Abortion Legalization and Adolescent Substance Use*

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Abstract

We assess whether adolescents who faced a higher risk of having been aborted are more likely to use controlled substances. We find that adolescents born in states which legalized abortion before national legalization in 1973, during the years when only those states permitted abortion, were much less likely to use drugs than persons from the same birth cohorts born elsewhere. These differences do not exist for earlier or later cohorts. Our results are much the same when we characterize abortion risk by either the birth rate or abortion rate in the year and place of the person's birth.

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1. Introduction

This paper examines how the legalization of abortion in the early 1970s affected the tendency of children born during those years to drink, smoke, and use illicit substances as teenagers. Recent work on legalization's effects has focused on early life outcomes (Gruber, Levine and Staiger (1999)) and on one later life activity - the propensity to commit serious crime (Donohue and Levitt (2001) and Joyce (2002)).[1] Not only has a single later life outcome been studied, the papers on legalization and crime reach dramatically different conclusions, making the impact of abortion reform on later life outcomes an open question. Our study focuses on a different outcome, about which there is likely independent interest and which, as we outline later, is ideal for assessing abortion legalization's later life effects.

Two main theoretical reasons have been posited for why abortion legalization could have affected later life outcomes like substance use and crime. The one most emphasized in the literature is the mechanism of selection - specifically, whether relatively disadvantaged women were more or less likely to have abortions after legalization. The early life circumstances of the average child born after legalization should have risen if they were and should have fallen otherwise.[2] If a person's early life environment affects later life outcomes, selective use of abortion should have changed the incidence of bad teenage outcomes for persons born after abortion reform.

[1] There has also been work by Angrist and Evans (1999) on how abortion legalization affected human capital acquisition and labor market outcomes for potential mothers exposed to abortion reform.

[2] Selection of the first type could occur if adjusted their fertility to ensure that children are born when the mother's (or family's) economic position is most favorable. The second type of selection could occur if disadvantaged women lack the means to pay for abortions, or if they live relatively farther from providers than their better off counterparts.

The second mechanism does not depend on selection. Even if the fraction of children in a given cohort born into disadvantaged circumstances was unaffected by abortion availability, the reductions in cohort sizes arising from abortion reform could affect the rate of use in a cohort because the cost structure faced by drug dealers, the size and efficiency of drug distribution networks and, especially, the nature of peer pressure influences adolescents might all be affected by the size of the cohort.[3]

Several authors have shown that legalization was indeed associated with dramatic increases in the number of abortions (e.g., Bauman, Anderson, Freeman, and Koch (1977), and Levine, Kane, Staiger, and Zimmerman (1996)).[4] With respect to the selective use of abortion, Levine et al. (1996) find that relatively disadvantaged groups such as teenagers, nonwhites, and unmarried women were more likely to have abortions after legalization. But they also find that older women, who tend not to be disadvantaged, were also more likely to have abortions after legalization. Moreover, even though groups like teenagers are more likely to be disadvantaged, there might have been systematic selection in the *types* of teenagers who had abortions. Hence, this evidence is only suggestive as to the selective use of abortions by the disadvantaged.

Gruber, Levine, and Staiger (1999) study how legalization affected the circumstances into which a child was born. They show that children who would have been born but for abortion legalization would have been more likely to be born into poverty, to die in infancy, to receive welfare and to live in a single parent family. The

[3] Jacobson (2001) discusses these and other reasons why cohort size might affect the rate of use.
[4] The association between abortion availability and fertility has been found by many other authors. See Blank, George and London (1994), and Levine, Trainor and Zimmerman (1996), and Kane and Staiger (1996).

improvement in the average early life circumstance of children born after abortion legalization supports the idea that there was positive selection (relatively more use by the disadvantaged) of abortion.[5]

Did the improvement in average childhood circumstances after legalization translate into better later-life outcomes for these children? Donohue and Levitt (2001) examine the relationship between legalization and the dramatic nationwide reduction in serious crime such as homicide, violent crime and property crime, which began in the early 1990s. There is a strong *prima facie* case that legalization had a causal effect on crime reduction since the decline in crime began earlier in the five states that liberalized abortion prior to the *Roe* decision.[6] In their formal analysis, Donohue and Levitt relate changes over time in a state's abortion ratio (the proportion of abortions to live births) to crime within the state in the 1990s and find that fully one-half of the reduction in crime can be explained by changes in the abortion ratio.

Joyce (2004) critically examines Donohue and Levitt's study. He argues that since information on abortions may be unreliable, and that even if reliable, need not correlate neatly with unintended fertility, the abortion ratio does not provide the proper source of variation for analyzing the relationship between legalization and crime.[7] Joyce uses an empirical strategy, previously employed by Gruber, Levine and Staiger (1999) and

[5] Other research finds a similar association between abortion access and improvements in birth outcomes. See for example, Grossman and Jacobowitz (1981), Grossman and Joyce (1990) and Currie, Nixon and Cole (1993).

[6] In 1970, four states explicitly legalized abortion by repealing state abortion laws. These states were New York, Washington, Alaska and Hawaii. A State Supreme Court ruling in late 1969 that held that existing anti-abortion laws were unconstitutional meant that, as of 1970, California was a state with "de facto" legalization.

[7] One explanation for this is that there may have been a correlation between the availability of abortion in a state and the use of contraceptives, or even the likelihood of engaging in sexual activity, for people in a state.

Angrist and Evans (1999), which exploits the fact that five "repeal" states effectively legalized abortion three years before national legalization in 1973. Joyce compares crime outcomes for people born in the 1970-1972 interval, across repeal and non-repeal states. He finds no evidence of a difference in crime between them, and concludes that the relationship Donohue and Levitt find between crime and abortion ratios is spurious.[8]

Because these studies of the subsequent criminal behavior of the cohorts exposed to abortion legalization give mixed results, understanding whether the impact of abortion reform carries over to later life outcomes is an open, and important, topic to analyze. Here, we focus on adolescent use of controlled substances.[9] Like serious crime, use by adolescents of controlled substances, particularly illicit narcotics, is likely a function of the person's childhood circumstances. Thus, any effect of abortion on early life environment should be manifested later in substance use.[10] Also, to the extent that use of illicit substances is an avenue by which adolescents and young adults get introduced to more serious criminal activity, our results may identify a key mechanism by which legalization affected serious crime, if it did. Finally, particular features of available data on substance use make it a very interesting subject for the study of legalization's effect.

[8] Though they do not emphasize these results, Donohue and Levitt also use the repeal state/non repeal state comparison to study legalization and crime. Overall, their results from this exercise were mixed. Donohue and Levitt argue that the comparison of early versus late legalizers is not a strong approach in their context because the early legalizers have only a three year "head start". The debate between the authors about the crime results continues in Donohue and Levitt (2004) and Joyce (2004b).

