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The Impact of Ignition Interlock Laws on DUI Arrests

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by

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A thesis submitted in partial fulfillment of the requirements
for graduation with Honors in the Economics

Jeffrey DeSimone
Thesis Mentor

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All requirements for graduation with Honors in the
Economics have been completed.

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Abstract

We study the impact of Ignition Interlock Laws (IILs), an increasingly popular public policy aimed at reducing drunk driving, on driving under the influence (DUI) arrests. While past studies have found that IILs reduce alcohol-impaired motor vehicle fatalities and short-term recidivism rates, little evidence exists on how IILs impact DUI arrests as a more direct indicator of DUI prevalence. Using state-level monthly panel data from the Uniform Crime Reporting Program, we find that DUI arrests decrease in response to IIL adoption, though more substantively and significantly when all DUI offenders are required to use them for a period after their conviction. The results suggest deterrence may be at least partially responsible for this effect, contrary to what a previous analysis of motor vehicle fatalities concluded. Our results add to the understanding of how IILs work, as policy makers continue to address the over 100 million episodes of driving while potentially under the influence of alcohol that are reported annually.[1]

Drunk driving continues to be a prevalent issue in the United States. Though motor vehicle fatalities decreased from 2016-2018, the National Highway Traffic Safety Administration estimates that alcohol-impaired-driving led to 10,511 deaths in 2018.[2] In 2010, the costs of mortality and other damages resulting from drunk driving were estimated to be $44 billion.[3] Because of the issue's persistence, the magnitude of its harmful effects, and the fact that a substantial share of these effects are negative externalities imposed on others, policymakers continually evaluate strategies for reducing drunk driving and its adverse consequences.

Ignition interlock laws (IILs) have been promoted as a cost-effective measure to combat drunk driving. These laws mandate the installation of an ignition interlock device in a vehicle after the driver has been convicted of a driving under the influence (DUI) offense. Devices have a built-in breathalyzer into which a driver must blow to start the ignition and periodically while driving to ensure their Blood Alcohol Concentration (BAC) is under the legal limit. Used as both a punishment and a deterrent, IILs are an alternative to unequivocal license suspension for DUI offenses. While costs differ across states, Mothers Against Drunk Driving, an enthusiastic proponent of IILs, estimates that a device costs $70 to $150 to install and $60 to $80 per month for monitoring and calibration.[4] These costs are intended to be borne by drivers, though some states offer financial assistance.

As a policy tool, IILs started slowly: after Iowa became the first to enact an IIL in 1997, it was nearly a decade before another state, New Mexico, followed suit in 2005. However, the rate of adoption accelerated quickly thereafter. By 2018, 40 states and the District of Columbia had implemented an IIL.

We follow the literature by categorizing IILs as either strong or weak. While strong laws require all offenders to install a device upon conviction, weak laws tend to man-

[1]Walker acknowledges funding from the Public Policy Center at the University of Iowa and the Iowa Center for Research by Undergraduates.

[2]See https://crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/812826; an alcohol-impaired-driving fatality is defined as a death resulting from a crash involving a driver with a blood alcohol concentration of 0.08 grams per deciliter or greater.

[3]See https://www.nhtsa.gov/risky-driving/drunk-driving.

[4]See https://www.madd.org/the-solution/drunk-driving/ignition-interlocks/

date devices only for first time offenders with a BAC of at least 0.08 and/or repeat offenders. Sentencing length also varies across states, with device usage is typically required for one to two years. Using this classification, 30 states and D.C. had enacted a strong IIL by 2018.

Numerous studies have evaluated the effectiveness of IILs in reducing motor vehicle accidents, fatalities, and recidivism. An early comprehensive review deemed only two such studies on accidents to be reliable, with neither showing a significant effect (Elder et al. 2011). Similarly, assessments of IILs in Sweden, Nova Scotia, and Ontario provided no evidence that IILs influenced collision-related outcomes (Bjerre and Thorsson 2008; Vanlaar et al. 2017; Wu et al. 2015). However, several later studies of U.S. laws agree that IILs significantly reduce motor vehicle fatalities that are specifically related to alcohol use (Carter et al. 2015; Fell and Lacey 2011; Kaufman and Wiebe 2016; McGinty et al. 2017; Teoh et al. 2018; Ullman 2016).

