April 2020 and recover somewhat over the next few years.

3.2 Relative Change in IID Installation Rates Use to Identify the Effect of IID on Recidivism

The modest pre-post 2019 increase in IID installations statewide mask much larger shifts in installation rates for individual counties. Here, we document this cross-county heterogeneity. Table 3 displays the proportion of arrests in which an IID is installed

[17]For the earliest year in the data extract provided to us, we are able to observe a six month post-arrest observation window. Slightly more than 80 percent of these arrests result in a conviction, with roughly 99 percent of the convictions occurring within two years of arrest.

Figure 2: **Percent of DUI Arrests Resulting in a Conviction Within Two-Year by Month of Arrest**



within two years of arrest by whether the arrest occurred before the implementation of SB 1046, after the implementation of SB 1046, and by whether the arrest occurred in an AB 91 county or in one of the remaining 54 counties. We present four cross-county group contrasts: tabulations for all offense groups combined (Panel A), tabulations for people arrested for the first offense (Panel B), people arrested for a first offense but where an injury occurs (Panel C), and arrested persons who have prior DUI convictions (Panel D).

Beginning with the results in panel A, we observe a pre-post increase in two-year installation rates in the non-AB 91 counties of roughly 9.3 percentage points. In the AB 91 counties, we observe a decline of 15.8 percentage points. These changes imply a 25 percentage point relative increase in the likelihood that an IID is installed for arrests occurring in the 54 non-AB 91 counties.

The results for the different offense groups reveal several interesting patterns. Beginning with the results for first-offense arrests (panel B), installation rates are very low in the pre-period in the non-AB 91 counties (roughly 3 percent), while in the AB 91 counties, 33 percent of these arrests resulted in an IID installation. In the post-period, IID installation rates in non-AB 91 counties increases to 13 percent, most certainly reflecting individuals arrested for the first time taking advantage of the option to install an IID and avoid the APS suspension. In AB 91 counties,

installations for this group fall by nearly 20 percentage points. Again, we see a large, statistically significant relative increase in installation rates of 30 percentage points.

The magnitude of the levels and changes in installation rates for people with no priors who injure someone (tabulations presented in Panel C) are comparable to those for first-time offenses without injury. Here, however, part of the increase in installation rates for those in non-AB 91 counties most likely reflects increased installations due to the mandatory installation requirement. Here again, the table documents a large relative increase in installation rates in non-AB 91 counties of 28 percentage points.

Finally, we see the same, yet more muted pattern among arrested persons with priors. In the non-AB 91 counties roughly 13 percent of arrests in the pre-period resulted in an IID installation. This increases to 19.3 percent in the post period with the introduction of mandatory IID requirement for such arrests. In the AB 91 counties, we observe a seven percentage point decrease, yielding a relative increase in installation rates in the non-AB 91 counties of 13.4 percentage points.

One might be concerned that the contrast between AB 91 and non-AB 91 counties documented in Table 3 is driven by the experience of specific large counties in each of the groups. This is not the case. In fact, there are declines in overall installation rates in each of the early pilot counties and increases in installation rates in each of the remaining counties. Figure 3 shows the percent of DUI arrests occurring prior to the 2019 policy changes where an IID is installed within by county. Figure 4 presents comparable tabulations for post-2019 arrests, while Figure 5 displays the before-after change in installation rates. AB 91 counties are marked with red bars, while the remaining 54 counties are marked with blue bars. The counties in each figure are sorted from highest to lowest values.

In Figure 3 , we see that the installation rates were uniformly and substantially higher in the four early pilot counties prior to 2019. In the post-period (Figure 4) , the installation rates for these four counties are distributed throughout the distribution for all counties, with Alameda having one of the lowest installation rates, Tulare and Los Angeles being somewhere near the center, and Sacramento having a relatively high installation rate. Finally, in Figure 5 we document the fact that installation rates increased in each of the 54 non-AB 91 counties and decreased in each of the four AB 91 counties.[18]

[18] Appendix Table A1 presents two-year installation rates by arrest year for each county. The table reveals clear pre-post 2019 shifts in installations for all counties.

Table 3: **Relative Changes in the Proportion of Arrests Where an IID is Installed Within Two Years of Arrest Between Non-AB 91 and AB 91 Counties: All and by Offense Group**

Panel A: All Offense Groups Combined			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0602	0.1528	0.0926^a
	(0.0003)	(0.0007)	(0.0007)
AB 91 counties	0.3055	0.1475	-0.1580^a
	(0.0011)	(0.0013)	(0.0019)
Difference	-0.2452^a	0.0053^a	0.2506^a
	(0.0009)	(0.0015)	(0.0017)
Panel B: No priors, no injuries			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0283	0.1322	0.1039^a
	(0.0003)	(0.0009)	(0.001)
AB 91 counties	0.3301	0.1337	-0.1964^a
	(0.0014)	(0.0015)	(0.002)
Difference	-0.3017^a	-0.0015	0.3032^a
	(0.0010)	(0.0018)	(0.0019)
Panel C: No priors, injury			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0335	0.1464	0.1129^a
	(0.0011)	(0.0027)	(0.003)
AB 91 counties	0.3128	0.1461	-0.1667^a
	(0.0046)	(0.0047)	(0.007)
Difference	-0.2792^a	0.0003	0.2795^a
	(0.0033)	(0.0055)	(0.0061)
Panel C: Priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.1285	0.1929	0.0644^a
	(0.0008)	(0.0014)	(0.002)
AB 91 counties	0.2469	0.1781	-0.0688^a
	(0.0020)	(0.0026)	(0.003)
Difference	-0.1184^a	0.0147^a	0.1331^a
	(0.0019)	(0.0031)	(0.0035)

Standard errors in parentheses.
a. Difference statistically significant at the one percent level of confidence.

Appendix Table A2 presents similar tabulations in which we stratify DUI arrests by the number of prior convictions using the categories no priors, one prior, two priors, and three or more priors. Again, we observe the largest relative increase in IID installations in non-AB 91 counties for those with no priors (a relative change of 29.8 percentage points). Although we observe statistically significant relative increases for those with priors as well, they are smaller and decline with the number of priors.

Figure 3: **Percent of DUI Arrests Where an IID is Installed within Two Years of Arrest by County: Arrests Occurring 2014 through 2018**

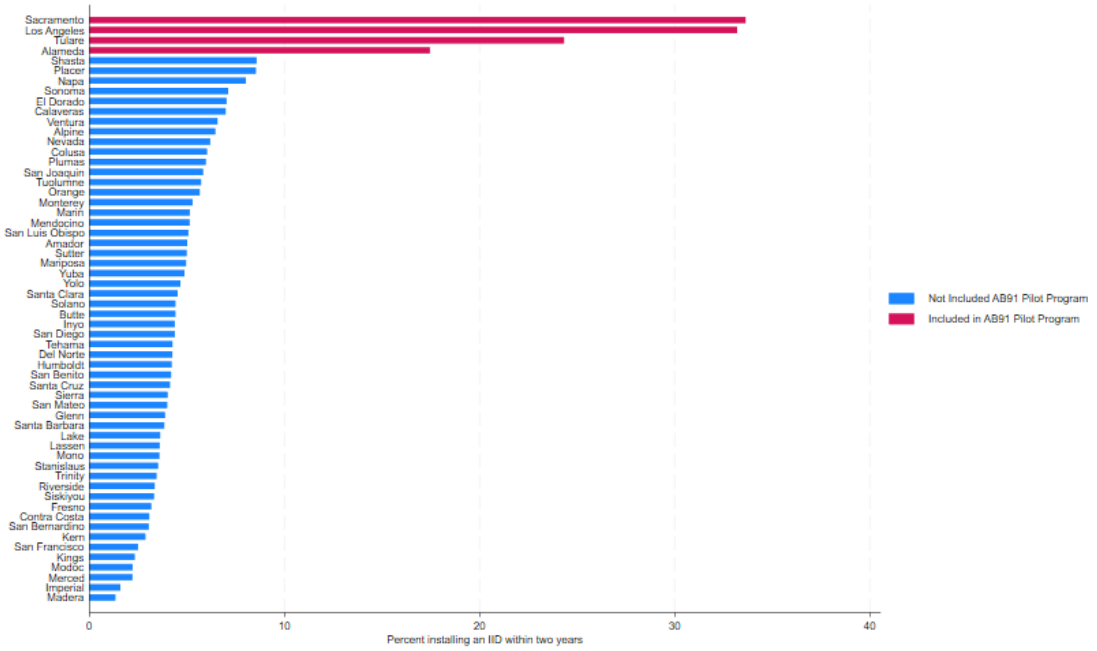


Figure 4: **Percent of DUI Arrests Where an IID is Installed within Two Years of Arrest by County: Arrests Occurring 2019 through 2022**

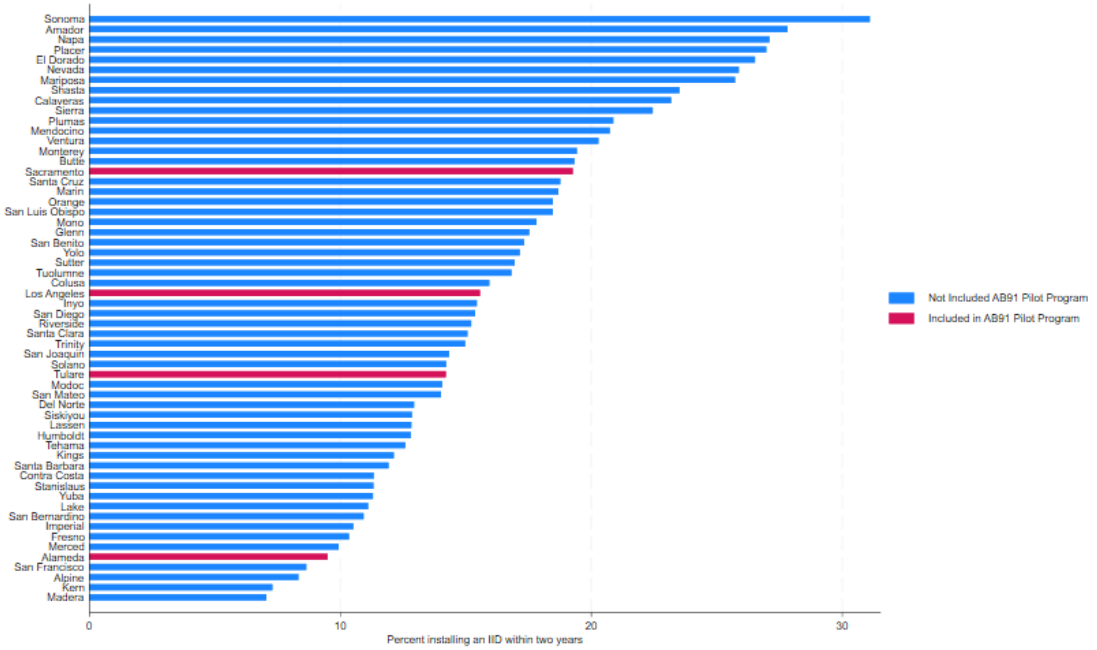
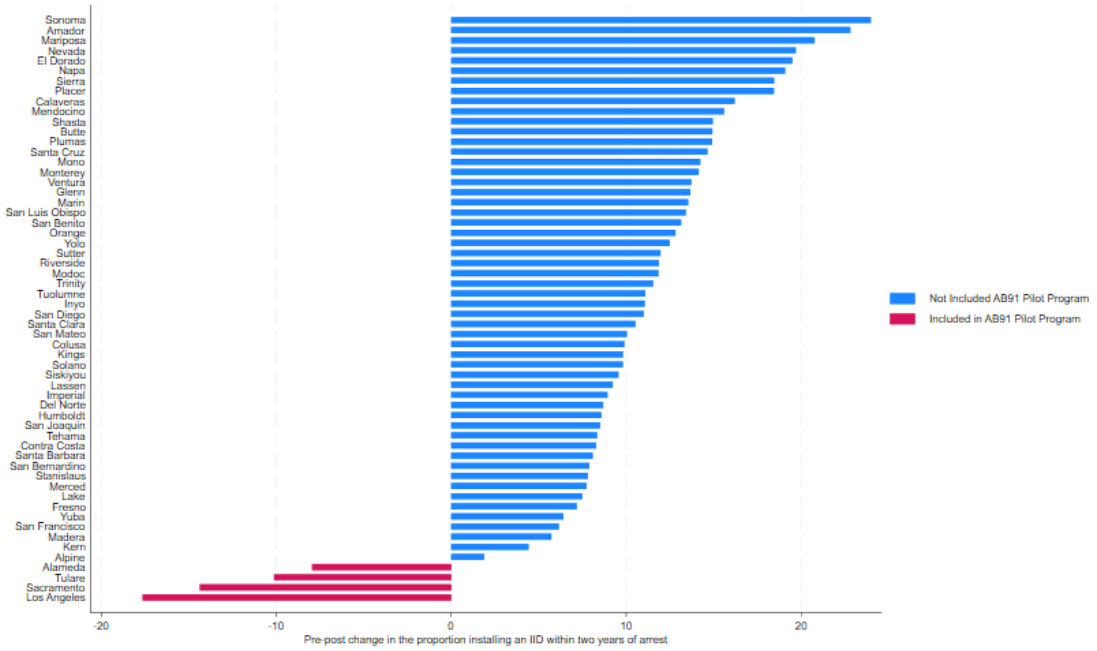


