Chapter 8: Substance Use Prevention

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I. Introduction

The relationship between substance use and labor market outcomes is complex and remains an important public policy issue. Substance use can directly impact labor market outcomes by influencing the likelihood of being employed (both keeping a job as well as getting a job) as well as productivity on the job. Some studies find that substance use may indirectly impact earnings and labor market participation by influencing health, education, marriage, fertility, and job experience (e.g., Kandel, Chen and Gill, 1995; Kaestner, 1999; Fergusson, Horwood, and Swain-Campbell, 2002; Ringel, Ellickson, and Collins, 2006). For welfare participants, substance use may also impede the efforts to complete job training and become self sufficient (Jayakody, Danziger, and Pollack, 2000). Despite a general consensus on the mechanisms through which substance use could negatively influence earnings, there is tremendous variation in the findings from studies examining the effects of substance use on labor market outcomes.

This chapter examines whether substance use prevention programs targeted at adolescents can influence the probability that they will be in poverty as an adult. Since we are not aware of any studies which have directly addressed this question, we draw conclusions from two different literatures: 1) The literature on the effectiveness of programs intended to prevent substance use among adolescents, and 2) the literature on the effect of substance use on educational attainment and labor market outcomes. The next section begins with a discussion of the etiology of substance use and Section 3 presents an overview of interventions intended to prevent adolescent substance use. Section 4 reviews the experimental studies of these prevention programs, with a special emphasis on the long-term outcomes. Section 5 reviews the literature

on how substance use influences labor market outcomes as well as how substance use influences educational attainment. Section 6 summarizes these findings and lists some ideas for future research in this field.

II. Background on the Etiology of Substance Use

According to information from the National Survey on Drug Use or Health, 29 percent of 16 to 17 year old adolescents report use of alcohol in the past 30 days, and approximately one in five (19.4 percent) report binge drinking in the past thirty days (SAMHSA, 2008).[1,2] Rates of illicit drug use are similar to binge drinking rates as 16% of youth ages 16-17 reporting use of an illicit substance (mostly marijuana) in the past month. Given the illegality of alcohol and drugs for this particular age group, the relatively high use rates are often viewed as troubling. When considered within the context of other decisions made by youths at this age regarding finishing high school, applying to college, and engaging in unprotected sex, the relatively high prevalence rates become even more disconcerting.

Figures 1 and 2 illustrate the trends in consumption among 10th grade students from the Monitoring the Future Survey since 1991.[3] Figure 1 shows that the current use rate for any illicit drug (primarily marijuana) in 2007 is below its peak but still nearly 50% higher than it was in 1992. Figure 2 shows that daily use of marijuana in 2007 (2.8%) is also below its peak value (4.5%), but still more than three times as high as its low value in 1991 (0.075%). There have

[1] Binge drinking refers to the consumption of five or more drinks in a single drinking occasion (i.e., within a few hours).
[2] Rates are even higher among 18-20 year olds, were 50.7 percent report drinking in the past thirty days and 35.7 percent report binge drinking in the past month.
[3] The Monitoring the Future survey is a school-based survey of students while the NSDUH is a survey of the household population. By focusing on use rates among 10th graders, we hope to capture those who are still required to stay in school due to their age.

also been fluctuations in the prevalence rates for being drunk over this period, but they have not been as dramatic as the fluctuations in marijuana use.

[Insert Figures 1 and 2 about here]

Rates of initiation for the various substances confirm the notion that substance abuse is often a problem that begins at a very early age. Whether discussing cigarettes, alcohol, or illicit substances, substance use generally begins in early adolescence, peaks during late adolescence and early adulthood (ages 18-25), and then (in the case of illegal substances) diminishes in the late 20s and early 30s (Kandel and Logan, 1984; Johnston, O'Malley, Bachman and Schulenberg, 2005; SAMHSA, 2008). These patterns of use across substances in the general population are amazingly consistent, although the age of initiation can differ in important ways across the substances. For example, cigarettes, alcohol and inhalants are generally substances that are initiated prior to illicit substances and can begin as early as 5th and 6th grade (Johnson and Gerstein, 1998; Chen and Kandel, 1995). As for "harder" substances, the average age of first use among the household population for marijuana was 17.6 years, cocaine and ecstasy was 20.2 years, heroin and pain relievers were 21.2 years and tranquilizers were 24.5 years (SAMHSA, 2008).