In addition to decreasing alcohol-related motor vehicle fatalities, IILs have been touted as a relatively inexpensive alternative for reducing DUI recidivism. All reviewed analyses in Elder et al. (2011) report that "interlocked" DUI offenders are less likely to be arrested for a subsequent offense when compared to drivers without devices or with license suspensions. However, these effects were no longer observed once the device was removed from the vehicle (Bjerre and Thorsson 2008; Kerns 2017; McCartt et al. 2013; Vanlaar et al. 2017; Willis et al. 2004). A controlled trial that attempted to account for selection issues surrounding the type of person to choose a device over a license suspension showed comparable results (Beck et al. 1999).

There is comparatively little evidence on whether IILs impact DUI arrests more generally. To our knowledge, the only work on this question is from Soper (2020), who in a two-way fixed effects analysis of annual state panel data from 2001-2016 found that DUI arrests did not significantly change in response to IIL implementation.

This paper likewise uses a difference-in-differences (DD) framework with state panel data to estimate the causal impact of IIL adoption on DUI arrests. We extend the Soper (2020) analysis by expanding the time period slightly and accounting for other potential correlates of arrests rates that vary across states and time, such as other alcohol policies, labor market measures, and demographics. More substantively, we utilize monthly rather than annual arrest data, which not only captures additional variation but also enables more precise measurement of law implementation timing. Furthermore, we pay special attention to the parallel trends assumption that validates DD models by allowing for DUI arrests to trend both differentially and non-linearly by state, while also restricting comparisons to geographically proximate states likely to be more inherently similar with each other even across dimensions that are difficult to measure.

Our preferred estimates, from regressions that include both state-specific cubic time trends and year-by-Census division fixed effects, indicate that DUI arrests decline by 6.2% upon the adoption of a strong IIL. This effect varies little by gender (-6.2% for males vs. -6.5% for females). Weak IILs also reduce arrests by a marginally significant 3.7%, although the impact is larger and more significant among males (4.4%) and in recent years (7.9%). Strong laws passed more recently are also slightly more effective (6.6% pre-2012 compared with 8.4% afterward).[5] However, the fact that the magnitude

[5]As of January 1, 2012, 15 states had implemented strong IILs, half of the ultimate sample total of 30

of the impact of strong IILs is not significantly larger over the second half of our sample period, with the laws having been in place for longer and also becoming more prevalent, is consistent with deterrence being at least partially responsible for explaining their impact. This contrasts with the conclusion of Ullman (2016) for alcohol-related motor vehicle fatalities, as an effect that is attributable purely to incapacitation would be expected to increase over time.

An important caveat is that DUI arrests are not necessarily a direct reflection of overall drunk driving behavior. DUI arrests depend critically on enforcement decisions regarding resources devoted to DUI policing and the discretion used by the officers involved. Additionally, DUI arrests represent only a small fraction of alcohol-impaired driving episodes: the Centers for Disease Control and Prevention estimate that only about 1% of drunk drivers are arrested.[6] Therefore, while we interpret our results as implying that IILs have lowered the prevalence of driving under the influence, that interpretation must be considered cautiously.

The rest of the paper proceeds as follows: section 1 provides background on the behavioral models used to explain drunk driving behavior, section 2 describes our data sources and methodology, section 3 presents our results, section 4 outlines robustness tests, and section 5 discusses implications and concludes.

1 Background

As pointed out by Ullman (2016), deterrence theory has been assumed, sometimes explicitly but often implicitly, by most studies of drunk driving behavior. Based on the framework of criminal behavior developed by Becker (1968), deterrence theory argues that individuals are deterred from participating in criminal activities when the associated benefits are outweighed by the associated costs. Others, including Shavell (1987) and Polinsky and Shavell (2007), have used incapacitation to model the optimal usage of crime prevention measures. This model implies that the number of incapacitated individuals increases with time as long as the cost of their incapacitation is less than the cost of the harm they would cause in a free state. Miceli (2010, 2012) adapted the standard economic model of crime to include both of these ideas, arguing for increases (decreases) in the level of incapacitation when the deterrence level is low (high).

IILs could potentially affect drunk driving behavior through either or both of deterrence and incapacitation. The latter is straightforward, as driving under the influence is impossible with a vehicle in which an ignition interlock device has been installed. The prospect of being incapacitated in this way may also act as a deterrent to driving after drinking in the future, as will explicit costs of installation and upkeep along with implicit costs such as the likelihood that the device will reveal to family, friends, and co-workers that the driver has been convicted of a DUI.

If incapacitation is the main reason that IILs decrease DUI arrests, then we would expect effect magnitudes to rise as the number of individuals mandated to install interlock devices increases. Therefore, the fact that our estimated effects for strong IILs,

plus D.C. Additionally, ignoring Iowa's early implementation, 2012 equally separates the sample treatment period.