Figure 5: **Pre-Post SB 1046 Change in the Percent of DUI Arrests Where an IID is Installed within Two Years of Arrest by County**



4 Methodological Strategy and Data Description

The primary objective of this study is to assess whether the installation of an IID reduces the likelihood of future DUI arrests and other measures of recidivism. It is certainly the case that a simple comparison of recidivism outcomes among persons who install an IID to those who do not reveals lower recidivism outcomes among installers relative to non-installers, a fact we document in Table 4. Panel A presents counts by year for arrests occurring in 2019, 2020, and 2021 for those who install an IID within two years of arrest for the following recidivism outcomes (also measured within two years of the violation date): DUI arrests, crashes, alcohol-involved crashes, injury crashes, alcohol-involved injury crashes, fatal crashes, and alcohol-involved fatal crashes. Panel B presents these outcomes as a percentage of the total number of cases in each year. Panels C and D present similar tabulations for cases where the person does not install an IID within two years. Focusing on the percentage panels (panels B and D), we observe future DUI arrest rates for those who install an IID that are less than one-quarter the comparable rates for people who do not install IIDs. Moreover, for all crash outcomes in the tables, we observe markedly lower recidivism rates among people who install an IID within two years of arrest relative to persons who do not.

Of course, we cannot infer from the differences in recidivism in Table 4 that the installation of an IID causes these declines in recidivism. There are likely many differences between those who install an IID and those who do not. For example, those who install an IID may be more intrinsically motivated to not drink and drive, may be more likely to comply with court orders, DMV requirements, or other mandates, and may generally be less risky relative to those who do not install. The installation of an IID may thus be a visible manifestation of such differences in unobserved characteristics.

An additional challenge concerns the strong trends in conviction rates, alcohol related crashes, and alcohol related crashes involving injury that likely reflect behavioral changes associated with the pandemic. Although the timing of SB 1046 and the COVID pandemic do not perfectly align, most of the post-SB 1046 period coincides with the timing of the pandemic. Alcohol-related traffic fatalities increased during the first years of the pandemic in California and the rest of the nation.[19] Simple

[19]There are documented changes in driving behavior coinciding with the onset of the pandemic.

before-after comparisons of recidivism outcomes in California that are likely to yield changes in recidivism that in part reflect these larger forces.

To address these methodological challenges, we employ difference-in-difference analysis to calculate the effect of installing an IID on the likelihood that persons arrested for a DUI recidivate within two years of their violation date.

4.1 Estimation Strategy

Table 3 documents very large and precisely measured relative changes in IID installation rates in the non-pilot counties relative to the four AB 91 counties, both overall and within the broad offense groupings separately specified by the 2019 mandate. We exploit these relative changes to perform a difference-in-difference analysis of recidivism outcomes. The intuition behind our strategy is relatively straightforward. We observe a large relative increase in IID installation rates in non-AB 91 counties coinciding with the 2019 policy changes. To the extent that IID installation reduces recidivism, we should observe a corresponding relative decrease in recidivism in these counties. We estimate these relative changes in recidivism and employ a two-stage least squares estimator to convert these relative changes in recidivism patterns into estimates of the effect of IID installation on the likelihood of a subsequent DUI arrest, as well as other outcomes.

Regarding the specifics of the estimation strategy, define $NonAB91_i$ as a dummy

Traffic volume and total crashes decreased at the beginning of the pandemic (Katrakazas et al., 2020; Lotan and Shinar, 2021; Islam et al., 2022). Associated with this decline in traffic volume was an increase in driving speed, but serious crashes appear to have declined initially. Early estimates of the effect of the pandemic on fatal crashes are conflicting (Vandoros, 2022; Barnes et al., 2020; Qureshi et al., 2020). Specifically, Islam et al. (2022) find a decline in alcohol-related crashes during the pandemic, while Thomas et al. (2020) finds an increase in crashes involving drugs. In a later analysis, Tefft and Steinbach (2024) generates an estimate of excess traffic deaths during the pandemic using pre-pandemic fatality trends to project future fatalities levels and then calculate excess deaths as the difference between actual observed fatality levels and projection. The study finds sizable increases in traffic fatalities during the pandemic beyond what would have been expected based on pre-pandemic trends. There is also evidence of an increase in risky driving behavior, such as a reduced use of the seat belt and consequently higher passenger ejection rates during automobile crashes (NHTSA, 2021). Our own tabulations of data from the National Highway Transportation Safety Administration Fatality Analysis Reporting System (FARS) find notable increases in traffic fatalities with the onset of the pandemic. We use data for each year from 2005 through 2022 and tabulated alcohol-related traffic fatalities per 100,000 residents for California, for each of the remaining states, and for an average of all states other than California. We observe clear increases in alcohol-related crash fatalities in California, in many of the other states, and certainly in the aggregate for states other than California. These tabulations are available upon request.

Table 4: **Two-Year Recidivism Outcomes by Year for People who Install an IID Within Two Years and People Who Do Not**

Panel A: Two-year recidivism outcome counts among people who install an IID within two years of arrest

Year	Total	DUI	Crash	Alcohol-involved crash	Injury crash	Alcohol-involved injury crash	Fatal crash	Alcohol-involved fatal crash
2019	20,270	433	1,136	123	491	67	< 50	< 50
2020	12,650	354	746	97	335	56	< 50	< 50
2021	15,045	404	878	110	405	70	< 50	< 50

Panel B: Two-year recidivism outcome percentages among people who install an IID within two years of arrest

Year	Total	DUI	Crash	Alcohol-involved crash	Injury crash	Alcohol-involved injury crash	Fatal crash	Alcohol-involved fatal crash
2019	100.00	2.14	5.60	0.61	2.42	0.33	< 0.2	< 0.2
2020	100.00	2.80	5.90	0.77	2.65	0.44	< 0.2	< 0.2
2021	100.00	2.69	5.84	0.73	2.69	0.47	< 0.2	< 0.2

Panel C: Two-year recidivism outcome counts among people who do not install an IID within two years of arrest

Year	Total	DUI	Crash	Alcohol-involved crash	Injury crash	Alcohol-involved injury crash	Fatal crash	Alcohol-involved fatal crash
2019	99,327	10,142	8,132	1,916	3,673	1,269	130	66
2020	80,121	9,541	7,942	1,890	3,793	1,403	152	66
2021	88,998	9,838	8,101	1,983	3,844	1,384	146	< 50

Panel D: Two-year recidivism outcome percentages among people who do not install an IID within two years of arrest

Year	Total	DUI	Crash	Alcohol-involved crash	Injury crash	Alcohol-involved injury crash	Fatal crash	Alcohol-involved fatal crash
2019	100	10.21	8.19	1.93	3.70	1.28	0.13	0.07
2020	100	11.91	9.91	2.36	4.73	1.75	0.19	0.08
2021	100	11.05	9.10	2.23	4.32	1.56	0.16	< 0.05

variable equal to one for arrests made in one of the 54 non-AB 91 counties and equal to zero otherwise, where i indexes a specific arrest. Using all arrests for which we can observe two years of post-arrest outcomes, we estimate the following equation:

$$Y_i = \alpha_0 + \alpha_1 Post_i + \alpha_2 NonAB91_i + \alpha_3 Post_i x NonAB91_i + \gamma X_i + \epsilon_i, \quad (1)$$

where Y_i is an indicator variable for a recidivism outcome (subsequent DUI, subsequent crash, subsequent crash with injury, all measured within two years of arrest), $Post_i$ is a dummy variable indicating that the arrest occurred on January 1, 2019 or later, $NonAB91_i$ is as defined above, α_1, α_2, α_3, and γ are parameters to be estimated (γ being a parameter vector), X_i is a vector of control covariates describing the arrest and the person arrested, and ϵ_i is a mean-zero error term. The coefficient α_3 measures the pre-post average change in Y_i in the non-AB 91 counties minus the comparable change for AB 91 counties (basically, the reduced-form effect of SB 1046 on the relative recidivism outcomes for the two county groups). Given the relative increase in IID installation rates in non-AB 91 counties, a significant negative estimate of α_3 would indicate that IID installation reduces recidivism.

We also employ this framework to estimate the effect of installation on recidivism using the second-stage equation

$$Y_i = \beta_0 + \beta_1 Post_i + \beta_2 NonAB91_i + \beta_3 Installation_i + \theta X_i + \varepsilon_i, \quad (2)$$

where the interaction term between $Post_i$ and $NonAB91_i$ is used as an instrument for $Installation_i$. Applying two-stage least squares to equation 2 provides the local average treatment effect for those whose installation outcome complies with the relative changes in policy (measured by the estimate of the parameter β_3).

The key identification assumption underlying the estimation strategy presented in equations 1 and 2 is that in the absence of SB 1046 the average recidivism outcomes in pilot and nonpilot counties would have followed parallel trends. This seems reasonable given that all counties are nested within the same state and the AB 91 counties are represented in the state's major southern and northern population centers. However, to assess whether the main estimation results are robust, we estimate several specifications of equations 1 and 2 with increasingly inclusive sets of covariates in the

vector X_i. We also present model estimates where we first match each arrest in an AB 91 county with an arrest in a non-AB 91 county using exact matching on age, sex, arrest date, and number of previous alcohol-related convictions and nearest-neighbor matching for the highest BAC reading at arrest.[20]

4.2 Outcome variables and data description

Our analysis focuses on three post-arrest outcomes: subsequent DUI arrests, subsequent automobile crashes, and subsequent automobile crashes that involve injuries to the driver or another person. All outcomes are measured over the two-year period after arrest. A priori, we hypothesize that IID installation should reduce the likelihood of a subsequent DUI arrest.

Regarding the two crash outcomes,the effects of SB 1046 could plausibly go in either direction. To the extent that IIDs prevent driving under the influence and driving under the influence increases the likelihood of an automobile crash,[21] an increase in IID installation rates would be expected to reduce crash rates. However, the 2019 policy changes also creates a new opportunity for individuals arrested for a DUI to completely avoid the pre-conviction license suspension through the installation of an IID, likely increasing the time on the road during the two-year follow-up window. To the extent that people arrested for a DUI are on average more risky drivers even when not under the influence, or that crash risk increases with time driving, the legislation may increase crash rates.

In supplementary analysis,[22] we also test for effects of IID installation on crashes (with and without injury as well as fatal crashes) where the accident record indicates that the driver has been drinking. However, we are cautious in interpreting these results given the very low incidence for some of the outcomes (deaths in particular) and the fact that information pertaining to the sobriety of the involved drivers is missing for a large share of the crashes. The DMV notes that data on alcohol involvement is incomplete due to the fact that the police do not always report information for this field and may also report with a lag. In addition, crash records come from two sources: law enforcement (with records from individual law enforcement agencies reported to

[20] Each arrest typically has information on two BAC measurements.

