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Baby Booms and Drug Busts: Trends in Youth Drug Use in the United States, 1975-2000

Mireille Jacobson*

March 2002

Abstract

This paper explores trends in rates of youth marijuana use in the United States, in particular the increase in the 1990s that followed the secular decline of the 1980s. A strikingly large positive relationship exists between youth cohort size and rates (not just levels) of youth illicit drug use at the national level. This relationship also holds at the census division and block group levels. I explore various explanations for this phenomenon: scale economies in drug markets, strained monitoring resources, and intergenerational attitude transfers. I find that: (1) larger youth cohort size is related to lower marijuana prices, (2) larger youth cohort size is associated with lower drug sales arrest rates for both youth and the population more generally but lower possession arrest rates for youth alone and (3) although parental attitudes matter for youth marijuana use they cannot explain the impact of cohort size on rates of use. The size of the youth cohort affects the demand for illegal drugs by lowering the possession arrest probability, but the main effect of cohort size is on the supply of illegal drugs. The explanation most consistent with the observed patterns in the data – larger cohorts associated with higher rates of use, lower prices, and lower sales arrest rates – is that an increase in youth cohort size produces a thicker youth drug market that, through lower sales arrest risk and informational economies, generates cost-savings in illicit drug distribution.

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*Department of Health Care Policy, Harvard Medical School, jacobson@hcp.med.harvard.edu. I am extremely grateful to Claudia Goldin and Larry Katz for their suggestions throughout the research process. I also thank John Bound, David Cutler, Edward Glaeser, Nora Gordon, Michael Grossman, Caroline Hoxby, Steve Levitt, Jeff Miron, Sendhil Mullainathan, Karen Norberg, Gary Solon, all the participants of Harvard's labor seminar, and SAMHSA's lunchtime seminar series for their helpful comments and the Social Science Research Council and The Lindesmith Center for financial assistance. Thanks as well go to Chris Foote for providing cross-state birth rates, Jessica Wolpaw Reyes for providing many state-level covariates, and Karen Norberg for helping me get access to the restricted portions of the National Longitudinal Survey of Adolescent Health. All remaining mistakes are my own.

Why did teen drug use increase in the 1990s after declining precipitously in the 1980s? These trends are puzzling because the rate of adult drug use remained essentially flat or declined in the 1990s (depending on the precise age group considered) and rates of other risky behavior among youth, such as teen pregnancy and crime, rose in the mid-1980s and declined in the 1990s (Gruber 2001). And past work has shown that changes in background characteristics, such as race, sex, education, family structure, religious commitment, work behavior, and urbanicity, explain little of the time series trends in youth drug use (Bachman et al. 1998). In the work that follows I explore the role of changing demographic structure in accounting for these apparently anomalous trends.

Although government policies probably play a crucial role in determining the extent of youth drug use, cohort size seems to matter as well. Indeed, the rate of past month or past year marijuana use among high school seniors, between 1975 and 2000, closely tracks the population or cohort size of those 15 to 19 years old (see Figures 1 and 2). Why should the rate of use correlate so well with cohort size?

I consider several hypotheses – scale economies in drug markets, strained monitoring resources, and intergenerational attitude transfers. "Scale economies" suggests that due to fixed costs of illicit drug distribution, an increase in cohort size lowers the per unit costs of drugs, reducing drug prices and increasing use. "Strained resources," proposes that efforts to prevent youth drug use are overwhelmed when cohorts are large, reducing the risks of punishment and again increasing use. The final explanation considered here, "attitude transfer," suggests the link between cohort size and drug use merely reflects the baby boomers' bequeathing to their kids, the baby boomlet, a relative acceptance of illicit drug use. These hypotheses are discussed in greater detail after reviewing the basic trends in youth drug use.

I then show that the relationship between drug use and cohort size is not driven solely by national trends. Cohort size within a census division matters for rates of youth drug use even after controlling for aggregate time effects. Moreover, across census block groups, the lowest level of geography for which the Census Bureau publishes data and an area that in 1990 averaged 452 housing units or 1,110 people, cohort size is an important predictor of a teen's past month and lifetime marijuana use.

Once I have established that the relationship is not spurious, I try to distinguish among the possible explanations. I present a collage of evidence consistent with an important role for the scale economies hypothesis. I show that the price of commercial-grade marijuana, the variety typically smoked by casual users, is *lower* when youth cohorts are large. I also find that rates of drug sales arrests for all age-groups decline when youth cohorts are large, lowering the expected costs of drug dealing, and rates of possession arrests fall for youth alone, raising the net benefits of drug use for teens. Importantly, however, the size of the youth cohort is unrelated to arrest rates for other "youth crimes," such as larceny or vandalism. And, the size of the cohort 20 to 24 years old, another high crime demographic, has no detectable effect on drug arrest rates. These results suggest the relative importance of efficiency gains in the illicit drug trade over a general strain on police resources in explaining the relationship between cohort size and drug use. Finally, I show that, although a parent's attitudes helps predict her teenager's marijuana use, intergenerational attitude transfers account for little of the relationship between cohort size and drug use over time.

Taken together, these findings suggest that an increase in youth cohort size produces a thicker youth drug market, which, through a decline in the risk of engaging in the drug trade, informational economies, and so on, generates cost-savings in drug distribution. Cohort size also affects youth demand through changes in possession arrest probabilities but the supply effect dominates, as evidenced by the negative relationship between marijuana prices and cohort size.

The rest of this paper is organized as follows. Section 1 reviews trends in youth drug use, establishes the basic relationship between youth drug use and cohort size, and discusses potential explanations. Throughout this section, and the paper more generally, marijuana is the illicit drug considered because it is by far the most widely used illicit drug and is typically the first of any illicit substances used (with the exception of alcohol use by those below the legal drinking age). Section 2 shows that the link between drug use and cohort size is not spurious. Section 3 distinguishes among the explanations proposed for this surprising relationship and Section 4 concludes.

Section 1. Trends in Drug Use and Cohort Size

Nationally representative surveys of drug use in general and youth drug use in particular do not exist prior to the 1970s. Retrospective studies, however, suggest that the World War II birth cohort marks a major turning point in drug use (Johnson et al. 1996). Individuals born before 1940 were unlikely to have ever used an illicit substance, with less than 7% ever using marijuana by the time they reached 35. In contrast, roughly 12% of high school seniors reported using marijuana in the past month in 1992, at the trough of youth marijuana use over the past 25 years. By all indications, marijuana use, and illicit drug use more generally, rose throughout the late 1960s and 1970s.

The first year for which representative national data are available on youth illicit drug use is 1975. These data are from *Monitoring the Future*, (MTF), a survey of high school students that has interviewed seniors since its inception but only recently began interviewing 8th and 10th graders. MTF is the primary source of information on youth drug use in the United States (Johnston 2000), but because it is a school-based survey, it leaves out two groups at high risk of drug use – institutionalized (e.g., imprisoned or hospitalized) youth and high school dropouts. Because of these deficiencies, I also use the *National Household survey on Drug Abuse* (NHSDA). As its name suggests, the NHSDA is a household rather than school-based survey and has interviewed the non-institutionalized population aged 12 and over since 1971. But because NHSDA surveys were done erratically before 1990, they enable only a limited view of trends in drug use.[1] Taken together, however, MTF and NHSDA give a consistent story of casual marijuana use among teens aged 15 to 19 in the United States over the past two and a half decades.

As shown in Figure 1, MTF data show that past month marijuana use among high school seniors increased between 1975 and 1978, at the same time that drug use in general peaked in the United States. Between 1978 and 1992, past month marijuana use fell steadily, from a peak of just over 37% to a nadir of roughly 12%, and then rebounded considerably from 1992 to 1999 to over 23%. Figure 2, which shows reported past month marijuana use among 15 to 19 year olds in the NHSDA, tells a similar story for the 1979 to1998 period. In particular, it confirms the steep decline in rates of use over the 1980s and the rebound in the 1990s.

The time-series pattern of youth marijuana use is not driven by compositional shifts in the characteristics of youth. In fact, changes in background characteristics such

[1] The first survey was completed in 1971 but the earliest publicly available data is from 1979. Data is also available for 1982, 1985, 1988 and 1990-1999. See NHSDA (1998) for more information on this survey.

as race, sex, marital status, parental education, urbanicity, region of the country, income, employment status, and so on explain little of the variation in rates of youth marijuana use over time.[2] The absence of much explanatory power over time can be seen in Fig 3.

Figure 3 plots the year effects from a regression of past month marijuana use among MTF respondents on a constant and year dummies, both with and without a broad set of demographic covariates (see Appendix Table A1 for names of and coefficients on demographic covariates). The year effects represent the deviation or forecast error in a given year from the mean rate of past month marijuana use over the full sample period. As can be seen by the close correspondence between both sets of year effects, adding individual-level demographics does little to reduce the forecast error; in both cases, however, the year effects are highly significant.[3]

Although all three figures look at past month use, the trends (though not levels) are almost identical for past year or lifetime youth marijuana use. More interestingly, both Figures 1 and 2 show a surprisingly strong relationship between trends in youth marijuana use and cohort size. Rates of use follow a strikingly similar pattern to the population 15 to 19 years old – peaking with the baby boom, falling with the baby bust, and rebounding with the kids of the baby boomers or the baby boomlet. In other words, not only is the absolute number of users larger in big cohorts but the fraction as well.[4]

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[2] They do, however, help predict youth marijuana use. For example, as reported in Table A1, blacks are much less likely to use marijuana than whites, as are older students, those from rural communities, those going to school in the South or the West, those with more siblings, etc. Note that these conclusions do not always hold (and in some cases such as race are reversed) when intensity of use is considered.

[3] Similarly, comparing time series fluctuations in past month marijuana use to changes predicted by cross-sectional models based on the center of the sample period, an approach used by Gruber and Zinman (2001) to analyze smoking, explains little of either the decline in youth marijuana use in the 1980s or increase in the 1990 and confirms that changes in background characteristics cannot explain trends in youth drug use.

[4] The relationship between rates of youth marijuana use and cohort size is also found in Canada (see Ontario Student Drug Use Survey, 1977-1999). It is impossible to see if the relationship holds in other countries, however, as none, to the author's knowledge provides a time series of youth drug use behavior.

How might we understand the striking relationship between youth drug use and cohort size? Three explanations – economies of scale in illicit drug distribution, a resource squeeze, and intergenerational attitude transfers – are explored here.

The first explanation, economies of scale, involves fixed costs of illicit drug dealing. Dealers need to make connections with clients, establish safe pick-up and drop-off locations, and maintain viable financing arrangements. Moreover, the penalty structure for, and thus expected cost of, drug trafficking is highly non-linear with respect to quantity, even flattening out above a certain threshold. The existence of such fixed costs in distribution may generate price fluctuations in response to idiosyncratic changes in cohort size. Assuming a given fraction of youth users, a larger cohort means a larger number of youth users. Yet, the increase in output required to meet the increased demand should require a less than proportionate increase in resources needed to evade the authorities, deliver drugs, and so on. For example, a thicker market might provide a better network of information on where to "safely" buy and sell illicit drugs. Since youth are disproportionately used in street-level drug sales, a larger youth cohort might also mean lower search costs for "employers." The reduction in the unit cost of distributing illicit drugs would translate into lower prices, which feeds back to youth use.

An alternative explanation for higher drug use among larger youth cohorts is that larger cohorts strain society's resources for monitoring adolescents. As school resources are spread thin when cohorts are large (Poterba 1997), crowding may make it difficult for teachers to monitor students. Police may be less able to patrol neighborhoods and clamp down on drug trafficking to and among youth. And, the relatively fixed slots for incarceration may necessitate police turning a blind eye to the drug trade. By lowering the probability of getting caught such congestion would affect users, by raising the net benefits of consumption, or dealers, by lowering total supply costs.

The final possibility considered here is that baby boomers, the cohort with the highest known rates of drug use in U.S. history, passed on to their kids a relative acceptance of illicit drug use. I term this the "intergenerational attitude transfer" hypothesis. Indeed, it is the children of the baby boomers, those who reached adolescence in the 1990s, who are responsible for rising rates of drug use in the 1990s, after a decade of declines. Thus, the increase in rates of youth drug use may not be substantively related to the increase in cohort size but rather reflect parental attitudes towards drug use. Although appealing, I will show that this hypothesis cannot explain the relationship between drug use and cohort size.[5]

Figure 4, panels A and B offer simple, stylized illustrations of how the first two hypotheses, those implying a substantive relationship between youth drug use and cohort size, might work. D_0 represents the downward sloping demand for marijuana by youth at time t. Assuming stable individual youth demand for marijuana, an increase in the youth cohort, Y, leads to a shift out in market demand, from D_0 to D_1, by ΔY. As pictured in Panel A, if there are fixed costs to engaging in the illicit drug trade, aggregate supply, S, will be downward sloping, and the shift out in demand will increase marijuana use more than proportionately and decrease prices.[6] More specifically, marijuana use will increase

[5] Another possibility, not explicitly evaluated here, involves peer effects. Exposure to a drug-using peer, which increases in likelihood when cohorts are large, may have a multiplicative effect on teen drug use. Moreover, the relative size of a cohort may itself impact culture and thus the acceptance of youth drug use.
[6] Figure 4 implicitly assumes perfect competition. Although this assumption is made merely to simplify the picture, it constrains Panel A to depict external economies since economies of scale at the firm level are inconsistent with perfect competition at the industry level. The sources of economies discussed above are in some cases internal (e.g. the nonlinear penalty structure for drug dealing) and others external (e.g. search costs for recruiting dealers). Little is known, however, about the precise economies in and structure of the illicit drug market.

by $(\Delta Y + \alpha\Delta P)$, where α is the slope of demand curve, and ΔP, the change in the money price of marijuana. Alternatively, Panel B shows a standard upward sloping supply curve with the shift out in demand by ΔY. With this shift in demand alone, use will increase less than proportionately to the increase in cohort size (or proportionately if supply is perfectly elastic). A decrease in arrest risk for dealers caused by congestion would lead to a shift down in supply from S_0 to S_1, which, if it exceeds the shift out in demand, will lead to higher use rates and lower prices. A decrease in arrest risk for users would raise the net benefits of consumption, shift demand out further (not pictured here) and, with stable supply and a sufficiently large shift, raise both prices and use. A combination of shifts out in demand and down in supply could also generate an increase in use rates, with the effect on prices depending on the relative shifts of supply and demand. The relevance of each explanation is an empirical issue to be sorted out in the work that follows.

