THE EFFECT OF DRUG-FREE SCHOOL ZONE LAWS ON TEEN DRUG USE

by

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ABSTRACT

One often stated goal of drug policy in the United States is keeping drugs and drug related violence away from children. Drug-free school zone laws, which impose harsher penalties on those caught on drug related offenses near schools, are perhaps the most ubiquitous policies with this specific aim in mind. While some form of drug-free zone is currently enforced in all 50 states, several have made reforms to their laws in the last 10 years. Using data from the Youth Risk Behavior Survey, I estimate the relationship between weakening drug-free school zone laws and teen drug use. I find little evidence to suggest these reforms have led to increases in teen drug use.

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INTRODUCTION

While many states have either passed or considered passing various legislation aimed at reforming current drug laws, a common concern is how a potentially increased drug presence may affect minors. According to data from Monitoring the Future, approximately 80% of high school seniors over the last decade have said that marijuana is "fairly easy" or "very easy" to get, and 20%-40% said the same for harder drugs like cocaine and heroin (Johnston et al. 2018). These numbers may be concerning given previous work examining the relationship between drug use and other outcomes of interest. For example, van Ours and Williams (2011), van Ours and Williams (2012), and van Ours et al. (2013) find that cannabis use increases the likelihood of mental health problems for men and women, as well as suicide ideation among males. In addition to mental health concerns, there is evidence drug use may lead to worse educational outcomes. Bray et al. (2000) finds that marijuana users are 2.3 times as likely to drop out of high school as non-users. Chatterji (2006) finds that cocaine use, as well as marijuana use, is associated with completing fewer years of education, and van Ours and Williams (2009) find that earlier initiation into cannabis use leads to larger reductions in years of education completed.[1] Concerns about a higher level of drug presence near schools can also stem from potentially related violence and crime. In addition to direct negative consequences if students are victims, researchers have also found that higher levels of neighborhood and school violence lead to worse educational outcomes and more high school dropouts.[2]

One of the most ubiquitous policies in the United States explicitly aimed at

[1]For a broader overview of the literature, see van Ours and Williams (2015) and Hall (2015)

[2]For examples, see Grogger (1997), Bowen and Bowen (1999), and van Ours and Williams (2015)

keeping drugs and drug-related crime away from minors is drug-free school zone laws, which are currently enforced in every state and Washington D.C. For example, the Sacramento county sheriff's department described drug-free zones as "one way to give students a place where they can play and talk without being threatened by drug dealers and drug users," and "a community-wide commitment to reduce drug use among young people." They also urged others: "don't stop at the school's boundaries. Expand your drug-free zone efforts to any area besieged by problems associated with drug and alcohol abuse" (Sacramento Sheriff's Department 2003). These laws impose harsher penalties for those caught on drug related offenses near schools and, depending on the state, often other locations such as parks, buses, community centers, and public housing complexes. The zones typically range from 500-1000ft from the edge of school property, and a drug-free zone infraction usually implies an increased mandatory minimum sentence or a higher felony class.

Over the last decade, nine states have made changes to their drug-free zone laws, and yet little is known about their original effect or that of the reforms themselves.[3] These reforms either reduce the applicability of the zones or the severity of the penalty associated with zone infractions. While some have celebrated these changes and called for more states to enact similar legislation, others have been concerned about undermining the purported effectiveness of drug-free school zone laws in keeping drugs and drug-related violence away from children. When Connecticut was considering legislation that would reduce the size of the drug-free zones, State Representative Prasad Srinivasan testified that he was "appalled as to why anyone would support a bill that puts our children at greater risk by easing restrictions on drug dealers. As drugs get closer to our schools, other forms of crime and violence will certainly

[3]These states are Connecticut, Delaware, Illinois, Indiana, Kentucky, Massachusetts, New Jersey, South Carolina, and Utah.

follow" ("Srinivasan Pushes to Stop the Shrinking of Drug Free Zones" 2014). Thus, to better understand and evaluate these recent reforms, it is crucial to estimate their effect on the policies' originally stated goal: to keep drugs away from students.

In this paper, I test whether reforms to drug-free school zone laws in the past decade have led to changes in teen drug use and school safety outcomes using Youth Risk Behavior Survey data from 1991-2017. To the best of my knowledge, I am the first to attempt to estimate a casual effect of these policies by using a difference-in-differences framework. While there were concerns that reforms would lead to increases in teen drug use and make schools less safe, I find little evidence that weakening drug-free school zone laws has had any such effect. Rather, I estimate a small, negative, and statistically insignificant association between these reforms and marijuana, cocaine, and methamphetamine use. I also find small effects that are statistically indistinguishable from zero on the level of drug presence at schools and the number of students skipping class due to feeling unsafe.

BACKGROUND

Literature Review

While previous studies have not examined the relationship between drug-free school zone laws and teen drug use, there is a large literature examining other policies aimed at drug use prevention, as well as determinants in general. Some of the earliest papers studying teen drug use look at their elasticity of demand for drugs. Chaloupka et al. (1999) find that high school student cocaine and marijuana use is responsive to both prices and increased penalties for possession. Pacula et al. (2000) and Williams (2004) both find evidence that teen's demand for marijuana is elastic, and Grossman et al. (2002) finds that teens are more sensitive to price than adults.[1]

There have also been programs aimed at reducing demand for drugs among students. One of the most well-known anti-drug programs is Project Drug Abuse Resistance Education (D.A.R.E.). West and O'Neal (2004) surveys the literature regarding D.A.R.E.'s effectiveness at reducing drug use, finding it to be small and statistically insignificant. Palmgreen et al. (2001) suggests that targeted television public service announcements can reduce marijuana use, while Hornik et al. (2008) finds that the National Youth Anti-Drug Media Campaign did not lead to a reduction in youth marijuana use. Another anti-drug initiative aimed at reducing demand, the Montana Meth Project, was widely thought to have caused a large reduction in teen methamphetamine use, and multiple states later adopted similar campaigns. However, Anderson (2010) and Anderson and Elsea (2015) show that the estimated effect goes away after accounting for the existing downward trend in student meth use in Montana.

[1] For a broader survey of the literature examining drug price and use, see Gallet (2014).

Several papers, such as Kuziemko and Levitt (2004) and DeSimone and Farrelly (2007), have found that drug prices increase in response to harsher drug punishments. However, the effect appears to be sensitive to the type of enforcement. For example, while Chaloupka et al. (1999) finds that increases to the penalties for possession decrease marijuana and cocaine use by minors, the authors note that drug use does not appear to be sensitive to penalties associated with sale, manufacture, and distribution. Anderson et al. (2015) looks at the effect of medical marijuana legalization on teen marijuana use, finding small, negative effects that are statistically insignificant.