Age of initiation is a particularly important indicator of problematic substance use. Numerous studies have shown that early initiates are at greater risk of serious mental illness, poor schooling outcomes, and dependence (Bray et al., 2000; Patton et al, 2002; Wells, Horwood, and Fergusson, 2004; Patton et al., 2007). Indeed simple descriptive evidence from the NSDUH survey confirms the association with dependence. In 2007, 15.9 percent of adults who reported that they initiated alcohol use at 14 years or younger were classified as alcohol dependent or abuse compared to only 3.9 percent of adults who first had alcohol at age 18 or older. Similarly, adults who reported first trying marijuana prior to the age of 15 were classified

with a dependence or abuse of an illicit substance, whereas only 2.7 percent of adults who initiated marijuana after age 18 were. The link between early initiation and subsequent dependence as well as duration of dependence has held up in multivariate analyses of data from the U.S. and other countries (Fergusson, Horwood, and Beautrais, 2003; Pudney, 2004; van Ours, 2006; Patton et al., 2007; van Ours and Williams, 2007).

III. Taxonomy of Interventions to Prevent Adolescent Substance Use

This section highlights the major types of prevention programs for adolescents and the causal mechanisms by which they are supposed to influence substance use. Prevention programs are typically divided into three categories: Universal (for the general population), selective (for those at risk or just beginning to use), and indicated (for those already using). While this is a logical way to think about prevention, from a policy perspective it is also useful to think about prevention in the context in where it is provided (School, Family, and Community). We focus on these categories and include sub-sections on drug testing and brief interventions which are becoming increasingly popular and can occur in any of these contexts.

Since many policies and programs can be construed as having preventive effects, we excluded interventions that were not generally considered drug prevention (e.g., Head Start, retail drug enforcement).[4] This does not mean that investing in these programs will not reduce substance use; indeed, these interventions may be more effective at reducing consumption than more traditional prevention programs.

[4] Drug enforcement can be considered a type of prevention in that the goal of is to raise prices (by increasing the risk to dealers; Reuter and Kleiman, 1986) and deter future consumption (by increasing the price and the expected sanction of using; Becker, 1968; Moore 2001).

School-based programs. The vast majority of middle school students receive some sort of school-based training designed to reduce short-run and long-run demand for alcohol, tobacco, and illegal drugs (Gottfredson and Wilson, 2003). Prevention programs sometimes target elementary and high school students and/or include booster sessions. Decisions about the types of programs are usually made at the school-district level, where officials are often courted by vendors for particular programs.

School-based programs can generally be classified into three categories: Information only, skill building (understanding social influences and learning how to respond to different situations), and normative education (change perceptions about substance use norms) (Manski et al., 2001). Programs are taught by teachers, peers, outside speakers (e.g., police officers, trained health educators), or some combination of these. Sessions can occur within classrooms or in auditoriums with the entire student body.

Family-based programs. These programs generally focus on families with a child or parent who is currently using drugs or is at high risk. Interventions can range from psychotherapy to programs intended to improve intra-family communication and promote a discussion about the consequences of consumption. Sessions can occur in a variety of locations, including a physician's/therapist's office or inside the home. It is also important to note that many school-based prevention programs include components intended to extend the discussion about substance use to the home.

Targeted community-based programs. Community based programs can be differentiated into two groups: targeted programs and universal programs. Targeted programs are those which target a specific population that may be at particularly high risk for drug use and abuse. Mentoring programs, sponsored by community organizations like the YMCA and Big Brothers Big Sisters, are one such example. Mentoring programs are intended to promote

healthy relationships and accomplish many of the goals of school-based prevention by offering positive role models to disadvantaged or high-risk youths.[5] It is also the case that participation in these programs reduces the amount of time that adolescents spend by themselves or with substance-using peers, and hence some of their effect may be through channels not originally anticipated.

Other types of programs that also fall into this category include drug treatment and criminal justice interventions. Community-based drug treatment in many respects is just an indicated prevention program focused on relapse. Since this chapter is intended to focus on more traditional prevention programs, we do not review the literature on treatment programs.[6] Criminal justice interventions aimed at those youth who get arrested for alcohol or drugs or believed to have substance use problems are another form of targeted prevention. The range of prevention programs targeted at high risk kids vary from drug education, treatment diversion programs, and boot camps. For those arrested for drunk driving, many states require that offenders complete a "DUI School" before having their driving privileges restored. These programs can require weekly group meeting as well as individual sessions, and at least one state (Nevada) allows offenders to take the classes entirely on-line.

Universal community-based programs. This category includes programs and policies that provide universal coverage to all individuals living within a community, regardless of their risk of use. Examples of universal programs include mass media campaigns, restrictions on sales to minors, policies raising the price of legal and illicit substances, and advertising restrictions. All of these approaches represent community-wide attempts to prevent the initiation of or reduce use of alcohol, tobacco, and/or illicit drugs.

[5] The popularity of these programs has grown six-fold in the past ten years and it is now estimated that over 3 million adolescents currently have adult mentors (Rhodes, 2008).