[9] Though this paper is about substance use and not crime, it should be noted that, strictly speaking, a minor who uses any of the substances we study is engaged in criminal activity, albeit of a decidedly less serious variety than the types studied by Joyce and Donohue and Levitt. Also, substance use may be a gateway into serious criminal activity. For example see Markowitz (2000), Parker and Auerhahan (1998) and Baumer et al (1998).

[10] The relationship between background and family conditions has been documented by numerous authors. In *Risky Behavior Among Youth: An Economic Analysis*, Gruber ed., papers by Cook and Moore (alcohol), Gruber and Zinman (smoking) and Pacula et al (marijuana use) estimate an a significant relationship between these factors and use.

One of these features is that national substance abuse trends among adolescents displayed a very different time series pattern to those for serious crime. Whereas crime began a sharp decrease in the early nineteen nineties, data from the *Monitoring the Future* survey shown in Figure 1 reveals that the fraction of U.S. 12th graders who report using controlled substances such as tobacco, alcohol and illegal substances like marijuana and cocaine started to trend *upwards* at around the same time. The figure shows only use patterns for use within the past month, but the patterns for having ever used the substances or for having used them within the past year are very similar. The pattern in Figure 1 is exactly the opposite of the first order relationship one might expect if legalization affected adolescent substance use. In light of these trends, formal evidence consistent with an effect of abortion would be quite convincing.

The most interesting aspect of studying substance use is that available data on this behavior permit a much sharper test of the effect of legalization than is possible with available crime data. Both Donohue and Levitt and Joyce, in their respective studies, use the Unified Crime Report data, compiled by the Federal Bureau of Investigation. These data measure all reported violent crime, property crime, and homicide. But because crime reports are not disaggregated by the specific age of the offender, they cannot be used in to test the impact of abortion legalization on criminal activity *by specific birth cohorts*.

The other measures used in the crime studies to help get around this problem have equally serious limitations. One measure is the arrest rate, which is age-disaggregated. But the fact that a given number or fraction of people of a certain age are arrested for a particular type of crime in a year is, at best, a highly imperfect indicator of the number or

fraction of persons of a given age who *commit* the particular crime in the year – presumably the outcome of interest. Arrest rates depend on the actions of police, and are not conviction rates. Indeed, even if every arrested person were an offender, arrest rates by age might still systematically mis-measure criminal behavior by age if the capacity to escape arrest varies with age. Donohue and Levitt and Joyce also look at age-disaggregated crime victimization data. The limitations of these data for testing the prediction of the effect of legalization on criminal behavior are obvious. Crime victims are not criminal offenders, and even if they were, it is not clear how the ages of the two groups would line up. [11]

There are none of these problems with substance use data. As we discuss in detail below, we are able to examine the proportion of persons, from specific birth cohorts, who use controlled substances in a given year, at a particular age. In addition, because we use data from multiple years, there is little risk that our estimates conflate aging and time effects, as is possible with analyses similar to ours, but which use data from a single year. Gruber, Levine, and Staiger (1999), for example, in their analysis of the effect of legalization on early life outcomes, use data from the 1980 Census. The use of these data means that children born at different years are of different ages when observed in the data, making it difficult to separate aging and time effects.[12]

Using several generations of 12th graders from the Monitoring the Future Survey, we classify adolescents by whether they were born in one of the states that legalized abortion before nationwide legalization in 1973. We then compare substance use

[11] Joyce also examines data from the FBI's Supplemental Homicide Reports.
[12] Gruber, Kane and Staiger attempt to carefully deal with this potential problem by adding state-specific trends.

between those from repeal and other states. For birth cohorts *in utero* when abortion was legal in only the repeal states, we find that people from those states were significantly less likely to use controlled substances, particularly the most serious like illegal narcotics, than were persons in their birth cohorts from other states. In addition, we find no evidence of differences in substance use by whether the person was born in a repeal state or not for cohorts born after national legalization.

In addition to these reduced form estimates, we also examine how early legalization affected use through its estimated effect on the birth rate and the abortion rate. These results, in which birth and abortion rates in a state are instrumented by the differential timing of abortion reform across states, show strongly significant relationships between substance use and (legalization-induced) changes in the birth rate and the abortion ratio. We emphasize the birth rate results because of some of the controversy in the previous literature about the abortion ratio, but the results from both measures support the paper's main conclusion that *in utero* exposure to abortion legalization was associated with a reduction in the tendency to use controlled substances for 12^{th} grade adolescents.

In the next section we describe our empirical framework and the data in greater detail. Section 3 presents results from the differential timing of abortion reforms in the early legalizing states and national abortion legalization. In Section 4 we exploit variation in state birth and abortion rates arising from the differential of legalization across states to estimate two stage least squares estimates of abortion's effects on use. These results help assess the plausibility of the reduced form estimates in Section 3. Section 5 concludes.

2. Data and Empirical Framework

We use data from several waves of the Monitoring the Future (*MTF*) data set. The *MTF* is a repeated cross-sectional national survey that, since 1975, has collected information each spring from high school seniors about their behaviors and attitudes. *MTF* also surveys 8th and 10th graders although data collection for these two student groups did not commence until 1991. Our analysis focuses on the annual 12^{th} grade surveys because the late date at which data collection began for 8^{th} and 10^{th} graders makes it impossible to assess their substance prior to abortion legalization. The focus on different generations of 12^{th} graders means that we have a sample, drawn from different birth cohorts, who are (approximately) the same age when observed. Each cross-section of *MTF* 12^{th} graders consists of about 16,000 students from 130 schools (Johnston, O'Malley, and Bachman (2001)).[13] All of our analyses are weighted using the sampling weights provided with the study.

Information about the use of five types of controlled substances is available in the *MTF*. Information about three types comes directly from the 12^{th} graders' responses: use of cigarettes, use of alcohol, and use of marijuana. Information on other types of illicit substance use, such as cocaine, heroin, and amphetamines, is also collected in *MTF*. We use two composite measures constructed by the *MTF* staff. One indicates the use of any illicit drug; the other denotes use of any illicit drug excluding marijuana. For each of these five substances, students report two measures of use: whether they have ever used the substance at all in their lifetimes, and whether they used the substance within the

previous thirty days. We only present results for use within the previous month, as this is likely a better measure of habitual, serious use. The results for the "ever used" indicators of use are qualitative identical to the results presented in the paper.[14]

There is limited demographic data available in the *MTF*. We know the student's gender and race, and the educational attainment of the student's parents.[15] Students who give missing responses for any of these demographic data are dropped from our analysis.[16]

Ideally, we would like to know precisely where a student's mother was when he or she was *in utero*. The public use version of *MTF* does not include state identifiers, but we made a restricted analysis data agreement with *MTF* that enabled us to merge the student's state of residence as of the survey date onto the dataset. These state of residence identifiers are the only indication of student location available. Our analysis therefore assumes that the students' state of residence is the same as their state of birth.[17] Note, measurement error in the state of birth variable biases us *away* from finding any significant effect consistent with the selection hypothesis because of attenuation bias.