Section 2. Robustness Checks on the Drug Use-Cohort Size Relationship

2a. Within Census Divisions

Before considering these explanations, however, it is important to confirm that the relationship between cohort size and drug use exists at more disaggregated levels. With the exception of the impact of the baby boom, which was a national phenomenon, the other explanations concern the effects of cohort size on drug use at a local level, possibly at the level of a town, neighborhood, or even school.

Time-series data on drug use at such local levels are extremely difficult, if not impossible, to find. Although MTF data provide the longest, most consistent time-series of youth marijuana use, the public-use version allows identification only at the (four) census region levels. NHSDA does not provide as long a series but identifies respondents at the (nine) census division levels. Thus, I merge estimates of the youth share of the population by division-year, based on the Current Population Surveys, with the respondent-level NHSDA data from 1979 to 1997.[7]

To determine the effect of idiosyncratic changes in cohort size on drug use, I use a simple linear probability model, regressing an indicator of past month or past year marijuana use, d_{igt}, among respondents ages 15 to 19 in year t and division g on the share of the population 15 to19 years old in that division and year, sh_{gt}, the unemployment rate in that division and year, ur_{gt}, some basic demographic characteristics, X_{igt}, such as age dummies (or age and age-squared for adults), sex and race (5 categories) and division and year fixed effects, α_g and δ_t, respectively:

$$\Pr(d_{igt}=1) = X_{igt}\beta + \log(sh_{gt})\gamma + ur_{gt}\pi + \alpha_g + \delta_t + \varepsilon_{igt} \qquad (1)$$

(See Table A2 for descriptive statistics). I use the youth share rather than the absolute population 15 to 19 years old so as not to overweight large areas, although the results are not sensitive to this specification.[8] I express the youth share in logs for ease of interpretation of later regressions, although this choice, as well, has no impact on the general conclusions drawn and only a minor effect on the implied elasticities (see Table A3 for a comparison of the log versus levels specification). An individual-specific error term, ε_{igt}, captures other sources of variation in drug use that are orthogonal to the youth share of the population.[9]

Using division and year fixed effects enables me to separate coincidental national

[7] Although the NHSDA is available through 1999, I use the 1979-1999 surveys to avoid inconsistencies due to changes in the coding of geographic divisions after 1997.
[8] As shown in Table A2, the youth share ranges from 9.8% in the late 1970s to 6.0% in the early 1990s. The trends are similar across divisions with only the precise years and levels of peaks and troughs differing.
[9] The linear probability model (LPM) is used for ease of interpretation; using probit or logit models lead to similar conclusions. Moreover, all estimated probabilities from the LPM lie between 0 and 1. All standard errors are cluster-adjusted to correct for correlation at the division-year level.

trends as well as cross-sectional differences or heterogeneity in drug use and demographics across regions from the true effect of changes in the youth share of the population on rates of youth drug use within a region over time.[10] Note that the effect of the log (sh_{gt}) on drug use, γ, can be estimated only to the extent that it is not predicted by the year and division fixed effects. Since a regression of log (sh_{gt}) on year and division fixed effects explains about 91.9% of the variation in log (sh_{gt}) over the NHSDA sample period, only the remaining 8.1% of the variation can be used to estimate γ. Using 12 years of individual-level drug use data, however, allows me to obtain reasonably precise estimates. Including division-year unemployment rates allows me to control for any effect of regional economic conditions on substance use.[11]

Table 1A shows the effect of a 1% increase in the youth share of the population on the probability of past month (Panel A) or year use (Panel B) among 15 to 19 year olds. For comparison purposes, Tables 1B and 1C show the effect on cigarette and alcohol consumption over the same intervals. For further comparison, the effect on past month and past year marijuana, cigarette, and alcohol use among those 30 years and older is also included. Col. (1) and col. (2) in each panel show the results from the basic regression without year fixed effects, not controlling for national trends. All remaining columns include year fixed effects and col. (3) and col. (6) of each table also control for the division-year unemployment rate. All standard errors are clustered at the division-year level to correct for correlation imposed because the youth share of the population

[10] Indeed rates of drug use vary considerably across divisions, with the Pacific typically having the highest and the South the lowest rates of youth use (See NHSDA 1999). Since the youth share is relatively high in Southern states, cross-sectional estimates alone would associate big youth cohorts with low rates of use.
[11] As Ruhm (2000) shows, mortality (and smoking and drinking, which contribute to it) is procyclical. The coefficients on unemployment rates in these regressions, however, suggest that while economic conditions impact adult use (particularly of marijuana and alcohol), their impact on youth use is more ambiguous.

varies at the division-year level while the dependent variable is at the respondent level. As a sensitivity check, standard errors are clustered by division (shown in brackets) to account for division-level serial correlation (Bertrand et al. 2002).

In the case of youth, the relationship between marijuana use and the division-year youth share of the population is positive and highly significant even after taking out aggregate year effects, controlling for fixed differences across geographic divisions, and controlling for the respondent's basic demographic characteristics.[12]

In contrast, the impact of the youth share of the population on *adult* drug use is imprecisely estimated and, although positive, the coefficients imply considerably smaller effects. For neither youth nor adults does the youth share have any explanatory power in the probability of past month or year cigarette use once national trends are taken out. As in the case of marijuana, however, the youth share is positively and significantly related to past month and year alcohol use for youth, but not for adults.[13]

More specifically, the results from Table 1A imply that a 10% increase in the youth share of the population *within a division* leads to an almost 4 percentage point increase in the probability of past month marijuana use, suggesting an elasticity of about 3 evaluated at the sample mean of past month marijuana use. Similarly a 10% increase in the youth share of the population within a division leads to a 4.4 percentage point increase in the probability of past year marijuana use, suggesting an elasticity of about 2, again evaluated at the sample mean of the dependent variable. Changes in the youth share of the population therefore appear to have a greater impact on current drug use.

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[12] Although clustering the standard errors at the division-level to adjust for correlation both within a division and over time lowers the levels of significance from 1% to 5% (or raises p-values from about .001 to .03), this correction does not alter the youth marijuana use conclusions.

[13] The youth share is significantly related to past month adult alcohol use at the 10% level but this result is not robust (e.g., see IV results, Table 2).

The coefficients on log share of the population imply elasticities of about 2 and 1 for past month and year adult marijuana use, evaluated at each sample mean. If the youth share has a truly independent effect on adult marijuana use, then a resource strain story, at least at the school level, would appear questionable. Complementarities between youth and adult drug use, or a link between the added stress of a large youth cohort and adult alcohol and marijuana use, however, could give this theory some validity.

An economies of scale story, with increases in the youth share leading to lower illicit drug prices, could spill over to adult marijuana use. The effect of the youth share of the population on past month and year alcohol use in both youth and adults could also be interpreted as evidence of complimentarity between marijuana and alcohol use.[14]

The implications of these results may be speculative, but the results themselves are clear and robust. What can be concluded with relative certainty from Table 1a is that national trends alone cannot account for the observed relationship between drug use and cohort size. Rather, idiosyncratic changes in youth share at the census division level actually *affect* rates of drug use.

In light of Shimer (2001), which shows that an increase in the youth share of the population is related positively to aggregate youth unemployment rates but negatively to state or division rates, the similarity between the time-series and panel evidence presented here suggests that the relationship between youth cohort size and drug use cannot simply be explained by economic booms. More directly, controlling for annual division-level unemployment rates leaves the effect of the youth share of the population

[14] The evidence for this in the literature is mixed. DiNardo and Lemieux (2001) find that youth substitute towards marijuana in response to increases in the legal drinking age and that individuals living in states that decriminalize marijuana are less likely to use alcohol. In contrast, Pacula (1998a and 1998b) and Farrelly (1999) find that higher beer prices are associated with reduced levels of both drinking and using marijuana.

on youth marijuana use essentially unchanged. In other words, the relationship between the youth share and youth marijuana use is independent of any effect the youth share has on local economic conditions.

Another possibility for the observed relationship is that families may flock to regions with relatively high rates of use for reasons unobserved in the data but correlated with higher rates of youth marijuana use. For example, areas that are growing faster than usual may devote relatively more resources to education than to enforcing the marijuana laws. If so, the estimated relationship between the youth share and rates of youth drug would not be causal. To identify the causality of the observed relationship, I adopt an instrumental variables approach, similar to Shimer (2001), that uses the birth rates in a division 15 to 19 years earlier to capture exogenous variation in the youth share of the population as opposed to that due to migration.[15]

As noted in Shimer (2001), the sum of births per 1000 persons in a given state 15 to 19 years earlier is a good predictor of the youth share in that state. Consequently, the same is true at the division-year level. Regressing log (sh_{gt}) on log($birth_{gt}$), year and division fixed effects yields an elasticity of 0.652, standard error of 0.049, and R^2 of 0.987. Table 2 shows the effect of cohort size on drug use when instrumenting for the youth share with birth rates. For adults, the precision of the estimates falls considerably and the magnitude of the effects is slightly reduced. In sharp contrast, the effect of the youth share on youth marijuana use is still fairly precisely estimated and of similar, although again slightly reduced, magnitude. The implied elasticities for past month and year youth marijuana use are 2.6 and 1.6 respectively.

[15] Chris Foote generously provided state-level birth rates, which he obtained from Robert Shimer, who constructed them from the *Statistical Abstract of the United States*.

The probability of past month or year youth cigarette use is now negatively related to cohort size, although again too imprecisely estimated to distinguish from zero. The magnitude of the effect on alcohol is significantly reduced in the case of past month use and relatively imprecisely estimated for both past month and past year use.

In short, the IV estimates are consistent with a causal relationship between the youth share of the population and youth rates of marijuana use. The relationship is not driven solely by aggregate trends. Moreover, the relationship appears to be particular to youth (in contrast to adults) and to illicit drugs (in contrast to alcohol or tobacco).[16]

2b. Across Schools and Census Block Groups

To probe the relationship between youth drug use and cohort size further, I use restricted data from the National Longitudinal Survey of Adolescent Health (Add Health). Add Health is a nationally representative survey of high school students (7th through 12th graders) from 134 schools in 80 communities.[17] It questions respondents about a broad array of risky behaviors, from cigarette use to suicidal tendencies and, to allow for analysis of community impact on such behavior, links these data to the 1990 Census of Population and Housing. The rich set of information Add Health provides on the respondent's school and community in addition to the respondent and her family helps control for heterogeneity across areas. But because these data are non-experimental and cross-sectional, the Add Health results should be treated as suggestive and not meant

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[16] As a further robustness check, I compare the effect of other demographic groups – the share 5 to14 years old, 20 to 24 years old, and the share 65 and older – on drug use. Table A4, which reports these results for both youth and adult drug use, suggests that an increase in the share of the population 5 to 14 years old or 20 to 24 has no statistically significant impact on youth marijuana use rates. Moreover, controlling for the share 5 to14 years old, 20 to 24, or 65 and older does not alter the relationship between cohort size and youth marijuana use. That the share of 20 to 24 year olds, another high crime demographic, has no or even a negative effect on rates of youth marijuana use and its own use (not shown here), suggests that burdened police resources may not be the key to the relationship between cohort size and drug use.

[17] For a thorough discussion of Add Health and many examples of its possible uses, see Jessor (1998).

to imply causal relationships.[18]

I run a linear probability model of any lifetime or past month marijuana use on a respondent's demographics (including information about her parents demographics and attitudes towards drug use), X_i, the log of the respondent's school size, SCH_i, the log share of 15 to 19 year olds in the respondent's census block group, sh_{ib}, and in her census tract, sh_{iT}, where i indexes individuals, b block groups, and T tracts (see Table A5 for means).[19] For lifetime or past month marijuana use, the regression run is as follows:

$$\Pr(d_{ib_T}=1) = X_i\beta + SCH_i\lambda + \log(sh_{ib})\gamma + \log(sh_{i_T})\eta + \varepsilon_{ib_T} \qquad (2)$$

Standard errors are cluster-adjusted at the census tract level.