[6] See https://www.cdc.gov/transportationsafety/impaired_driving/impaired-drv_factsheet.html

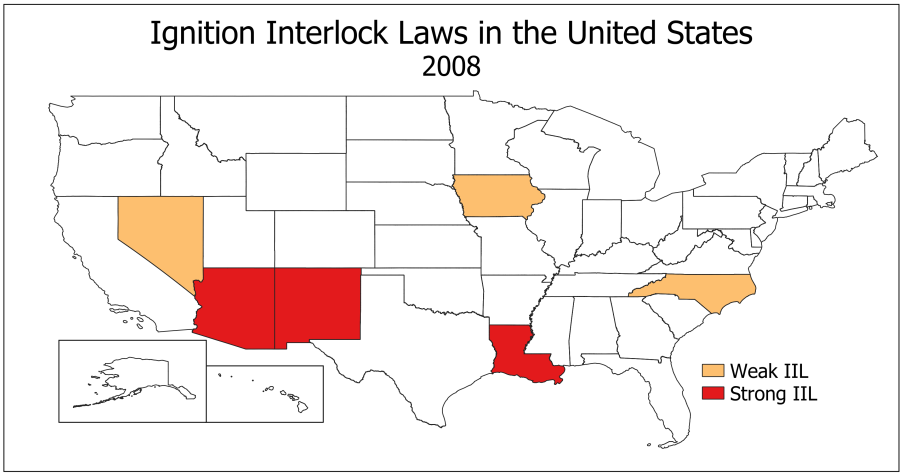

Figure 1: Ignition Interlock Laws in 2008

which over time incapacitate more offenders and have steadily increased in presence, are statistically the same in the first and second halves of the sample period provides evidence that deterrence is at least partially responsible for their impact.

2 Data and Methods

2.1 Data Sources

We utilize information on IILs from the Mothers against Drunk Driving website (Driving 2013). Following the literature, IILs are classified as ”strong” when all DUI offenders are mandated to install them and ”weak” otherwise (e.g. only for offenders with a BAC above 0.08 or repeat offenders). As mentioned, adoption of IILs quickly became widespread once New Mexico became the second state to establish one in 2005, with 40 states implementing an IIL by 2018, 31 of which are strong (with some states having initially enacted a weak law that has since been strengthened). Despite the absence of a state-wide law, we follow Ullman (2016) by including California in our treatment group upon the implementation of their pilot program in four counties: Los Angeles, Alameda, Sacramento, and Tulare[7]. Figure 1 and Figure 2 indicate the pattern of IIL law adoption across states by comparing the presence of IIL laws in 2008 and 2018, respectively.

Information on DUI arrests from 1999 to 2018 comes from the Uniform Crime Reporting Program (UCR) of the FBI. Florida is excluded from our sample because it reports DUI arrest information only very rarely. Because other states have less extreme reporting gaps varying from isolated months to several years, our panel is unbalanced. Our threshold for including data from a particular state in a given sample month is a

[7]A statewide IIL became effective in California on January 1, 2019.

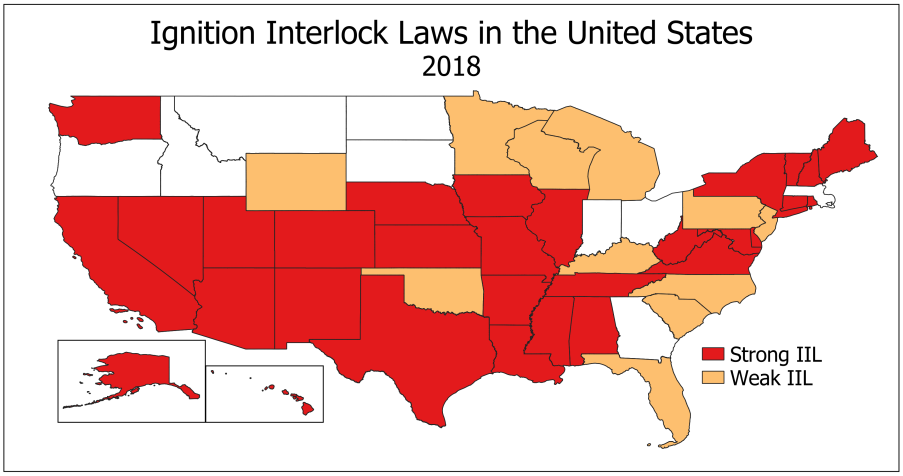

Figure 2: Ignition Interlock Laws in 2018

specific percentage of the state's population that is covered by the agencies reporting data for that month. All estimates reported in the subsequent tables represent the average from nine regressions in which this threshold is varied from 50% to 90% at 5% intervals. This strategy allows us to examine robustness of the results to incrementally trading off data selectivity for sample size.[8]