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[13] One limitation of the data is that MTF does not sample high school dropouts. However, to the extent that dropouts are likely to be children born into relatively disadvantaged circumstances, the absence of dropouts from the data biases us *against* finding evidence for the predicted effect of legalization.

[14] These results are available from the authors upon request.

[15] We do not create a table of means as the set of demographics is so limited. One half of the students are female. Roughly 75% of the sample is white, 12% are African-American, 6% are Latin-American, and 2% are Asian-American. Forty-eight percent of the mothers have education beyond high school, and about 15% are less than high school education. Seventeen percent of fathers have less than high school education, and fifty-one percent have education beyond high school. There no statistically significant differences in these demographics across repeal and non-repeal states.

[16] These exclusions result in a loss of nearly 10% of the observations. Additional results not shown here indicate that the results are not affected by deleting these observations.

[17] We think this assumption fairly innocuous. In the 1980 and 1990 Census IPUMS about eighty percent of people enrolled in school, and aged 16-17, resided in their state of birth. The number was slightly larger for New York and California – two of the repeal states in our study. We note that even if state of birth were known, there would be the problem that some people are born in states different than the ones in which they spent the relevant part of their gestation.

Students' birthdays are reported in the survey. We wish to identify when people were *in utero,* and at risk to have been aborted. Birthdays do not capture this well. Instead, we use the birthday information to create the variable YU_i, which measures the year covering the first six months of the respondent's gestation. We code this variable as year t^* if the student was born between July 1 of year t^* and June 30th of year t^*+1. Henceforth, whenever the paper refers to "year" of birth, we will be referring to this variable.[18]

Our analyses control for time-varying state-level factors that likely affect substance use. We include state cigarette and beer taxes, the fraction of the state's population that resides in a dry county, state per capita income, and state unemployment rate. All of these variables correspond to the year of the survey. Information on state cigarette taxes, including the federal cigarette tax, is from the Tobacco Institute's *The Tax Burden on Tobacco*.[19] The state tax on a case of twenty-four twelve-ounce beers was obtained from the Beer Institute's *Brewer's Almanac*. An estimate of the number of a state's residents residing in dry counties was also obtained from the *Brewer's Almanac*. Dividing these estimates by inter-censal estimates of the total population in a state taken from the Census Bureau, we create the fraction of the state population residing in dry counties for each year. Information on each state's annual per capita income was obtained from the Bureau of Economic Analysis, and state adult unemployment rates were obtained from the Bureau of Labor Statistics. All dollar figures are converted to 1999 dollars using the CPI-U.

[18] Constructing the year-of-birth variables this way also helps us align the students' years of birth with state abortion ratios, which we discuss in detail later.

[19] Phillip DeCicca graciously provided us these data. See DeCicca et al (2002) for the details of data.

3 The Impact of Early vs. Late Legalization of Abortion

Setup

Our first set of results exploits the fact that the "repeal" states legalized abortion three years prior to nationwide legalization in 1973. We estimate the model:

$$\begin{aligned} \text{Use}_{ijt} = {} & \beta_1\left(\text{Repeal}_j * D_{7072}\right) + \beta_2\left(\text{Repeal}_j * D_{7476}\right) + \beta_3\left(\text{Repeal}_j * D_{7880}\right) + \\ & \beta_4 D_{7072} + \beta_5 D_{7476} + \beta_6 D_{7880} + \beta_7 X_{it} + \beta_8 \Gamma_{jt} + \beta_9 \delta_j + \beta_{10}\tau_t + \beta_{11} YU_i + \varepsilon_{ijt} \end{aligned} \quad (1)$$

where Use_{ijt} is an indicator variable which equals 1 if student i in state j in year t uses the particular substance. The variables X_{it} and Γ_{jt} are, respectively, the vectors of individual and time-varying state-level controls described above. The terms δ_j and τ_t are, respectively, state and survey year fixed effects which account for differences over time and across states in unobserved factors related to substance use. The variables YU_i are the earlier discussed "year of birth" (or more accurately, "year in utero") dummies which control for idiosyncratic changes in substance use from one birth cohort to the next. The binary variables D_{7072}, D_{7476} and D_{7880} represent, respectively, three different "epochs" during which the person could have been *in utero*: *any year* during the interval 1970-1972; *any year* during the interval 1974-1976; or any year in the interval 1978-1980.[20] The variable Repeal_j in (1) denotes whether the person's state is one of the early legalizing states in which abortion was effectively legalized in 1970.[21]

[20]Two persons born in different years during the same epoch will have the same values for the value for their epoch of birth dummy, but will have different year of birth dummies YU_i, so the year of birth and epoch dummies are not collinear. We also estimated the models without the specific birth cohort dummies, and the results (shown later) are essentially the same.

[21] The use of state fixed effects in equation (1) precludes the use of a main effect for the Repeal_j variable.

The interactions between the variables for the epoch of a person's birth and the repeal variable are the variables of interest in (1). The first interaction asks: Among high school seniors born during 1970-1972, was substance use higher for those born in states in which they could have been aborted in those years? The second interaction asks: Among high school seniors born during 1974-1976, who could each have been aborted because abortion had recently been nationally legalized, was there a difference in adolescent use for those who happened to be born in early legalizing states ? Finally, the last interaction asks: Among high school seniors born during 1978-1980, at a time when abortion had long been nationally legalized for all women, was there a difference in substance use for those who happened to be born in early legalizing states?

Under the cohort size or selection effects described earlier, high school seniors of a given birth cohort who were exposed to legalized abortion *in utero* should have lower rates of substance use than those of the same birth cohort who were not exposed. The coefficient β_1 should therefore be negative since, among people born between 1970 and 1972, only those from repeal states were exposed to legalized abortion. All persons born after national legalization in 1973 were, in principle, exposed to the same *in utero* environment of legalized abortion. Thus, whether a person born after 1973 was born in a repeal state or not should be irrelevant for their use of controlled substances. The coefficients β_2 and β_3 should thus both be zero. This reasoning presumes that the convergence of abortion activity in late legalizing states to levels in the repeal states was immediate after 1973. If, instead, it took a few years for abortion activity in late legalizing states to catch up, the abortion environments faced by people from the two

types of states would have become similar only some years after 1973. Such a lag would imply that β_3 should be 0, although β_2 might be negative rather than 0.

We estimate (1) on a sample of different cohorts of 12th graders born over the interval 1966 to 1980. We exclude three birth cohorts: people born in years 1969, 1973 and 1977. For the 1969 and 1973 cohorts it is not possible to say precisely what abortion regime prevailed during the time the person was *in utero*. We drop the 1977 cohort for consistency: each epoch studied consists of 3 birth years. The final sample consists of *MTF* respondents spanning the survey years 1983-1999.