This category also includes multi-pronged community-level initiatives intended to provide comprehensive strategies for reducing substance use. For example, Holder et al.'s (1997) community project included five components in one city: community mobilization, responsible beverage service, reducing drinking and driving, reducing underage drinking, and reducing alcohol access.[7] One justification for such an approach is that the synergies can be achieved and may improve the salience of an overall message or effect when done as part of a comprehensive effort. While this chapter focuses on specific interventions rather than attempts to combine many interventions, it is important to recognize that the reported effectiveness of an initiative may differ if it is part of a larger program.

Drug and alcohol testing. Although offered by the Office of National Drug Control Policy as a prevention program in and of itself, drug and alcohol testing generally can occur in any type of setting or as part of any specific program and hence we thought it appropriate to consider it separately. It is important to note that simply detecting whether someone is using alcohol or drugs does not influence consumption itself; a change in behavior depends on how that information is used. If an individual believes there will be sanctions associated with testing positive, this could influence use if the expected sanction is larger than the expected benefit of consumption (Becker, 1968). If detection forces someone into treatment or into a fruitful discussion with a caring adult, this may also influence future consumption.

Although most frequently used by employers and criminal justice agencies, drug testing is becoming increasingly popular in school settings. Studies from the late 1990s suggest that between 9-16% of high schools had testing programs (MacCoun, 2007) and there was a strong effort by the Bush Administration to increase the number of programs (Office of National Drug

[6] For reviews of the treatment literature, we refer readers to Manski et al. (2001, Chapter 8) and NIDA (1999).
[7] Another example is Komro et al.'s (2008) *Project Northland Chicago*, which included four components: Curricula,

Control Policy, 2002). Additionally, the Supreme Court ruled in 2002 that it is legal for schools to require students participating in all extra-curricular activities be subject to random drug testing, not just athletes.

Brief interventions. Often rooted in motivational interviewing, the goals of brief interventions (BIs) are to help the users identify problem use and enhance their motivation to change this behavior (Tevyaw and Monti, 2004). Depending on the setting, BI can be administered to an individual or group, as one session or a series. While BI is often associated with primary care settings and emergency rooms, there are a growing number of interventions targeted at college students (e.g., see review in D'Amico and Stern, 2008). The model has recently been adapted to several different settings for adolescents, including schools (D'Amico and Fromme, 2000; 2002; D'Amico and Orlando, 2005), shelters (Baer et al., 2004; D'Amico et al., 2006b), and teen courts (D'Amico et al., recently funded).

IV. Review of the Long Term Effects of Drug Prevention

There are many reviews and meta-analyses of the short-term impact of a variety of prevention programs on drug use (e.g., Manski et al., 2001; Caulkins et al., 2002; Gottfredson and Wilson, 2003; Faggiano et al., 2005; Gates et al., 2006). Most conclude that skills-based prevention programs (in schools or elsewhere) appear to be effective at deterring early-stage drug use, by delaying initiation and reducing the frequency of tobacco, alcohol and marijuana use among young adolescents, although the effect sizes are small (Faggiano et al., 2005; Manski et al., 2001).[8] A much smaller literature exists examining the longer term effects of drug

family interventions, youth-led community service projects, and community organizing.

[8] The NRC panel was pessimistic about much of the prevention literature: "At least 20 reviewers and meta-analyses of drug prevention programs were published during the 1980s and 1990s. The most recent of these generally conclude that substance abuse prevention efforts are 'effective' for preventing substance use, in the sense that the studies reviewed report statistically significant differences between subjects receiving and no receiving the

prevention and whether program effects observed between 7th and 8th grade or even earlier are sustained longer than a year.

Our review focuses on prevention programs that target adolescents, have been evaluated in randomized-controlled trials, include follow-up information at least 12 months out, and were evaluated in the United States. To find longer-term evaluations of randomized controlled trials, we conducted our own literature reviews and heavily relied on the reviews by Skara and Sussman (2003), Foxcroft et al. (2003), Faggiano et al., 2005; Gates et al., 2006, and D'Amico and Stern (2008). We report information on alcohol, drug, and tobacco use as well as information about attrition. As mentioned in the previous section, we will not present evidence about treatment, excise taxes, and law enforcement practices. We will also not consider multi-component community-level prevention since is it is extremely difficult to identify the main mechanism driving the change (the message itself, parental involvement, accountability, etc.). Further, we do not include brief interventions since we are unaware of studies examining long term outcomes for adolescents (Gates et al., 2006; D'Amico and Stern, 2008).[9] Finally, we do not consider mass media campaigns given the difficulties of evaluating them using an experimental approach (Hornik et al., 2003a; 2003b).

The vast majority of evaluations reviewed here do not include outcome information related to education and employment because they are usually not the primary outcomes evaluated in these interventions. We do report this information when it is available. We also include a summary of long term effects of the non-experimental Seattle Social Development

preventive intervention on some measure of substance use, at least immediately following the termination of the prevention activity, and in rare cases months or years beyond that point. However, certain practices in the reporting of original research and in the summaries of these findings have tended to overstate the effectiveness of prevention activities" (Manski et al., 2001, 213).