Table 3 presents results for participation in marijuana, cigarette, and inhalant use (glues and solvents). Inhalants are of particular interest because, unlike marijuana or cigarettes, they can be bought legally by minors and are often found in the home. Table 4 considers a highly distinct, though not uncorrelated, set of "behaviors" – serious thoughts of committing suicide in the past year, suicidal attempts in the past year, and medically treated attempts in the past year.

School size is related positively to teen marijuana use. A 10% increase in school size is associated with a 0.3 percentage point increase in the probability of lifetime use,

[18] The survey is technically longitudinal but the attrition rate of 25% between waves seriously weakens any results. Moreover, since the waves are only a few years apart the contextual variables from the census are not updated. Consequently, the present analysis uses information from first wave of the survey only.
[19] The demographics used are age, age-squared, sex, race (5 categories), a Hispanic indicator, an indicator for born in the US, grade in school, number of siblings in the household and its square, employment status in the past month, and the importance of religion. Indicators for the respondent's twin status, male-male, and female-female twin status are also included because of evidence of protective effects of the first factor and partial counteracting effects of the last two. This is the subject of ongoing research. I also include the following parental information: education, log of income, unemployment status, and food stamp recipiency; and the following locational attributes: the median age in the census tract, the tract-level unemployment rate, the county non-marital fertility rate, the county non-marital birth rate for 15 to 19 year olds, log of total serious crimes per 100,000 in the county, log of total serious juvenile crimes per 100,000, log of per capita spending on police by local government, and the proportion of local spending going to police.

implying an elasticity of about 0.1 evaluated at the sample means of the data. This result suggests that congestion at the school level, possibly allowing drug dealers and buyers in the area to act with relative impunity, may help explain the relationship between drug use and cohort size.[20] On the other hand, the importance of peer effects is suggested by the positive and significant relationship between school size and medically treated suicide attempts, a measure of the gravity of suicidal tendencies (Cutler et al. 2001).

The youth share in a block group is also positively related to marijuana use. A 10% increase in the share of 15 to 19 year olds in a block group raises the probability of both lifetime and past month use by almost 0.5 percentage points, implying elasticities of about 0.2 and 0.3. In contrast, the share in a census tract, an area that typically contains four to five block groups and had roughly 2,500 to 8,000 people in 1990, has a small but statistically significant negative effect on marijuana use, implying elasticities of about -0.03 for past month and -0.05 for lifetime use, evaluated at the sample means of the data.

The distinction between the block group and the census tract findings coupled with the school size findings suggest that a teen's decision to use marijuana may be most directly affected by youth in the immediate vicinity. Moreover, together with the census division results, this suggestive evidence on school size and block groups reinforces the possibility of a substantive relationship between youth drug use and cohort size.

Section 3. Distinguishing Among Competing Explanations

3a. Marijuana Prices and the Importance of Economies of Scale

One explanation for the relationship between youth marijuana use and cohort size

[20] Sibling size also warrants discussion. Having more siblings lowers the probability of drug use. This may reflect the type of families with more kids (e.g. religious or poor) or may protect against boredom, a prime motive for youth drug use (Glassner and Loughlin 1987). Alternatively, larger families may have fewer resources per child and their kids less spending money for drugs. This is a topic of ongoing research.

is that increases in cohort size lead to thicker markets and lower the delivered price of the drug. For example, when the population of youth in an area increases, drug dealers may find it worthwhile to make the fixed investment in setting up a local supply network, effectively lowering the marginal cost of illicit drugs in the area. A testable implication of this hypothesis is that marijuana prices fall when youth cohort size increases. I take advantage of two different data sources, each with its own drawbacks, to generate three distinct marijuana price series.

The first source of data is *High Times*, a marijuana "fanzine" that has been published monthly since 1975. In each issue, contributors write in to the "Trans High Market Quotations" (THMQ) section with descriptions and prices per ounce of the marijuana available in their part of the country. For a given month and year, a typical observation lists a contributor's state, describes the marijuana available according to source country or state (Mexico, Colombia, Jamaica, California, Hawaii, etc.) and quality (commercial grade, sensimilla, etc.), and finally gives a price. Ideally one would like to deflate these prices by a measure of potency but such information is unavailable. I take a second best approach, identifying price observations that fit one of two categories: low quality or commercial grade Colombian and Mexican "weed" and high quality or Californian and Hawaiian sensimilla. For tractability, within each quality category, I follow the ten most commonly represented states: Alaska, California, Georgia, Hawaii, Michigan, Missouri, New York, Oregon, Tennessee, and Texas.

The second source of price data is the Drug Enforcement Administration's (DEA) System to Retrieve Information from Drug Evidence (STRIDE), which records purchases and seizures of illegal drugs made by undercover DEA agents and informants as well as by the Metropolitan Police of the District of Columbia. A typical observation in STRIDE reports the type of drug acquired, the weight and purity of the drug, the city where it was acquired, the date the "transaction" occurred, and, if the drug was purchased, the price paid. These data are also available from 1974 onwards.[21]

Because the DEA focuses their efforts on harder drugs, however, marijuana observations are only a small fraction of cocaine or heroin purchases (roughly 6,500 compared with 90,000 for cocaine and 50,000 for heroin over the period 1974 to 2000). In fact, over 40% of the marijuana observations in STRIDE are from the District of Columbia Metropolitan Police. The next most highly represented state, Texas, contributes less than 7% of the marijuana observations. In addition, less than 1/10 of the marijuana observations include purity estimates.[22] Since anecdotal evidence suggests there have been significant increases in purity over the past 25 years (Harrison et al. 1996) and STRIDE, unlike THMQ, gives no other indication of quality or potency, these price data are far from ideal. Nonetheless, I use them for comparative purposes.

Figure 5 plots median prices per gram in 1999 dollars for the low quality marijuana in THMQ and all marijuana observations in STRIDE from 1975-1999. For comparison, it also shows the population 15 to 19 years old in thousands. The high quality marijuana price series is omitted because it is even noisier than these two and greatly obscures the figure. Moreover, the low quality category is most relevant for the present analysis as it is the type of marijuana most likely used by casual users. As can be seen in Table 5, a gram of sensimilla (high quality) marijuana is almost 3 times as

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[21] STRIDE data were first recorded in 1970, but there are few observations before 1974. See Frank (1987) for a thorough discussion of STRIDE.
[22] Pacula et al. (2001) uses secondary DEA sources to generate marijuana prices for 1981-1998. These data are quite crude, however, with prices given in broad ranges and purity available only at the national level.

expensive as a gram of commercial (low quality) marijuana. Figure 6 shows the same price series but plots rates of past month marijuana use among high school seniors (from MTF) for comparative purposes.

In the aggregate, both the THMQ low quality and STRIDE marijuana price series are negatively related to the population 15 to 19 years old and thus rates of past month marijuana use among this age group. In other words, marijuana prices are low when cohort size is large and rates of use are high. Judging from the THMQ series, the increase in marijuana prices in the 1980s followed the reduction in cohort size while the decrease in the 1990s was contemporaneous with the increase in cohort size. In contrast, the STRIDE data suggest that the increase in marijuana prices may have began in the mid 1970s, before the decrease in cohort size, whereas the decrease in the 1990s occurred after the initial increases in cohort size. These inconsistencies, however, may be related to the different composition of states in each series.

To better interpret the relationship among marijuana use, prices, and cohort size, I would like to supplement the basic marijuana use regressions in equation (1) with marijuana prices. The goal would be to determine whether the relationship between drug use and cohort size works exclusively through prices.[23] Due to data limitations, I instead run a simple panel regression of the log of marijuana prices per gram in state, s, and year, t, on the log of the youth share, sh_{st}, the annual state unemployment rate, ur_{st}, to capture state economic conditions, state and year fixed effects, α_s and δ_t, and an error ε_{st} term:

$$\log(\text{price per gram}_{st}) = \log(sh_{st})\gamma + ur_{st}\pi + \alpha_s + \delta_t + \varepsilon_{st} \qquad (3)$$

The regression is run separately for each price series – THMQ low quality, THMQ high

[23] Since prices are only available for 10 states and neither NHSDA nor MTF provides state-identifiers, such analysis is currently infeasible. Restricted access to MTF state-identifiers will help remedy this problem.

quality, and STRIDE from 1975 to 2000. For the STRIDE series, I can generate a balanced panel for only fives states – California, the District of Columbia, Florida, New York and Texas. For the high quality THMQ regressions, all 10 states – Alaska, California, Georgia, Hawaii, Michigan, Missouri, New York, Oregon, Tennessee, and Texas – are included. The low quality regressions omit Alaska and Hawaii because of significant numbers of missing observations. Standard errors are again clustered by state.

Table 6, Panel A gives the OLS results from these basic regressions and Panel B shows the results when instrumenting for the youth share of the population with lagged birth rates. Although both the high quality THMQ and the STRIDE estimates are too imprecisely estimated to draw any conclusions, the low quality THMQ prices are clearly negatively related to cohort size. In particular, they suggest an elasticity of about -2 with respect to the share of the population 15 to 19 years old.

Table 7 tries to get at the differences between the effect of youth share on low and high quality marijuana prices. In addition to the share of the population 15 to 19 years old, I also include the share of 20 to 24 year olds in the state because high quality marijuana users tend to be older. Both the OLS and IV results, which use lagged birth rates to instrument for the share of the population between 15 and 19 years old and 20 and 24, reveal that the teen cohort continues to have a negative effect (of similar magnitude) on the low quality marijuana prices. In contrast, the share of 20 to 24 year olds has a negative effect on high quality, but not low quality, marijuana prices.

Tables 6 and 7 provide perhaps the strongest evidence for the economies of scale hypothesis. Why should a larger youth cohort lead to lower marijuana prices, particularly given the higher rates of use associated with larger cohorts? Clearly supply-side factors such as economies of scale must dominate for use and prices to move in opposite directions. The additional finding that high-quality marijuana prices are negatively related to the size of the slightly older cohort suggests more generally that marijuana prices reflect the size of the target group of consumers. Thicker markets may generate cost-savings in distribution, translating into lower prices and higher rates of use.[24]

3b. Drug Arrests Rates and the Role of Strained Police Resources

A complimentary explanation to that of scale economies concerns the possible strain a larger youth cohort places on police resources. To probe the role of strained resources, I turn to drug arrest data. I look at sales and possession offenses separately to get a sense of the differential impact on the supply and demand sides of the market.[25]

The impact of the youth share on youth marijuana arrest rates is a priori ambiguous. Offense rates presumably parallel use rates. Thus, to the extent that the police can keep up with these trends, youth marijuana arrest rates, particularly for possession, should follow use rates and cohort size. If law enforcement resources are strained, however, arrests could increase less than proportionately. They might even decrease if a more efficient illicit drug market emerges.

To assess these possibilities, I use data from the Uniform Crime Reporting (UCR) program's "Arrest Reports by Age, Sex and Race for Police Agencies in Metropolitan Statistical Areas." Table 8 gives means for 1976-97 of marijuana and all drug arrests rates (sale or possession arrests among the specified group as a fraction of those in the

[24] This finding is consistent with Pacula et al. (2001), which suggests that marijuana prices and potency explain much of the trend in youth marijuana use, but goes a step further by establishing a mechanism behind the trends in both prices and use rates.

[25] The distinction between sales and possession may be somewhat artificial, however, because it is based on the amount of a substance found on an offender. Those charged with drug sales offenses, however, are almost certainly involved in distribution at a high level. Thus, the evidence on sales arrest rates provides the clearest picture of what is happening on the supply-side of the drug market.

group) aggregated up to the division-year level.[26] The "risk" of arrest for marijuana sales or possession is about 8 times greater for 15 to19 year olds than for those 30 years and older, about 8 times greater for sales and over 10 times greater for possession. In addition, and in contrast to adults, youth drug arrests occur disproportionately in the marijuana (rather than cocaine or heroin) trade.

Table 9A looks at the effect of changes in the youth population share on marijuana sales and possession arrest rates of youth and adults separately as well as for the entire population. The basic set-up is similar to the drug use regressions in (1) except the dependent variable is at the division-year level. More specifically, I run a regression of the log of the arrest rate in division g at time t, a_{gt}, by age group (youth, adults, and all ages) and offense (sales or possession) on the log of the share of the population 15 to 19 years old in that division and year, sh_{gt}, the annual division unemployment rate, ur_{gt}, to capture regional economic conditions, division and year fixed effects, α_g and δ_t, and an error ε_{gt} term to capture variation in drug arrest rates that is orthogonal to the youth share:

$$\log(a_{gt}, \text{by age group and offense}) = \log(sh_{gt})\gamma + ur_{gt}\pi + \alpha_g + \delta_t + \varepsilon_g \qquad (4)$$

To account for the fact that even unanticipated increases in arrest rates dissipate gradually over time, at least in part because of the somewhat artificiality of year intervals, standard errors are cluster-adjusted at the division-level.

The OLS results (Panel A) for youth marijuana sales arrest rates are negative but insignificant at conventional levels. In contrast, the effect on youth drug possession arrest rates is negative and significant, implying that a 1% increase in the youth share leads to a 1.5% decrease in drug possession arrest rates. The elasticities for total sales

[26] UCR data have been collected since 1960 but reporting was spotty until 1976, when local police were required to submit data. See Schneider and Wiersema (1990) for a discussion of the limits of UCR data.

and possession and adult possession arrest rates are negative but imprecisely estimated.