Drug-Free School Zone Laws

Concerns about teen drug use, and potential danger stemming from drug trafficking near children are not new: one early attempt to address this was to enact drug-free school zones. The first was part of the Comprehensive Drug Abuse Prevention and Control Act, signed into law by President Nixon in 1970, who said "drugs are alarmingly on the increase in use among our young people. They are destroying the lives of hundreds of thousands of young people all over America, not just of college age or young people in their twenties, but the great tragedy: The uses start even in junior high school, or even in the late grades."[2] In the 1980s states started passing their own drug-free school zone laws, and they are currently enforced in every state and Washington D.C. These laws impose harsher penalties for those caught on drug-related offenses near schools and, depending on the state, often other locations. For example, 19 states include parks, 13 include school busses, 11 create zones around arcades and youth centers, and 12 include public housing complexes. Arkansas created drug-free zones around schools, public parks, school

[2]President Nixon's full remarks, including this excerpt, are available from The American Presidency Project by Gerhard Peters and John T. Woolley.

bus stops, skating rinks, YMCAs, community centers, public housing complexes, substance abuse treatment facilities, day care centers, and churches. Utah enforces a drug-free zone around schools, child care centers, parks, arcades, recreation centers, amusement parks, churches, shopping malls, sports facilities, movie theaters, parking lots, and libraries. However, Arkansas and Utah have two of the most expansive laws: 11 states only place zones around schools. The most common zone radius is 1,000ft, although 7 states have larger zones, and Alabama's stretch 3 miles in each direction.

Penalties vary by state as well. Separate charges carrying mandatory minimum sentences from one to eight years are included in 13 state's drug-free zone laws, while 25 others treat zone infractions as sentence enhancers, often increasing maximum penalties, or raising the felony class of the charge. For example, in Arkansas, a drug-free zone infraction, which can come from possession as well as delivery, manufacture, or sale, carries a 10-year additional sentence, with no parole. Alabama's zones only apply to drug sales, but carry a 5-year mandatory minimum, again with no parole. Utah treats possession with intent, delivery, sale, or manufacture within a drug-free zone as a first degree felony. Washington doubles the maximum sentence, while Kansas increases the felony level. The extent to which these zones apply to minors varies by state: a few allow minors to be tried as adults, while four others explicitly exempt them from being charged with zone infractions. However, in practice, drug-free zone laws are rarely used against high-school students. For specific details on each state's law, see Porter and Clemons (2013).

There is some previous descriptive work examining the use of drug-free zones. Brownsberger et al. (2004) conducted a review of how drug-free school zone laws were being enforced in three Massachusetts cities, finding that the zones covered 29% of the cities, but 56% of the high-poverty areas, and while 80% of arrests were inside drug-free zones, less than 1% actually involved dealing drugs to minors. Furthermore,

while only 74% of those arrested inside zones were charged with a zone infraction, 39% of those arrested outside zones were still charged with a zone infraction. In December 2005, the New Jersey Commission to Review Criminal Sentencing released a report finding that large swathes (over 50%) of their largest cities were covered by these zones (Figure 2.1). They also found that those impacted by the zones were disproportionately black and Hispanic: in 2004, there were 12,567 individuals arrested for drug-free zone infractions, 72.3% of whom were black, and in 2005, 96% of offenders incarcerated for a drug-free zone offense were either black or Hispanic.

Similar results were found in other states as well. In 2010, the Illinois Disproportionate Justice Impact Study (DJIS) Commission released a report showing that 89% of those arrested on drug-free zone infractions in 2005 were nonwhite (Lurigio et al. 2010). The very next year, Florida's Committee on Criminal Justice submitted a report to the Florida Senate which found that from 2009 to 2010, 55.9% of those admitted to prison on drug charges were black, but 88.5% of those admitted to prison on drug-free zone charges were black.

The primary limitation of these data is that they do not tell us the net effect of drug-free school zone laws as they are single snapshots of equilibrium outcomes. However, these reports have led to concerns that these laws have had a discriminatory impact on minorities, and a potentially large social cost. Partially because of this, nine states have reformed their drug-free zone laws over the last 10 years by either reducing the applicability of the laws by adding conditions for zones to apply or by reducing the severity of the penalty associated with zone infractions. The most common condition added was that a minor must be reasonably expected to be present when the offense occurred, and most penalty reductions involved reducing enhancements for possession. Many reforms have also reduced the size of drug-free zones. Table 2.1 details how each of the nine states has changed their drug-free school zone laws, and Table 2.2

provides a simple chart showing which of the three types of changes were included in the reform. To the best of my knowledge, this paper is the first to estimate the impacts of these reforms. I find little evidence that weakening drug-free school zone laws has led to either an increase in teen drug use or a decrease in reported school safety.