Main Results

Table 1 presents the results for whether students, as of their senior year in high school, report having recently use one of the five controlled substances. Means of the dependent variable as well as sample sizes are reported at the bottom of Table 1 for each dependent variable.[22] The standard errors allow for arbitrary correlation for individuals within the same state over time. The table shows that the demographic controls are all statistically significant determinants of substance use. Boys are more likely to have recently used all controlled substances, except tobacco. Controlling for family background, whites are more likely to have used. Increased parental education, both mother's and father's, reduces the likelihood of use for all substances, except for alcohol.

The beer tax has a consistently negative and significant effect on the recent use of all of the controlled substances, except smoking. By contrast, the cigarette tax has no effect on the recent use of any of these substances. The effect of the state unemployment

rate, the state's per capita income, and the fraction of the state's population living in a dry county also have no discernible effect on use in any of the specifications.

The results for the parameters of primary interest, β_1, β_2 and β_3 are shown in the first three rows of Table 1. For the most serious substances – all illicit substances and illicit substances excluding marijuana – 12th graders who were exposed to legalized abortion *in utero* because their states were early legalizers were significantly less likely to have recently used. For the less serious categories – cigarette smoking, marijuana and drinking alcohol – the estimated effect on the interaction is also negative, but these effects are either only weakly or not statistically significant. Reassuringly, all of the point estimates for β_1 point strongly in the direction suggested by the earlier discussion.

The estimated effect of having been born in a repeal state in 1974-1976 is zero for all of the use categories. This result is consistent with a causal effect of abortion exposure, since all persons from these birth cohorts were exposed to legal abortions *in utero*. Unfortunately, the results for students born during 1978-1980 are not as strong. Despite the fact that abortion had long been nationally legalized, we find that use of marijuana and for any illicit drug (including marijuana), was higher for persons who happened to be born in repeal states. There should have been no difference in use these late years, provided that abortion and fertility behavior had by this time become roughly the same in all states. Reassuringly, these two coefficients are the only ones that are wrong-signed in a statistically significant way. For the most serious dimension of use – illegal drugs except marijuana – the β_3 coefficient is 0, as predicted.

[22] Sample sizes vary across dependent variables since students do not answer all substance use questions.

Overall, the results strongly support the idea that exposure to legalized abortion because of early legalization, was associated with decreased substance use. We generally find reduced substance use for adolescents born in repeal states during the period when only they were exposed to legalized abortion, and no difference in use between persons born in repeal states and those born in other states in the time both shortly and several years after nation legalization. [23] Moreover, this pattern is most sharply evident for the most serious dimension of substance use, and generally not found for those less serious substances which nearly all adolescents try at some point. This is precisely what we would predict if abortion exposure affected behavior either through the selection or other effects outlined in the Introduction. Finally, the results are both statistically and economically significant. For example, 10% of all students report having recently used an illicit drug other than marijuana. Our estimated coefficient of –0.026 therefore implies a about a twenty-five percent average reduction in the likelihood of ever having used associated with *in utero* exposure to legal abortion.

Robustness Tests

How sensitive are the benchmark results in Table 1 to alternative model specifications? The framework in (1) is a difference-in-difference estimator, with distinct time intervals: the "before" period, when no person in any state was exposed to legalized abortion *in utero*; the "during" period, when only persons in the repeal state

[23] We test whether the interaction term β_1 is statistically different from the other two interaction terms β_2 and β_3. We find they are significantly different in all the specifications at the 10% level, for all substances excluding drinking and smoking..

were exposed to abortion; and two "after" period, when both the repeal and non-repeal states had legal abortion due to the national law change.

Table 2 presents the estimated interaction effects from different versions of the model (1). The results in Panel A of the table are from the most basic difference-in-difference specification. This regression has only the dummy variable indicating that the state is was an early legalizer, dummies for the "epoch" of the person's birth, and the three interaction terms. It drops the state, year of birth and survey year fixed effects, and all of the demographic and time varying state controls. These results show that the strong conclusions from the benchmark model hinge on our having controlled for all of the other factors in the benchmark model of Table 1. While the β_1 interaction coefficient is always negative and statistically significant, as predicted, we consistently find that those born in repeal states after national legalization continue to exhibit lower rates of use.

Are the various controls used in the benchmark model differentially important in explaining the difference between the simple difference-in-difference results in panel A of Table 2 and the benchmark results in Table 1? If a particular factor is unimportant for the difference between the two sets of results, then excluding only that factor from the benchmark regression should leave the estimates basically unchanged from what we find in Table 1. Panels B, C, and D show, in turn, the results when the regressions exclude birth year and survey year fixed effects, observations from New York, and observations from California. [24] The results in these three panels are virtually identical to the main results in Table 1. As with the benchmark model, the results show that use is lower by

[24] While there is not an exact one-to-one correspondence between the survey year and cohort fixed effects, these coefficients exhibit a similar pattern when only one of these sets of effects is included in the model. Therefore, we only present the results when both sets of these effects are excluded.

statistically significant amounts for people from repeal states when only these people were exposed to legalized abortion. And, except for some of the use categories that include marijuana smoking, there is no statistically significant difference in use for people born in repeal states after national legalization. The benchmark results thus appear to be due neither to something peculiar about adolescents from these two largest states, nor to secular trends in substance use.

Panels D and E show the results when model (1) excludes, in turn, state fixed effects, and all demographic and time varying state controls.[25] The results show that it is these factors that, to differing degrees, account for the difference between the simple difference model in Panel A and the benchmark results in Table 1. Both sets of regressions find, as does the benchmark model, lower use for persons born in the repeal states when only those states allowed legalized abortion. However, unlike the benchmark regression, both models find statistically different levels of use for people from these states for all of the use categories in one or the other of the post-national legalization period. The differences with the benchmark results in Table 1 are especially pronounced for the results that drop the demographic and time varying state controls.

In analysis not shown, we explore why excluding these controls might lead to significant coefficients for post 1973 interactions. The operating hypothesis of the difference in difference framework is that any differences in observable factors between repeal and non-repeal states remain fixed across the various "epochs". We measure the relative difference in means across epochs and repeal and non-repeal states for each of

[25] When the state fixed effects are dropped, we include the binary variable Repeal_j so as to remain in the difference in difference framework.

the time varying state variables, and find that this assumption is strongly rejected by the data, for all of the state controls and for all pairs of "epochs". For example, the difference in the average beer tax confronted by students in the "after" period from repeal states versus those from non-repeal states born in the 1970-1972 period is 40 cents difference than the comparable difference in the tax faced by students from these types of states but born in the period after 1973. To the extent that higher beer taxes lower student demand for drugs, excluding this control imparts a negative omitted variables bias to estimated interaction terms for the period after 1973.