Panel B, which uses 15 to 19 year lagged birth rates to instrument for the youth share, raises the magnitude and significance of the estimated elasticity of youth sales arrests: a 1% increase in the youth share of the population leads to a 2.5% decrease in youth marijuana sales arrest rates.[27] In addition, it confirms the OLS estimate of −1.5 for the elasticity of youth marijuana possession rates. Since a larger youth cohort also leads to higher rates of youth drug use, these estimates understate the change in marijuana arrest risk for youth. Using the IV estimate of the elasticity of past year youth marijuana use rates with respect to the youth share of the population (1.6), a 10% increase in the youth share translates into a roughly 4% reduction in youth sales arrests per drug user and 2.7% reduction in possession arrests per user.

To get closer to the level of a drug market, I rerun the IV regressions with the log of arrest rates by *state*, *s* and year, *t*, on the log of the youth share of the population, sh_{st}[28]:

$$\log(a_{st}\text{, by age group and offense}) = \log(sh_{st})\gamma + ur_{st}\pi + X_{st}\beta + \alpha_s + \delta_{g(s),t} + \varepsilon_{st} \qquad (5)$$

I also include the unemployment rate by state and year, ur_{st}, state fixed effects, α_s, and division-year fixed effects, $\delta_{g(s),t}$, to pick up regional fluctuations in arrest rates, and a set of state characteristics, X_{st}, including the log of prisoners per capita and the log of police per capita to capture law enforcement intensity (both are lagged one year to minimize endogeneity between crime rates and enforcement), the log of state income per capita (in 1997 dollars) as an additional measure of state economic conditions, and a dummy for the

[27] The instrumental variables approach is particularly useful here because it gets around the division bias imposed by regressing youth drug arrests per 100,000 15 to 19 year olds on the share of 15 to 19 year olds.
[28] Using the state by year specification increases the number of observations from 198 to 858. Due to significant missing observations, data from AK, DE, FL, HI, IL, KS, MT, NH, SD, VT and WY are not included in the state or division level regressions. For the division-level analysis, these states were also excluded from the youth share figures.

presence of a concealed handgun law to control for the relative ease of owning a gun on crime. For comparison, I also consider arrest rates for all illicit drug sales and possession offenses (Panel B, Table 9B) as well as for larceny and vandalism (Panel C, Table 9B), two arrest categories that, according to Table 8, are also dominated by 15 to 19 year olds.

As shown in Table 9B, moving to the state-level, but controlling for regional fluctuations, significantly raises the precision and magnitude of the estimated elasticities of sales arrests with respect to youth share. For youth sales, the elasticity is roughly -4.4. More importantly, the coefficient on total marijuana sales arrests rates suggests an elasticity of -4. The results for total drug sales are remarkably similar.[29] That all drug sales arrest rates fall in response to an increase in the youth share suggests that something is occurring on the supply-side of the market – a squeeze on police resources, a more efficiently operating drug trade, a greater acceptance of youth culture. Panel C of Table 9B, which shows that a larger youth cohort is associated with higher arrest rates for larceny, confirms my basic conclusion: something particular to illicit drug markets is driving the negative relationship between cohort size and drug sales arrest rates.

The fall in total drug sales arrest rates, despite the inclusion of 20 to 24 year olds, a relatively high-crime demographic excluded from both the youth and adult groups, also suggests that the impact of the youth share on police resources is not purely compositional. In other words, police, perhaps exasperated by the relative number of youth in their area, do not shift their resources to monitor the drug involvement of other high-crime age groups. Rather, an increase in the youth share at the division-level is

[29] The results are also similar (not shown here) without controls for law enforcement intensity, handgun laws, or local economic conditions, suggesting these covariates are not driving the relationship between cohort size and marijuana arrest rates. Similarly, using year rather than division-year fixed effects has little effect on the magnitude or precision of the results, suggesting regional fluctuations do not drive the results.

associated with an effective decrease in enforcement of the drug laws or a greater ability of illicit drug sellers to evade the authorities. This can be seen more clearly in Table 9C, which adds to (5) the share of 20 to 24 year olds in state *s* and year *t*. Although an increase in 20 to 24 year olds may also overwhelm the police, it does little to alter the relationship between the youth share and either youth or total sales arrest rates. In short, a strain on police resources may not capture the full effect of cohort size on use rates.

Ethnographic studies of drug markets suggest another reason an increase in the youth share of a population could benefit illicit drug suppliers. In the 1970s, criminal penalties became more severe for adults or more lenient for juveniles. In consequence teenagers were explicitly recruited for and employed in street-level drug sales (Padilla 1992). Thus, an increase in the youth share of the population may offer "employers" a bigger source of talented illicit drug dealers, effectively lowering search costs and the unit costs of illicit drug dealing.

Possession arrest rates display a different pattern. Increases in youth cohort size are associated with declines in possession arrest rates for youth but increases for adults. In other words, a larger youth cohort raises the net benefit of consumption for youth, through a decline in the expected costs of using illicit drugs, but lowers the net benefit for adults. The differential effect of the youth share of the population on adult (versus youth) possession arrest risk may explain why, despite the decline in marijuana prices, increases in youth cohort size have little effect on rates of adult marijuana use. Alternatively, adults may simply be less sensitive to these price changes.

3c. Intergenerational Attitude Transfers

The impact of parental attitudes on teen preferences for marijuana may be another factor leading to the relationship between cohort size and drug use. This hypothesis, which links the high rates of drug use among baby boomers and their kids, implies a shift out in demand concurrent with the increase in cohort size in the 1990s. Given the evidence on cohort size and marijuana prices, it can only be part of the full explanation.

To test the importance of parental attitudes, I again turn to the Add Health survey estimates from equation (2), shown in Tables 3 and 4. Although Add Health does not ask parents about their own illicit drug use, they do ask whether low teen drug use was an important factor in neighborhood choice.[30] Since parents are also asked about the extent of drug dealing and use in the neighborhood, I am able to capture how parental attitudes, not just the availability of illicit drugs, impacts their children's behavior (see Table A6 for the exact questions used). In addition, I also consider the impact of a parent's smoking, and heavy drinking on her child's lifetime and past month marijuana, cigarette, and inhalant use, and year suicidal tendencies. The questions on parental smoking and drinking may be especially important because parental behavior strongly affects directly related youth behaviors (see Case and Katz 1991).

Table 3 presents the results for participation in marijuana, cigarette, and inhalant use and Table 4 for suicidal tendencies. As expected, respondents whose parent's say they chose their residence partly because of low teen drug use in the area are less likely to participate in marijuana use, even when controlling for a parent's assessment of drug availability. The other measures of parental attitudes – their own drinking or smoking – confirm this story. A teen whose parent admits to having five or more drinks at a time at least weekly over the past month has a roughly .1 percentage point higher probability of

[30]The survey goes to great lengths to ensure that the parent surveyed is the adult female in the family. In over 85% of cases, the teen's biological mother is interviewed; another 5% are step or adoptive mothers.

having ever used marijuana. A teen whose parent smokes is similarly affected.

A parent's cigarette and alcohol use is also positively related to her children's use of inhalants or cigarettes. In contrast, as shown in Table 4, they have no clear effect on suicidal tendencies. For example, a parent's smoking status is positively related to her child's admission of suicidal thoughts but has a negative, though insignificant, impact on medically treated attempts. Whereas little can be said about the relationship between parental views on drug use and teen suicidal tendencies, it is unambiguously related to drug use, providing evidence in support of the intergenerational attitude transfer story.

These Add Health results, however, do not test the impact of parental attitudes on changes in drug use over time. To do so, I would need to supplement the NHSDA regressions of past month and past year substance use among teens in equation (1) with measures of their parent's substance use. Since the public-use NHSDA does not provide family identifiers, I use the fraction of 37 to 55 years olds within a division and year, who have ever used marijuana, $MJ_{37\text{-}55gt}$, cigarettes, $Cig_{37\text{-}55gt}$, or alcohol, $Alc_{37\text{-}55gt}$, to proxy for a respondent's parent's use:

$$\Pr(d_{igt}=1) = X_{igt}\beta + \log(sh_{gt})\gamma + MJ_{37\text{-}55gt}\eta + Alc_{37\text{-}55gt}\phi + Cig_{37\text{-}55gt}\chi + ur_{gt}\pi + \alpha_g + \delta_t + \varepsilon_{igt} \qquad (6)$$

I also use 15 to 19 year lagged birth rates to instrument for the youth share.

Table 10 shows that higher rates of lifetime marijuana use among 37 to 55 year olds (34.8%) within a division over time translate into higher probability of past month or year marijuana use among teens in the same region. Evaluated at the sample means of the data, these estimates suggest that a 10% increase in rates of lifetime marijuana use among those likely to currently have teenage children is associated with a 9% increase in past month marijuana use among teens and a 5% increase in past year use. Moreover, marijuana experience among 37 to 55 year olds is not only associated with higher marijuana use among teens but also higher cigarette and alcohol use.

Despite the strong link between a teen's marijuana use and the fraction of 37 to 55 year olds who have ever used marijuana in the same division and year, the estimated impact of the youth share on either past month or year marijuana use is little different from the basic instrumental variables estimates in Table 2. In other words, these results, which were previewed by the limited effect of youth cohort size on adult marijuana use (col. 4, Panels A and B in Table 2), suggest that intergenerational attitude transfers explain little of the relationship between cohort size and youth drug use.[31]

3d. Adding it All Up

Can the relationship between youth drug use and cohort size be entirely explained by economies of scale and a reduction in the probability of drug sales arrest? Since any given youth is a price-taker, supply-side cost-savings can be depicted as a downward shift in perfectly elastic supply to an individual youth (see Appendix B, Panel A for details). Assuming that individual demand is stable, this shift in supply to an individual youth caused by the change in cohort size allows us to back out a price elasticity of youth demand. In particular, the IV estimates of the elasticity of past year marijuana use with respect to youth share (1.6) and annual marijuana prices with respect to youth share (-1.88) imply a price elasticity of -0.85. If we further assume that long-run supply is stable and that aggregate demand shifts by the full change in cohort size, we can back out the long-run supply elasticity, $\gamma = \left[\left(\frac{\Delta \log P}{\Delta \log Y} \right)^{-1} + \beta \right]$ = -1.38 (see appendix B, Panel B for

[31] This basic conclusion also holds if you consider lifetime use net of past year use among 37 to 55 year olds within a division and year, to purge the measure of current substance use behavior, or if you restrict the parental cohort to 40 to 44 year olds using either measure of use (lifetime or lifetime net of past year).

details of these calculations). Of course, there is reason to believe that these assumptions are not entirely valid. For example, Pacula et al.'s (2001) estimates of the price elasticity of past year marijuana participation, based on differences in marijuana prices and use rates across cities, range from -0.47 to -0.06 and, thus, the implied elasticity for stable per youth demand of -0.85 from may be implausibly large.[32] This informal calculation suggests that some outward shift in individual youth demand for illegal drugs is also associated with an increase in youth cohort size.

The evidence on the effect of cohort size on youth possession arrest rates supports the view that demand is also affected by cohort size. By lowering the likelihood of getting caught, an increase in youth cohort size should raise the net benefits of marijuana consumption. Using the -0.85 estimate of the price elasticity of marijuana demand, an outward shift in supply would explain about 60% of the increase in youth use between 1992 and 1999. Explaining the other 40% through changes in arrest probabilities alone seems unlikely, however, and further suggests that social multiplier effects may play a role in the relationship between youth drug use and cohort size. For example, an increase in cohort size may raise the benefits of drug use by increasing the probability of having a peer to share with in the drug experience.

Section 4. Conclusions

This paper establishes a strikingly large positive relationship between youth cohort size and rates of youth marijuana use. In other words, teens are more likely to use illicit drugs when their cohort is large. I first show that this relationship is not driven solely by national trends. Cohort size within a census division, state, or neighborhood

[32] As indicated by the wide range of estimates, however, the Pacula et al. (2001) results are quite sensitive to the form of time trends included in their models.

matters for rates of youth involvement with illicit drugs, even when controlling for time effects. I then explore various explanations for the relationship: scale economies in drug markets, resource strains, and intergenerational attitude transfers.

I find that economies of scale in drug distribution and reductions in the probability of arrest for drug dealing play key roles in this phenomenon. In effect, these two factors are flip sides of the same coin. Thicker youth markets provide better networks of information concerning where to "safely" buy and sell illicit drugs, and they require a less than proportionate increase in resources to evade the authorities and deliver drugs. The reduction in the unit cost of distributing illicit drugs translates into lower prices of commercial-grade marijuana, the variety typically smoked by casual users. And, lower prices feed back to youth use.

Although the supply channel dominates, as reflected in the negative relationship between cohort size and marijuana prices, supply side changes alone cannot fully explain the relationship between youth marijuana use and cohort size. Rather, cohort size also appears to affect demand. Indeed, I find that drug possession arrest rates for youth decline when cohorts are large, raising the net benefit of consumption. Informal calculations suggest that this may not be the only factor affecting demand, however. Cohort size may also affect youth demand for marijuana through peer multiplier effects.