Figure 2.1: Map of Drug-free Zones in Newark, NJ

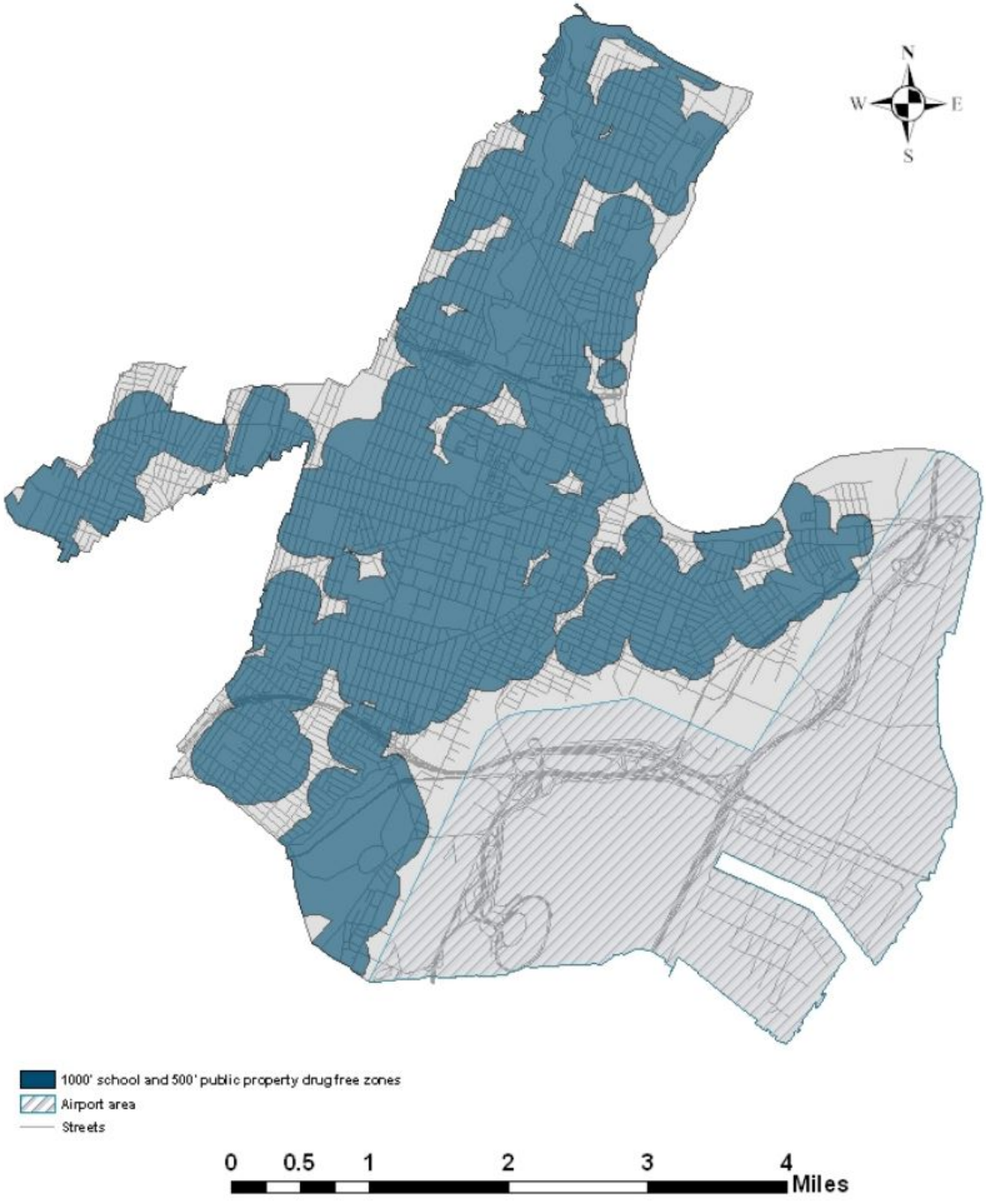


Table 2.1: Drug-Free School Zone Changes

State	Year	Change
Connecticut	2015	Mandatory two year minimum for possessing drugs within 1500 feet of a school is eliminated and replaced with a misdemeanor with a maximum sentence of one year. Manufacturing, distributing, and selling drugs still qualifies for a three year mandatory minimum sentence.
Delaware	2011	Radius of zones around schools is reduced from 1000 feet to 300 feet to mach those around parks and churches. Created three categories of offenses: possession, dealing, and aggravated possession, which requires large amounts with the intent to deal.
Illinois	2018	Zone radius was reduced from 1000 to 500 feet, and zone applicability requires that either a minor be present at the time, or be reasonably expected to be present, such as during school hours.
Indiana	2013	Reduced radius of zones from 1000 to 500 feet and eliminated zones around public housing complexes and youth program centers. Also added a requirement that a minor must be reasonably expected to be present when the offense occurs for the zone to apply.
Kentucky	2011	Reduced zone radius from 1000 yards to 1000 feet, rescaled penalties to correspond to amount sold, and reduced the maximum sentence for possession.
Massachusetts	2012	Reduced zone radius from 1000 to 300 feet and limited the hours zones apply to 5AM-midnight.
New Jersey	2010	Allowed judges to waive mandatory minimum sentences for those charged with zone infractions.
South Carolina	2010	For drug-free school zone enhancements to apply, intent to commit a drug offense inside a zone must be established.
Utah	2015	Reduced zone radius from 1000 to 100 feet and added a requirement that minors must be reasonably expected to be present for the zone to apply. Also limited automatic sentence increases to offenses involving sale and distribution.

Table 2.2: Drug-Free School Zone Changes

State	Year	Penalty Reduction	Zone Size Reduction	Added Conditions
Connecticut	2015	X		
Delaware	2011	X	X	
Illinois	2018		X	X
Indiana	2013		X	X
Kentucky	2011	X	X	
Massachusetts	2012		X	X
New Jersey	2010	X		
South Carolina	2010			X
Utah	2015	X	X	X

DATA

To estimate the effect of drug-free school zone law reforms on teen drug use and school safety outcomes, I use data from the state Youth Risk Behavior Survey (YRBS) containing individual-level survey responses from high school students for the period 1991-2017. Each observation in the dataset includes a given student's response to each question in the survey, on topics such as school, family, health, drugs, and other risky behaviors.[1] The YRBS data also includes individuals' demographic information, allowing for analysis of heterogeneous treatment effects by subgroups. One drawback is aggregation of location to the state level as a student privacy measure, which prevents sub-state analysis and more local controls.

The YRBS is conducted every two years at two distinct levels: the national survey, conducted by the CDC, and the state surveys, conducted by the local departments of health and education. Early papers using YRBS data exclusively used the national YRBS data, as it was intended by the CDC to be nationally representative.[2] However, the national data did not include samples from each state-year, and state samples are often small, and not intended to be representative at the state level. Later studies used the state YRBS as a supplement to the national YRBS, running separate regressions for each data set.[3] To increase sample size and the amount of usable policy variation, later studies pooled the state and national YRBS which limits the number of missing state-year observations.[4] These papers

[1] For example, students were asked "During the past 30 days, how many times did you use marijuana?" with six possible responses: 0 times, 1 or 2 times, 3 to 9 times, 10 to 19 times, 30 to 39 times, and 40 or more times.

[2] see Merrill et al. (1999), Gruber and Zinman (2001), Chatterji et al. (2004), and Lynne-Landsman et al. (2013)

[3] see Carpenter and Cook 2008, Carpenter and Stehr 2008

[4] see Anderson 2010, Anderson et al. 2015, Sabia and Anderson 2016, Hansen et al. 2017, and Dave et al. 2019

constructed weights for the pooled sample using state-level information on age, race, and sex from the National Cancer Institute's Surveillance Epidemiology and End Results (SEER).

I exclusively use the state YRBS due to the sampling design of the national survey. While the national data are intended to be nationally representative, the CDC samples schools at different rates based on characteristics researchers cannot observe. To increase precision, they implement a three stage sampling design to oversample black and Hispanic students. First, they increase the probability of selection for high-black and high-Hispanic primary sampling units (PSUs). Then, within selected PSUs, they increase the probability of selection for schools with disproportionately high minority enrollment. Finally, for selected schools with high minority enrollment, they sample two classes per grade rather than one.

Because high minority enrollment is likely correlated with unobservable factors that impact teen drug use, the national YRBS represents a potentially endogenous sample that needs to be weighted by the inverse probability of selection to provide consistent coefficient estimates (Solon et al. 2015). However, because the student location is aggregated to the state level, researchers are unable to observe the actual probability of selection. The weights used in previous studies–including those provided by the CDC–are exclusively based on observable factors such as age, grade, race, and sex. The weights are not based on the actual probability of selection and therefore do not correct for the unobserved bias still present in the data. Because the state YRBS do not employ similar oversampling techniques, each state's survey is representative of the state itself. Thus, to avoid a potentially endogenous sample, I do not include the national YRBS data in my sample.