Similar difference-in-difference results are evident for the other state controls, all of which will likely impart a negative bias on β_2 and β_3. Adding these time-varying state controls, as we do in the baseline regressions, leaves the 1970-1972 interaction strongly negative, but causes most of the post-1973 differential effects to vanish. Had the difference in use for people born in 1970-1972 only been due to changes in observable factors rather than to abortion legalization in the repeal states, we would have expected the coefficient β_1 to go to 0 as well. The fact that this interaction term is not impacted by the inclusion of the state-level factors suggests that the estimated effects are truly due to differential abortion exposure from early legalization.

In the final row of the table, we add state-specific time trends to the baseline model. This specification asks a lot of the data, since the state trend terms use up much of the variation used to identify the earlier effects. The results show that the only difference of note between these estimates and those in the benchmark case and the various extensions, is that there appears to be slightly slower "bounceback". That is, the reduced use for people from the repeal states takes slightly longer to disappear when state

trends are controlled for than is true with the other results. The results continue to show strong evidence of reduced use in the 1970-73 period for persons born in the repeal states, and also show that these difference vanish by a five years after national legalization. These findings strongly support the main results.

4. Birth Rates, Abortion Rates, and the Plausibility of the Estimates

Early Legalization and Variation in Birth Rates and Abortion Ratios

Our main results show that teens exposed *in utero* to legalized abortions because of early legalization in their states were less likely to use controlled substances than their counterparts in other states. These effects are relatively large. For example, the results show that young people who were exposed *in utero* to early legalization were, on average, between 2.1 percentage points (for any illicit drug) and 2.6 percentage points (for any illicit drug, excluding marijuana) less likely to use. These numbers represent a 10 and 25 percent lower average probability, respectively, of using these substances.

Presumably, any effect of exposure to legalized abortion operated through its effect on abortion and birth rates in the affected states. It is changes in these variables that initiate the selection and cohort size effects on substance use. We do not adjudicate between the relative importance of these two effects. Instead, we assess how reform affected fertility behavior as summarized by the birth and abortion rate, and how these change in turn, changed substance use.

To answer the first question, we estimate regressions of the form

$$\begin{aligned}\text{Rate}_j = {} & \gamma_1\left(\text{Repeal}_j * D_{7072}\right) + \gamma_2\left(\text{Repeal}_j * D_{7476}\right) + \gamma_3\left(\text{Repeal}_j * D_{7880}\right) \\ & + \gamma_3 D_{7072} + \gamma_4 D_{7476} + \gamma_5 D_{7880} + \gamma_6 X_{it} + \gamma_7\, \Gamma_{jt} + \gamma_8 \delta_j + \gamma_9 \tau_t + \gamma_{10} YU_i + u_{ijt},\end{aligned} \tag{2}$$

where $Rate_j$ is a measure of the birth or abortion rate in the person's state in their year of birth. In (2), the first epoch/repeal interaction measure how much the birth (abortion) rate was differentially lower (higher) in states which reformed early, during the years in which only they permitted legalized abortion. The second and third interactions measure how birth (abortion) rates in the repeal states compared to those in the rest of the country after abortion was nationally legalized.

Table 3 shows the results for regression (2) for two measures of state birth rates, and two measures of the abortion rate. Birth rates are measured as the number of live births per hundred women. Abortion rates are measured both in terms of state of residence of the woman, and state of occurrence.[26] The "state of residence" abortion ratio equals the number of abortions had by women who live in a state, divided by the number of live births to women in that state. Total live births are measured from the year from July 1 to June 30, ensuring that the denominator corresponds to the same set of pregnancies as the numerator. The numerator for the state of occurrence abortion measure is the number of abortions occurring in the state in a given year.

The first column presents the results for the birth rate measured in levels. The regression in the first panel includes, apart from the three interaction terms, only state fixed effects. This simple specification shows that birth rates in the early repeal states were about 3.5 births per hundred lower in the repeal states during the repeal years of 1970-1973. The effect is strongly statistically significant. There was no difference in

[26] Joyce (2001) criticizes the abortion ratio by state of occurrence because it includes abortions by people who do not live in the state. This consideration is a potentially important one in the years immediately preceding Roe v. Wade, since women from states that had not legalized abortion could travel into states in which the abortion laws had been repealed to have the procedure performed. Ted Joyce graciously provided us with the "state of residence" abortion data used in our analysis.

birth rates between the two types of states in either the immediate or late post-1973 periods.

The other two specifications in the first column control for year and cohort effects, and then for year effects, cohort effects and the demographic and time varying state controls. Controlling for these other factors, and especially the demographic controls, reduces the amount by which birth rates are lower in repeal states in 1970-1972 from 3.5 to around 2.5. Otherwise, the results in these other specifications are essentially the same as in the simple specification in the first panel: birth rates are significantly lower in repeal states during the repeal period and not different later time periods. In the second column, we run the same three specifications, but with the birth rate measured in logs. The results are qualitatively the same. These results are nearly identical to those found by Levine et al (1996), who use a similar specification.[27]

The last two columns in the table measure the effect of early legalization on abortion behavior in the repeal states relative to the rest of the country. Whether measured in terms of state of occurrence or state of residence, abortion rates were, as expected, higher in repeal states during the years when abortion was illegal in other states. However, the abortion ratio continued to be higher in repeal states, even after abortion had been nationally legalized. The fact that there are relatively more abortions in repeal states after national legalization raises the question of what besides changes in unintended fertility, as measured by the birth rate, the abortion ratio captures. Some of the debate in the abortion literature has therefore centered on whether it is appropriate to

[27] Note that Levine et al perform their analysis at the state level while our analysis is performed at the individual level using the sample of *MTF* respondents. In addition, they use the log of the birth rate rather than the level of the birth rate.

use the abortion ratio as opposed to the birth rate, or categorical measures of legalization. Joyce, for example, argues that if the only effect of legalized abortion was its effect on birth rates, then abortion behavior should track that for the birth rate results. Given these concerns, we do not emphasize the abortion ratio results in what follows.

TSLS Estimates of Effect of Early Legalization on Use

How did early legalization, through its effect on birth and abortion ratios shown in the previous section, affect substance use? This question can be answered straightforwardly by letting the results of regression (2) be the first stage in a Two Stage Least Squares (TSLS) analysis, in which the second stage regression is

$$\text{Use}_{ijt} = \alpha_1 Rate_j + \alpha_2 D_{7072} + \alpha_3 D_{7476} + \alpha_4 D_{7880} + \alpha_5 X_{it} + \alpha_6 \Gamma_{jt} + \alpha_7 \delta_j + \alpha_8 \tau_t + \alpha_9 YU_i + \nu_{ijt} \quad (3)$$

The three interaction terms indicating the state's early legalization status are instrumental variables for the state's birth rate or abortion ratio in this analysis. [28]

In the TSLS approach, variation in the birth rate or abortion ratio in (3) comes from the differential timing of legalization in the repeal and non-repeal states and thus isolates the effect of abortion reform. Because the relative timing of reform is arguably exogenous, these results measure how an exogenous reduction in cohort size coupled with positive selection, because of changes in abortion activity and fertility, causally affect adolescent substance use. The coefficient α_1 from the TSLS exercise is thus the structural estimate of this effect.