Rising rates of illicit drug use among teens in the 1990s had seemed anomalous and inexplicable because it came after more than a decade of decline in youth drug use. But the positive relationship between youth drug use and cohort size allows us to better understand changes in youth drug use over time. Because of their relatively large cohort size and the existence of fixed costs in illicit drug dealing, teens in the 1990s affected society's ability to cope with the illicit drug trade and faced lower drug prices than the cohorts just preceding them. It is important to point out, however, that the fixed costs of drug distribution and thus economies of scale are in large part by-products of the illicit nature of the drug trade. Under a legal regime, cohort size might play a more limited role in determining trends in youth drug use. And the efficacy of policy instruments such as taxation, which has been effective in discouraging smoking among high school seniors (Gruber and Zinman 2001), might prove unaffected by variations in cohort size.

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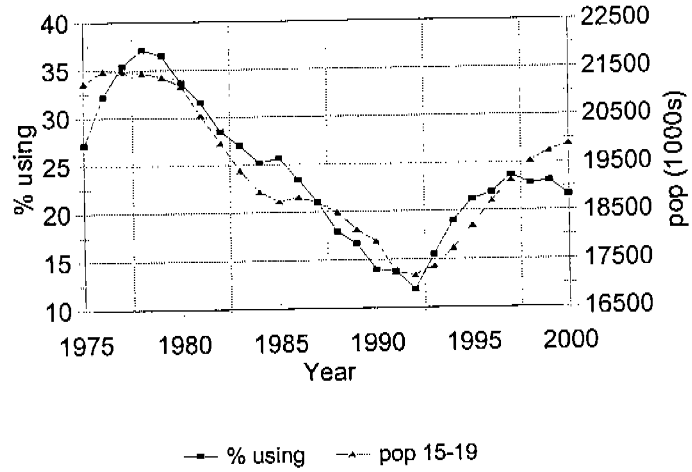
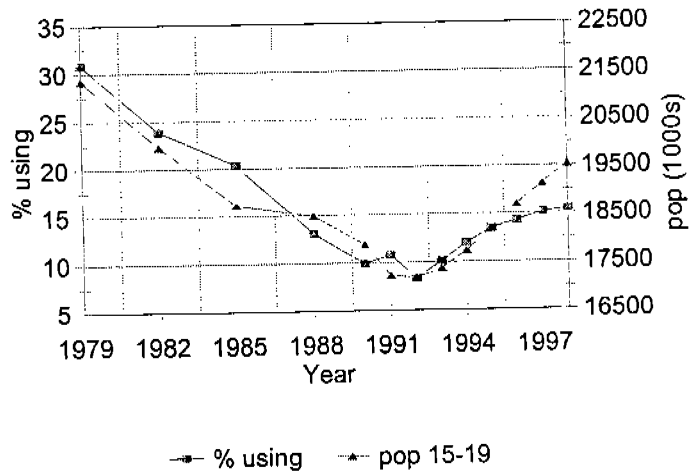
Fig 1. Past Month Marijuana Use Among 12th Graders and Pop 15-19: 1975-2000



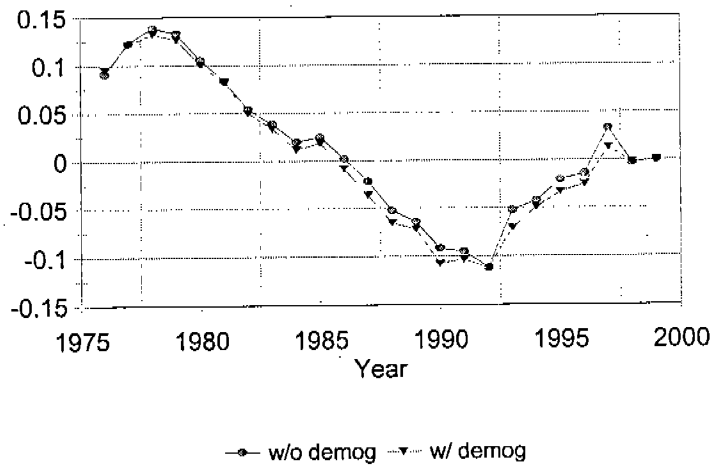
Sources: Monitoring the Future, Current Population Surveys.
Notes: "% using" represents past month marijuana use among high school seniors. The population of 15 to 19 year olds is in 1000s.

Fig 2. Past Month Marijuana Use and Population 15-19 Year Olds: 1979-1998



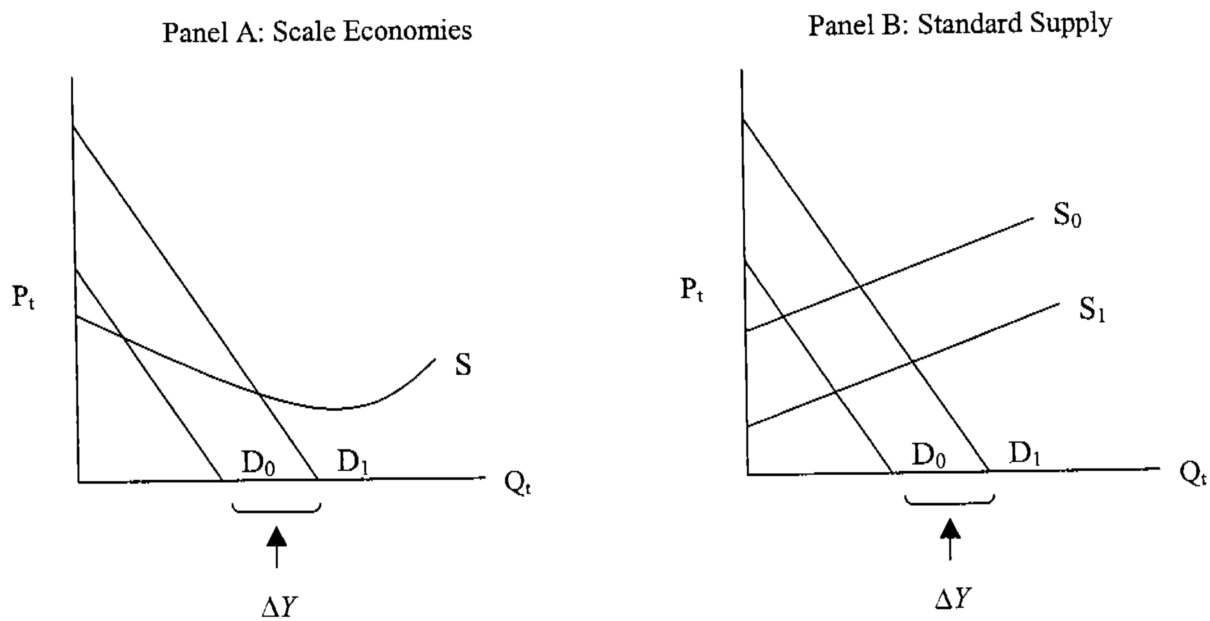
Sources: National Household Survey on Drug Abuse, Current Population Surveys.
Notes: "% using" represents past month marijuana use among 15 to 19 year olds. The population of 15 to 19 year olds is in 1000s.

Figure 3. Demographics and Trends in Rates of Past Month Marijuana Use



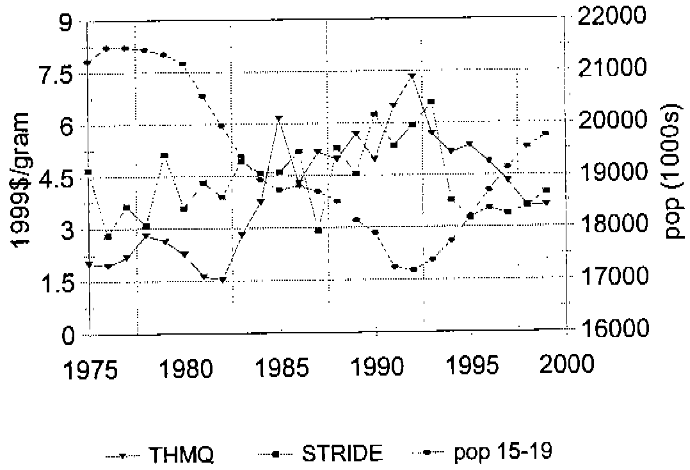
Sources: Monitoring the Future.
Notes: Plotted above are year effects from a regression of past month marijuana use among high school seniors in on a constant and year dummies, both with and without individual demographic covariates.

Fig 4. Hypothetical Aggregate Supply to and Demand for Marijuana by Youth



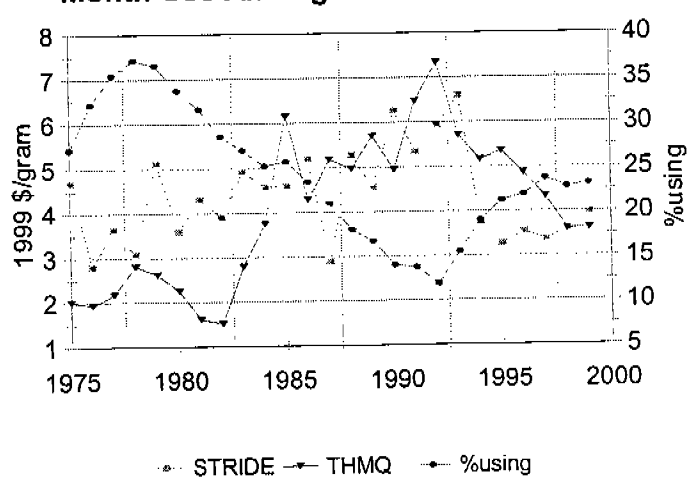
Notes: P_t is the money price of marijuana and Q_t the total quantity in the youth market at time t.

Fig 5. Real Marijuana Prices and Youth Cohort Size: 1975-1999



Sources: *High Times*, STRIDE, and Current Population Surveys.
Notes: "THMQ" is the median price per gram of commercial-grade marijuana. STRIDE is the median price per gram of marijuana from DEA busts. Both are in 1999 dollars, with the national CPI as the deflator. The population of 15 to 19 year olds is in 1000s.

Fig 6. Real Marijuana Prices and Past Month Use Among HS Seniors: 1975-99



Sources: *High Times*, STRIDE, and Monitoring the Future.
Notes: "THMQ" and "STRIDE" are defined as above. All prices are again in 1999 dollars per gram. "% using" is the rate of past month marijuana use among high school seniors as reported in Monitoring the Future.

Table 1A. Impact of Youth Share of the Population on Probability of Past Marijuana Use Among Youth and Adults: National Household Survey on Drug Abuse: 1979-1997

Independent Variable	Panel A: Past Month Marijuana Use 15-19 Year Olds Mean Use = 12.4%			30 Years & Older Mean Use = 4.55%		
Ln (Share Pop. 15-19)	.523*** (.038) [.046]	.369*** (.106) [.136]	.390*** (.106) [.137]	.081*** (.021) [.023]	.104** (.043) [.043]	.086* (.045) [.046]
Division UR	--	--	.003 (.003) [.003]	--	--	-.003** (.002) [.001]
Division FEs	√	√	√	√	√	√
Year FEs		√	√		√	√
R-squared	.035	.039	.039	.032	.033	.033
Number Obs.	40780	40780	40780	81117	81117	81117

Independent Variable	Panel B: Past Year Marijuana Use 15-19 Year Olds Mean Use = 22.1%			30 Years & Older Mean Use = 8.10%		
Ln (Share Pop. 15-19)	.608*** (.040) [.051]	.427** (.138) [.162]	.436** (.139) [.164]	.080*** (.028) [.033]	.114 (.060) [.076]	.090 (.062) [.080]
Division UR	--	--	.001 (.004) [.006]	--	--	-.004* (.002) [.002]
Division FEs	√	√	√	√	√	√
Year FEs		√	√		√	√
R-squared	.044	.048	.048	.050	.052	.052
Number Obs.	40780	40780	40780	81117	81117	81117

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level. Standard Errors in parentheses are cluster-adjusted at the division-year level; those in brackets are cluster-adjusted at the division (9) level to allow for correlation over time. Tests of significance shown above are based on the division-year level clustering.

Coefficients represent the effect of a 1% change in the youth share of the population on marijuana, participation in the specified interval.

Estimates are based on pooled NHSDA data from 1979-1997. The youth share of the population is measured at the division-year level. All regressions include controls for the respondent's sex, and race as well as division fixed effects and division-year unemployment rates. All but the first and forth columns of results also include year fixed effects. Regressions for 15-19 year olds include age dummies whereas regressions for those 30 years and older control for the respondent's age and age-squared.