[9] D'Amico and Stern (2008) note that one reason for the lack of longitudinal data is that this type of intervention is relatively new for adolescents.

Program since it includes rich information on substance use as well as on work and school outcomes.

School-based programs. Table 1 presents the results from long-term evaluations of randomized controlled prevention experiments with schools or students. This table only focuses on experiments conducted in the United States and is limited to studies with a minimum follow-up time of one year. For programs that have been studied at multiple waves, we only present the information from the latest wave since we are primarily interested in whether program effects can be sustained over time.[10]

[Insert Table 1 about here]

There is no evidence suggesting that school-based prevention programs have any long term effect (>5 years) on marijuana use. While some of these programs do appear to have an impact several months after the intervention (e.g., AAPT, ALERT), the six year follow-up for ALERT and Life Skills as well as the four/five year follow up for Project Towards No Drug Use (TND) showed no effect on marijuana use. While Project Towards No Drug Use did find a small effect on hard drug use at the four or five year evaluation, the authors note that this effect should be interpreted cautiously.[11]

The effect of these programs on the long term alcohol use is slightly better. The evaluation of Life Skills found no effect on frequency of use, but that it did reduce the probability of being drunk in the previous 30 days at the six year follow up. An evaluation of the Adolescent Alcohol Prevention Trial (AAPT) using five waves of data and latent growth curve modeling produced more promising results (Taylor et al., 2000). The analysis found that the 7th

[10] The one exception is AAPT where the five-year evaluation did not include information about marijuana use; thus we include information from the one year follow-up (Hansen and Graham, 1991).
[11] The authors note: "Because we did not find the significant maintenance effects for 30-day hard drug use from year 2 and 3, this might only be a random finding or a finding reflecting a program that was successful only in those who were retained in the follow-up sample" (191).

grade program had a beneficial effect on lifetime alcohol use, recent alcohol use, and lifetime drunkenness through the 11th grade. The authors also found that those randomly assigned to the normative education program had lower rates of growth for self-reported alcohol use. The long-term evaluations of ALERT and TND found no effects on alcohol.

The results with respect to smoking appear to be very program specific. The Hutchinson Smoking Prevention Program is based on the social influence model and includes 65 sessions over the course of 4th-10th grades (Peterson et al., 2000). The authors were able to follow up with 94% of the original sample at two years after high school and found that the program had absolutely no effect on daily smoking or other smoking outcomes. Projects ALERT and TND also did not find any long term effects of prevention on smoking behaviors; however both of these programs had higher attrition rates (57% and 46%, respectively) that may have influenced these results.

The six year wave of Know Your Body (2 hours per week of teacher instruction for every school week from 4th through 9th grades; 384 total sessions) found that 13.1% of the control group initiated use in 9th grade compared to 3.1% of those assigned to the intervention (Walter et al., 1989). However, it should be noted that they were only followed up with 65% of those surveyed at baseline and the evaluation was conducted at the end of the program (9th grade). Thus, it is unclear whether these effects persist after the program is over and how attrition influences the results. The six year Life Skills evaluation included 61% of the initial seventh-grade sample and also found that it decreased smoking. Finally, AAPT noted a significant effect on cigarette use for those receiving the normative education program.

There is a long term evaluation that did not utilize a randomized controlled design that should be mentioned because it directly assessed the impact of the intervention on schooling outcomes. The Seattle Social Development Project focused on teacher training, skill

development for students, and parent training. There were three conditions: "Full"—at least one semester of intervention in grades 1-4 and at least one semester of intervention in grades 5-6; "Late"—at least one semester of intervention in grades 5-6; and the "Control" received no intervention. Hawkins et al. (2005) were able to follow up with 94% of the participants (n=605) when they were 21 years old (9 years after the intervention). While they did not find noticeable effects on substance use,[12] they did find statistically significant effects in terms of functioning at school or work at age 21 among the treatment group. The authors attempt to reconcile the contradictory findings by stating that "it is also possible that at 21 years of age, the use of various substances is relatively normative, even among those progressing positively in the domains of school and work" (Hawkins et al., 2005; p. 30). If this argument is correct, it may be the case that the programs highlighted in Table 1 do influence human capital development even if they have no noticeable long-term effect on substance use.