Monthly demographics are calculated by interpolating annual population estimates from the Census Bureau. Open container and 0.08 BAC drunk driving laws, along with beer taxes, from the Alcohol Policy Information System are also included as control variables. Table 1 displays summary statistics: a comparison of columns 1 and 2, and likewise of columns 3 and 4, provides preliminary evidence that IIL presence is negatively associated with DUI arrests. In general, we see that the control and treatment groups in our sample are relatively similar. Nonetheless, as previously described, we implement a DD framework that in our preferred specification allows DUI arrest rates to have separate nonlinear trends in each state while also restricting comparisons to states in the same U.S Census division.

2.2 Empirical Strategy

Our analysis examines the relationship between IILs and DUI arrests in a panel data framework, allowing us to control for factors common to both states and time that influence both DUI arrests and IIL implementation. State fixed effects account for potentially conflating factors that vary slowly over time within each state, while month-by-year fixed effects (i.e. separate indicators for each month of each year) do the same for month-to-month temporal variation that affects all states. The two-way state and

[8] Our main effect for strong IILs is significant at the 1% level for all nine inclusion thresholds; the effect of weak IILs is significant at the 10% level at seven of the nine thresholds.

Table 1: Summary Statistics

	(1) Pre-treat (Strong)	(2) Post-treat (Strong)	(3) Pre-treat (Weak)	(4) Post-treat (Weak)	(5) Control
DUI Arrests (per 100,000)	42.18	31.87	41.07	23.13	35.74
Youth (6-20) %	.2141	.1983	.2102	.2051	.2080
Adult (21-61) %	.5579	.5518	.5541	.5474	.5521
Old (62+) %	.1456	.1730	.1564	.1678	.1617
Hispanic %	.1883	.2221	.1214	.1813	.0596
Black %	.1303	.1236	.1422	.1380	.1484
Male %	.4925	.4936	.4907	.4931	.4896
Income ($1000)	36.93	49.61	36.60	44.04	38.83

Note: All estimates are weighted by state-year population.

month-by-year fixed effects regression compares, from month to month, the change in DUI arrests for states that implemented an IIL that month with the change in arrests over the same period in states that either already or do not have an IIL. The DD estimator from this model reflects the average of these comparisons across all sample months, allowing us to causally interpret the estimated average effect of IILs implemented across states and time.

Our analysis estimates variations of the following model:

$$log(D_{sm}) = \beta_0 + \beta_1 S_{sm} + \beta_2 W_{sm} + \beta_3 X_{sm} + \beta_4 Z_{sm} + \lambda_s + \lambda_m + \beta_t T_s + \lambda_{dy} + \varepsilon_{sm}$$

where s and m denote state and sample month, respectively and D_{sm} denotes DUI arrests. $S_{sm} = 1$ if a strong IIL is in effect in that state for the entire month and $W_{sm} = 1$ is the same for a weak ILL, meaning that the initial month of adoption is considered untreated unless the law became effective on the 1st of the month. We include state fixed effects (λ_s) and sample month fixed effects (λ_m) to account for factors that are constant within states over time and that vary over time similarly across states, respectively.

To increase statistical precision and at least partially account for the possibility that IIL implementation represents a broader effort by the state to combat drinking and driving, we include as regressors X_{sm} the alcohol policy measures described above as well as socioeconomic factors (race/ethnicity, gender, age, per capita income, the unemployment rate, and log population). Specifying log population as a control variable performs the dual purpose of standardizing arrests by population while also allowing arrest rates themselves to depend on population size. Z_{sm} represents various additional covariates inserted to test for robustness and mechanisms, upon which we elaborate after presenting the main results.

All regressions are weighted by the average state population over the sample period so that estimates reflect predicted effects across individuals rather than states. Standard errors are clustered by state to allow for within-state serial correlation in unobserved DUI arrest determinants. Our estimates of interest are β_1 and β_2; given the logged dependent variable, these represents the average percentage increases in the DUI arrest rate resulting from enacting strong and weak IILs, respectively.