Table 1B. Impact of Youth Share of the Population on Probability of Past Cigarette Use Among Youth and Adults: National Household Survey on Drug Abuse: 1979-1997

Independent Variable	Panel A: Past Month Cigarette Use					
	15-19 Year Olds Mean Use =22.9%			30 Years & Older Mean Use = 33.5%		
Ln (Share Pop. 15-19)	.165*** (.050) [.032]	.139 (.106) [.133]	.119 (.109) [.137]	.161*** (.039) [.035]	.061 (.076) [.159]	.063 (.079) [.161]
Division UR	--	--	-.003 (.004) [.006]	--	--	-.000 (.003) [.003]
Division FEs	√	√	√	√	√	√
Year FEs		√	√		√	√
R-squared	.048	.063	.063	.040	.045	.045
Number Obs.	40780	40780	40780	81117	81117	81117

Independent Variable	Panel B: Past Year Cigarette Use					
	15-19 Year Olds Mean Use = 31.5%			30 Years & Older Mean Use = 36.9%		
Ln (Share Pop. 15-19)	.270*** (.047) [.040]	.104 (.126) [.137]	.069 (.128) [.142]	.217*** (.040) [.030]	.092 (.077) [.161]	.092 (.078) [.160]
Division UR	--	--	-.006 (.005) [.006]	--	--	.000 (.003) [.003]
Division FEs	√	√	√	√	√	√
Year FEs		√	√		√	√
R-squared	.050	.056	.056	.045	.049	.049
Number Obs.	40780	40780	40780	81117	81117	81117

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level. Standard Errors in parentheses are cluster-adjusted at the division-year level; those in brackets are cluster-adjusted at the division (9) level to allow for correlation over time. Tests of significance shown above are based on the division-year level clustering.

Coefficients represent the effect of a 1% change in the youth share of the population on cigarette, participation in the specified interval.

Estimates are based on pooled NHSDA data from 1979-1997. The youth share of the population is measured at the division-year level. All regressions include controls for the respondent's sex, and race as well as division fixed effects and division-year unemployment rates. All but the first and forth columns of results also include year fixed effects. Regressions for 15-19 year olds include age dummies whereas regressions for those 30 years and older control for the respondent's age and age-squared.

Table 1C. Impact of Youth Share of the Population on Probability of Past Alcohol Use Among Youth and Adults: National Household Survey on Drug Abuse: 1979-1997

Independent Variable	Panel A: Past Month Alcohol Use: 15-19 Year Olds Mean Use = 36.2%			30 Years & Older Mean Use = 53.0%		
Ln (Share Pop. 15-19)	.624*** (.046) [.069]	.321** (.148) [.209]	.290* (.161) [.216]	.193 (.077) [.117]	.446** (.120) [.173]	.390* (.137) [.186]
Division UR	--	--	-.005 (.005) [.006]	--	--	-.009 (.005) [.008]
Division FEs	√	√	√	√	√	√
Year FEs		√	√		√	√
R-squared	.075	.077	.077	.079	.080	.080
Number Obs.	40780	40780	40780	81117	81117	81117

Independent Variable	Panel B: Past Year Alcohol Use: 15-19 Year Olds Mean Use = 57.0%			30 Years & Older Mean Use = 68.4%		
Ln (Share Pop. 15-19)	.517*** (.057) [.068]	.335** (.142) [.117]	.304** (.153) [.129]	.050 (.074) [.103]	.341** (.123) [.143]	.309 (.134) [.168]
Division UR	--	--	-.005 (.005) [.005]	--	--	-.005 (.005) [.009]
Division FEs	√	√	√	√	√	√
Year FEs		√	√		√	√
R-squared	.081	.086	.086	.090	.091	.091
Number Obs.	40780	40780	40780	81117	81117	81117

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level. Standard Errors in parentheses are cluster-adjusted at the division-year level; those in brackets are cluster-adjusted at the division (9) level to allow for correlation over time. Tests of significance shown above are based on the division-year level clustering.

Coefficients represent the effect of a 1% change in the youth share of the population on marijuana, participation in the specified interval.

Estimates are based on pooled NHSDA data from 1979-1997. The youth share of the population is measured at the division-year level. All regressions include controls for the respondent's sex, and race as well as division fixed effects and division-year unemployment rates. All but the first and forth columns of results also include year fixed effects. Regressions for 15-19 year olds include age dummies whereas regressions for those 30 years and older control for the respondent's age and age-squared.

Table 2. IV Estimates of the Impact of Youth Share of the Population on Probability of Past Marijuana, Alcohol and Cigarette Use: National Household Survey on Drug Abuse: 1979-1997

Independent Variable	Panel A: Used in Past Month					
	15-19 Year Olds			Adults, 30 Years and Older		
	Marijuana	Cigarettes	Alcohol	Marijuana	Cigarettes	Alcohol
Ln (Share of Pop. 15-19)	.334*** (.123) [.209]	-.017 (.132) [.189]	.193 (.177) [.246]	.070 (.056) [.061]	-.017 (.097) [.198]	.168 (.182) [.278]
Mean Use	12.4%	22.9%	36.2%	4.55%	33.5%	53.0%
Number Obs.	40780	40780	40780	81117	81117	81117

Independent Variable	Panel B: Used in Past Year					
	15-19 Year Olds			Adults, 30 Years and Older		
	Marijuana	Cigarettes	Alcohol	Marijuana	Cigarettes	Alcohol
Ln (Share of Pop. 15-19)	.346** (.165) [.270]	-.079 (.149) [.201]	.310* (.176) [.160]	.068 (.076) [.097]	.029 (.092) [.190]	.106 (.180) [.255]
Mean Use	22.1%	31.5%	57.0%	8.10%	36.9%	68.4%
Number Obs.	40780	40780	40780	81117	81117	81117

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level. Standard Errors in parentheses are cluster-adjusted at the division-year level; those in brackets are cluster-adjusted at the division (9) level to allow for correlation over time. Tests of significance shown above are based on the division-year level clustering.

Coefficients represent the effect of a 1% change in the youth share of the population on drug (marijuana, cigarette or alcohol) participation in the specified interval. The birth rate (or sum of the number of births per person) in division *d* 15 to 19 years earlier is used to instrument for the share of the population 15-19 years old.

Estimates are based on pooled NHSDA data from 1979-1997. All regressions include controls for the respondent's sex and race as well as division and year fixed effects and division-year unemployment rates. Regressions for 15-19 year olds include age dummies whereas regressions for those 30 years and older control for the respondent's age and age-squared.

Table 3. Impact of School Size, Family Structure, and Parental Attitudes on Teen Substance Use: National Longitudinal Survey of Adolescent Health

	Marijuana Use		Cigarette Use		Inhalant Use	
Independent Var.	Ever	Past Mo.	Ever	Past Mo.	Ever	Past Mo.
Ln (Share 15-19, tract)	-.008* (.004)	-.007** (.003)	.008*** (.002)	-.009*** (.003)	-.002*** (.001)	-.001*** (.000)
Ln (Share 15-19, block grp)	.046* (.026)	.049** (.020)	.003 (.031)	.011 (.024)	-.001 (.004)	.001 (.002)
Ln (School Size)	.031** (.015)	.016 (.010)	.011 (.014)	.013 (.011)	-.002 (.004)	-.001 (.001)
Neighborhood less teen drug use	-.102*** (.033)	-.029 (.028)	.017 (.055)	.008 (.020)	-.001 (.006)	-.003 (.003)
Parent Drinks Heavily	.100*** (.045)	.049 (.039)	.052 (.046)	.073** (.036)	.042 (.031)	.007 (.018)
Parent Smokes Cigarettes	.074** (.035)	.105*** (.035)	.101*** (.030)	.098*** (.035)	.011** (.005)	.008*** (.003)
Number of Siblings	-.041** (.013)	-.028** (.014)	-.046**8 (.014)	-.004 (.019)	-.005 (.003)	.002 (.001)
Missing Parent Survey Dummy	.029 (.060)	.071 (.054)	.321*** (.057)	.093* (.055)	-.010 (.007)	-.004 (.003)
Missing Census Information	-.028 (.036)	-.080** (.040)	-.061* (.035)	-.025 (.046)	-.029** (.013)	-.014*** (.004)
Mean Use	28.6%	14.4%	57.0%	26.0	6.04%	1.56
Observations	17147	16936	17200	17167	16836	16836
R-Squared	.147	.136	.158	.110	.023	.010

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level. Standard errors are cluster-adjusted by census tract and shown in parentheses.

Regressions control for age, age-squared, sex, race (5), whether Hispanic, whether native born, grade in school, the employment status of the adolescent respondent in the last month, the importance of religion to the respondent, size of the school the respondent attends, number of siblings in household and its square. Controls from parental (90% maternal) surveys include parent's education, log of family income, unemployment status, smoker status, food stamp recipiency status, parent's assessment of problems with drug-dealing and drug use in their neighborhood, whether they live in the neighborhood because of low crime, and/or low levels of teen drug use. All regressions also control for the median age in the census tract, the tract-level unemployment rate, the county non-marital fertility rate, the county non-marital birth rate for 15 to 19 year olds, log of total serious crimes per 100,000 in the county, log of total serious juvenile crimes per 100,000, log of per capita spending on police by local government, and the proportion of local spending going to police.

Table 4. Impact of School Size, Family Structure, and Parental Attitudes on Suicidal Tendencies: National Longitudinal Survey of Adolescent Health

Independent Variable	Suicidal Tendencies in Past Year		
	Serious Thoughts	Suicidal Attempt	Medically Treated Attempt
Ln (Share 15-19, census tract)	-.003**	.000	-.001
	(.001)	(.001)	(.001)
Ln (Share 15-19, block group)	.020	-.009	-.005
	(.015)	(.006)	(.005)
Ln (School Size)	.003	.003	.008*
	(.006)	(.005)	(.004)
Neighborhood less teen drug use	-.003	-.012	-.008
	(.017)	(.010)	(.008)
Parent Drinks Heavily	.029	.016	-.011
	(.034)	(.022)	(.008)
Parent Smokes Cigarettes	.002	.004	-.006
	(.013)	(.006)	(.005)
Number Siblings	.001	.014	.015
	(.011)	(.010)	(.009)
Missing Parent's Dummy	-.037**	-.019**	-.011
	(.018)	(.009)	(.008)
Missing Census Information	.006	.011	.008
	(.030)	(.012)	(.009)
Mean Dependent Variable	13.4%	3.89%	0.96%
Observations	17177	17243	17243
Adj. R-Sqd	.077	.062	.140

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level. Standard errors are cluster-adjusted by census tract and shown in parentheses.

Regressions control for age, age-squared, sex, race (5), whether Hispanic, native born, grade in school, employment status of the teen respondent in the last month, the importance of religion, size of the school the respondent attends, number of siblings in household and its square. Controls from parental (90% maternal) surveys include parent's education, log of family income, unemployment status, smoker status, food stamp recipiency status, assessment of problems with drug-dealing and drug use in the neighborhood, whether they live in the neighborhood because of low crime, and/or low levels of teen drug use. All regressions also control for the median age in the census tract, the tract-level unemployment rate, the county non-marital fertility rate, the county non-marital birth rate for 15 to 19 year olds, log of total serious crimes per 100,000 in the county, log of total serious juvenile crimes per 100,000, log of per capita spending on police by local government, and the proportion of local spending going to police.

Table 5. Descriptive Statistics for Illicit Drug Price Data

	Median Prices Per Gram (1999 Dollars)		
	All	Retail	Wholesale
High Quality MJ, THMQ	9.17 [2.25, 18.25]	--	--
Low Quality MJ, THMQ	3.17 [.529, 10.6]	--	--
All MJ, STRIDE	3.14 [.128, 29167]	--	--
Cocaine, Pure Grams	215 [36.6, 5982]	180 [57, 22740]	45.3 [.444, 569]
Heroin, Pure Grams	306 [35.4, 112676]	1676 [209, 2820513]	246 [.784, 40000]

The high quality THMQ marijuana statistics are based on 250 observations, 10 states over 25 years. The states included are: AK, CA, GA, HI, MI, MO, NY, OR, TN, and TX. The low quality THMQ statistics are based on 200 observations, all states listed above, except AK and HI, over 25 years. The STRIDE statistics are based on 100 observations, CA, DC, FL, NY, and TX, over 25 years.

Table 6. OLS & IV Estimates of the Impact of Youth Share of the Population on THMQ and STRIDE Marijuana Prices, by State: 1975-1999

Panel A: OLS Regressions			
	Median Marijuana Prices Per Gram		
Independent Variable	THMQ, Low Quality	THMQ, High Quality	STRIDE, All buys
Ln (Share Pop. 15-19 Years Old)	-1.55 (.868)	-.308 (.261)	-.867 (.134)
Observations	200	244	125
R-squared	.981	.801	.287

Panel B: Instrumental Variables Regressions			
	Median Marijuana Prices Per Gram		
Independent Variable	THMQ, Low Quality	THMQ, High Quality	STRIDE, All buys
Ln (Share Pop. 15-19 Years Old)	-1.88** (.646)	.362 (.548)	-1.48 (1.84)
Observations	200	239	125

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level.

Observations are at the state-year level. Price data comes from *High Times'* monthly Trans High Market Quotations (THMQ) and the DEA's System to Retrieve Drug Information from Evidence (STRIDE). Each price observation is the median price per gram of either high or low quality marijuana in a given state and year or of all DEA buys in a given state and year. Quality is based on author-assessment and on the source country and type of marijuana listed. Low quality is generally Colombian or Mexican "commercial weed" whereas high quality is Californian sensimilla or a Hawaiian variety such as "Puna Gold."