It is also important to acknowledge that even if these school-based prevention programs do not have a long-term effect on consumption, the fact that they delay initiation of particular substances for some students may influence subsequent educational attainment and labor market outcomes. Using two-to-three year follow-up data from a variety of middle school-based prevention evaluations (both randomized and non-randomized), Caulkins et al. (2002) calculated the initiation effects of a composite, hypothetical "best practice" prevention program on alcohol, tobacco, and marijuana use (Table 2).[13] They estimate that a one percent reduction in substance use observed at the end of a good prevention program could generate anywhere from a 14 to 51 percent reduction in lifetime quantity consumed of that substance, depending on the substance

[12] Specifically, they note that the full-intervention group participants "were also less likely to have used a substance in the recent past (alcohol or tobacco in the past month or any other illicit drug in the past year), but this finding did not achieve statistical significance (p=0.09). Subsequent analyses examining different substances separately found no significant effects of the full- or the late-intervention condition, compared with controls, for past month alcohol or tobaccos use or for past year marijuana or other illicit drug use" (29).

targeted by the prevention program. Even if the true effect on initiation is indeed the lower bound, this is not negligible.

[Insert Table 2 about here]

A further point to consider about these programs is that school-based drug prevention programs are relatively cheap to implement on a per child basis. A study by Aos et al. (2004) reports the cost of a variety school-based substance abuse prevention programs and compares these program costs to that of a variety of general prevention programs, community initiatives and mentoring programs. They show that the per child cost of many of the school based programs just mentioned range from a low of ~$5 dollars per child (for Project ALERT and TNT, respectively) to a high of only $112 (for DARE).[14] In all cases, however, the costs exclude the cost of teacher's time getting trained and preparing lessons as well as the opportunity cost of the time that could have been used teaching alternative subjects. Most of the school-based programs are on the lower end of the range, with Life Skills and Start Taking Alcohol Risk Seriously (STARS) costing $33 and $20, respectively. The one exception was the Seattle Social Development project, for which they report a per child cost of $5,172, but this program includes a teacher training and parent training on top of the interventions planned in early and later grades.

The fact that the average cost per child of implementing these programs is so low means that the programs do not have to have very large outcome effects in order to have a benefit-cost ratio greater than one. Caulkins et al. (2002) point out in their assessment of an ideal school-based drug prevention program that 95 percent of the time the benefits are more than twice the cost of actually implementing the program on a per child basis, even when programs are assumed

[13] The authors considered four approaches for generating these ranges and fully acknowledge the limitations.
[14] Aos et al. (2004) report all figures in $2003 and we converted them to $2007.

to have small effects. Of course, the fact that something creates cost savings does not mean that it is desirable to implement, as it depends on several factors, including the relative cost-effectiveness of other approaches that could be used to achieve the same end.

Family-based programs. As previously noted, many prevention programs include a school and home component. The latter can be as passive as sending anti-drug materials home to something as active as proving parenting training about how to talk to adolescents about substance use. This section focuses on those interventions that primarily focus on the family, and like the earlier section, we focus on the latest wave of outcome data.

The program that receives the most attention in the review literature is the Iowa Strengthening Families Program (ISFP). The program lasts seven weeks with parents and students meeting for two hours per week with trained program staff. In the first hour the parents and students are separated for their sessions, and in the second hour they are all brought together. Sessions primarily focus on parenting skills, peer resistance, and communication skills. Schools were randomly assigned to ISFP or a minimal contact control condition and "nonlinear growth curve analyses were conducted with school-level outcome variables aggregated over the available respondents in each school . . ." (Spoth et al., 2004). Analyses based on the six year follow up (for students with a data available at all waves, case-wise deletion was used for those missing any information) found that time to alcohol use without parental permission, drunkenness and cigarette use was significantly longer for those assigned to ISFP ($p<0.05$).

As part of the evaluation, Spoth et al. (2004) also randomly assigned some schools to a related program called Preparing for the Drug Free Years (PDFY), which primarily focuses on the parents separately and includes fewer sessions. Compared to the same control group, there was no difference in time to initiation for any of the substances for those assigned to PDFY, but there was a statically significant difference in growth rates for tobacco use ($p<0.05$).

The Focus on the Family program is intended to prevent substance use for children of heavy drug users in treatment (Catalano et al., 1999). The program included 33 hours of parental skills training and 9 months of home-based case management. A total of 140 adult methadone patients (and their 178 children, ranging from 3 to 14 years) were recruited and either assigned to the intervention or a no-intervention control. The program did not have much of an effect on the children at the one-year follow up interview, but it did have an effect on parental drug use.

Family based programs are generally more expensive per youth than school-based prevention programs because they involve more people and management. For example, Aos et al. (2004) report that the average cost per youth of the Iowa Strengthening Families Program was $959, which is substantially higher then the school based prevention programs mentioned before, but lower in cost than the Seattle Social Development Project, which included a school and family component.

Community-based programs. In addition to the benefits discussed in Chapter 6, participation in the Big Brothers Big Sisters (BBBS) mentoring program has a positive effect of substance use outcomes. Grossman and Tierny (1998) report that the adolescents assigned to the treatment condition were 46% less likely to initiate drug use and 27% less likely to initiate alcohol use during the 18-month follow period (information was not reported for the intensity of use). There are at least two reasons to believe that this study may underestimate the effects of mentoring. First, the authors present intent-to-treat results and nearly 20% of the treatment group was not matched to a mentor. Second, it is not clear whether those in the control group were mentored somewhere else, thus possibly diluting the treatment effect.