[21] See Blomberg et al. (2009) and Peck et al. (2008) for evidence related to the relative risk of a crash at various BAC levels.

[22] See table A10.

the California Highway Patrol who then provide information to the DMV) as well as reports by involved individuals (or their insurance companies) under a California Financial Responsibility law. Roughly 40 percent of crashes are reported by law enforcement alone, 40 percent by individuals or insurance companies only in compliance with their financial responsibility obligation, while 20 percent have reports from both law enforcement and individuals or their insurance companies. Information about whether drivers have been drinking is missing from all records that are not reported by law enforcement.[23]

The data for this project come from the California DMV Driver Record Master File. We received information for all drivers arrested for a DUI since 2014 from the drivers license basic record, the history sub-record, the arrest data sub-record, the drivers license abstract sub-record, and an abstract sub-record providing information on convictions. We also received information from the crash sub-record, a sub-record with information on IID installations and removals, and a sub-record providing information on drinking driver program participation. We focus on all arrests occurring between January 1, 2014 and the end of calendar year 2021. The end-date restriction ensures that we have a two-year post-arrest observation window for each arrest during which we measure IID installation and recidivism outcomes. During our study period, we observe 947,765 DUI arrests for which we can observe a complete two-year post-arrest observation window (631,354 arrests occurring during calendar years 2014 through 2018 and 316,411 occurring in calendar years 2019 through 2021).

Table 5 provides the averages before and after SB 1046 for the proportion of arrests where an IID is installed within two years, the proportion of arrests where we observe a subsequent DUI arrest, the proportion of arrests where we observe a subsequent crash, and the proportion of arrests where we observe a subsequent crash with injury. We observe an increase in the proportion of arrests resulting in an IID installation of 0.0288. The increase is statistically significant at the one percent level of confidence. Regarding the recidivism outcomes, in the pre-SB 1046 period, roughly 8.5 percent of people arrested for a DUI are rearrested for a DUI within two years of the original arrest. This figure increases to 9.7 percent post SB 1046. We observe a similar pattern for the percent of arrested persons who are involved in a crash within

[23]This information is drawn from a December 14, 2021, memo drafted by the Chief of the DMV Research and Development Branch, Bayliss J. Camp titled "Conceptual Overview of the Driver Record Master and Potential Sub-records of Interest for the Analysis of the Effects SB 1046."

Table 5: **Statewide Pre and Post SB 1046 Averages for IID Installation, DUI Recidivism, Post-Arrest Crashes, and Post-Arrest Crash with Injury for the Two-Year Period Following Arrest**

	Before SB 1046	After SB 1046	After-Before
IID Installed	0.1227	0.1516	0.0288[a]
	(0.0004)	(0.0006)	(0.0007)
DUI Recidivism	0.0848	0.0971	0.0123[a]
	(0.0004)	(0.0005)	(0.0006)
Crash	0.0698	0.0851	0.0154[a]
	(0.0003)	(0.0005)	(0.0006)
Crash with	0.0319	0.0396	0.0077[a]
Injury	(0.0002)	(0.0003)	(0.0004)

Standard errors in parentheses.
a. Difference statistically significant at the one percent level of confidence.

two years (6.98 percent in the pre period and 8.51 percent in the post period), and the percent of people involved in a crash with injury (from 3.19 percent to 3.96 percent). All of the pre-post changes are statistically significant.

5 Estimation Results

5.1 Unadjusted Difference-in-Difference Estimates

We begin by presenting the simple average recidivism outcomes for non-AB 91 and AB 91 counties for the pre-SB 1046 period (that is, 2014 through 2018), the post-SB 1046 period (that is, 2019 through 2021), the pre-post changes in outcomes for each set of counties, and the extent to which the change for non-AB 91 counties differs from the comparable change for AB 91 counties. Recall that in Table 3 we document a large relative increase (25 percentage points) in the likelihood that an arrest is followed by an IID installation in the 54 nonAB 91 counties compared to the remaining four counties. To the extent that IID installation impacts recidivism outcomes, we should observe corresponding relative changes in recidivism in the opposite direction, i.e., recidivism rates should fall in the non-AB91 counties relative to the AB91 counties.

Tables 6, 7, and 8 present these tabulations for two-year DUI recidivism rates, two-year crash rates, and two-year crash-with-injury rates. Beginning with DUI recidi-

vism, among the 54 non-AB91 counties we observe a statistically significant increase in DUI recidivism of 0.0110 (or 1.1 percentage points). Among AB 91 counties however, we observe a larger pre-post increase of 0.0152 (or 1.52 percentage points). The change for non-AB91 counties relative to AB 91 counties (the difference-in-difference estimate) is thus -0.0042 (or a 0.42 percentage point relative decline in recidivism). This relative decline is statistically significant at the one-percent confidence level.

The results by offense group reveal comparable relative declines in recidivism rates in the 54 non-AB 91 counties. We observe statistically significant difference-in-difference estimates for first-time offenses (a relative decline of 0.0044, statistically significant at the one percent level of confidence) and for offenses where there are no priors but where an injury occurs (a relative decline of 0.0074, statistically significant at the ten percent level of confidence). While we observe a relative decline for repeat offenses as well (with a point estimate of -0.0031), this estimate is not statistically significant,

Table 7 presents the difference-in-difference analysis for the outcome measuring automobile crashes within two years of the focal arrest. The patterns are similar to what we observe for DUI recidivism, although new crashes are generally somewhat less likely than new DUI arrests. Among people arrested in the non-AB 91 counties, the proportion involved in a new crash increases by 0.0143 (1.43 percentage points) while among arrests in AB 91 counties the proportion increases by 0.0200 (2 percentage points). Together, these imply a pre-post SB 1046 relative change in new crashes of -0.0057 (or .57 percentage points). This relative decline is statistically significant at the one-percent confidence level.

The outcomes by offense group are notably different. Among persons arrested for a first offense, we observe a relative decline in two-year crash rates of 0.0042 (significant at the five percent level of confidence). Among those arrested for a first offense causing injury and those arrested for a repeat offense, the relative declines are more than double the value for first timers. For those arrested for an offense with injury, we see a 0.0096 relative decrease in the proportion involved in a subsequent crash for non-AB91 counties relative to AB91 counties (statistically significant at the five percent level of confidence). The comparable estimate for people with prior convictions is a relative decline of 0.0096 (statistically significant at the one percent level of confidence). The larger estimate for those with priors is particularly striking given that we observe in Table 3 the smallest relative increase in IID installation for

Table 6: **Relative Changes in the Proportion of Arrests Where a New DUI Arrest Occurs Within Two Years of Arrest Between Non-AB91 and AB 91 Counties: All and by Offense Group**

Panel A: All Arrests			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0879	0.0989	0.0110[a]
	(0.0004)	(0.0006)	(0.0007)
AB 91 counties	0.0756	0.0908	0.0152[a]
	(0.0007)	(0.0011)	(0.0012)
Difference	0.0123[a]	0.0081[a]	−0.0042[a]
	(0.0008)	(0.0013)	(0.0015)
Panel B: No priors, no injuries			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0803	0.0905	0.0102[a]
	(0.0005)	(0.0007)	(0.0009)
AB 91 counties	0.0670	0.0816	0.0146[a]
	(0.0008)	(0.0013)	(0.0014)
Difference	0.0133[a]	0.0089[a]	−0.0044[a]
	(0.0010)	(0.0015)	(0.0018)
Panel C: No priors, injury			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0602	0.0662	0.0060[b]
	(0.0015)	(0.0019)	(0.0024)
AB 91 counties	0.0466	0.0603	0.0137[a]
	(0.0021)	(0.0032)	(0.0037)
Difference	0.0136[a]	0.0059	−0.0077[c]
	(0.0027)	(0.0038)	(0.0046)
Panel D: Priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.1079	0.1217	0.0137[a]
	(0.0008)	(0.0012)	(0.0014)
AB 91 counties	0.1023	0.1191	0.0169[a]
	(0.0014)	(0.0023)	(0.0026)
Difference	0.0056[a]	0.0025	-0.0031
	(0.0017)	(0.0025)	(0.0030)

Standard errors in parentheses.
a. Difference statistically significant at the one percent level of confidence.
b. Difference statistically significant at the five percent level of confidence.
c. Difference statistically significant at the ten percent level of confidence.

Table 7: **Relative Changes in the Proportion of Arrests Where the Person is in a Crash Within Two Years of Arrest Between Non-AB91 and AB91 Counties: All and by Offense Group**

Panel A: All Arrests			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0682	0.0824	0.0143[a]
	(0.0004)	(0.0006)	(0.0006)
AB 91 counties	0.0744	0.0943	0.0200[a]
	(0.0007)	(0.0011)	(0.0012)
Difference	−0.0061[a]	−0.0119[a]	−0.0057[a]
	(0.0007)	(0.0012)	(0.0014)
Panel B: No priors, no injuries			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0709	0.0834	0.0125[a]
	(0.0005)	(0.0007)	(0.0008)
AB 91 counties	0.0785	0.0952	0.0167[a]
	(0.0008)	(0.0014)	(0.0012)
Difference	−0.0076[a]	−0.0118[a]	−0.0042[b]
	(0.0009)	(0.0015)	(0.0017)
Panel C: No priors, injury			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0604	0.0669	0.0065[a]
	(0.0015)	(0.0020)	(0.0024)
AB 91 counties	0.0582	0.0742	0.0160[a]
	(0.0023)	(0.0035)	(0.0040)
Difference	0.0021	−0.0073[c]	−0.0094[b]
	(0.0028)	(0.0039)	(0.0047)
Panel D: Priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0642	0.0839	0.0197[a]
	(0.0006)	(0.0010)	(0.0011)
AB 91 counties	0.0684	0.0977	0.0293[a]
	(0.0012)	(0.0021)	(0.0022)
Difference	−0.0042[a]	−0.0138[a]	−0.0096[a]
	(0.0013)	(0.0022)	(0.0025)

Standard errors in parentheses.
a. Difference statistically significant at the one percent level of confidence.
b. Difference statistically significant at the five percent level of confidence.
c. Difference statistically significant at the ten percent level of confidence.

Table 8: **Relative Changes in the Proportion of Arrests Where the Person is in a Crash Involving an Injury Within Two Years of Arrest Between Non-AB 91 and AB 91 Counties: All and by Offense Group**

Panel A: All Arrests			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0310	0.0384	0.0075[a]
	(0.0003)	(0.0004)	(0.0004)
AB 91 counties	0.0346	0.0438	0.0092[a]
	(0.0005)	(0.0008)	(0.0009)
Difference	−0.0036[a]	−0.0054[a]	−0.0018[c]
	(0.0005)	(0.0008)	(0.0009)
Panel B: No priors, no injuries			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0321	0.0384	0.0063[a]
	(0.0003)	(0.0005)	(0.0006)
AB 91 counties	0.0371	0.0445	0.0075[a]
	(0.0006)	(0.0010)	(0.0010)
Difference	−0.0049[a]	−0.0061[a]	-0.0012
	(0.0006)	(0.0011)	(0.0012)
Panel C: No priors, injury			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0285	0.0332	0.0048[b]
	(0.0010)	(0.0014)	(0.0017)
AB 91 counties	0.0272	0.0360	0.0088[a]
	(0.0016)	(0.0026)	(0.0029)
Difference	0.0013	-0.0028	-0.0041
	(0.0019)	(0.0028)	(0.0033)
Panel D: Priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.0291	0.0395	0.0104[a]
	(0.0004)	(0.0007)	(0.0008)
AB 91 counties	0.0305	0.0442	0.0137[a]
	(0.0008)	(0.0014)	(0.0015)
Difference	−0.0015	−0.0047[a]	−0.0032[c]
	(0.0009)	(0.0015)	(0.0017)

Standard errors in parentheses.
a. Difference statistically significant at the one percent level of confidence.
b. Difference statistically significant at the five percent level of confidence.
c. Difference statistically significant at the ten percent level of confidence.

this group (13.32 percent, relative to 27.95 percent for first-time-with-injury arrests, and 30.03 percent for first arrests without injury). Combined these results suggest that IIDs have notably larger effects on subsequent crashes for those with priors, an issue we will return to in the two-stage-least-squared analysis below.