In my preferred specification, I use the provided weights, which also account for state population size, meaning state-year observations can be combined. While

the combined data are not nationally representative–not all states conduct a survey or provide their results–the sample is representative of the total population of the included states. Thirty-seven states freely provide their data and I have obtained an additional 5 states under various data agreements, for a total coverage of 42 states and over one million students surveyed. While I do not have post-treatment data for each state that reformed their drug-free school zone laws, I do have pre- and post-treatment data for six of the nine states (Delaware, Kentucky, Massachusetts, New Jersey, South Carolina, and Utah) totaling 33 pre-treatment state-year observations with 61,098 students and 18 post-treatment state-year observations with 28,317 students.[5] Appendix Table A.1 lists the number of observations for each state-year.

Table 3.1 presents descriptive statistics of the sample used, both unweighted and weighted, for treatment states which change their drug-free school zone laws and control states that do not. I drop observations that do not include demographic information, which is only about 3% of the original sample. Students in treatment and control states look mostly similar based on observable demographic characteristics and drug-use rates. Even though the practical level of difference is small, many of the means are statistically distinguishable as the total sample is large. The biggest difference is in demographics: the treatment states have about 4.6 percentage points more white students, with relatively fewer students identifying as black or other. In the full sample, about 20% of students report using marijuana at least once in the last 30 days. Slightly more than half of those students say they use marijuana more than 10 times in the last 30 days, which I define as frequent marijuana use, following

[5]For example, I have 6 years of pre- and 4 years of post-treatment data from Delaware, 5 years of pre- and 4 years of post-treatment data from Kentucky, 5 years of pre- and 3 years of post-treatment data from Massachusetts, 3 years of pre- and 2 years of post-treatment data from New Jersey, 5 years of pre- and 4 years of post-treatment data from South Carolina, and 9 years of pre- and 1 year of post-treatment data from Utah. While I have 6 years of data from Illinois, their reform took effect in 2018, and YRBS only has data through 2017. As the YRBS is conducted every other year, 5 years of data, for example are spread over 9 years.

Anderson et al. (2015). Nearly 10% have used a harder drug like cocaine, heroin, ecstasy, or meth at least once in their life, and about 25% say they have been offered, sold, or given drugs on school property in the last 12 months.

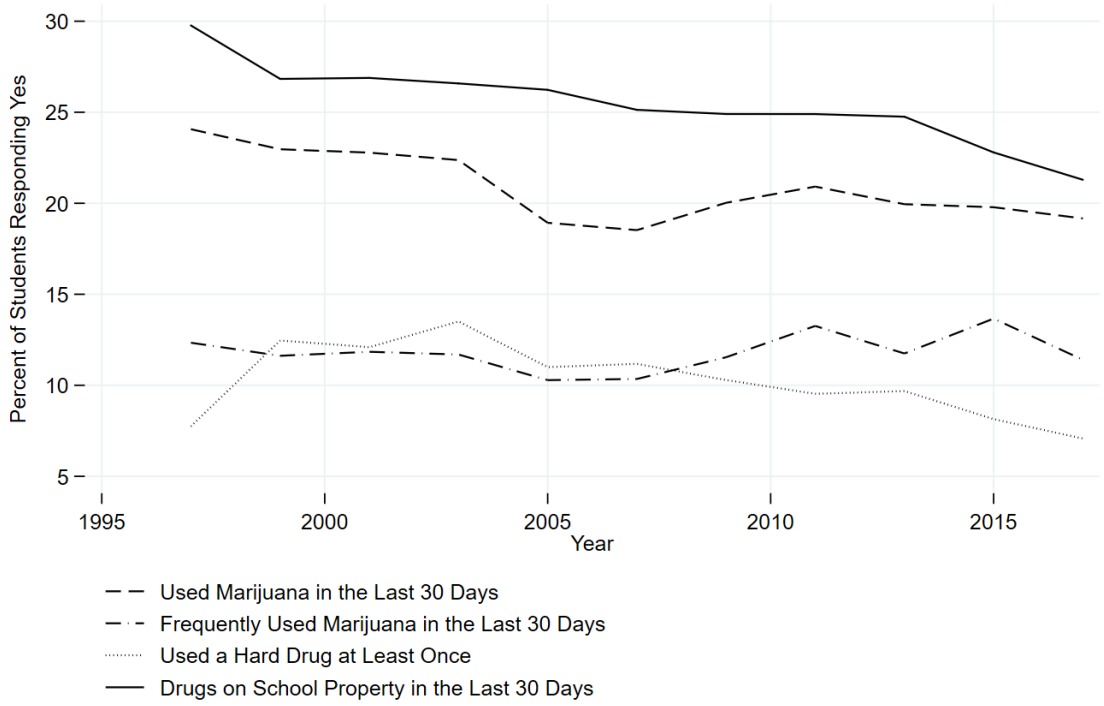
Drug use over the last 20+ years has not been constant, however. Figure 3.1 shows how the percentage of students reporting the above four behaviors has evolved over the sample period. Marijuana use was falling during the late nineties and early 2000s, but has flattened since while frequent marijuana use has been climbing over the same period. In contrast, hard drug use was increasing until the early 2000s, but has been slowly falling since. The prevalence of drugs on school property appears to be falling over the sample period as well.

Table 3.1: Descriptive Statistics: State YRBS 1991-2017

	Unweighted			Weighted		
	Control	Treatment	Total	Control	Treatment	Total
Marijuana Use in the Last 30 Days	0.201	0.209	0.202	0.204	0.202	0.204
Frequent Marijuana in the Last 30 Days	0.12	0.114	0.119	0.115	0.11	0.114
Hard Drug Use at Least Once	0.096	0.093	0.096	0.096	0.093	0.096
Drugs at School in the Last 12 Months	0.246	0.247	0.246	0.248	0.261	0.25
Age	15.877	15.986	15.888	16.014	16.034	16.016
Male	0.487	0.485	0.486	0.504	0.504	0.504
Grade 9	0.281	0.282	0.281	0.279	0.278	0.279
Grade 10	0.267	0.258	0.266	0.256	0.254	0.256
Grade 11	0.24	0.25	0.241	0.233	0.234	0.233
Grade 12	0.197	0.194	0.197	0.219	0.222	0.219
Black	0.142	0.16	0.144	0.176	0.149	0.173
White	0.539	0.581	0.544	0.583	0.629	0.589
Asian	0.057	0.04	0.055	0.035	0.034	0.035
Hispanic	0.165	0.143	0.162	0.143	0.141	0.143
Other	0.069	0.047	0.067	0.038	0.022	0.036
N	982352	137216	1119568	982352	137216	1119568