An experimental evaluation of a related BBBS mentoring program based in schools did not yield the same results (Herrara et al., 2007). Utilizing a similar waiting list approach with over 1,000 students in ten sites across the country, the study found only one major difference

between the control and treatment groups at the 15-month follow up: Those in the treatment group were less likely to skip school and more confident that they would attend and complete college.[15] They attribute the lack of effect to attrition from the mentoring (many students switched schools) and noted that the mentors did not as much time to cultivate relationships as they did in the previous BBBS community intervention.

Aos et al. (2004) estimate that the average cost of BBBS to taxpayers is $1,392 per youth participant; however, this does not include the opportunity costs associated with being a mentor or a mentee. We refer readers to Chapter 6 of this volume for a more detailed discussion of these costs and note that the benefits of the community-based BBBS extend beyond a decrease in the initiation substance use.

Drug testing. There are very few studies of the effectiveness of student drug testing (MacCoun, 2007), and we are only aware one using randomization to learn whether student-athlete drug testing influenced substance use among student-athletes (Goldberg et al., 2007). The researchers recruited 11 schools near Portland that wanted to start student-athlete drug testing programs. Five of these schools were randomly assigned to implement testing programs and the other six were assigned to defer implementation until the study was completed (653 student athletes in testing schools and 743 student athletes in the deferred testing schools). Substance use was serially assessed with voluntary, confidential questionnaires. The results of the two-year prospective study were mixed: Testing did not influence past-month drug use among student-athletes, but "prior year drug use was reduced in two of four follow-up self reports, and a combination of drug and alcohol use was reduced at two assessments as well." Even though this study used randomization, we cannot draw strong conclusions from these

[15] Specifically, they note: "We did not see benefits in any of the out-of-school areas we examined, including drug and alcohol use, misconduct outside of school, relationships with parents and peers, and self-esteem…"

results. In addition to high attrition levels, five of the 11 schools were removed from the study and two of the schools altered their testing programs during the study.

The California Youth Authority's (CYA) drug testing experiment in the early 1990s has also generated mixed results. Nearly 2,000 young parolees (12-24 years old, mean 19 years) were randomly assigned to one of five different levels of testing (including no testing), and graduated sanctions were supposed to be applied to those testing positive (Haapanen and Britton, 1998). Those assigned to higher levels of testing were more likely to be arrested for a violent crime and less likely to have a "good" parole outcome at 24 months post–release (Haapanen and Britton, 2002). The study could not address the causal effect on drug use since self-reported drug use information was not collected, although the authors noted that parolees assigned to lower levels of testing were more likely to test positive. Implementation problems plagued the study, and the authors note that this finding could be attributable to targeting of drug tests in the lower testing groups.

An analysis of this experiment focusing on human capital outcomes and accounting for non-compliance found that parolees randomly assigned to testing were more likely to be employed or in school in the month after being released from prison, with the effect being large for Hispanics and non-existent for Blacks (Kilmer, 2008). The lack of long-run employment and schooling data for these parolees makes it difficult to reconcile these findings, but it suggests we have more to learn about the heterogeneous and dynamic effects of this ubiquitous drug prevention intervention for at-risk youth.

Drug testing costs vary greatly depending on the testing method, the substances being tested, and whether the entity purchasing the test receives a quantity discount. For example, urine tests conducted by criminal justice agencies can cost less than $5 per test while over-the-counter hair tests can exceed $60. Full cost evaluations of testing programs should also include

the costs associated with addressing positive tests (e.g., additional probation supervision, admission to a treatment program).

V. Review of the Literature on Substance Use, Educational Attainment, and Labor Market Outcomes

Considerable attention has been given by social scientists to the impact of substance use on worker productivity and labor market outcomes. Substance use is believed to diminish a worker's productivity and lead to poor labor market outcomes for several reasons.[16] First, it may delay initiation into the work force, thereby reducing experience and human capital accumulation associated with on-the-job training (Johnson and Herring, 1989). Second, it may decrease the probability of being employed which, again, may interfere with human capital accumulation (Gill and Michaels, 1992; Register and Williams, 1992). Third, it may increase absenteeism which directly influences the productivity of not only the worker, himself, but also those individuals who work with him (French, Zarkin, and Dunlap, 1998). Finally, substance abuse may reduce an individual's productivity at the job, which should translate directly into lower wages if wages are indeed a good indicator of the worker's marginal productivity. Empirical studies that analyze the direct effect of substance use and abuse on earnings have generated very mixed findings, however. Even after accounting for the endogeneity of substance use, earnings of substance users are found to be higher by some researchers (Kaestner, 1991; Gill and Michaels, 1992; Register and Williams, 1992; French and Zarkin, 1995; Zarkin et al., 1998), and lower by others (Mullahy and Sindelar, 1993; Kenkel and Ribar, 1994; Burgess and Propper, 1998). The lack of a robust finding has led many economists to focus on other measures of a