Coefficients represent the elasticity of the indicated price per pure gram with respect to the share of the total population that is 15-19 years old. The birth rate (or sum of the number of births per person) in state *s* 15 to 19 years earlier is used to instrument for the share of the population 15-19 years old. All regressions include year and state fixed effects. Standard errors are cluster-adjusted at the state-level to allow for serial correlation. States included in the THMQ high quality price regressions are: AK, CA, GA, HI, MI, MO, NY, OR, TN, and TX. AK and HI are omitted from the low quality price regressions because over half of the observations for each state are missing. States included in the STRIDE regressions are: CA, DC, FL, NY, and TX.

Table 7. OLS & IV Estimates of the Impact of Share of the Population 20-24 Years Old on THMQ and STRIDE Marijuana Prices, by State: 1975-1999

	Panel A: OLS Regressions			
	Median Marijuana Prices Per Gram			
Independent Variable	THMQ, Low Quality		THMQ, High Quality	
Ln (Share Pop. 15-19 Years Old)	--	-1.56*	--	.233
		(.823)		(.339)
Ln (Share Pop. 20-24 Years Old)	-.321	.047	-1.17*	-1.24*
	(.447)	(.284)	(.555)	(.608)
Observations	200	200	244	244
R-squared	.978	.981	.816	.816

	Panel B: Instrumental Variables Regressions			
	Median Marijuana Prices Per Gram			
Independent Variable	THMQ, Low Quality		THMQ, High Quality	
Ln (Share Pop. 15-19 Years Old)	--	-1.87**	--	.664
		(.905)		(.793)
Ln (Share Pop. 20-24 Years Old)	-1.25	.113	-1.54	-1.98
	(.873)	(.458)	(1.29)	(1.67)
Observations	200	200	229	229

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level.

Observations are at the state-year level. Price data comes from *High Times'* monthly Trans High Market Quotations (THMQ) and the DEA's System to Retrieve Drug Information from Evidence (STRIDE). Each price observation is the median price per gram of either high or low quality marijuana in a given state and year or of all DEA buys in a given state and year. Quality is based on author-assessment and on the source country and type of marijuana listed. Low quality is generally Colombian or Mexican "commercial weed" whereas high quality is Californian sensimilla or a Hawaiian variety such as "Puna Gold."

Coefficients represent the elasticity of the indicated price per pure gram with respect to the share of the total population that is 20-24 years old. The birth rate (or sum of the number of births per person) in state *s* 15 to 19 years earlier is used to instrument for the share of the population 15-19 years old. The birth rate in state *s* 20-24 years earlier is used to instrument for the share of the population 20-24 years old. All regressions include year and state fixed effects. Standard errors are cluster-adjusted at the state-level to allow for serial correlation. States included in the THMQ high quality price regressions are: AK, CA, GA, HI, MI, MO, NY, OR, TN, and TX. AK and HI are omitted from the low quality price regressions because over half of the observations for each state are missing. States included in the STRIDE regressions are: CA, DC, FL, NY, and TX.

Table 8. Drug Sales and Possession Arrest Rates: 1976-1997, Descriptive Statistics

	Full Sample	15-19 Year Olds	30 Years & Older
Youth Share	.081 (.011)	--	--
Marijuana Sales Arrests Per 100,000	18.5 (13.0)	63.9 (62.7)	7.81 (5.11)
All Drug Sales Arrests Per 100,000	61.4 (49)	174 (163)	35 (31)
Marijuana Possession Arrests Per 100,000	96.6 (44.2)	395 (208)	33.5 (20.0)
All Drug Possession Arrests Per 100,000	170 (92)	518 (257)	92 (79)
Larceny Arrests Per 100,000	373 (206)	1394 (776)	187 (119)
Vandalism Arrests Per 100,000	68.4 (44.4)	289 (216)	22.0 (16.9)
Population (100000s)	23.2 (9.74)	1.85 (.758)	12.3 (5.45)
Number of Obs.	198	198	198

Mean is given in each cell. Standard deviation appears in parenthesis. Youth share of the population and arrest rates are measured at division-year level for 1976-1997. Arrest data is generated from MSA-level Uniform Crime Reports. Due to significant missing observations, the divisions do not include data from: AK, DE, FL, HI, IL, KS, MT, NH, SD, VT and WY. Arrest rates are given per 100,000 of the target population, i.e. 15-19 year olds, 30 years and older or total population.

Table 9A. Impact of Youth Share on Marijuana Arrest Rates, by Census Division: 1976-1997

	Panel A: OLS Regressions					
	Arrest Rate, 15-19 Year Olds		Arrest Rate, 30 Years Old +		Arrest Rate, All Ages	
Independent Variable	Marijuana Sales	Marijuana Possession	Marijuana Sales	Marijuana Possession	Marijuana Sales	Marijuana Possession
Ln (Share of the Pop. 15-19 Years Old)	-1.96 (2.11)	-1.52* (.766)	.047 (1.49)	-.466 (.867)	-.686 (1.65)	-.654 (.675)
Observations	198	198	198	198	198	198
R-squared	.692	.892	.809	.879	.689	.849

	Panel B: IV Regressions					
	Arrest Rate, 15-19 Year Olds		Arrest Rate, 30 Years Old +		Arrest Rate, All Ages	
Independent Variable	Marijuana Sales	Marijuana Possession	Marijuana Sales	Marijuana Possession	Marijuana Sales	Marijuana Possession
Ln (Share of the Pop. 15-19 Years Old)	-2.52 (1.56)	-1.40 (.868)	.212 (1.22)	-.471 (1.01)	-.885 (1.21)	-.640 (.767)
Observations	198	198	198	198	198	198

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level. Standard errors are cluster-adjusted at the division-level to correct serial correlation.

Coefficients represent the elasticity of the indicated arrest rate (sales or possession) with respect to the share of the total population that is 15-19 years old.

Observations are at the division-year level. OLS estimates are based on 198 observations, 9 census divisions over 22 years. Data is generated from MSA-level Uniform Crime Reports. Due to significant missing observations, the divisions do not include data from AK, DE, FL, HI, IL, KS, MT, NH, SD, VT and WY. All regressions include division and year fixed effects, annual division unemployment rates, The birth rate (or sum of the number of births per person) in division *d* 15 to 19 years earlier is used to instrument for the share of the population 15-19 years old.

Table 9B IV Estimates of the Impact of Youth Share of the Population on Marijuana, All Drug and Other Arrest Rates, by State with Division-Year Fixed Effects: 1976-1997
(State-year level observations)

Panel A: Marijuana Arrest Rates

	Arrest Rate, 15-19 Year Olds		Arrest Rate, 30 Years & Older		Arrest Rate, All Ages	
Independent Variable	Marijuana Sales	Marijuana Possession	Marijuana Sales	Marijuana Possession	Marijuana Sales	Marijuana Possession
Ln (Share of the Pop. 15-19 Years Old)	-4.44*** (1.00)	-1.67 (1.33)	-3.10*** (1.09)	1.69 (1.66)	-3.87*** (1.12)	.023 (1.28)
Observations	854	854	848	854	853	854

Panel B: All Drug Arrest Rates

	Arrest Rate, 15-19 Year Olds		Arrest Rate, 30 Years & Older		Arrest Rate, All Ages	
Independent Variable	Drug Sales	Drug Possession	Drug Sales	Drug Possession	Drug Sales	Drug Possession
Ln (Share of the Pop. 15-19 Years Old)	-6.70*** (1.75)	-1.98* (1.10)	-2.95*** (1.05)	1.12 (1.10)	-4.33*** (1.39)	-.539 (.955)

Panel C: Other Arrest Rates

	Arrest Rate, 15-19 Year Olds		Arrest Rate, 30 Years & Older		Arrest Rate, All Ages	
Independent Variable	Larceny	Vandalism	Larceny	Vandalism	Larceny	Vandalism
Ln (Share of the Pop. 15-19 Years Old)	.577 (.613)	-.824 (.525)	-.205 (.511)	.843 (.901)	.828* (.452)	.288 (.546)

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level. Standard errors are cluster-adjusted at the state-level to correct serial correlation.

Coefficients represent the elasticity of the indicated arrest rate (sales or possession) with respect to the share of the total population that is 15-19 years old. The birth rate (or sum of the number of births per person) in state *s* 15 to 19 years earlier is used to instrument for the share of the population 15-19 years old.

Observations are at the state-year level. OLS estimates should include 858 observations, 39 states over 22 years. Excluded are: AK, DE, FL, HI, IL, KS, MT, NH, SD, VT and WY. Deviations are due to missing data in some state-year cells.

All regressions include state and division-year fixed effects, annual state unemployment rates, the log of prisoners per capita lagged 1 year, the log of police per capita lagged 1 year, the log of state income per capita (in 97 $), and a dummy for the presence of a concealed handgun law.

Table 9C. IV Estimates of the Impact of the Youth Share and the Share 20 to 24 Years Old on Marijuana Arrest Rates, by State with Division-Year Fixed Effects: 1976-1997
(State-year level observations)

	Youth Marijuana Arrest Rates				All Marijuana Arrest Rates			
	Sales		Possession		Sales		Possession	
Ln (Share Pop. 20-24)	-8.53** (3.23)	-2.24 (1.62)	-2.19 (2.35)	.014 (1.85)	-6.54*** (3.22)	-1.25 (2.38)	.157 (2.11)	-.163 (1.77)
Ln (Share Pop. 15-19)	--	-4.69*** (1.36)	--	-1.67 (1.26)	--	-4.00*** (1.11)	--	.005 (1.20)
Obs.	854	854	854	854	853	853	854	854

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level. Standard errors are cluster-adjusted at the state-level to correct serial correlation.

Coefficients represent the elasticity of the indicated arrest rate (sales or possession) with respect to the share of the total population that is 15-19 years old. The birth rate (or sum of the number of births per person) in state *s* 15 to 19 years earlier is used to instrument for the share of the population 15-19 years old. Similarly, the birth rate in state s 20 to 24 years earlier is used to instrument for the share of the population 20-24 years old.

Observations are at the state-year level. OLS estimates should include 858 observations, 39 states over 22 years. Excluded are: AK, DE, FL, HI, IL, KS, MT, NH, SD, VT and WY. Deviations are due to missing data in some state-year cells.

All regressions include state and division-year fixed effects, annual state unemployment rates, the log of prisoners per capita lagged 1 year, the log of police per capita lagged 1 year, the log of state income per capita (in 97 $), and a dummy for the presence of a concealed handgun law.

Table 10. The Effect of Lifetime Substance Use Among 37-55 Year Olds on the Probability of Youth Marijuana, Cigarette, and Alcohol Use, NHSDA: 1979-1997

	Substance Use Among 15-19 Year Olds					
	Past Month Use			Past Year Use		
Indep. Variable	Marijuana	Cigarettes	Alcohol	Marijuana	Cigarettes	Alcohol
Ln (Share Pop. 15-19)	.322*** (.105)	-.064 (.114)	.180 (.169)	.318** (.145)	-.138 (.132)	.293 (.124)
Frac. 37-55 yr olds, Used MJ	.301** (.127)	.256*** (.065)	.179* (.225)	.224*** (.071)	.311*** (.081)	.236** (.208)
Frac. 37-55 yr olds, Smoked	-.048 (.078)	.149 (.103)	-.069 (.129)	-.001 (.104)	.190 (.123)	-.100 (.138)
Frac. 37-55 yr olds, Drank	.367*** (.069)	-.023 (.110)	.314** (.141)	.332*** (.088)	-.060 (.129)	.544*** (.129)
Mean Use	12.4%	22.9%	36.2%	22.1%	31.5%	57.0%
Observations	40780	40780	40780	40780	40780	40780

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level. Standard Errors in parentheses are cluster-adjusted at the division-year level.

Coefficients on youth use represent the effect of a 1% change in the youth share of the population on drug (marijuana, cigarette or alcohol) participation in the specified interval. The birth rate (or sum of the number of births per person) in division *d* 15 to 19 years earlier is used to instrument for the share of the population 15-19 years old.

The fraction of 37 to 55 year olds using marijuana, cigarette, or alcohol (separately) is measured at the division-year level. Coefficients on the fraction of 37-55 year olds who have ever used marijuana, cigarettes, and/or alcohol represent the effect of going from no 37 to 55 years old in a youth's division and year using a given substance to all adults in this age group, area, and year using on the youth's own probability of marijuana, cigarette, or alcohol use in the past month or year.

Estimates are based on pooled NHSDA data from 1979-1997. All regressions include controls for the respondent's sex and race, division and year fixed effects, the fraction of 37-55 year olds in that division and year who have ever used marijuana, cigarettes, or alcohol (separately), and division-year unemployment rates. Regressions for 15-19 year olds include age dummies whereas regressions for those 30 years and older control for the respondent's age and age-squared.