Notes: This table reports means for the sample of high school student respondents from the state YRBS (1991-2017). Treatment contains all states that weaken their drug-free school zone laws during the sample period; control contains all remaining states

Figure 3.1: Teen Drug Use Over Time



EMPIRICAL METHODS

To estimate the effect of weakening drug-free school zone laws on teen drug use and school safety outcomes, I implement a difference-in-differences framework exploiting variation in timing and location of reforms. Specifically, I estimate the following equation:

$$Y_{ist} = \alpha + \beta_1 WeakenDFSZ_{st} + \boldsymbol{X_{ist}\beta_2} + \boldsymbol{X_{st}\beta_3} + \delta_s + \lambda_t + \theta_s \cdot t + \epsilon_{ist}$$

where i indexes individuals, s indexes states, and t indexes years. Y_{ist} is the outcome variable of interest in any given regression, usually some measure of drug use or safety concern.[1] The variable $WeakenDFSZ$ is equal to one in states after they enact reforms weakening their drug-free school zone laws and zero otherwise. Thus, β_1 is the coefficient of interest, showing the average treatment effect of weakening drug-free school zone laws on teen drug use and school safety outcomes.

The vector δ_s represents state fixed effects, which control for time-invariant state-level factors related to teen drug use, while the vector λ_t represents year fixed effects, which control for country-wide changes to teen drug use in each year. $\theta_s \cdot t$ represents state-specific linear time trends, which control for smooth, linear changes in factors related to drug use that are state-specific, such as attitudes towards drug use. The vector $\boldsymbol{X_{ist}}$ includes individual level controls (age, sex, race, and grade), to account for demographic changes, while the other vector, $\boldsymbol{X_{st}}$, includes yearly

[1] For example, when testing the effect of reforms on marijuana use, Y_{ist} is equal to 1 if the student reports using marijuana in the last 30 days, and 0 otherwise. Similarly, when measuring the effect of reforms on school safety, Y_{ist} is equal to 1 if the student reports staying home from school due to feeling unsafe either at school or on their commute, and 0 otherwise. Specifically, students are asked "During the past 30 days, on how many days did you not go to school because you felt unsafe at school or on you way to or from school" with five possible responses: 0 days, 1 day, 2 or 3 days, 4 or 5 days, and 6 or more days. I create a binary measure indicating if the student reported a positive number of days.

state level unemployment and youth unemployment to control for state-level economic conditions. As there are a number of potentially confounding drug law changes, the vector also includes policy indicators for state laws that reduced mandatory minimums associated with drug offenses, restructured their felony classifications for drug-related crimes, or legalized medical or recreational marijuana, which are set to one for the state after the policy is changed.[2] All regressions are estimated as linear probability models using CDC provided survey weights, with standard errors clustered at the state level to correct for serial correlation (Bertrand et al. 2004).[3]

To achieve consistent estimates, the standard identifying assumption must hold: that the reforms to drug-free school zone laws are exogenous to other uncontrolled factors correlated with teen drug use. In other words, teen drug use in treatment states would have evolved similarly to teen drug use in control states had drug-free school zone reforms not been enacted. While Table 3 shows that student demographic information and teen drug use rates are similar in treatment and control states, that is neither necessary nor sufficient to establish that the parallel trends assumption holds. Perhaps the largest threat to identification is that while states were changing their drug-free school zone laws, some also passed other drug laws reducing mandatory minimums, restructuring felony classifications, or legalizing medical marijuana. Because these laws are likely correlated with teen drug use, their effects must be taken into account, otherwise the identifying assumption will not hold. As discussed earlier, I do add indicators for these policy changes in my regressions. Because these other types of reforms were passed by many other states in various years throughout my sample, I am able to control for their effect on teen drug use.

[2]I calculate unemployment rates using Current Population Survey data, and compile drug law changes from a variety of sources, including the Urban Institute Justice Policy Center, Pew Charitable Trusts, and Families Against Mandatory Minimums.

[3]I prefer the LPM because we are interested in marginal effects, it lends itself to easy interpretation, and it avoids making a fairly arbitrary choice of which non-linear model to use.

RESULTS

Tables 5.1-5.6 provide ordinary least squares estimates of the effect recent reforms to drug-free school zone laws have had on various drug use and school safety outcomes. Using a basic two way fixed effects model, reforms aimed at weakening drug-free school zone laws are associated with a statistically insignificant 0.5 percentage-point decrease in the probability than a student reports smoking marijuana in the last 30 days (Table 5.1). Adding the other drug law changes to the model, as well as observable student characteristics and state level covariates reduces the estimated size of the effect, but not to a statistically significant degree. After adding state specific linear time trends, I still find a small, negative, and statistically insignificant effect on marijuana use, with the 95% confidence interval excluding an effect size of a two percentage point increase. Table 5.1 also shows no statistically significant effects on frequent marijuana use, hard drug use, or being offered, given, or sold drugs on school property. For all three outcomes, adding individual and state level covariates as well as adding linear time trends result in negligible changes to the estimated treatment effect, and there are no effects in any model that are statistically distinguishable from zero.[1]

Although I find insignificant effects when using the entire sample, there could be heterogeneous effects for various subgroups that are masked in a pooled sample. Thus, I estimate separate equations by age and race to test for possible heterogeneous effects in Tables 5.2 and 5.3.

First, since drug-free school zone laws are not generally applied to minors, we might expect older students' drug use to be more sensitive to changes in their

[1]Unweighted results, run as a test for model misspecification, were largely similar (DuMouchel and Duncan 1983)

applicability. However, that is generally not what we observe. While the estimated effect size on general marijuana use is negative, small, and statistically insignificant for both those less than 17 years old and at least 17 years old, conditional on using marijuana at least once, I find a positive effect on frequent marijuana use for the former group, and a negative effect for the latter group. While the effect for those under 17 years old is marginally significant, the coefficient estimate is larger in absolute size for those 17 and older.[2] While the estimated effect on hard drug use is larger for older students, it is still statistically insignificant. The opposite is true for the likelihood that a student reports being given, offered, or sold drugs on school property–the estimated effect is larger for students under 17–but again, the effects are not statistically significant for either group, and in this case are fairly noisy: I cannot rule out effect sizes of 6 percentage point increases for either age group using a 95% confidence interval.[3]

Second, because previous studies suggest that the enforcement of drug-free school zone laws is racially disproportionate, we may expect reforms to have a differential impact by race on students as well.[4] I separately estimate effects for white and non-white students in Table 5.3 for each of the four main outcomes of interest.[5] The estimated effects for non-white students are always larger than those for white students. For example, I find that weakening drug-free school zone laws reduces the