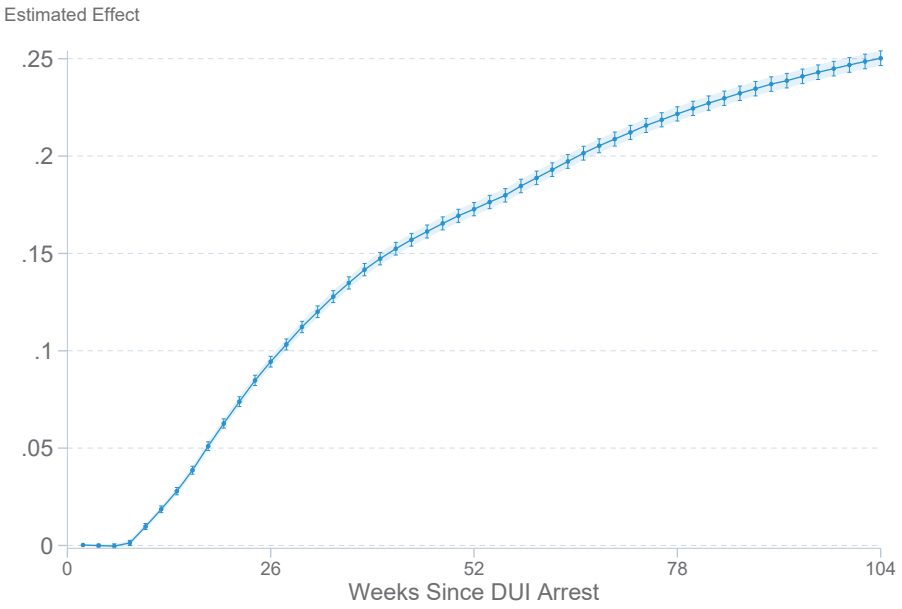
Finally, Table 8 presents the difference-in-difference analysis for crashes that result in an injury. Again, we see pre-post SB 1046 increases in this recidivism measure for both county groups, with the rate increasing by 0.0075 for non-AB91 counties and by 0.0092 for AB 91 counties, implying an overall relative decline in non-AB 91 counties of 0.0018 (statistically significant at the ten percent level of confidence). We observe relative declines within each offense group. However, the relative change is statistically significant (at the ten percent level of confidence) only for those who are arrested and who have priors.

Appendix Tables A3 through A5 reproduce this analysis by subgroups defined by the number of priors using the following groupings: no priors, one prior, two priors, and three or more priors. The results for those without priors are quite similar to what we observe in tables 6 through 8. This is not surprising given that those without priors basically combine first-timers without injury and first-timers who cause an injury. Among those with prior convictions, we see the strongest evidence of an effect of IID on the outcomes for people with one prior conviction. We find no evidence of a relative change in recidivism outcomes among people with two prior convictions and evidence of a relative decline in crashes for those with three priors. We note, however, that we have much smaller sample sizes for these latter two groups.[24]

Figures 6 through 9 visibly depict how the difference-in-difference estimates evolve with time since arrest. Specifically, each figure graphs the difference-in-difference estimate using different post-arrest time windows ranging from one to 104 weeks. Hence, in Figure 6 we observe the relative change in non-AB 91 counties in the proportion installing an IID within one week of arrest, within two weeks of arrest, and so on through two years. Note, the value for two years (the point estimate at 104 weeks) matches the installation difference-in-difference estimate presented in Table 3 for all offense groups combined, while the 104-week estimates for the recidivism outcomes in the subsequent figures match the difference-in-difference estimates for all

[24]Regarding sample size, we observe data for 654,112 arrests where the person arrested has no priors, 187,339 where the person arrested has one prior, 65,843 where the person arrested has two priors, and 40,471 where the person arrested has three or more priors.

Figure 6: **Difference-in-Differences Estimated Effect on Probability of Having Installed a IID Within x Weeks of Arrest**



offense groups combined presented in Tables 6 through 8. Each figure also depicts the 95 percent confidence interval around the individual estimates.

In Figure 6 we see no relative difference in IID installation until about four weeks following arrest. The difference in IID installation rates increases steadily to 25 percentage points by the end of the two-year period. While our observation window ends at two years, the figure suggests that the installation difference-in-difference grows continuously through the end of the observation window (and perhaps would continue to grow beyond two years).

The relative decline in subsequent DUI arrests in non-AB 91 counties (Figure 7) is basically zero for the first six months or so and then begins to emerge. By one year, we observe statistically significant relative declines that increases and stabilizes in year two.

Significant relative declines in crashes (Figure 8) and crashes with injury (Figure 9) appear more quickly. We observe statistically significant relative declines in both outcomes in non-pilot counties within a few months of arrest. The overall effect on crashes appears to grow larger with time, whereas the effect on crashes with injuries stabilizes after six months.

Figure 7: **Difference-in-Differences Estimated Effect on Probability of Another DUI Arrest Within x Weeks of Arrest**

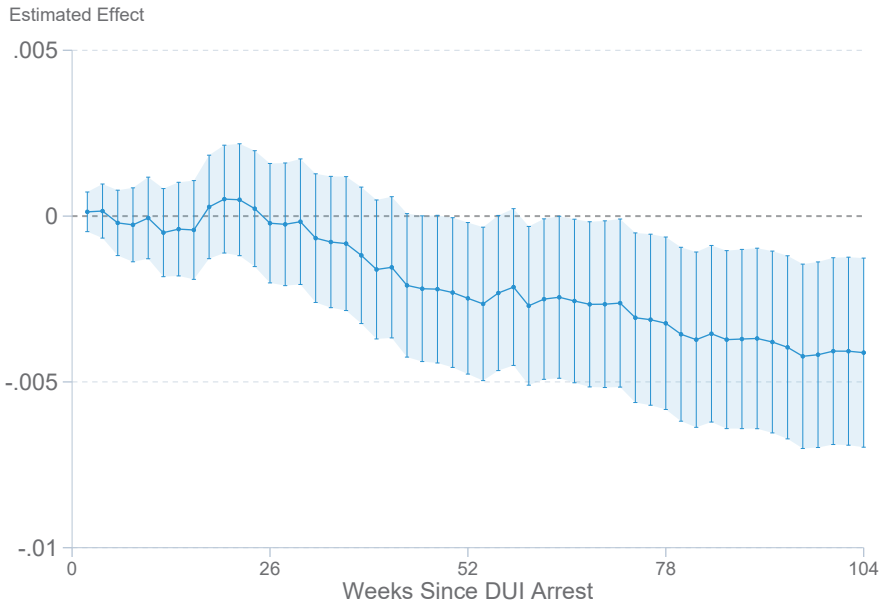


Figure 8: **Difference-in-Differences Estimated Effect on Probability of a Crash Within x Weeks of Arrest**

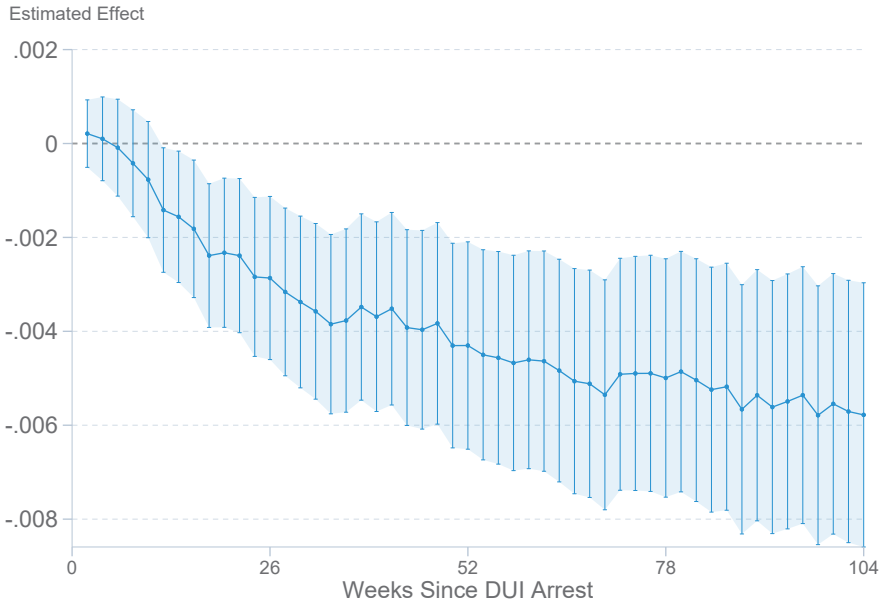
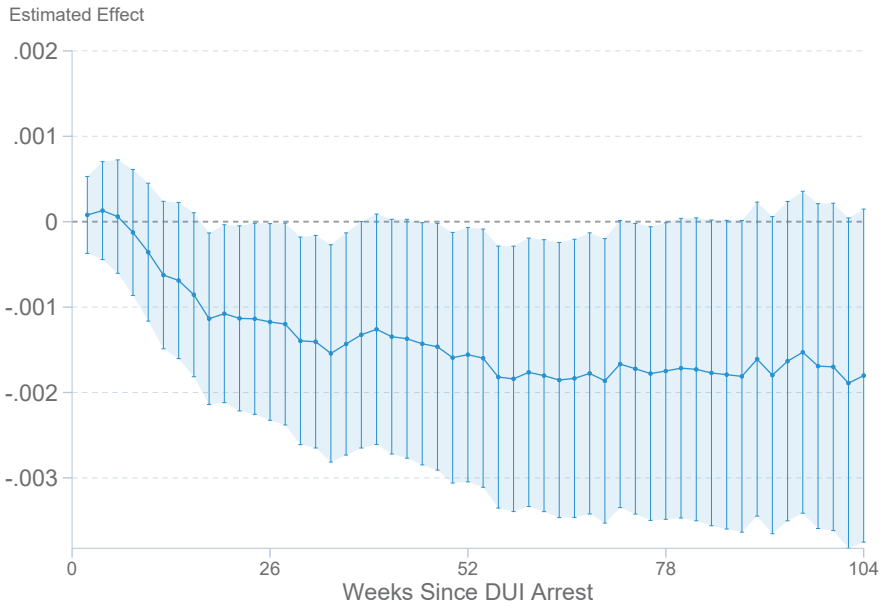


Figure 9: **Difference-in-Differences Estimated Effect on Probability of an Crash With Injury Within x Weeks of Arrest**



5.2 Regression Adjusted Difference-in-Difference Estimates

The results thus far indicate that the relative increase in IID installations in non-AB 91 counties corresponds in time with relative declines in the three recidivism measures under study. One might be concerned that DUI arrests in AB 91 and non-AB 91 counties are somehow fundamentally different from each other or that the composition of DUI arrests in these two county sets differentially change in a manner that may be spuriously creating this pattern. To explore whether this is the case, in this section, we present difference-in-difference estimates that employ multiple regression analysis along with case-level matching to adjust for potential differences in the observable characteristics of DUI arrests that may determine subsequent recidivism outcomes independently of whether an IID is installed.

We estimate regression-adjusted difference-in-difference models for the three recidivism outcomes analyzed in Tables 6 through 8 using the following model and sample specifications:

- **Model 1**: A baseline model including a dummy for non-AB9 91 county, post-SB 1046 and an interaction term between the two variables. The coefficient on

the interaction term provides the unadjusted difference-in-difference estimate corresponding to those provided in Tables 6, 7, and 8.

- **Model 2**: Adding to the specification in model 1 controls for the driver being male, dummy variables indicating having one, two, three, four, or five or more prior DUIs, a quadratic in age at the time of arrest, a quadratic in the maximum BAC reading at arrest, a dummy variable indicating BAC missing, and interactions of the male dummy with all of these additional control variables.