Appendix A: Tables A1-A6

Table A1. Effect of Demographics on Past Marijuana Use Among HS Seniors: 1976-99

Covariate	Past Month Use	Past Year Use
Smsa	.016*** (.002)	.023*** (.003)
Age>18	-.014*** (.008)	-.023*** (.003)
Black	-.045*** (.003)	-.082*** (.003)
Other non-white	-.026*** (.003)	-.046*** (.003)
Male	.052*** (.002)	.053*** (.002)
Grew up rural	-.070*** (.003)	-.094*** (.003)
Grew up town	-.025*** (.002)	-.033*** (.003)
Grew up urban	-.010*** (.003)	-.014*** (.003)
Dad's ed< hs	.003 (.003)	.003 (.003)
Dad's ed some college	.011*** (.003)	.014*** (.003)
Dad's ed college	017*** (.002)	.021*** (.003)
Mom's ed< hs	-.011*** (.002)	-.016*** (.003)
Mom's ed some college	.014*** (.002)	.021*** (.003)
Mom's ed college	.011*** (.002)	.018*** (.003)
Northeast	.039*** (.003)	.046*** (.003)
South	-.028*** (.002)	-.034*** (.003)
West	-.010*** (.003)	.003 (.003)
# of Siblings	-.039*** (.002)	-.048*** (.002)
Married	-.059*** (.006)	-.077*** (.007)
Engaged	-.019*** (.004)	.009*** (.004)
Sep/Divorced	.057*** (.014)	.049*** (.015)
General Ed. HS	.070*** (.002)	.080*** (.002)
Vocational HS	.071*** (.003)	.069*** (.003)
Other HS	.076*** (.004)	.075*** (.004)
Attend Church Regularly	-.114*** (.002)	-.144*** (.002)
Work part-time	.014*** (.002)	.023*** (.003)
Work >20hr/wk	.039*** (.003)	.057*** (.003)
Job inc/week	.0003*** (.00002)	.023*** (.0003)
Other inc/week	.0006*** (.00002)	.057*** (.003)
R-squared	.083	.093
Number of Obs.	308381	308431

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level. Standard errors are in parentheses.

Regressions are based on pooled samples of 12th graders from Monitoring the Future, 1976-1999. All regressions include year fixed effects. Excluded categories are: white for race, North Central for region, suburban for hometown type, never married for marital status, college preparatory school for high school type, and no job for work status.

Table A2. Descriptive Statistics for NHSDA Regressions: 1979-1997

	Full Sample	15-19 Year Olds	30 Years & Older
Share Pop 15-19	.071 (.007) [.060, .098]	--	--
Annual Division Unemployment Rate	6.71 (1.72)	--	--
Percent Male	44.5	49.2	41
Percent Black	23.3	24.1	22.9
Median Age	25	17	40
Past Year MJ Use	.138	.226	.079
Past Month MJ Use	.078	.127	.044
Past Year Cig Use	.340	.320	.363
Past Month Cig Use	.287	.236	.330
Past Year Alc Use	.617	.565	.678
Past Month Alc Use	.464	.362	.533
Number of Obs.	209559	40889	81207

Except where indicated, mean is given. Standard deviation appears in parenthesis. Youth share of the population is measured at the division-year level for 1979, 1982, 1985, 1988 and 1990-1997. Min and max of youth share of the population appear in brackets

Table A3. Specification Checks of IV Regressions of Youth Drug Use on Cohort Size

Panel A :Youth Share in Logs Versus Levels				
	Past Month Marijuana Use			
	Youth, 15-19 Year Olds		Adult, 30 Years & Older	
Youth Share	.334***	3.22*	.070	.941
	(.123)	(1.74)	(.056)	(.911)
Specification of Indep. Variables	Logs	Levels	Logs	Levels

Panel B: Youth Density vs. Youth Share, Effect of Population Density				
	Past Month Marijuana Use			
	Youth, 15-19 Year Olds		Adult, 30 Years & Older	
Log (youth share)	.334***	.321***	.070	.102*
	(.123)	(.121)	(.056)	(.056)
Log (population density)	--	.038	--	-.095*
		(.097)		(.054)

Panel C: Absolute Growth in or Size of Youth Population				
	Past Month Marijuana Use			
	Youth, 15-19 Year Olds		Adult, 30 Years & Older	
Youth Population	.321***	8.78×10^{-8}*	.102*	1.07×10^{-8}
	(.121)	(5.08×10^{-8})	(.056)	(3.28×10^{-8})
Total Population	-2.82	-3.43×10^{-9}	-.197**	-3.08×10^{-9}
	(1.64)	(2.97×10^{-9})	(.054)	(2.28×10^{-9})
Specification of Indep. Variables	Logs	Levels	Logs	Levels

*** Indicates significance at the 1% level, at the ** 5% level and at the * 10% level. Standard Errors are cluster-adjusted at the division-year level.

Coefficients in Panel A. represent the effect of a 1% change in the youth share of the population on marijuana participation in the past month. Coefficients in col (1) and col (3) of Panel B. represent the effect of a 1% change in the youth population and in col(2) and col(4) the effect of youth population size on marijuana participation in the past month. The birth rate (or sum of the number of births per person) in division *d* 15 to 19 years earlier is used to instrument for the population 15-19 years old.

Estimates are based on pooled NHSDA data from 1979-1997. All regressions include controls for the respondent's sex and race as well as division and year fixed effects and division-year unemployment rates. Regressions for 15-19 year olds include age dummies whereas regressions for those 30 years and older control for the respondent's age and age-squared.

Table A4. Placebo Run, Impact of Shares of Other Age Groups on Probability of Past Marijuana Use: National Household Survey on Drug Abuse: 1979-1997

	Panel A: Share of Children							
Indep.Variable	Marijuana Use, 15-19 Year Olds				Marijuana Use, 30 Years & Older			
	Past Month		Past Year		Past Month		Past Year	
Ln (Share Pop. 5-14)	.056 (.136) [.191]	.090 (.129) [.235]	.121 (.156) [.238]	.156 (.153) [.288]	-.075 (.076) [.122]	-.065 (.079) [.133]	-.107 (.099) [.180]	-.096 (.103) [.193]
Ln (Share Pop. 15-19)	--	.318*** (.113) [.187]	--	.319** (.153) [.245]	--	.078 (.052) [.050]	--	.080 (.071) [.083]
Observations	40780	40780	40780	40780	81117	81117	81117	81117

	Panel B: Share in High Crime Demographics							
Indep.Variable	Marijuana Use, 15-19 Year Olds				Marijuana Use, 30 Years & Older			
	Past Month		Past Year		Past Month		Past Year	
Ln (Share Pop. 20.24	-.027 (.110) [.120]	-.177 (.110) [.150]	-.049 (.127) [.169]	-.209* (.142) [.231]	.014 (.040) [.075]	-.015 (.049) [.096]	.044 (.060) [.116]	.010 (.069) [.141]
Ln (Share Pop. 15-19)	--	.394*** (.133) [.225]	--	.417** (.178) [.308]	--	.075 (.065) [.081]	--	.107 (.089) [.120]
Observations	40780	40780	40780	40780	81117	81117	81117	81117

	Panel C: Share 65 & Older							
Indep.Variable	Marijuana Use, 15-19 Year Olds				Marijuana Use, 30 Years & Older			
	Past Month		Past Year		Past Month		Past Year	
Ln (Share Pop. 65 +)	.486* (.290) [.390]	.403 (.263) [.326]	.483 (.341) [.484]	.398 (.308) [.406]	.284* (.148) [.179]	.270* (.151) [.183]	.413** (.188) [.251]	.402* (.192) [.265]
Ln (Share Pop. 15-19)	--	.312*** (.114) [.185]	--	.325** (.155) [.249]	--	.056 (.053) [.062]	--	.046 (.069) (.098)
Observations	40780	40780	40780	40780	81117	81117	81117	81117

*** Indicates significance at the 1% level, ** the 5% level and * the 10% level. Standard errors in parentheses are clustered by division-year and in brackets by division. Tests of significance are based on the division-year level clustering.

Coefficients represent the effect of a 1% change in the youth share on marijuana use. The birth rate (sum of births per person) in division *d* 15 to 19 years earlier is used to instrument for the share of the population 15-19 years old. Estimates are based on pooled NHSDA data. All regressions include the respondent's sex and race, division and year fixed effects, and division-year unemployment rates. Regressions for 15-19 year olds include age dummies and for those 30 years and older age and age-squared variables.

Table A5. Descriptive Statistics for Add Health Regressions

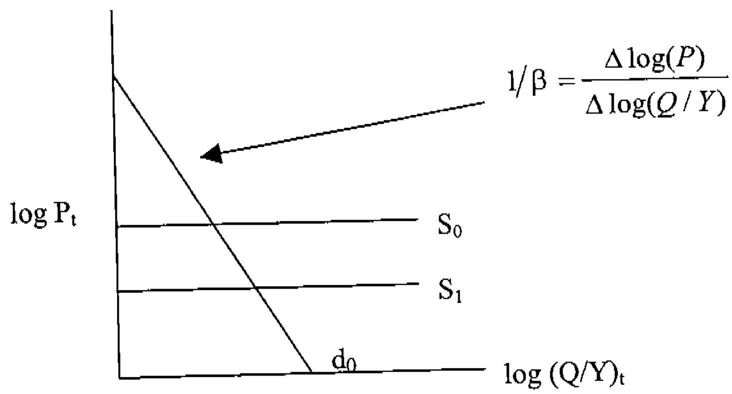
	Full Restricted Use Sample
Percent Male	49.4
Percent Black	23.2
Mean Age	15.2
Number of Siblings in Household	1.46 (1.23)
Age of Biological Mother	41.2 (5.55)
Family Income	45700 (51600)
Number of Students in School	1213 (831)
Ever Used MJ	28.6
Past Month MJ Use	14.4
Suicidal Thoughts Past Year	13.4
Suicidal Attempts Past Year	3.89
Medically Treated Suicide Attempt Past Year	0.96
Neighborhood less teen drug use	56.0
Neighborhood less crime	50.0
Neighborhood affordable	50.7
Number of Observations	20745

Table A6. Description of Add Health Data Used in Analysis of Teen Drug Use

Variable	Respondent	Question
Suicidal Thoughts Past Year	Teen	During the past 12 months, did you ever seriously think about committing suicide?
Suicidal Attempts Past Year	Teen	During the past 12 months, how many times did you actually attempt suicide? (coded as 0,1 in regressions)
Medically Treated Suicide Attempt Past Year	Teen	Did any attempt result in an injury, poisoning, or overdose that had to be treated by a doctor or nurse?
Neighborhood less teen drug use	Parent	You live here because there is less drug use and other illegal activity by adolescents in this neighborhood (Y/N).
Neighborhood less crime	Parent	You live here because there is less crime in this neighborhood than there is in other neighborhoods (Y/N).
Drug Problem in the Neighborhood	Parent	In this neighborhood, how big a problem are drug dealers and drug users? (no, small, big problem)
Parent Drinks Heavily	Parent	How often in the last month have you had five or more drinks on one occasion? (4 or more times coded as heavy drinking)
Smoke	Parent	Do you smoke?

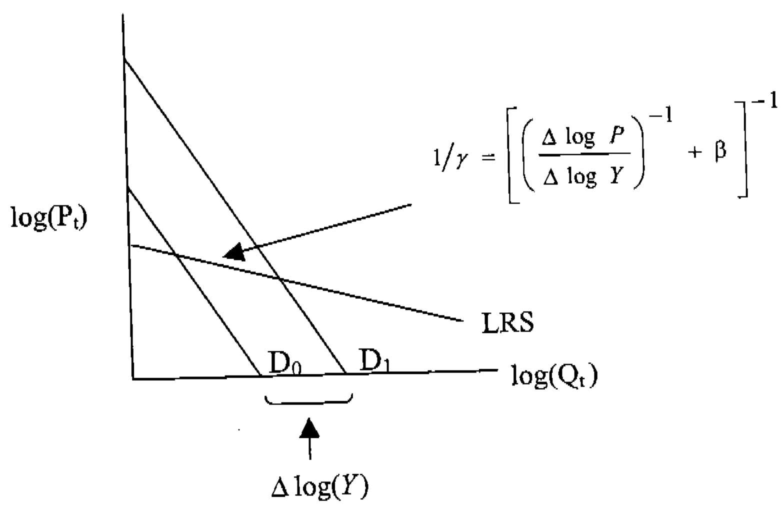
Appendix B: Hypothetical Youth Market for Marijuana and the Implied Elasticities

Panel A: Individual Youth Demand for Marijuana



Notes: P_t is the money price of marijuana, Q_t, quantity in the youth market, and Y_t the population of youth at time t. Since any given youth is a price-taker, supply-side cost-savings due to economies of scale can be depicted as a downward shift in supply, from S_0 to S_1. Assuming youth demand, d_0, is stable, i.e. unaffected by cohort size, the elasticity of youth demand can then be expressed as $\beta = \frac{\Delta \log(Q/Y)}{\Delta \log P} = \frac{\Delta \log(Q/Y)/\Delta \log Y}{\Delta \log P/\Delta \log Y}$. And, given the estimated elasticity of individual youth use with respect to cohort size (1.6) and price with respect to cohort size (-1.88), $\beta = (1.6/-1.88) = -.85$.

Panel B: Market Supply and Demand for Marijuana



Notes: P_t, Q_t, and Y_t are defined as above. Assuming market demand, D_0, shifts out fully by the change in cohort size, $\Delta \log(Y)$, we can express the change in quantity demanded as $\Delta \log(Q) = \Delta \log Y + \beta \cdot \Delta \log P$ and the change in quantity supplied as $\Delta \log(Q) = \gamma \cdot \Delta \log P$, where γ is the price elasticity of supply. Thus,

$$\gamma = \left[\left(\frac{\Delta \log P}{\Delta \log Y} \right)^{-1} + \beta \right] = \left[(-1.88)^{-1} + -.85 \right] = -1.38$$