[2]Since the coefficient on smoking marijuana is negative, we must consider how the sample is changing as marginal marijuana users may be less likely to be frequent marijuana users. However, the estimates are small enough to not create a large bias

[3]Unweighted results were largely similar, as were estimates without state-specific time trends

[4]see New Jersey Commission to Review Criminal Sentencing (2005), Lurigio et al. (2010), and Florida Committee on Criminal Justice (2011)

[5]The YRBS changed questions regarding race and ethnicity during the sample period. Currently, the questionnaire asks students if they are Hispanic or Latino, and then separately asks about their race. Prior to 2007, there was a single question asking "How do you describe yourself?" where students had to pick between identifying as Hispanic/Latino or choosing a race like white or black. About 35% of students in the non-white category chose Hispanic in either form of the question, and about 42% of the same group identify as black, and not Hispanic, either because they had to choose between the two before 2007, or they chose black and not Hispanic.

probability a white student reports using marijuana by 0.007, but that it also increases the probability a non-white student reports using marijuana by 0.019. However, the effects are not statistically distinguishable from each other, or from zero for either group. This is true for marijuana use and the other three outcomes.[6]

In addition to running separate regressions for various subsamples, I also estimate the effect of weakening drug-free zone laws on each of the four harder drugs (cocaine, methamphetamines, ecstasy, and heroin). As before, an important effect for one drug may be masked when pooling all four together to increase the sample response rate. Table 5.4 shows my results using my preferred specification including individual and state covariates, both with and without state-specific time trends. I fail to find effects statistically distinguishable from zero for either cocaine or methamphetamine use. Furthermore, like the estimates in previous tables, they are not sensitive to the inclusion of state-specific time trends. Using the "worst-case" model, 95% confidence intervals rule out effect sizes of increases to the probability of using cocaine and methamphetamines by 0.007 and 0.017, respectively.

The findings for ecstasy and heroin are more complicated. When I include state-specific linear time trends, I obtain estimated coefficients for ecstasy and heroin use that are both positive and statistically significant. Drug-free school zone law reforms are associated with 2.29 and 1.55 percentage point increases in the frequency of students reporting having used ecstasy and heroin, respectively, at least once. Note that relative to the average rate of use for these drugs (6.18 and 2.77 percent), the estimated effects are perhaps implausibly large. However, we must exercise some caution when interpreting these coefficients, as two other potential issues emerge. First, the direction of the effect is dependent on the inclusion of state-specific linear time trends; excluding them results in negative coefficient estimates. For heroin this

[6]Unweighted results were largely similar, as were estimates without state-specific time trends

effect is statistically significant at the 1% level, and we observe a similar decline in the use of needles, an outcome we may expect to be linked.[7]

Second, the YRBS data are surveys, and not every student responds to every question as students are allowed to skip any question they choose.[8] Table 5.5 shows that the number of students responding changes based on the question being asked. For example, 935,045 of the total 1,119,568 students (83.5%) answered the question asking about cocaine use, whereas only 716,244 students (64.0%) responded regarding their heroin use. If non-response is correlated with the truthful, but unobserved answer, this can lead to biased estimates. For example, if students who do not use heroin are less likely to answer the question at all, we will measure effect sizes that are too large, but if the converse is true, we will have attenuation bias in our coefficients. If non-response is systematically related to reforms in drug-free school zone laws, there could be much larger concerns regarding the accuracy of our estimates.

To test for such a relationship, I use my preferred model to predict non-response to particular questions. I find small, statistically insignificant effects for all drug related questions except those regarding heroin and ecstasy (Table 5.5). However, for some reason, students are much less likely to answer questions about their heroin and ecstasy use in states after reforms have passed, which could lead to biased results. As the salience of reforms to drug-free zone laws is likely low among students, to whom the law generally does not apply, and the estimated effect sizes are implausibly large, these effects on response rates seem unlikely to be causal.[9] Regardless of the cause, the change in sample response is large enough to prohibit putting significant weight,

[7]Bluthenthal et al. (2015) found that nearly 99% of heroin users injected the drug using needles at least once

[8]Brener et al. (2006) finds that students are more likely to report their drug use when surveys are conducted at school, like the YRBS, rather than at home.

[9]For example: a student in a state that has passed a drug-free school zone law reform is predicted to have a negative probability of answering the question regarding ecstasy use

or a causal interpretation, on the original coefficient estimates of interest.[10]

In addition to potential increases in teen drug use, another concern about reforming drug-free school zone laws is a potential increase in drug-related violence and crime near schools, which has been linked to worse educational outcomes.[11] While the data are not ideal, I use responses potentially correlated with these outcomes. Table 5.6 presents estimates of the effects of weakening drug-free school zone laws on whether a student reported skipping school due to safety concerns either at school or on their commute in the last 30 days. I find that students are only 0.0398 percentage points more likely to have skipped school, an effect that is small relative to the mean, and statistically insignificant. I cannot reject the null hypothesis of no effect on carrying a weapon on school property, being threatened or injured by someone with a weapon on school property, nor getting into physical fights, either at school or in general.[12] While these are not direct measures of the outcomes of interest, we do not observe increases in correlated behavior we might expect to increase if drug related crime and violence were increasing in and around schools.

[10]This is in addition to issue regarding linear time trends discussed earlier.
[11]For examples, see Grogger (1997), Bowen and Bowen (1999), and van Ours and Williams (2015)
[12]Unweighted results were largely similar, as were estimates without state-specific time trends

Table 5.1: Drug-Free School Zone Laws and Teen Drug Use

Panel A: Marijuana Use in the Last 30 Days			
Weaken DFSZ Laws	-0.00508	-0.0000329	-0.00452
	(0.00531)	(0.00705)	(0.0114)
Mean	0.204	0.204	0.204
N	1014519	1014519	1014519
Panel B: Frequent Marijuana Use in the Last 30 Days			
Weaken DFSZ Laws	-0.00203	0.0113	0.00206
	(0.0142)	(0.0130)	(0.0201)
Mean	0.413	0.413	0.413
N	203928	203928	203928
Panel C: Ever Hard Drug Use			
Weaken DFSZ Laws	0.0105	0.0129	0.0110
	(0.00740)	(0.0143)	(0.00744)
Mean	0.0927	0.0927	0.0927
N	1051195	1051195	1051195
Panel D: Drugs at School in the Last 12 Months			
Weaken DFSZ Laws	-0.00736	0.00814	0.0148
	(0.0104)	(0.0150)	(0.0221)
Mean	0.249	0.249	0.249
N	948390	948390	948390
State & Year FE	Yes	Yes	Yes
Covariates	No	Yes	Yes
State Specific Trends	No	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports OLS estimates of the effect of weakening drug-free school zone laws based on state YRBS data using CDC provided survey weights (1991-2017). Covariates are listed in Table 1. Robust standard errors, clustered at the state level, are reported in parentheses.