- **Model 3**: Adding a complete set of year-month and county-of-residence fixed effects to the specification of model 2.

- **Model 4:** Estimation of Model 3 in a sample where each AB 91 county arrest is matched to a non-AB 91 county arrest using exact matching in age, sex, arrest date, and number of previous alcohol-related convictions and nearest-neighbor matching for the highest BAC reading at arrest.

Table 9 presents these results. Here, we omit the bulk of the model coefficients and present only the difference-in-difference estimates from each model specification for each outcome. Beginning with the DUI recidivism outcome, we observe statistically significant relative declines in recidivism in non-AB 91 counties in all models, with point estimates varying from -0.0032 in model 3 (the full specification but not using the matched sample) to -0.0042 in model 1 (the unadjusted difference-in-difference estimate). The four estimates are within each other's confidence intervals. Interestingly, adding an extensive set of covariates has little impact on the standard errors, although we have less precision in the model based on the sample of AB91 county arrests matched to arrests from other counties.[25]

Turning to the crash recidivism outcome, we find statistically significant relative declines in crash recidivism in non-AB91 counties in all model specifications, with each estimate statistically significant at the one percent level of confidence. The estimates employing the full sample of arrests range from -0.0053 in model (3) to -0.0057 in model (1). We observe the largest point estimate for the model using the full covariate vector and estimated on the matched sample (a relative decline of 0.0074

[25]Note, this sample is by construction, smaller than the full sample of arrests and thus we have slightly larger standard errors.

in the non-AB 91 counties relative to the AB 91 counties). Again, all of the estimates lie within each others' confidence intervals.

Finally, for the crashes-with-injury recidivism outcome, we again find relative declines in non-AB 91 counties. These estimates are significant at the ten-percent confidence level in the unadjusted model (model 1), the model using the full sample and the full vector of covariates (model 3), and the model employing the matched sample (model 4). Again we find that the magnitude of the estimates is generally not sensitive to the model specification. For all three outcomes, the general stability of the estimates indicates that the relative change in IID installation is unrelated to observable characteristics of the arrests, and thus bolsters the identification assumption that the relative shift in IID installations provides exogenous variation in our key explanatory variable of interest.

These difference-in-difference estimates present tests of the hypothesis that recidivism rates decline in the county group experiencing relative increases in IID installation rates. The findings strongly suggest that this is indeed the case. However, it is difficult to interpret the magnitude of these estimates as they reflect the product of the change in policy in installation rates and the effect of IID installation on recidivism. We now turn to generating structural estimates of the effect of an IID installation on the three recidivism outcomes.

5.2.1 Two stage least squares estimates of the effect of IID installation

The difference-in-difference analysis clearly indicates that the relative increase in IID installation in non-AB 91 counties corresponded in time with a relative decrease in recidivism among people arrested for a DUI. Moreover, this result is robust to controlling for observable arrest characteristics and estimating the model using a matched sample.

For policy evaluation as well as comparison with results from previous studies, it is helpful to have structural estimates of the effects of IID installation on each of these recidivism outcomes. Here, we employ two-stage least squares to generate such estimates. As discussed in the methods section above, we estimate models where the first stage equation models IID installation and the specification is basically the right-hand side of equation 1 above, and the second stage models a recidivism outcome as a function of installing an IID within two years (with the specification corresponding to 2 above). The interaction term between the post-SB 1046 dummy variable and

Table 9: **OLS Difference-in-Difference Estimates: Change in Recidivism Outcomes, Non-AB 91 Relative to AB 91 Counties**

Two-year recidivism outcome	Model (1)	Model (2)	Model (3)	Model (4)
DUI arrest	-0.0042^{a}	-0.0045^{a}	-0.0032^{b}	-0.0034^{c}
	(0.0015)	(0.0014)	(0.0014)	(0.0018)
Crash	-0.0057^{a}	-0.0055^{a}	-0.0053^{a}	-0.0074^{a}
	(0.0014)	(0.0014)	(0.0014)	(0.0017)
Crash with injury	-0.0018^{c}	-0.0016	-0.0017^{c}	-0.0020^{c}
	(0.0010)	(0.0010)	(0.0010)	(0.0012)
Controls	No	Yes	Yes	Yes
Year-month effects	No	No	Yes	Yes
County effects	No	No	Yes	Yes
Matched sample	No	No	No	Yes
N	947,765	947,765	947,765	459,198

Robust standard errors are in parentheses. The vector of control variables include a male dummy, dummies for having one, two, three, four, or five or more prior DUIs, a quadratic in age at arrest, a quadratic in the maximum BAC reading at arrest, a dummy variable indicating BAC missing, and interactions of the male dummy with all other control variables. County effects measure county of resident and the year-month effects measure the year/month of arrest.

a. Estimate significant at the one percent level of confidence.
b. Estimate significant at the five percent level of confidence.
c. Estimate significant at the ten percent level of confidence.

the non-pilot county dummy variable serves as the instrument in this just-identified model.

We estimate two-stage least squares models for each of the four specifications that we employ in the OLS analysis. Table 10 presents the results. The top portion of the table presents the estimates of the effect of installing an IID on each recidivism outcome using the four model specifications discussed above. The bottom panel presents the first stage coefficient on the interaction term between post-SB 1046 and the non-AB 91 county dummy (the difference in difference in IID installation rates), along with the coefficient standard error, an F-test of the significance of the instrument in the first stage, and a corresponding p-value.

Installing an IID within two years reduces DUI recidivism in the next two years by between 1.3 and 2.3 percentage points. The estimate of the model without covariates is 1.68 percentage points. Three of the estimates are statistically significant at the one percent confidence level (models (1), (2), and (4)), while one estimate (model (3)) is statistically significant at the five percent level of confidence.

We see somewhat larger absolute effects on crash recidivism. The estimated effects of installing an IID range from a 2.19 percentage point reduction in crashes (model 3) to a 2.29 percentage point reduction (model 4). All estimates for this outcome are statistically significant at the one-percent confidence level.

Finally, estimates of the effects of installing an IID on subsequent crashes with injury suggest reductions ranging from 0.65 percentage points (model 2) to 1.11 percentage points (model 4). Two of the estimates (Models 1 and 3) are significant at the ten-percent level of significance, while the estimate using the matched sample is significant at the five-percent level of confidence.

To contextualize these estimates, we calculate a counterfactual baseline recidivism rate assuming zero IID installations and then measure the declines implied by the effects in Table 10 relative to this counterfactual. To be specific, in Table 5 we see that in the post-SB 1046 period, 15.16 percent of DUI arrests result in an IID installation within two years, 9.71 percent are re-arrested for a DUI within two years, 8.51 percent are involved in a subsequent crash, and 3.96 percent are involved in a crash that causes an injury. With the effect size estimates in Table 10, we can calculate what recidivism rates would be during these periods if no one installed an IID after arrest.[26] Using the estimates from model (1), we estimate counterfactual

[26]To do so, we use the estimates from the models with no covariates since the model estimates

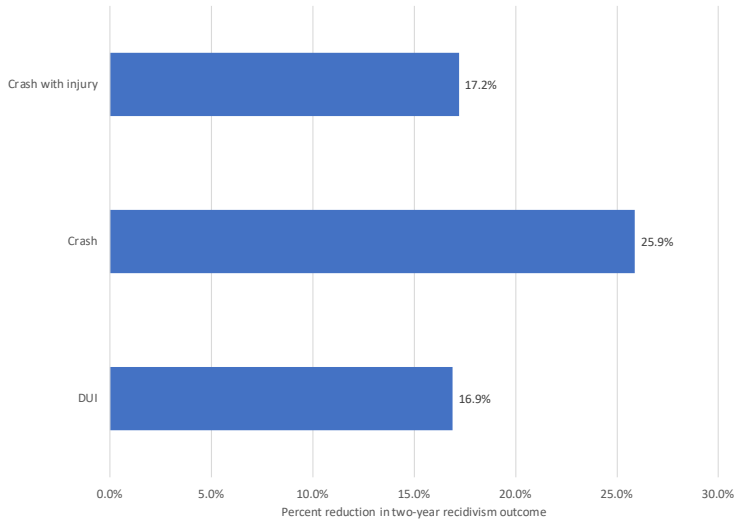
Table 10: **Two Stage Least Squared Estimates of the Effect of Installing an IID on Recidivism Outcomes Two Years Following Arrest**

Two-Year recidivism outcome	Model (1)	Model (2)	Model (3)	Model (4)
DUI arrest	-0.0168^{a}	-0.0179^{a}	-0.0129^{b}	-0.0234^{a}
	(0.0058)	(0.0058)	(0.0059)	(0.0071)
Crash	-0.0229^{a}	-0.0220^{a}	-0.0219^{a}	-0.0214^{a}
	(0.0057)	(0.0057)	(0.0059)	(0.0058)
Crash with injury	-0.0070^{c}	-0.0065	-0.0068^{c}	-0.0111^{b}
	(0.0040)	(0.0040)	(0.0040)	(0.0048)
Controls	No	Yes	Yes	Yes
Year-month effects	No	No	Yes	Yes
County effects	No	No	Yes	Yes
Matched sample	No	No	No	Yes
First-stage coefficient	0.2506^{a}	0.2503^{a}	0.2456^{a}	0.2538^{a}
	(0.0012)	(0.0019)	(0.0019)	(0.0020)
F-statistic	16,884	17,535	16,949	12,477
(p-value)	(< 0.0001)	(< 0.0001)	(< 0.0001)	(< 0.0001)
N	947,765	947,765	947,765	459,198

Robust standard errors are in parentheses. The vector of control variables include a male dummy, dummies for having one, two, three, four, or five or more prior DUIs, a quadratic in age at arrest, a quadratic in the maximum BAC reading at arrest, a dummy variable indicating BAC missing, and interactions of the male dummy with all other control variables. County effects measure county of resident and the year-month effects measure the year/month of arrest. The first-stage coefficient is the coefficient on the interaction term between non-pilot counties and the after dummy variable. Note, this interaction terms is omitted from the second-stage model. The F-statistic rest the significance of the instrument in the first-stage model.

a. Estimate significant at the one percent level of confidence.
b. Estimate significant at the five percent level of confidence.
c. Estimate significant at the ten percent level of confidence.

Figure 10: **Estimated Percent Reduction in Two-Year Recidivism Rate Caused by Installing an IID Within Two Years of Arrest**



two-year recidivism rates with zero IID installations of 9.96 percent, 8.86 percent, and 4.07 percent for DUI, crash, and crash-with injury recidivism, respectively.

Dividing the effect sizes presented in Table 10 by these counterfactual recidivism rates provides an estimate of the percentage reduction in two-year recidivism caused by installing an IID within two years of arrest. Figure 10 visualizes these tabulations. The findings suggest that installing an IID within two years reduces two-year recidivism rates by 16.9 percent for new DUIs, by 25.9 percent for new crashes, and by 17.2 percent for crashes with injury.

It is also instructive to characterize the findings in Table 10 with respect to the findings from past research on the effectiveness of IIDs. Regarding DUI recidivism, the results from the Maryland RCT evaluation by Beck et al. (1999) find two-year

are generally insensitive to the inclusion of controls. Here we illustrate this tabulation for the DUI recidivism outcome. Using the estimates from model (1) for DUI recidivism, the counterfactual recidivism rate with zero IID installations would be 0.0971 + 0.0168x0.1516, where 0.0971 is the observed recidivism rate in the post SB 1046 period, 0.0168 is the reduction in the likelihood of recidivating for this outcome caused by installing an IID, and 0.1516 is the proportion that installed an IID in the post period. This suggests a counterfactual recidivism rate with zero installations of 0.0996 for DUI arrest within two years. Performing similar calculations for crashes and crashes with injury yield counterfactual recidivism rates of 0.0886 and 0.0407, respectively.

recidivism rates of 9.1 percent for the experimental control group and 5.9 percent for the treatment group, yielding a two-year effect size of 3.2 percentage points.[27] In Table 10, our effect size estimates range from roughly 1.7 to 2.3 percentage points for this outcome. Although the estimates are a bit smaller than what was observed in the RCT, they are within the range and suggest that our findings on DUI recidivism are in line with the findings from previous research.