3 Results

Our DD estimates for the responsiveness of DUI arrests rates to IIL adoption are summarized in Table 2. Moving across columns reveals the importance of including two-way fixed effects to implement the DD design (column 2), state-specific cubic time trends to allow for DUI arrest rates to trend nonlinearly and differentially in each state (column 3), and division-by-sample month fixed effects to restrict comparisons to geographically proximate and thus intrinsically similar states (column 4). In particular, the notable reduction in both effect sizes and standard errors appears to justify the strategy of explicitly modeling the nonlinearity in state DUI arrest trends.

Our preferred specification in column 4 shows that adopting IILs predicts DUI arrest rate declines of 6.2% for strong laws and 3.7% for weak laws. It is not surprising that strong laws, by virtue of covering first time and/or relatively low BAC offenders to whom some weak laws do not apply, have larger effects, although the difference between the coefficients is not statistically significant. Moreover, statistical significance is high for strong laws, with an absolute t-statistic of well over 3, but only at 10% for weak laws. It should be noted, however, that in column 3, in which the weak IIL effect is actually slightly smaller in magnitude, the standard error is disproportionately smaller to the extent that the effect becomes significant at the 5% level.

Table 2: Main Results

	(1)	(2)	(3)	(4)
Strong IIL	-.0594	-.0962*	-.0637***	**-.0622****
	(.0713)	(.0457)	(.0157)	**(.0182)**
Weak IIL	-.0145	-.0843*	-.0357*	**-.0372**$^{+}$
	(.0719)	(.0377)	(.0169)	**(.0189)**
Demographics	X	X	X	**X**
Labor market	X	X	X	**X**
Alcohol policies	X	X	X	**X**
State FE		X	X	**X**
Sample month FE		X	X	
State cubic trends			X	**X**
Division-by-sample month FE				**X**

Note: $^{+}$:$p < .1$, *:$p < .05$, **:$p < .01$, ***:$p < .001$. Regressions are weighted by state population, with standard errors clustered by state. The dependent variable is the natural log of DUI arrests. Demographic variables include the natural logs of the population covered by the state's reporting agency, the state's overall population, and the fraction of residents who are male, youth (ages 6-20), adult (ages 21-61), elderly (ages 62+), Black, and Hispanic. Labor market measures include the unemployment rate and per capita personal income. Alcohol policies include the beer tax along with indicators for the presence of open container and 0.08 BAC drunk driving laws. Estimates are an average of nine models in which inclusion of data from a specific state-month cell is determined by the percentage of a state's population covered by the reporting agencies that month, with the threshold for inclusion varying from 50% to 90% in increments of 5%.

Although rates of alcohol use in general, and DUI arrests in particular, are somewhat higher for males than females, Table 3 shows that the average effect of strong laws is quite similar across genders. In contrast, weak laws on average lower DUI arrest rates by a significant 4.4% for males, but an insignificant 1.7% for females.

We also split our sample period in Table 3 to investigate whether law effectiveness changed once they became more widespread.[9] If incapacitation was responsible for the effect of IIL implementation, we should see an increase in effect size. For strong laws, while the effect is slightly larger in later years, the difference is not statistically significant, with the estimates being well within a standard error of each other. This suggests that deterrence is at least partially responsible for the impact strong IILs have on reducing DUI arrests. This conclusion is contrary to that of Ullman (2016), who finds that incapacitation is largely responsible for a decrease in alcohol-related motor-vehicle fatalities after strong IIL implementation. In contrast, the effect of weak laws is close to zero in early years, but grows to near the size of the strong law effect and becomes highly significant in later years.

Table 3: Secondary Results

	Male DUIs	Female DUIs	Time Periods
Strong IIL	-.0618**	-.0647**	
	(.0182)	(.0195)	
Weak IIL	-.0444*	-.0167	
	(.0183)	(.0223)	
1999-2011*Strong IIL			-.0656*
			(.0266)
2012-2018*Strong IIL			-.0840*
			(.0288)
1999-2011*Weak IIL			-.0131
			(.0227)
2012-2018*Weak IIL			-.0787*
			(.0307)

Note: $^{+}$:$p < .1$, *:$p < .05$, **:$p < .01$, ***:$p < .001$. Regressions represent the preferred model from column 4 of Table 2 and are weighted by state population, with standard errors clustered by state. The dependent variable is the natural log of DUI arrests. Estimates are an average of nine models in which inclusion of data from a specific state-month cell is determined by the percentage of a state's population covered by the reporting agencies that month, with the threshold for inclusion varying from 50% to 90% in increments of 5%.