Table 5.2: Drug-Free School Zone Laws' Effects by Age

	Age <17	Age >=17
Panel A: Marijuana Use in the Last 30 Days		
Weaken DFSZ Laws	-0.00445	-0.00328
	(0.0140)	(0.0161)
Mean	0.176	0.250
N	675341	339178
Panel B: Frequent Marijuana Use in the Last 30 Days		
Weaken DFSZ Laws	0.0343*	-0.0409
	(0.0180)	(0.0336)
Mean	0.381	0.452
N	118983	84945
Panel C: Ever Used Hard Drugs		
Weaken DFSZ Laws	0.00785	0.0148
	(0.00834)	(0.0133)
Mean	0.0796	0.115
N	699824	351371
Panel D: Drugs at School in the Last 12 Months		
Weaken DFSZ Laws	0.0255	-0.00352
	(0.0202)	(0.0325)
Mean	0.251	0.245
N	628573	319817
State & Year FE	Yes	Yes
Covariates	Yes	Yes
State Specific Trends	Yes	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports OLS estimates of the effect of weakening drug-free school zone laws based on state YRBS data using CDC provided survey weights (1991-2017). Covariates are listed in Table 1. Robust standard errors, clustered at the state level, are reported in parentheses.

Table 5.3: Drug-Free School Zone Laws' Effects by Race

	White	Non-White
Panel A: Marijuana Use in the Last 30 Days		
Weaken DFSZ Laws	-0.00725	0.0194
	(0.00816)	(0.0147)
Mean	0.191	0.214
N	575835	438684
Panel B: Frequent Marijuana Use in the Last 30 Days		
Weaken DFSZ Laws	0.0114	0.0162
	(0.0221)	(0.0272)
Mean	0.412	0.420
N	110178	93750
Panel C: Ever Used Hard Drugs		
Weaken DFSZ Laws	-0.00252	0.000827
	(0.0100)	(0.0152)
Mean	0.0854	0.101
N	589653	461542
Panel D: Drug School in the Last 12 Months		
Weaken DFSZ Laws	0.0133	0.0181
	(0.0261)	(0.0271)
Mean	0.242	0.260
N	554524	393866
State & Year FE	Yes	Yes
Covariates	Yes	Yes
State Specific Trends	Yes	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports OLS estimates of the effect of weakening drug-free school zone laws based on state YRBS data using CDC provided survey weights (1991-2017). Covariates are listed in Table 1. Robust standard errors, clustered at the state level, are reported in parentheses.

Table 5.4: Drug-Free School Zone Laws and Hard Drug Use

Panel A: Ever Used Cocaine		
Weaken DFSZ Laws	-0.00517	-0.00616
	(0.00616)	(0.00676)
Mean	0.0636	0.0636
N	935045	935045
Panel B: Ever Used Methamphetamines		
Weaken DFSZ Laws	-0.00213	-0.00457
	(0.00421)	(0.0109)
Mean	0.0466	0.0466
N	838974	838974
Panel C: Ever Used Ecstasy		
Weaken DFSZ Laws	-0.00462	0.0229**
	(0.00444)	(0.00993)
Mean	0.0618	0.0618
N	716244	716244
Panel D: Ever Used Heroin		
Weaken DFSZ Laws	-0.0124***	0.0155*
	(0.00324)	(0.00781)
Mean	0.0277	0.0277
N	823459	823459
Panel E: Ever Used a Needle to Inject Illegal Drugs		
Weaken DFSZ Laws	-0.0122***	0.00651
	(0.00321)	(0.00440)
Mean	0.0237	0.0237
N	847536	847536
State & Year FE	Yes	Yes
Covariates	Yes	Yes
State Specific Trends	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports OLS estimates of the effect of weakening drug-free school zone laws based on state YRBS data using CDC provided survey weights (1991-2017). Covariates are listed in Table 1. Robust standard errors, clustered at the state level, are reported in parentheses.

Table 5.5: Drug-Free School Zone Laws and Non-Response

Panel A: Marijuana Use in the Last 30 Days		
Weaken DFSZ Laws	-0.00156	0.00129
	(0.0142)	(0.0159)
Mean	0.0295	0.0295
N	1051195	1051195
Panel B: Drugs at School in the Last 12 Months		
Weaken DFSZ Laws	-0.132	0.0194
	(0.0973)	(0.103)
Mean	0.0851	0.0851
N	1051195	1051195
Panel C: Meth		
Weaken DFSZ Laws	-0.301	0.0378
	(0.245)	(0.246)
Mean	0.222	0.222
N	1051195	1051195
Panel D: Cocaine		
Weaken DFSZ Laws	0.0276	0.0283
	(0.0531)	(0.0316)
Mean	0.0710	0.0710
N	1051195	1051195
Panel E: Ecstasy		
Weaken DFSZ Laws	-0.679**	-0.416
	(0.283)	(0.295)
Mean	0.361	0.361
N	1051195	1051195
Panel F: Heroin		
Weaken DFSZ Laws	-0.319	-0.163
	(0.196)	(0.182)
Mean	0.197	0.197
N	1051195	1051195
State & Year FE	Yes	Yes
Covariates	Yes	Yes
State Specific Trends	No	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports OLS estimates of the effect of weakening drug-free school zone laws based on state YRBS data using CDC provided survey weights (1991-2017). Covariates are listed in Table 1. Robust standard errors, clustered at the state level, are reported in parentheses.

Table 5.6: Drug-Free School Zone Laws and School Safety

Panel A: Skipped School due to Safety Concerns in the Last 30 Days	
Weaken DFSZ Laws	0.000398
	(0.0139)
Mean	0.0630
N	981765
Panel B: Carried a Weapon on School Property in the Last 30 Days	
Weaken DFSZ Laws	0.00224
	(0.00768)
Mean	0.0520
N	954440
Panel C: Threatened or Injured by Someone with a Weapon on School Property in the Last 30 Days	
Weaken DFSZ Laws	-0.0169
	(0.0123)
Mean	0.0725
N	954184
Panel D: Got in a Physical Fight in the Last 12 Months	
Weaken DFSZ Laws	-0.00297
	(0.0133)
Mean	0.273
N	835821
Panel E: Got in a Physical Fight on School Property in the Last 12 Months	
Weaken DFSZ Laws	-0.00857
	(0.00912)
Mean	0.101
N	944724
State & Year FE	Yes
Covariates	Yes
State Specific Trends	Yes

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: This table reports OLS estimates of the effect of weakening drug-free school zone laws based on state YRBS data using CDC provided survey weights (1991-2017). Covariates are listed in Table 1. Robust standard errors, clustered at the state level, are reported in parentheses.