Our review of the literature noted the conflicting findings related to the effects of IID installation on crashes. For example, Kaufman and Wiebe (2016) find in an analysis of state panel data that a mandatory IID requirement for all DUI convictions reduces alcohol-related crash fatalities by 15 percent. In a more recent and similar analysis, Teoh et al. (2021) finds that universal IID requirements reduce alcohol-related crash fatalities by 26 percent. In contrast, earlier AB91 evaluations found that while alcohol-related crashes decreased, IIDs increased the overall number of crashes when those who install an IID are compared with people subject to a hard license suspension (Department of Motor Vehicles, 2016). The results here, which essentially contrast people who are arrested for a DUI who install an IID to similar people who do not, find reductions in both overall crashes and crashes involving injury. The magnitudes (25.9 and 17.2 percent reductions, respectively) align with the findings from the state-level panel data studies.

Tables 11 through 13 present two-stage least squares estimates of the effects of IID installations on the three recidivism outcomes where we estimate separate models by the three offense subgroups specified by SB 1046. In Table 11 and 12 we observe statistically significant and sizable negative effects of IID installation on future DUI arrests for persons whose focal arrest was a first offense, as well as for persons whose first focal arrest was a first offense involving an injury, with the largest effects for the latter group. We also find significant preventive effects of IIDs on future crashes for these two groups, again with effect size larger in absolute value for cases where the focal offense involved an injury. Turning to people with priors, in Table 13 we observe negative point estimates for the effects of IIDS on future DUIs, although none of the estimates are statistically significant. However, we observe the largest effects of IID on future crashes for this group as well as future crashes involving injury.

[27]It is interesting to note that the two-year recidivism rate for the control group in this RCT of 9.1 percent is quite close to our counterfactual calculation of what DUI recidivism would have been in the post SB 1046 period with zero installations (9.96 percent).

All subgroup estimates are generally less precise than the estimates that pool all DUI arrests, due to the smaller sample sizes used to estimate the two-stage least squares models. Hence, for several of the subgroup estimates where we do not find statistically significant effects, the lower power associated with smaller sample sizes may prevent us from measuring meaningful effects of an IID for the outcome in question. However, we find evidence of the effectiveness of IID installation for each individual group for at least two of the three outcomes that we study.

We also estimated these models for the subgroups defined by the number of prior convictions. These results are presented in appendix tables A6 through A9. The results are quite similar to what we see when we estimate by offense subgroups. We observe the strongest evidence or an effect of IIDs on DUI recidivism among people with no prior convictions (appendix table A6) and effects on crashes and crashes with injury for people with one prior conviction (appendix table A7). We do not find statistically significant effects for any of the outcomes for people with two priors or people with three or more priors. However, the samples used to estimate these models are comparatively small and our estimated effects are imprecise.

Finally, appendix table A10 presents two-stage least squares estimates of the effects of IID installation on four additional outcomes: subsequent alcohol-involved crashes, subsequent alcohol-involved crashes with injury, subsequent fatal crashes, and subsequent fatal crashes involving alcohol. All outcomes are measured over the two-year period after focal arrest. For reasons we discussed above, we know that information pertaining to whether a motorist had been drinking is missing for at least 40 percent of crashes we observe in the data. Moreover, crashes involving fatalities are quite rare, and thus it is difficult to detect an impact for this outcome. With these caveats in mind, we find no evidence of an impact of IIDs on these additional outcomes.

6 Discussion

The previous section finds sizable effects of IID installation on the various measures of recidivism among people arrested for DUI. Of course, these devices are effective only to the extent that persons who are required to install an IID actually install a device. The net effect of a mandatory IID requirement will be a function of both the effectiveness of the device in preventing future driving under the influence and the

Table 11: **Two Stage Least Squared Estimates of the Effect of Installing an IID on Recidivism Outcomes Two Years Following Arrest: First-Time Offenses**

Two-Year recidivism outcome	Model (1)	Model (2)	Model (3)	Model (4)
DUI arrest	-0.0147^{a}	-0.0158^{a}	-0.0134^{b}	-0.0112
	(0.0058)	(0.0058)	(0.0059)	(0.0072)
Crash	-0.0140^{b}	-0.0134^{b}	-0.0132^{b}	-0.0209^{a}
	(0.0061)	(0.0060)	(0.0061)	(0.0073)
Crash with injury	-0.0040	-0.0036	-0.0041	-0.0061
	(0.0042)	(0.0042)	(0.0043)	(0.0092)
Controls	No	Yes	Yes	Yes
Year-month effects	No	No	Yes	Yes
County effects	No	No	Yes	Yes
Matched sample	No	No	No	Yes
First-stage coefficient	0.3003^{a}	0.3000^{a}	0.2954^{a}	0.2981^{a}
	(0.0023)	(0.0023)	(0.0023)	(0.0020)
F-statistic	17,047	17,013	16,495	22,215
(p-value)	(< 0.0001)	(< 0.0001)	(< 0.0001)	(< 0.0001)
N	595,327	595,327	595,327	301,656

Robust standard errors are in parentheses. The vector of control variables include a male dummy, dummies for having one, two, three, four, or five or more prior DUIs, a quadratic in age at arrest, a quadratic in the maximum BAC reading at arrest, a dummy variable indicating BAC missing, and interactions of the male dummy with all other control variables. County effects measure county of resident and the year-month effects measure the year/month of arrest. The first-stage coefficient is the coefficient on the interaction term between non-pilot counties and the after dummy variable. Note, this interaction terms is omitted from the second-stage model. The F-statistic rest the significance of the instrument in the first-stage model.

a. Estimate significant at the one percent level of confidence.
b. Estimate significant at the five percent level of confidence.
c. Estimate significant at the ten percent level of confidence.

Table 12: **Two Stage Least Squared Estimates of the Effect of Installing an IID on Recidivism Outcomes Two Years Following Arrest: First-Time Offenses With Injury**

Two-Year recidivism outcome	Model (1)	Model (2)	Model (3)	Model (4)
DUI arrest	-0.0276^{c}	-0.0296^{c}	-0.0234	-0.0271
	(0.0162)	(0.0162)	(0.0164)	(0.0193)
Crash	-0.0336^{c}	-0.0313^{c}	-0.0308^{c}	-0.0116
	(0.0174)	(0.0175)	(0.0178)	(0.0205)
Crash with injury	-0.0145	-0.0128	-0.0129	-0.0046
	(0.0123)	(0.0124)	(0.0126)	(0.0145)
Controls	No	Yes	Yes	Yes
Year-month effects	No	No	Yes	Yes
County effects	No	No	Yes	Yes
Matched sample	No	No	No	Yes
First-stage coefficient	0.2795^{a}	0.2779^{a}	0.2724^{a}	0.2899^{a}
	(0.0072)	(0.0071)	(0.0071)	(0.0084)
F-statistic	1,507	1,532	1,471	1,191
(p-value)	(< 0.0001)	(< 0.0001)	(< 0.0001)	(< 0.0001)
N	58,785	58,785	58,785	30,934

Robust standard errors are in parentheses. The vector of control variables include a male dummy, dummies for having one, two, three, four, or five or more prior DUIs, a quadratic in age at arrest, a quadratic in the maximum BAC reading at arrest, a dummy variable indicating BAC missing, and interactions of the male dummy with all other control variables. County effects measure county of resident and the year-month effects measure the year/month of arrest. The first-stage coefficient is the coefficient on the interaction term between non-pilot counties and the after dummy variable. Note, this interaction terms is omitted from the second-stage model. The F-statistic rest the significance of the instrument in the first-stage model.

a. Estimate significant at the one percent level of confidence.
b. Estimate significant at the five percent level of confidence.
c. Estimate significant at the ten percent level of confidence.

Table 13: **Two Stage Least Squared Estimates of the Effect of Installing an IID on Recidivism Outcomes Two Years Following Arrest: Repeat Offenses**

Two-Year recidivism outcome	Model (1)	Model (2)	Model (3)	Model (4)
DUI arrest	-0.0236	-0.0236	-0.0076	-0.0182
	(0.0225)	(0.0225)	(0.0235)	(0.0274)
Crash	-0.0717^a	-0.0707^a	-0.0692^a	-0.0860^a
	(0.0199)	(0.0120)	(0.0207)	(0.0238)
Crash with injury	-0.0242^c	-0.0235^c	-0.0238^c	-0.0220
	(0.0138)	(0.0138)	(0.0144)	(0.0165)
Controls	No	Yes	Yes	Yes
Year-month effects	No	No	Yes	Yes
County effects	No	No	Yes	Yes
Matched sample	No	No	No	Yes
First-stage coefficient	0.1332^a	0.1327^a	0.1280^a	0.1394^a
	(0.0037)	(0.0036)	(0.0036)	(0.0047)
F-statistic	1,296	1,359	1,264	880
(p-value)	(< 0.0001)	(< 0.0001)	(< 0.0001)	(< 0.0001)
N	293,653	293,653	293,653	126,608

Robust standard errors are in parentheses. The vector of control variables include a male dummy, dummies for having one, two, three, four, or five or more prior DUIs, a quadratic in age at arrest, a quadratic in the maximum BAC reading at arrest, a dummy variable indicating BAC missing, and interactions of the male dummy with all other control variables. County effects measure county of resident and the year-month effects measure the year/month of arrest. The first-stage coefficient is the coefficient on the interaction term between non-pilot counties and the after dummy variable. Note, this interaction terms is omitted from the second-stage model. The F-statistic rest the significance of the instrument in the first-stage model.

a. Estimate significant at the one percent level of confidence.
b. Estimate significant at the five percent level of confidence.
c. Estimate significant at the ten percent level of confidence.

extent to which people comply with the requirements or respond to the opportunities to install a device in lieu of some other sanction.

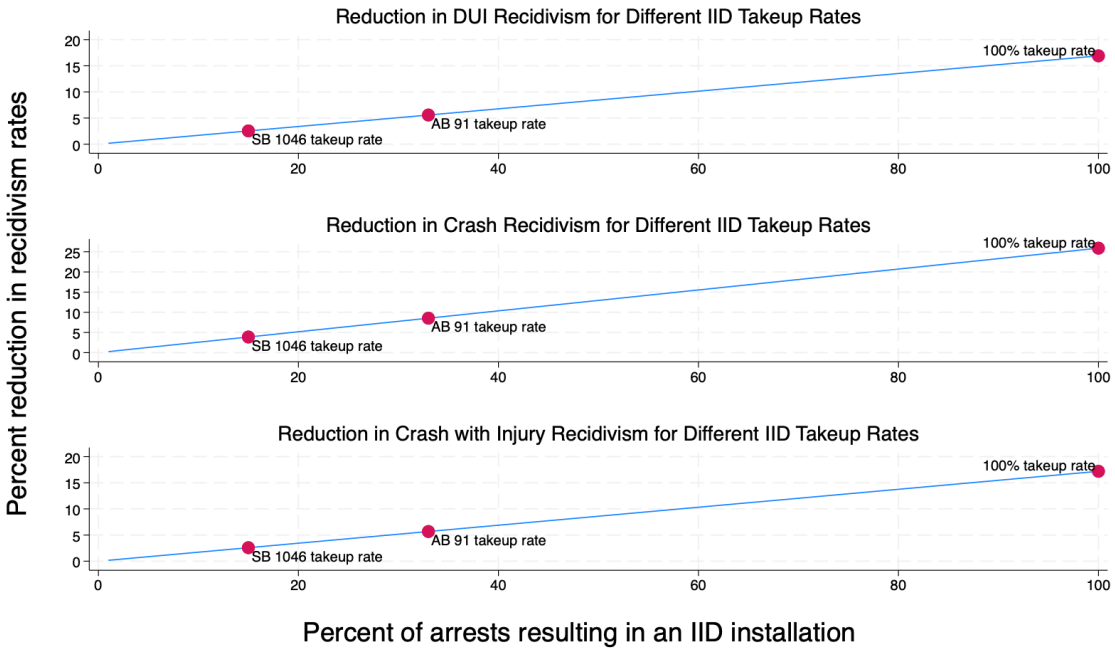
What we observe in the post-SB1046 period is that most DUI arrests do not result in an IID installation (roughly 15 percent install). As we have discussed in detail, this low rate is due to many factors, including conviction rates that are less than 100 percent and lower in recent years, the non-universality of the mandatory IID requirements following the 2019 policy changes, and people choosing to not install an IID even when it is a requirement for restoring driving privileges.