[28] In the first stage regressions, the F-tests for the excluded instruments shown at the bottom of Table 3 are large enough to avoid concerns about finite sample biases that may contaminate TSLS estimates (E.g., see Bound, Jaeger, and Baker 1995).

Each entry in Table 4 is the estimated coefficient and standard error of a separate TSLS analysis of the relationship between the particular birth or abortion ratio measure and substance use within the past month. As noted above, the excluded instruments are the three interaction terms denoting early legalization. These structural estimates are, if anything, stronger confirmation of the fact that abortion legalization affected subsequent substance use than the results presented thus far. The first two rows show that increases in the birth rate due to the early legalization of abortion were associated with strongly statistically significant increases in the probability of recent use for every substance except smoking.

Assessing the Plausibility of the Estimates

Are these estimated effects plausible? The TSLS approach finds that that each 1 in 100 decrease in birth rate arising from early legalization lowered average recent use by about 1 percentage point for both of most serious substances (illicit drugs, and illicit drugs except marijuana). From the first stage results, we know that birth rates were about 3 in 100 lower in the repeal states during the early legalization years. This implies that the actual average reduction in use of 2.1 percent and 2.6 percent for the two most serious substances found in the benchmark results can be comfortably explained by the changes in birth rate changes in repeal states.

What do these results imply about use among the adolescents who, but for abortion, would have been born? The estimates of the birth rate reductions from early legalization allow a straightforward calculation of this effect. The reduction of about 3 in 100 in births in repeal states represents lower births of around 6 percent. If $U_{aborted}$ is the average rate of use among persons aborted because of reform, and $U_{non-aborted}$ is the

average rate of use among other persons, then the percentage point change in average use occasioned by abortion reform is $0.06(U_{non-aborted} - U_{aborted})$.[29] Thus, the percentage point *difference* in the average use rate between the aborted adolescents and those not aborted is approximately 16.6 times the overall change in average use due to abortion reform. This implies that the aborted persons would have recently used illicit substances at a rate of 34.9 percentage points $(16.6 * 2.1)$ higher than people who were born - or at a rate of about 56.9 percent, given the average use rate at the bottom of Table 1. And, aborted persons would have recently used illicit substances, excluding marijuana, at a rate 43.1 percentage points $(16.6 * 2.6)$ larger than people who were born, or at a rate of about 53.1 percent.

At first blush, these estimates seem large. However, it should be remembered that that even these large implied use propensities suggest that nearly one half of adolescents who were not born because of abortion would *not* have used illicit substances. That said, are the results plausible?

We believe that they are under either a selection or cohort size interpretation of what abortion reform did. If reform was associated principally with a selection effect, then the adolescents who would have been born would have been born into the worst childhood circumstances. We do not directly observe these very worst circumstances, but there is information on differences in average use between adolescents whose backgrounds differ in observable ways. For example, Table 1 shows the effect of being

[29] Average use without abortion reform is $0.06 * U_{aborted} + 0.94 * U_{non-aborted}$ since aborted persons are approximate 6% of the pre-reform sample. Average use after reform is $U_{non-aborted}$. The change in average use associated with reform is thus $U_{non-aborted} - 0.06 * U_{aborted} - 0.94 * U_{non-aborted}$.

born to parents with different levels of schooling. We find a 1.4 percentage point difference in recent use of illicit substances excluding marijuana between students whose mothers completed less than high school and those who completed only a high school education. For the same substance, students whose fathers completed exactly a high school education were less 1.3 percentage points less likely to use than students whose fathers were high school dropouts. These are very large effects, considering that the average level of use of these substances, across all students, is ten percent. If the effect of these small differences in observable disadvantage is this large, we think it entirely plausible that differences between people in the most limited circumstances and the average person can be as large as we find.

If, in addition or instead of selection, abortion reform initiated changes in use because of peer and other effects associated with changes in cohort size, our estimates seem even more plausible. If there are large peer effects in teen behavior, then the estimates measure both the greater relative use of persons who were aborted, *and* the spillover effects of their use on the behavior of other teens.

Our evidence is consistent with Gruber, Levine and Staiger (1996), who find very large cohort specific relative improvements in early life outcomes for people from repeal states. They are also consistent with Donohue and Levitt (2001) who find that nearly half of the reduction in serious crime in the 1990's can be attributed to abortion reform from the early 1970's.

The TSLS for the two abortion ratio measures are shown in the last two rows of the table. Nearly all of the point estimates are negative, and thus consistent with the notion that increased abortion activity following legalization was associated with lower

use among 12th graders who were exposed to these changes *in utero*. However, none of these estimates is statistically significant at conventional levels. The fact that first stage results in the earlier section showed that there was more abortion activity in repeal states after national legalization raises the question of what besides changes in unintended fertility, as measured by the birth rate, the abortion ratio captures. Because of this concern, we do not emphasize the abortion ratio results, even though the abortion ratio results are, on the whole, very similar to those from those forthcoming from both the categorical measures of legalization and the birth rate.

6. Conclusion

This paper studies the impact of abortion legalization on the use of controlled substances for people exposed to legalization *in utero*. We compare substance use among adolescents born in states that legalized abortion early, to use among youths from the same birth year cohorts but from states that did not legalize early. We find that *in utero* exposure to legalized abortion is associated with diminished substance use, especially of illegal narcotics. We then study how early legalization in the repeal states affected birth rates, and relate this variation in birth rates and abortion ratios to substance use using a two-stage procedure. We find the lower birth rates reduced teen substance use, but only for the very specific cohorts exposed to legalized abortion. Moreover, the results suggest that the average differences in substance use between repeal and non-repeal states can be attributed exclusively to abortion's effect on fertility behavior, and not to some other difference between states. Using the same two-stage procedure, we find that using the abortion ratio rather than the birth rate yields qualitatively similar results.

Abortion legalization has been shown by previous work to have resulted in large improvements in early childhood circumstances. We argue that either this selection effect, or an effect due to the effect of cohort size on peer effects, can explain our results. Our findings illustrate how changes in the quality of childhood circumstances can affect outcomes later in life. In addition, our study of illicit substance use may identify the mechanism by which abortion legalization could have caused the effect on serious crime found by other authors.

Our analysis focuses on the *relatively* lower rates of drug use for adolescents who faced higher risks of having been aborted. It remains a puzzle why, in the aggregate, adolescent drug use increased slightly beginning in the early 1990s. Bachman et al. (1998) consider and ultimately dismiss the role of changes in standard background factors like race, sex, family structure and urbanicity. Others have stressed the potential importance of government policy on substance use. For example, fewer resources were spent on prevention in the 1990s after the very popular "Just Say No" efforts of the 1980s. And, drug interdiction efforts may have become less effective in the 1990s, as evidenced by the increase in drug quality and the reduction in drug prices over this interval (Pacula et al (2001)). Finally, young people who came of age in the 1990s were the children of baby boomers – the highest drug-using generation in American history. If these parents had more lax views about drug use, these ideas may have been passed on to their children, who themselves used drugs as adolescents or young adults. Jacobson (2001) argues against this effect, suggesting instead that changes drug distribution, peer interactions and other effects having to do with cohort size may account for much of the aggregate trends. Our paper does not address the issue of aggregate movements, but the

results suggest that increases in substance use in the early 1990s would have been even higher, had there been no abortion reform in the early 1970s.