CONCLUSION

While drug-free school zone laws remain standard policy across the United States, little is known about their effect. Despite this, several states have enacted reforms over the last decade by shrinking zone radii, adding conditions for the zones to apply such as the presence of a minor, or reducing the penalty associated with zone infractions. While many who were concerned about the potentially discriminatory application of these laws have pushed for these changes, and called on other states to pass similar legislation, others have remained skeptical, and worry that reducing the cost of possessing and dealing drugs near schools may lead to an increase in teen drug use and a decrease in school safety.

To test for evidence of the latter, I use Youth Risk Behavior Survey data from 1991-2017 and estimate linear probability models exploiting spatial and temporal variation in drug-free school zone law changes. I fail to find convincing evidence that weakening these laws has led to an increase in teen drug use. While statistically significant effects on heroin and ecstasy use were found, the direction of the effects is sensitive to the model's inclusion of state-specific time trends, and the reforms themselves were correlated with reductions in response rates, removing confidence in their accuracy. While the other non-statistically significant results did not have the same issues of time trend sensitivity or reductions in response rates, there are still limitations worth noting. First, the sample only contains teens who are still in school, and who were attending the day of the survey. If the treatment effect differs for drop outs or students who skip school more often, this will lead to biased estimates. Second, all of the data used in this study is self-reported, which could lead to biased estimates as well if the response truthfulness is systematically related to the policy changes of interest.

While these limitations make this study less than ideal, to the best of my knowledge, it is the first to attempt to estimate the causal effect of reforming drug-free school zone laws. Understanding the potential effects on teen drug use and school safety outcomes has been made urgent by previous work creating maps of drug-free zones and providing statistics on those being charged with drug-free zone infractions, both of which have shown the policies' potential unintended costs. To the extent this paper's results suggest reforms to drug-free zone laws have not lead to large increases in teen drug use, it is even more important to understand their effects on the other potential outcomes. More work needs to be done to study the direct effect of drug-free school zone laws, as well as recent reforms, on those arrested on drug charges, and to test for potential discriminatory application of drug-free zones, which will allow for a more complete cost-benefit analysis.

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APPENDIX A

YRBS DATA COVERAGE

Table A.1: Number of Observations by State-Year: State YRBS

State	1997	1999	2001	2003	2005	2007	2009	2011	2013	2015	2017	Total
Alabama	3787	2095	1578	1088	1140	-	1520	1358	1574	1565	-	15705
Alaska	-	-	-	1481	-	1318	1373	1327	1235	1418	1332	9484
Arizona	-	-	-	3431	3307	3095	2596	2899	1623	2582	2139	21672
Arkansas	1991	1454	1694	-	1615	1608	1690	1375	1547	2880	1682	17536
California	-	-	-	-	-	-	-	-	-	1943	1778	3721
Colorado	-	-	-	-	1498	-	1511	1523	-	-	1493	6025
Delaware	-	2180	2915	3048	2717	2627	2407	2299	2756	2777	2906	26632
Florida	-	-	4237	4080	4564	4523	5664	6212	6089	6359	6171	47899
Georgia	-	-	-	-	1755	2465	1882	1969	1992	-	-	10063
Hawaii	1409	1248	-	-	1662	1191	1511	4329	4631	6089	6031	28101
Idaho	-	-	1714	1731	1457	1440	2164	1702	1886	1760	1818	15672
Illinois	-	-	-	-	-	2438	3051	3616	3276	3282	5010	20673
Iowa	1521	-	-	-	1359	1440	-	1535	-	-	1691	7546
Kansas	-	-	-	-	1654	1733	2026	1876	1941	-	2413	11643
Kentucky	1465	-	-	1613	3282	3595	1777	1829	1626	2577	1997	19761
Louisiana	5563	-	-	-	-	1349	1035	1160	1107	-	1273	11487
Maine	1837	-	1351	1680	1375	1324	9275	9918	9017	9605	9501	54883
Maryland	-	-	-	-	1414	1528	1644	2920	53785	55596	51087	167974
Massachusetts	-	-	-	3624	3522	3131	2707	2729	2718	3120	3286	24837
Michigan	3933	2690	3630	3452	3253	3532	3411	4194	4266	4816	1626	38803
Mississippi	1532	1565	1806	1488	-	1614	1795	1828	1584	2154	-	15366
Missouri	1483	1652	1650	1551	1878	1561	1624	-	1616	1502	1864	16381
Montana	2457	2917	2755	2781	3077	4030	1852	4148	4889	4486	4741	38133

Table A.1 Continuted: Number of Observations by State-Year: State YRBS

State	1997	1999	2001	2003	2005	2007	2009	2011	2013	2015	2017	Total
Nebraska	-	-	-	2933	3755	-	-	3832	1885	1688	1427	15520
Nevada	1464	1677	1464	1982	1556	1783	2085	-	2133	1452	1667	17263
New Hampshire	-	-	-	1327	1276	1638	1493	1413	1634	14837	12050	35668
New Jersey	-	-	2142	-	1495	-	1756	1657	1701	-	-	8751
New Mexico	-	-	-	-	5634	2638	5047	5875	5451	8304	5781	38730
New York	3741	3312	-	9320	9708	13439	14870	13201	10643	10834	11411	100479
North Carolina	-	-	2548	2553	3874	3506	5702	2278	1846	6178	3151	31636
North Dakota	-	1823	1599	1666	1725	1768	1838	1911	1981	2121	2142	18574
Oklahoma	-	-	-	1384	1715	2612	1413	1147	1474	1611	1649	13005
Pennsylvania	-	-	-	-	-	-	2080	-	-	2899	3761	8740
Rhode Island	1528	-	1392	1814	2362	2210	3213	3961	2453	3462	2221	24616
South Carolina	5539	4597	-	-	1309	1241	1108	1493	1606	1358	1501	19752
South Dakota	1604	1674	1614	1829	1590	1611	2170	1543	1320	1313	-	16268
Tennessee	-	-	-	1940	1540	2069	2220	2635	1904	4138	2043	18489
Utah	1388	1509	1071	1451	1549	1976	1598	1729	2195	-	1848	16314
Virginia	-	-	-	-	-	-	-	1440	6935	5195	3697	17267
West Virginia	1818	1323	-	1748	1368	1393	1670	2170	1793	1622	1563	16468
Wisconsin	1325	1336	2120	2121	2389	2094	2434	3043	2843	-	2067	21772
Wyoming	2081	1619	2770	1552	2500	2244	2902	2519	3015	2424	-	23626