What then is the ultimate impact of this policy on recidivism rates among persons arrested for a DUI? How does this effect depend on the proportion of people who install an IID? Although we cannot answer these questions with certainty, we can use the calculations from the previous section along with the observed installation rates to answer these two questions.

Figure 11 presents the projected percent reduction in recidivism as a function of the percent of arrests that result in an IID installation for the three recidivism outcomes that are the focus of this study. Although the line in each figure shows the complete projected relationship between the percentage reduction in recidivism and the IID installation rate, each figure also highlights three specific points: the recidivism reduction associated with a 100 percent installation rate, the recidivism reductions associated with the installation rates under the earlier more inclusive pilot, and the recidivism reduction associated with the statewide installation rates observed post 2019 (roughly 15 percent). The recidivism reductions with 100 percent installation rates correspond to the estimated effects of installing a device presented in Figure 10 (16.9 percent reduction for DUI recidivism, 25.9 percent reduction for crash recidivism, and 17.2 percent reduction for crash-with-injury recidivism).

This exercise suggests that the overall effects of requiring IIDs on these recidivism measures are small. The declines among those arrested for a DUI during the post period are on the order of 2.5 percent for the likelihood of another DUI within two years, 3.9 percent for the likelihood of a crash within two years, and 2.6 percent for the likelihood of a crash with an injury within two years relative to a counterfactual with zero installations. The figure also illustrates that higher installation rates would correspond with larger policy impacts.

Figure 11: **Reduction in Statewide Two-Year Recidivism Rates Among Those Arrested for a DUI as a Function of the Percent of Arrests where an IID is Installed Within Two Years**



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Table A1: **Percent of Arrests where an IID is Installed within Two Years of the Arrest, by Arrest Year and County**

County	2014	2015	2016	2017	2018	2019	2020	2021
Alameda	21.9	20	15.6	15.7	14.6	10.3	7.9	10.1
Alpine	-	-	-	-	-	-	-	-
Amador	6.7	6.8	4.9	3.2	2.8	35.5	29.5	16.8
Butte	3.8	4.6	5.4	3.8	4.4	18	18.3	21.4
Calaveras	5.3	6.9	10.8	4.1	7.8	24.7	24.1	19.8
Colusa	1.9	8.3	4.1	7.1	10.5	24	16.7	9.4
Contra Costa	3.6	3.1	2.1	3.4	3.1	11.9	11.8	10.2
Del Norte	3.6	1	7.8	4.7	3.6	13.7	12.3	12.8
El Dorado	4.9	6.4	7.9	9.5	6.9	27.9	25.8	25.8
Fresno	2.7	3.1	3.9	3.3	2.9	9.8	10.3	11.1
Glenn	5.8	6.7	2.1	2.8	1.5	18	18.1	16.7
Humboldt	3.7	3.3	5.3	4.5	4.3	13.9	11.6	12.4
Imperial	1.8	0.9	1.4	1.8	2.1	11.9	10.5	8.5
Inyo	1.6	4.3	8.4	3.8	3	19.6	9.9	16.7
Kern	3.1	2.5	3.3	2.8	2.6	7.6	6.9	7.2
Kings	1.6	1.8	2.2	3.4	2.9	11.7	12.3	12.5
Lake	4.3	2.7	3.5	4.9	2.8	11.3	8.5	12.8
Lassen	4.3	6	0.6	1	5.1	12.8	10.4	15.4
Los Angeles	34	33.4	33.1	33.4	31.7	17.9	13.8	14.2
Madera	1.2	0.7	1.7	1.8	1.2	8.1	6.9	6.1
Marin	3.9	4.2	4.7	5.2	7.7	21.1	13.2	20.5
Mariposa	1.8	5.1	8	4.2	6.2	29.3	22.6	25.5
Mendocino	5.9	4.5	5.5	3.6	6.1	22.5	18.2	20.7
Merced	1.1	2.7	1.9	2.5	2.6	11.1	9.5	9.1
Modoc	1.9	0	5.3	0	4.8	9.1	20.8	9.7
Mono	5.5	3.1	2.3	3.4	3.8	19.7	14.5	18.1
Monterey	3.9	4.9	5.7	5.4	6.6	22.6	17.8	17.4
Napa	7.2	6.3	8.2	10.2	9.1	27.6	28.9	24.9
Nevada	4.4	6.2	7.5	6.5	7.1	25.4	25.8	26.7
Orange	5.9	5.9	6.1	5.8	4.5	20.1	16.8	17.9
Placer	6.7	9.2	8.8	9.1	9.7	25.7	27.4	27.8

Table A1: **Percent of Arrests where an IID is Installed within Two Years of the Arrest, by Arrest Year and County**

County	2014	2015	2016	2017	2018	2019	2020	2021
Plumas	5.3	3	7.8	7.9	5.9	23.7	17.8	19.6
Riverside	2.7	3.3	3.1	3.3	4.4	18.4	13.3	13.5
Sacramento	33.7	33.3	32.9	35.5	32.4	23.6	16.6	16.8
San Benito	4.6	3.1	4.8	3.8	4.4	17.1	12.8	21.3
San Bernardino	3.1	2.6	3.3	3.1	3.2	12.4	10.2	10
San Diego	4.1	4.3	4.8	4.1	4.7	18.9	12.5	13.8
San Francisco	3.2	2.7	1.9	1.6	3	10.4	7.2	7.8
San Joaquin	6	5.8	5.8	5.7	5.9	16.6	12.9	13.4
San Luis Obispo	4.2	5.4	4.5	5.2	6	21.4	15.8	17.7
San Mateo	3.1	3.3	3.2	5.2	5.3	15.4	12.5	13.6
Santa Barbara	4.2	3.3	3.6	3.7	4.2	13.1	10.4	11.9
Santa Clara	4	4.5	4.4	5.1	4.7	15.1	12.8	16.9
Santa Cruz	3.8	3.6	3.3	5.1	4.8	20.1	16.4	19.3
Shasta	9.1	6.8	8.6	6.9	11.8	25.5	23.5	21.7
Sierra	-	-	-	-	-	-	-	-
Siskiyou	3.5	3	3.9	3.8	2.5	14.7	12.9	10.8
Solano	6.7	4	4.1	3.9	3.7	15.1	12.9	14.5
Sonoma	7.6	6.4	7.7	7.5	6.5	30.8	28.9	33.6
Stanislaus	3.3	2.9	3.5	3.7	4.4	12.8	10.8	10.1
Sutter	4.9	4.7	5	6.3	4	18.8	15.8	16.3
Tehama	3.4	3.2	5.5	3.7	5.4	14.6	9.8	12.6
Total	12.5	11.7	11.4	11.1	10.4	16.9	13.6	14.5
Trinity	2.8	7.1	1.8	1.2	4.7	16.9	15.9	12.4
Tulare	24.7	21.9	26	26	23.4	15.9	14.3	12.3
Tuolumne	7.3	5.5	4.4	3.4	7.9	18.5	14.9	16.5
Ventura	6.6	7	6.6	6.2	6.4	23.5	16.8	19.6
Yolo	4.1	3.2	6	6.1	4.2	19.6	14.8	16.5
Yuba	4.4	5.5	5.3	5.6	2.6	10.7	9.3	13.2

Figures are suppressed for cells with fewer than 30 cases.

Table A2: **Relative Changes in the Proportion of Arrests Where an IID is Installed Within Two Years of Arrest Between Non-AB 91 and AB 91 Counties by the Number of Prior Convictions**

Panel A: No Priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.029	0.134	0.105^a
	(0.000)	(0.001)	(0.001)
AB 91 counties	0.329	0.135	-0.193^a
	(0.001)	(0.002)	(0.002)
Difference	-0.300^a	0.001	0.298^a
	(0.001)	(0.002)	(0.002)
Panel B: One prior			
	0.001	0.002	0.002
Non-AB 91 counties	0.143	0.218	0.076^a
	(0.001)	(0.002)	(0.002)
AB 91 counties	0.284	0.205	-0.079^a
	(0.003)	(0.004)	(0.005)
Difference	-0.141^a	0.013^a	0.154^a
	(0.003)	(0.004)	(0.005)
Panel C: Two prior			
	0.003	0.004	0.005
Non-AB 91 counties	0.126	0.184	0.057^a
	(0.002)	(0.003)	(0.003)
AB 91 counties	0.218	0.161	-0.057^a
	(0.004)	(0.005)	(0.007)
Difference	-0.091^a	0.023^a	0.114^a
	(0.004)	(0.006)	(0.007)
Panel C: Three or more priors			
	0.004	0.006	0.007
Non-AB 91 counties	0.063	0.097	0.035^a
	(0.002)	(0.003)	(0.003)
AB 91 counties	0.117	0.087	-0.029^a
	(0.004)	(0.005)	(0.007)
Difference	-0.054^a	0.010^c	0.064^a
	(0.004)	(0.006)	(0.007)

Standard errors in parentheses.
a. Difference statistically significant at the one percent level of confidence.

Table A3: **Relative Changes in the Proportion of Arrests Where a New DUI Arrest Occurs Within Two Years of Arrest Between Non-AB 91 and AB 91 Counties by the Number of Prior Convictions**

Panel A: No Priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.079	0.088	0.009^a
	(0.000)	(0.001)	(0.001)
AB 91 counties	0.065	0.079	0.014^a
	(0.001)	(0.001)	(0.001)
Difference	0.014^a	0.009^a	-0.005^a
	(0.001)	(0.001)	(0.002)
Panel B: One prior			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.101	0.112	0.012^a
	(0.001)	(0.001)	(0.002)
AB 91 counties	0.092	0.107	0.015^a
	(0.002)	(0.003)	(0.003)
Difference	0.009^a	0.006^c	-0.003
	(0.002)	(0.003)	(0.004)
Panel C: Two priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.114	0.128	0.014
	(0.002)	(0.002)	(0.003)
AB 91 counties	0.110	0.128	0.018
	(0.003)	(0.005)	(0.006)
Difference	0.004	0.000	-0.004
	(0.004)	(0.006)	(0.007)
Panel C: Three or more priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.132	0.152	0.020^a
	(0.002)	(0.003)	(0.004)
AB 91 counties	0.138	0.160	0.022^a
	(0.004)	(0.007)	(0.008)
Difference	-0.007	-0.008	-0.001
	(0.005)	(0.007)	(0.009)

Standard errors in parentheses.
a. Difference statistically significant at the one percent level of confidence.

Table A4: **Relative Changes in the Proportion of Arrests Where the Person is in a Crash Within Two Years of Arrest Between Non-AB 91 and AB 91 Counties by the Number of Prior Convictions**

Panel A: No Priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.070	0.082	0.012^a
	(0.000)	(0.001)	(0.001)
AB 91 counties	0.077	0.093	0.016^a
	(0.001)	(0.001)	(0.001)
Difference	-0.007^a	-0.011^a	-0.005^a
	(0.001)	(0.001)	(0.002)
Panel B: One prior			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.065	0.083	0.018^a
	(0.001)	(0.001)	(0.001)
AB 91 counties	0.070	0.098	0.028^a
	(0.001)	(0.003)	(0.003)
Difference	-0.005^a	-0.015^a	-0.010^a
	(0.002)	(0.003)	(0.003)
Panel C: Two priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.061	0.085	0.024^a
	(0.001)	(0.002)	(0.002)
AB 91 counties	0.064	0.093	0.030^a
	(0.002)	(0.004)	(0.005)
Difference	-0.002	-0.009^c	-0.006
	(0.003)	(0.005)	(0.005)
Panel C: Three or more priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.064	0.087	0.023^a
	(0.002)	(0.003)	(0.003)
AB 91 counties	0.069	0.103	0.034^a
	(0.003)	(0.006)	(0.006)
Difference	-0.005	-0.016^a	-0.011^c
	(0.004)	(0.006)	(0.007)

Standard errors in parentheses.
a. Difference statistically significant at the one percent level of confidence.