The paper undertakes none of the many philosophical and moral questions raised by abortion legalization and availability. Nor does our examination of the effect of abortion exposure on substance use address any of the other economic costs or gains associated with legalization. It would thus be wrong to read our results as supportive of abortion legalization in particular, or greater abortion availability in general. However, we believe that policy questions about abortion should be informed by credible estimates of the policy's various effects. Research that examines additional later life outcomes attributable to the reforms in abortion laws, both within the United States as well as in other countries, is needed to provide a more complete picture of the impact of changes in abortion policy.

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Table 1. Reduced Form Estimates of Effect of in utero Legalized Abortion Exposure on Whether Used Controlled Substance Within Last Month

	Controlled Substance				
Variable	Smoke	Drink	Marijuana	Any Illicit Drug	Any Illicit Except Marijuana
Repeal*D_7072	**-0.017**	**-0.025**	**-0.012**	**-0.021**	**-0.026**
	(0.012)	**(0.020)**	**(0.010)**	**(0.013)**	**(0.011)**
Repeal*D_7476	**0.011**	**-0.002**	**0.008**	**0.012**	**0.012**
	(0.027)	**(0.030)**	**(0.022)**	**(0.028)**	**(0.015)**
Repeal*D_7880	**0.004**	**0.031**	**0.044**	**0.042**	**0.008**
	(0.025)	**(0.024)**	**(0.020)**	**(0.019)**	**(0.019)**
D_7072	-0.097	-0.008	-0.017	-0.011	-0.014
	(0.0221)	(0.0260)	(0.0149)	(0.0174)	(0.0128)
D_7476	-0.197	-0.075	-0.047	-0.047	-0.030
	(0.0402)	(0.0353)	(0.0218)	(0.0237)	(0.0180)
D_7880	-0.262	-0.142	-0.059	-0.068	-0.052
	(0.0442)	(0.0473)	(0.0292)	(0.0303)	(0.0207)
Male	-0.003	0.080	0.059	0.051	0.020
	(0.0082)	(0.0059)	(0.0040)	(0.0039)	(0.0019)
White	0.139	0.171	0.050	0.062	0.044
	(0.0095)	(0.0121)	(0.0113)	(0.0110)	(0.0038)
Mother HS Grad	-0.022	0.014	-0.006	-0.011	-0.014
	(0.0066)	(0.0062)	(0.0062)	(0.0073)	(0.0037)
Mother Some College	-0.027	0.024	0.003	-0.003	-0.011
	(0.0074)	(0.0071)	(0.0078)	(0.0086)	(0.0038)
Mother College Grad	-0.029	0.026	-0.007	-0.012	-0.017
	(0.0073)	(0.0085)	(0.0063)	(0.0075)	(0.0037)
Father HS Grad	-0.021	-0.006	-0.013	-0.014	-0.013
	(0.0065)	(0.0049)	(0.0061)	(0.0055)	(0.0038)
Father Some College	-0.028	-0.005	-0.008	-0.012	-0.015
	(0.0075)	(0.0065)	(0.0058)	(0.0049)	(0.0033)
Father College Grad	-0.038	-0.003	-0.011	-0.017	-0.019
	(0.0086)	(0.0069)	(0.0048)	(0.0051)	(0.0038)
Cigarette Tax	-0.00029	-0.00002	0.00025	0.00000	-0.00039
	(0.0006)	(0.0005)	(0.0002)	(0.0003)	(0.0002)
Beer Tax	-0.0003	-0.0007	-0.0010	-0.0010	-0.0005
	(0.0002)	(0.0004)	(0.0002)	(0.0003)	(0.0002)
Fraction Dry	0.093	-0.159	-0.195	-0.241	0.005
	(0.255)	(0.416)	(0.252)	(0.299)	(0.255)
State Per Capita Income	-0.000003	-0.000002	-0.000004	-0.000001	0.000000
	(0.000005)	(0.000005)	(0.000004)	(0.000005)	(0.000005)
State Unemployment Rate	-0.0017	0.0003	0.0035	0.0005	-0.0045
	(0.0029)	(0.0037)	(0.0028)	(0.0031)	(0.0039)
State Fixed Effects?	Yes	Yes	Yes	Yes	Yes
Survey Year Fixed Effects?	Yes	Yes	Yes	Yes	Yes
Cohort Effects?	Yes	Yes	Yes	Yes	Yes
Mean of Dependent Var	**0.30**	**0.60**	**0.19**	**0.22**	**0.10**
R-squared	0.026	0.061	0.029	0.028	0.017
N	126,389	116,751	125,674	122,867	122,867

Regressions are weighted using MTF sampling weights.
Standard errors allow for arbitrary clustering within a state. See text for additional details.

Table 2. Robustness of Results in Tables 1: Alternative Model Specifications for Use in Past 30 Days