[16] There is also research examining whether income-support programs encourage substance use by providing resources to support a drug habit, and the evidence is mixed (Phillips et al., 1999; Shaner et al., 1995; Rosen et al., 2006; Chatterji and Meara, 2007).

worker's productivity, such as the probability of being employed or unemployed (Kandel and Davies, 1990; Register and Williams, 1992). Here, too, the evidence is mixed. Using the 1984 and 1985 waves of the NLSY, Kandel and Davies (1990) find that use of marijuana and cocaine in the past year is positively associated with the total number of weeks unemployed. However, Register and Williams (1992) find using data from the 1984 wave of the NLSY that use of marijuana on the job in the past year and long term use of marijuana both have a positive impact on the probability of being employed. General use of marijuana, on the other hand, did lower the probability of being employed.

The lack of a robust finding is driven by a number of factors. First, studies examine the impact of substance use on earnings and labor market outcomes for populations of varying ages. While some studies focus on young adults (Kandel and Yamaguchi, 1987), others focus on mature young adults (Kandel and Davies, 1990; Register and Williams, 1992) while others focus on the full adult population (Zarkin et al., 1998). It is quite possible that the nature of the relationship between substance use and labor market outcomes changes over the life-cycle as job market experience and job tenure begin to dominate the effects of other individual determinants of labor market outcomes. Indeed a few studies have explicitly considered this fact and noted the differential effects of substance use on wages conditional upon age (Mullahy and Sindelar, 1993; French and Zarkin, 1995), but it is not a factor that is consistently considered in the literature.

A second factor complicating the interpretation of findings from the literature is the inconsistent treatment of indirect mechanisms through which substance abuse could impact earnings, for example through educational attainment, health, fertility and occupational choice. Given that these inputs have been established as important determinants of labor market participation and wages (Becker, 1964; Mincer, 1970; Willis and Rosen, 1979), and that there are

strong findings in the literature about associations with each of these (Chatterji, 2006; Bray et al., 2000; Kenkel and Wang, 1998; Cook and Moore, 1993; Mullahy and Sindelar, 1994), it is important to consider whether analyses looking at the impact of substance abuse on earnings consider the indirect effects as well.

Finally, the literature is pretty inconsistent in terms of its definition of substance use. "Current" use has been define as daily use (Kandel and Yamaguchi, 1987), use in the past month (Cook and Moore, 1993; Chatterji, 2006) and use in the past year (Kandel and Davies, 1990; Register and Williams, 1992; Mullahy and Sindelar, 1993). A few studies attempt to differentiate the effects of chronic use from casual use (Kenkel and Ribar, 1994; Roebuck, French and Dennis, 2004) or to proxy chronic use with measures of early initiation (Bray et al., 2000; Ringel et al., 2006). Given all the different ways that substance use can be operationalized, with some representing more chronic or persistent use while others represent more casual use, it is not surprising that findings vary across the studies.

It is clear that the relationship between substance use and abuse and labor market outcomes is dynamic and can be potentially influenced by the relationship between early substance use and human capital production. The potential for reverse causality is also real. Just as substance use and abuse can lead to job separations and other poor labor market outcomes, job separations may lead to increased substance use and abuse. Statistical methods used to date to try to separate out these two effects include event history analysis (e.g., Kandel and Yamaguchi, 1987; Ringel et al., 2006), fixed effects modeling (Cook and Moore, 1993; Kaestner, 1994a and 1994b; Kenkel and Ribar, 1994) and instrumental variable techniques (e.g., Bray et al, 2000). The problem, however, is not purely statistical in nature. At least some of the problem stems from how and when substance use is measured.

In Table 3 we highlight studies that have attempted to deal in a significant way with the particular issues relevant for understanding the link between substance use and employment, earnings and schooling. The table is far from exhaustive, as there are many more studies that have been done in this area. However, these particular studies represent major steps forward in the literature in attempting to deal with the statistical and measurement problems so as to get a cleaner estimate of the causal associations.

[Insert Table 3 about here]

The top of Table 3 focuses on studies examining the relationship between substance use and employment or earnings. For most of these studies, the major issue has been dealing with the simultaneity of current substance use and current labor market outcomes. The main approach for dealing with the problem is to employ instrumental variable (IV) methods. The difficulty comes in trying to identify appropriate instruments, and the particular choice of instruments (religiosity, family stability, non-earned income or illegal acts) used in the first few studies could all be viewed as problematic. The Register and Williams (1992) study, however, remains particularly insightful because it was the first (and as far as we know only) study that differentiated the effects of on-the-job substance use from off-the-job substance use. Indeed, they find that in the case of marijuana, off-the-job use was positively associated with earnings, while they find on the job use and long term use to be associated with lower wages. They interpret their results as evidence that recreational marijuana use may help reduce stress in a fashion similar to moderate alcohol consumption. However, only current substance use was instrumented, so endogeneity issues are still likely influencing the model due to the endogeneity of on-the-job use and chronic use in the past.