Table A5: **Relative Changes in the Proportion of Arrests Where the Person is in a Crash Involving an Injury Within Two Years of Arrest Between Non-AB 91 and AB 91 Counties by the Number of Prior Convictions**

Panel A: No Priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.032	0.038	0.006[a]
	(0.000)	(0.000)	(0.001)
AB 91 counties	0.036	0.044	0.007[a]
	(0.001)	(0.001)	(0.001)
Difference	−0.004[a]	−0.006[a]	-0.001
	(0.001)	(0.001)	(0.001)
Panel B: One prior			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.030	0.039	0.009[a]
	(0.001)	(0.001)	(0.001)
AB 91 counties	0.032	0.046	0.014[a]
	(0.001)	(0.002)	(0.002)
Difference	-0.002	−0.007[a]	−0.005[a]
	(0.001)	(0.002)	(0.002)
Panel C: Two priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.027	0.040	0.013[a]
	(0.001)	(0.001)	(0.002)
AB 91 counties	0.028	0.041	0.013[a]
	(0.002)	(0.003)	(0.003)
Difference	-0.001	0.000	0.000
	(0.002)	(0.003)	(0.004)
Panel C: Three or more priors			
	Before SB 1046	After SB 1046	After - Before
Non-AB 91 counties	0.029	0.042	0.013[a]
	(0.001)	(0.002)	(0.002)
AB 91 counties	0.029	0.042	0.012[a]
	(0.002)	(0.004)	(0.004)
Difference	-0.001	0.000	0.001
	(0.002)	(0.004)	(0.005)

Standard errors in parentheses.
a. Difference statistically significant at the one percent level of confidence.

Table A6: **Two Stage Least Squared Estimates of the Effect of Installing an IID on Recidivism Outcomes Two Years Following Arrest: No Priors**

Two-Year recidivism outcome	Model (1)	Model (2)	Model (3)	Model (4)
DUI arrest	-0.0161^a	-0.0175^a	-0.0146^a	-0.0302^a
	(0.0055)	(0.0055)	(0.0055)	(0.0067)
Crash	-0.0154^a	-0.0146^b	-0.0144^b	-0.0167^b
	(0.0057)	(0.0057)	(0.0058)	(0.0069)
Crash with injury	-0.0047	-0.0042	-0.0046	-0.0022
	(0.0040)	(0.0040)	(0.0041)	(0.0048)
Controls	No	Yes	Yes	Yes
Year-month effects	No	No	Yes	Yes
County effects	No	No	Yes	Yes
Matched sample	No	No	No	Yes
First-stage coefficient	0.2985^a	0.2980^a	0.2936^a	0.2996^a
	(0.0022)	(0.0022)	(0.0022)	(0.0026)
F-statistic	17,876	18,502	18,038	13,649
(p-value)	(< 0.0001)	(< 0.0001)	(< 0.0001)	(< 0.0001)
N	654,112	654,112	654,112	332,590

Robust standard errors are in parentheses. The vector of control variables include a male dummy, dummies for having one, two, three, four, or five or more prior DUIs, a quadratic in age at arrest, a quadratic in the maximum BAC reading at arrest, a dummy variable indicating BAC missing, and interactions of the male dummy with all other control variables. County effects measure county of resident and the year-month effects measure the year/month of arrest. The first-stage coefficient is the coefficient on the interaction term between non-pilot counties and the after dummy variable. Note, this interaction terms is omitted from the second-stage model. The F-statistic rest the significance of the instrument in the first-stage model.

a. Estimate significant at the one percent level of confidence.
b. Estimate significant at the five percent level of confidence.
c. Estimate significant at the ten percent level of confidence.

Table A7: **Two Stage Least Squared Estimates of the Effect of Installing an IID on Recidivism Outcomes Two Years Following Arrest: One Prior**

Two-Year recidivism outcome	Model (1)	Model (2)	Model (3)	Model (4)
DUI arrest	-0.0205	-0.0198	-0.0063	0.0203
	(0.0234)	(0.0234)	(0.0244)	(0.0308)
Crash	-0.0675^{a}	-0.0656^{a}	-0.0641^{a}	-0.0775^{a}
	(0.0216)	(0.0217)	(0.0226)	(0.0277)
Crash with injury	-0.0356^{b}	-0.0347^{b}	-0.0356^{b}	-0.0371^{c}
	(0.0151)	(0.0152)	(0.0158)	(0.0195)
Controls	No	Yes	Yes	Yes
Year-month effects	No	No	Yes	Yes
County effects	No	No	Yes	Yes
Matched sample	No	No	No	Yes
First-stage coefficient	0.1543^{a}	0.1535^{a}	0.1477^{a}	0.1485^{a}
	(0.0049)	(0.0048)	(0.0048)	(0.0060)
F-statistic	987	1,023	954	603
(p-value)	(< 0.0001)	(< 0.0001)	(< 0.0001)	(< 0.0001)
N	187,339	187,339	187,339	83,956

Robust standard errors are in parentheses. The vector of control variables include a male dummy, dummies for having one, two, three, four, or five or more prior DUIs, a quadratic in age at arrest, a quadratic in the maximum BAC reading at arrest, a dummy variable indicating BAC missing, and interactions of the male dummy with all other control variables. County effects measure county of resident and the year-month effects measure the year/month of arrest. The first-stage coefficient is the coefficient on the interaction term between non-pilot counties and the after dummy variable. Note, this interaction terms is omitted from the second-stage model. The F-statistic rest the significance of the instrument in the first-stage model.

a. Estimate significant at the one percent level of confidence.
b. Estimate significant at the five percent level of confidence.
c. Estimate significant at the ten percent level of confidence.

Table A8: **Two Stage Least Squared Estimates of the Effect of Installing an IID on Recidivism Outcomes Two Years Following Arrest: Two Priors**

Two-Year recidivism outcome	Model (1)	Model (2)	Model (3)	Model (4)
DUI arrest	-0.0343	-0.0336	-0.0100	-0.0168
	(0.0575)	(0.0571)	(0.0597)	(0.0785)
Crash	-0.0547	-0.0542	-0.0520	-0.0621
	(0.0483)	(0.0481)	(0.0502)	(0.0643)
Crash with injury	0.0037	0.0040	0.0071	0.0000
	(0.03300	(0.0329)	(0.0344)	(0.0440)
Controls	No	Yes	Yes	Yes
Year-month effects	No	No	Yes	Yes
County effects	No	No	Yes	Yes
Matched sample	No	No	No	Yes
First-stage coefficient	0.1142[a]	0.1145[a]	0.1102[a]	0.1082[a]
	0.0076	0.0075	0.0075	0.0097
F-statistic	224	232	215	123
(p-value)	(< 0.0001)	(< 0.0001)	(< 0.0001)	(< 0.0001)
N	65,843	65,843	65,843	27,716

Robust standard errors are in parentheses. The vector of control variables include a male dummy, dummies for having one, two, three, four, or five or more prior DUIs, a quadratic in age at arrest, a quadratic in the maximum BAC reading at arrest, a dummy variable indicating BAC missing, and interactions of the male dummy with all other control variables. County effects measure county of resident and the year-month effects measure the year/month of arrest. The first-stage coefficient is the coefficient on the interaction term between non-pilot counties and the after dummy variable. Note, this interaction terms is omitted from the second-stage model. The F-statistic rest the significance of the instrument in the first-stage model.

a. Estimate significant at the one percent level of confidence.
b. Estimate significant at the five percent level of confidence.
c. Estimate significant at the ten percent level of confidence.

Table A9: **Two Stage Least Squared Estimates of the Effect of Installing an IID on Recidivism Outcomes Two Years Following Arrest: Three or More Priors**

Two-Year recidivism outcome	Model (1)	Model (2)	Model (3)	Model (4)
DUI arrest	-0.0180	-0.0156	0.0099	0.0837
	(0.1404)	(0.1417)	(0.1452)	(0.1570)
Crash	-0.1748	-0.1792	-0.1752	-0.0270
	(0.1128)	(0.1140)	(0.1164)	(0.1215)
Crash with injury	0.0134	0.0129	0.0110	0.0343
	(0.0746)	(0.0752)	(0.0767)	(0.0814)
Controls	No	Yes	Yes	Yes
Year-month effects	No	No	Yes	Yes
County effects	No	No	Yes	Yes
Matched sample	No	No	No	Yes
First-stage coefficient	0.0640[a]	0.0633[a]	0.0620[a]	0.0809[a]
	(0.00730	(0.0072)	(0.0072)	(0.0104)
F-statistic	76	76	73	61
(p-value)	(< 0.0001)	(< 0.0001)	(< 0.0001)	(< 0.0001)
N	40,471	40,471	40,471	14,936

Robust standard errors are in parentheses. The vector of control variables include a male dummy, dummies for having one, two, three, four, or five or more prior DUIs, a quadratic in age at arrest, a quadratic in the maximum BAC reading at arrest, a dummy variable indicating BAC missing, and interactions of the male dummy with all other control variables. County effects measure county of resident and the year-month effects measure the year/month of arrest. The first-stage coefficient is the coefficient on the interaction term between non-pilot counties and the after dummy variable. Note, this interaction terms is omitted from the second-stage model. The F-statistic rest the significance of the instrument in the first-stage model.

a. Estimate significant at the one percent level of confidence.
b. Estimate significant at the five percent level of confidence.
c. Estimate significant at the ten percent level of confidence.

Table A10: **Two Stage Least Squared Estimates of the Effect of Installing an IID on Recidivism Outcomes Two Years Following Arrest**

Two-Year recidivism outcome	Model (1)	Model (2)	Model (3)	Model (4)
Alcohol-involved crash	0.0031	0.0027	0.0036	0.0031
	(0.0026)	(0.0026)	(0.0026)	(0.0032)
Alcohol-involved injury crash	0.0002	0.0000	0.0004	0.0007
	(0.0022)	(0.0022)	(0.0022)	(0.0027)
Fatal crash	0.0000	0.0000	-0.0000	0.0003
	(0.0007)	(0.0017	(0.0007)	(0.0009)
Alcohol-involved fatal crash	0.0003	0.0003	0.0004	0.0005
	(0.0004)	(0.0004)	(0.0004)	(0.0005)
Controls	No	Yes	Yes	Yes
Year-month effects	No	No	Yes	Yes
County effects	No	No	Yes	Yes
Matched sample	No	No	No	Yes
First-stage coefficient	0.2506[a]	0.2503[a]	0.2456[a]	0.2538[a]
	(0.0012)	(0.0019)	(0.0019)	(0.0020)
F-statistic	16,884	17,535	16,949	12,477
(p-value)	(< 0.0001)	(< 0.0001)	(< 0.0001)	(< 0.0001)
N	947,765	947,765	947,765	459,198

Robust standard errors are in parentheses. The vector of control variables include a male dummy, dummies for having one, two, three, four, or five or more prior DUIs, a quadratic in age at arrest, a quadratic in the maximum BAC reading at arrest, a dummy variable indicating BAC missing, and interactions of the male dummy with all other control variables. County effects measure county of resident and the year-month effects measure the year/month of arrest. The first-stage coefficient is the coefficient on the interaction term between non-pilot counties and the after dummy variable. Note, this interaction terms is omitted from the second-stage model. The F-statistic rest the significance of the instrument in the first-stage model.

a. Estimate significant at the one percent level of confidence.
b. Estimate significant at the five percent level of confidence.
c. Estimate significant at the ten percent level of confidence.