Robustness Test	*Interaction Terms*	Smoke	Drink	*Controlled Substance* Marijuana	Any Illicit Drug	Any Illicit Drug Except Marijuana
A. Simple Difference in Difference:						
No Survey Year, State or Birth Year Fixed Effects, And No Demographic or Time Varying State Controls	Repeal*D_7072	-0.022	-0.033	-0.027	-0.040	-0.042
		(0.008)	(0.021)	(0.015)	(0.014)	(0.009)
	Repeal*D_7476	-0.018	-0.058	-0.032	-0.043	-0.036
		(0.019)	(0.031)	(0.014)	(0.016)	(0.015)
	Repeal*D_7880	-0.020	-0.009	-0.004	-0.020	-0.044
		(0.017)	(0.024)	(0.013)	(0.013)	(0.019)
B: No Survey Year or Cohort Fixed Effects						
	Repeal*D_7072	-0.016	-0.017	-0.012	-0.021	-0.027
		(0.012)	(0.019)	(0.011)	(0.013)	(0.011)
	Repeal*D_7476	0.028	0.009	0.015	0.013	0.005
		(0.023)	(0.031)	(0.023)	(0.027)	(0.014)
	Repeal*D_7880	0.019	0.036	0.050	0.043	0.002
		(0.024)	(0.021)	(0.015)	(0.016)	(0.020)
C: No New York Observations						
	Repeal*D_7072	-0.016	-0.047	-0.012	-0.023	-0.032
		(0.014)	(0.013)	(0.011)	(0.014)	(0.009)
	Repeal*D_7476	-0.017	-0.021	0.006	-0.002	-0.001
		(0.024)	(0.037)	(0.030)	(0.035)	(0.015)
	Repeal*D_7880	-0.022	0.029	0.065	0.044	-0.016
		(0.027)	(0.037)	(0.021)	(0.026)	(0.017)
D. No California Observations						
	Repeal*D_7072	-0.024	0.012	-0.016	-0.028	-0.014
		(0.011)	(0.017)	(0.014)	(0.018)	(0.012)
	Repeal*D_7476	0.015	0.016	0.003	0.007	0.021
		(0.031)	(0.045)	(0.032)	(0.039)	(0.016)
	Repeal*D_7880	0.004	0.036	0.032	0.037	0.029
		(0.028)	(0.027)	(0.017)	(0.019)	(0.018)
E: No State Fixed Effects						
	Repeal*D_7072	-0.022	-0.049	-0.029	-0.040	-0.037
		(0.009)	(0.022)	(0.018)	(0.017)	(0.008)
	Repeal*D_7476	0.006	-0.048	-0.022	-0.029	-0.023
		(0.015)	(0.026)	(0.020)	(0.020)	(0.014)
	Repeal*D_7880	0.000	-0.009	0.006	-0.006	-0.028
		(0.017)	(0.020)	(0.020)	(0.015)	(0.018)
F: No Demographic or Time-Varying State Controls	Repeal*D_7072	-0.027	-0.036	-0.026	-0.039	-0.042
		(0.007)	(0.019)	(0.014)	(0.013)	(0.009)
	Repeal*D_7476	-0.019	-0.053	-0.030	-0.041	-0.034
		(0.020)	(0.032)	(0.015)	(0.017)	(0.015)
	Repeal*D_7880	-0.027	-0.008	0.002	-0.012	-0.038
		(0.016)	(0.022)	(0.013)	(0.013)	(0.019)
G: Add State Specific Time Trends						
	Repeal*D_7072	-0.016	-0.005	-0.023	-0.027	-0.022
		(0.004)	(0.003)	(0.004)	(0.003)	(0.003)
	Repeal*D_7476	-0.015	-0.001	-0.026	-0.031	-0.019
		(0.004)	(0.003)	(0.004)	(0.003)	(0.003)
	Repeal*D_7880	0.003	0.003	0.010	0.002	-0.008
		(0.004)	(0.003)	(0.004)	(0.004)	(0.003)

The regressions from which these estimates come are identical to the regressions in Table 1, except for the specific modification noted.
Data from multiple waves of Monitoring the Future. See text for additional details.
All regression except that in panel G are weighted using MTF sampling weights. Standard errors allow for arbitrary forms of clustering within states.

Table 3. Effect of Early Legalization on Abortion Activity and Birth Rates

	Birth Rate	Log Birth Rate	Abortion by State Of Occurrence	Abortion by State Of Residence
Treated*70-72	-3.525	-0.048	300.5	218.7
	(0.816)	(0.010)	(60.9)	(15.2)
Treated*74-76	-0.373	-0.014	316.3	263.2
	(1.528)	(0.026)	(58.0)	(32.0)
Treated*78-80	0.377	-0.003	312.6	283.5
	(2.934)	(0.048)	(51.6)	(41.2)
Demographics?	No	No	No	No
Year Effects?	No	No	No	No
State Effects?	Yes	Yes	Yes	Yes
Cohort Effects?	No	No	No	No
F-test on excluded instruments (p-value)	9.22 (0.00)	9.34 (0.00)	14.44 (0.00)	80.22 (0.00)

No Demographics

	Birth Rate	Log Birth Rate	Abortion by State Of Occurrence	Abortion by State Of Residence
Treated*70-72	-3.422	-0.047	301.7	220.1
	(0.842)	(0.011)	(61.3)	(16.4)
Treated*74-76	-0.206	-0.012	315.5	261.8
	(1.515)	(0.025)	(58.0)	(32.1)
Treated*78-80	0.451	-0.002	310.2	280.8
	(2.885)	(0.047)	(51.2)	(40.7)
Demographics?	No	No	No	No
Year Effects?	Yes	Yes	Yes	Yes
State Effects?	Yes	Yes	Yes	Yes
Cohort Effects?	Yes	Yes	Yes	Yes
F-test on excluded instruments (p-value)	11.03 (0.00)	9.84 (0.00)	14.18 (0.00)	70.95 (0.00)

Demographic Controls

	Birth Rate	Log Birth Rate	Abortion by State Of Occurrence	Abortion by State Of Residence
Treated*70-72	-2.390	-0.027	251.0	179.6
	(0.933)	(0.014)	(58.0)	(19.5)
Treated*74-76	0.199	0.008	213.8	172.8
	(1.368)	(0.022)	(50.2)	(37.9)
Treated*78-80	0.512	0.011	231.3	210.8
	(1.731)	(0.029)	(37.4)	(36.5)
Demographics?	Yes	Yes	Yes	Yes
Year Effects?	Yes	Yes	Yes	Yes
State Effects?	Yes	Yes	Yes	Yes
Cohort Effects?	Yes	Yes	Yes	Yes
F-test on excluded instruments (p-value)	7.55 (0.00)	6.17 (0.00)	33.47 (0.00)	42.99 (0.00)

Regressions Include State, Survey Year, and Cohort Fixed Effects
All regression weighted using MTF sample weights.

Table 4. Two Stage Least Squares Estimates of Effect of Changes in Birth Rate and Abortion Rate Induced by Early Abortion Legalization on Adolescent Substance Use

	Use Within Past Month:				
Regressor	Smoke	Drink	Marijuana	Any Illicit Drug	Any Illicit Drug Except Marijuana
Birthrate	0.008	0.013	0.011	0.014	0.012
	(0.006)	(0.007)	(0.004)	(0.005)	(0.004)
Log(Birthrate)	0.642	1.059	0.908	1.177	0.955
	(0.597)	(0.668)	(0.404)	(0.600)	(0.473)
Abortion Rate * 1000	-0.053	-0.065	-0.015	-0.046	-0.082
- *State of Occurrence*	(0.052)	(0.095)	(0.045)	(0.055)	(0.065)
Abortion Rate * 1000	-0.054	-0.033	0.034	-0.001	-0.080
- *State of Residence*	(0.075)	(0.109)	(0.061)	(0.074)	(0.082)

Each entry is from a separate regression and is the estimated TSLS estimate and standard error of the relationship between the indicated regressor and the particular measure of subject use. The excluded instruments in each regression are the three interaction terms from earlier tables, showing the "epoch" in which the state legalized abortions. Each regression includes the full set of time varying demographics, state, cohort of birth and survey year fixed effects shown earlier.
The Abortion Ratio is the number of abortions by women from a state per the number of live births to women from that state.
Standard errors allow for arbitrary clustering by state. Regressions are weighted using MTF sampling weights.

Figure 1. Fraction of 12th Graders Who Used Controlled Substance in Past Month