4 Robustness Tests

As one attempt to test whether the estimated effects might somehow be picking up other factors that changed coincidentally with law implementation, we add lead and lag terms to our preferred specification. Table 4 shows that none of the eight 12- and 24-month lead and lag terms for the two law types are significant at 5%. For both strong and weak laws, the 12-month lead term has the largest coefficient among the four leads and lags, suggesting an anticipatory effect that could be consistent with deterrence.

[9]The year 2012 was chosen because 15 states had enacted a strong IIL by December 2011, about half of the 31 states (including D.C.) that had done so by the end of the sample period.

Table 4: Timing of Effect

	Pre-24 months	Pre-12 months	Post-12 months	Post-24 months
Strong IIL	.0010	$-.0305^{+}$	.0008	.0141
	(.0198)	(.0162)	(.0286)	(.0229)
Weak IIL	-.0087	-.0220	-.0072	-.0060
	(.0287)	(.0265)	(.0266)	(.0365)

Note: $^{+}$:$p < .1$, *:$p < .05$, **:$p < .01$, ***:$p < .001$. Regressions represent the preferred model from column 4 of Table 2 and are weighted by state population, with standard errors clustered by state. The dependent variable is the natural log of DUI arrests. Estimates are an average of nine models in which inclusion of data from a specific state-month cell is determined by the percentage of a state's population covered by the reporting agencies that month, with the threshold for inclusion varying from 50% to 90% in increments of 5%.

Other possible explanations for a decrease in DUI Arrests being spuriously correlated with IIL implementation are simultaneously occurring declines in overall criminal behavior or a more widespread increase in enforcement behavior. We test these hypotheses by estimating our preferred specification but with the log of arrests for stealing and gambling, also recorded by the Uniform Crime Reporting program, substituted as dependent variables in place of DUI arrests. Table 5 indicates that while effect sizes are similar for weak IILs and actually larger for strong IILs on gambling arrests, signs are inconsistent, while standard errors are sufficiently large to render the relationships statistically insignificant. These results cast doubt on the possibility that IIL adoption is systematically associated with changes in criminal activity or enforcement intensity more generally.

Table 5: Other Criminal Outcomes

	DUI Arrests	Stealing Arrests	Gambling Arrests
Strong IIL	.0622**	-.0030	.1346
	(.0182)	(.0336)	(.0804)
Weak IIL	$.0372^{+}$	.0347	-.0470
	(.0189)	(.0409)	(.1223)

Note: $^{+}$:$p < .1$, *:$p < .05$, **:$p < .01$, ***:$p < .001$. Regressions represent the preferred model from column 4 of Table 2 and are weighted by state population, with standard errors clustered by state. The dependent variable is the natural log of DUI arrests. Estimates are an average of nine models in which inclusion of data from a specific state-month cell is determined by the percentage of a state's population covered by the reporting agencies that month, with the threshold for inclusion varying from 50% to 90% in increments of 5%.

5 Discussion

Our analysis finds that IILs reduce DUI arrests, with strong laws being particularly effective. DD estimates show that strong IIL adoption predicts a subsequent decline of 6.2% in the DUI arrest rate.

Regarding plausibility, this effect size seems consistent with the number of ignition interlock devices in use nationally. In 2014, the most recent year for which data are available, the estimated number of devices installed in states that reported data was 318,714, or about 10.1 per 10,000 residents; in states with strong laws in 2014, 135,899 devices were installed.[10] Based on this count, our estimated effect of 6.2% implies that strong IILs averted about 41,199 DUI arrests in 2014, suggesting an upper bound of roughly 0.3 DUI arrests avoided for every device installed. As a first approximation, this seems reasonable given that every device represents a driver who has already been convicted of a DUI offense.

To the best of our knowledge, this is the first study to investigate the impact of IILs on DUI arrests using monthly arrest data, controlling for correlates of arrest rates across time and states, and explicitly modeling temporal and spatial relationships in an effort to ensure that the parallel trends assumption of the DD framework is satisfied. We estimate a causal effect of strong IILs that is statistically and economically significant, yet plausible in size. Our results provide additional evidence of the effects of IILs beyond their impact on fatal accidents, further highlighting their usefulness as a potential contributor toward mitigating societal harm from drunk driving.

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[10] This is a lower bound, as data are unavailable for Alaska, California, Kansas, or Mississippi: see http://www.rothinterlock.com/2014_survey_of_currently_installed_interlocks_in_the_us.pdf.

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