The results from the two Kaestner (1994a and 1994b) studies demonstrate how substance abuse can differentially influence different aspects of labor market outcomes even for the same

population being considered. In the first study, for example Kaestner (1994a) finds a positive effect on cocaine use on earnings for young adult women, but in his second study (Kaestner, 1994b) he shows no effect of the same measure of cocaine use on women's hours worked. Interestingly, Mullahy and Sindelar (1996) also show a positive effect of heavy drinking on women's labor market participation suggesting that women with a serious substance abuse problem may be more tied to the labor market.

Findings for men are far less clear. Neither of the Kaestner (1994a, 1994b) studies shows a consistent result for the effects of cocaine on employment or wages, nor does Register and Williams (1992). However, DeSimone, using the same data set as Kaestner, does find a large statistically significant negative association between marijuana and cocaine use and labor market participation for men. DeSimone emphasizes in his work the importance of excluded instruments satisfying overidentification and exclusion restrictions, which he argues are not met by Kaestner's instruments. DeSimone also employs more proximal measures of substance use, capturing frequency of use in the past year rather than in the lifetime. However, DeSimone does not account for a number of intermediate mechanisms through which substance use might impact labor markets, most notably labor market experience and marital status. So, the omission of these intermediate factors may also be contributing to the finding of a large effect.

The Mullahy and Sindelar (1996) paper, which was the last in a series of papers they coauthored examining alcohol dependence and labor market outcomes, was one of the first to carefully test the validity and appropriateness of the instruments used for estimation and consider the extent to which the relationship between alcohol dependence and labor market outcomes might differ over the life course. In another paper they show that the relationship between alcohol dependence and earnings clearly differs by age (Mullahy and Sindelar, 1993), but in this

study they find no statistically significant or consistent evidence that alcohol dependence influences employment or unemployment, at least for men.

Another question is whether alcohol dependence might influence the types of jobs people get, not just whether or not they work. Kenkel and Wang (1998) use data from the 1979 NLSY to compare job attributes of alcoholic and non-alcoholic men. They find that male alcoholics are less likely to be in white-collar occupations, less likely to receive a variety of fringe benefits, and they tend to work for smaller firms than their non-alcoholic counterparts. They further find, however, that alcoholic men who work in white-collar occupations earn about as much as non-alcoholics, while alcoholic men working in blue-collar professions earn about 15% less on average. If substance users are able to self-select into specific job occupations that enable them to continue their substance use, then studies that examine the relationship between substance use and wages and other measures of productivity may be biased because they are attributing differences to substance use behavior instead of characteristics of the job.

Finally, using data from the 1991 and 1992 National Household Survey on Drug Abuse, Zarkin et al. (1998) examine the effects of current use (i.e., in the past 30 days) and previous substance use on hours worked in the past month. They use an instrumental variables approach, identifying instruments within the survey using information on self-reported risks and availability. They estimate the models separately for 1991 and 1992, both years representing an independent nationally representative cross-section of U.S. households. In general they find no consistently significant effect of any of the substances examined. However, their results for light marijuana users (those reporting use of 1 to 3 joints in the past 30 days) were particularly interesting. Using the 1991 cross-section, they found that light marijuana use was statistically significantly associated with working *more* hours (42 hours more than nonusers). However, using the exact same method, same controls, and the same measure of substance use with the

1992 cross-section, they found that light marijuana use was statistically significantly associated with working *fewer* hours (41 fewer hours than nonusers). The authors interpret these completely opposite results despite the same methodology and controls as evidence of the necessity to continue investigating the relationship and the need for careful inspection of models that get estimated.

Schooling outcomes, which are shown in the latter part of Table 3, are of particular interest because of their close proximity to the delivery of adolescent prevention programs and because educational attainment is such an important factor for labor market outcomes. As in the literature just reviewed on earnings and employment, much of the focus of the schooling literature in economics remains focused on the identification of causal effects, but a much more serious debate over the proper tools for identification has ensued (Cook and Moore, 1993; Dee and Evans, 1997; Chatterji, 2006). For example, Cook and Moore (1993) use cross-state variation in the minimum legal drinking age and beer taxes, two significant predictors of adolescent drinking behaviors, to identify the causal effects of teen drinking on educational attainment. They find that after controlling for socio-demographic factors and family environment, high school seniors who are frequent drinkers complete 2.3 fewer years of college compared to seniors who are not frequent drinkers. Dee and Evans (1997), however, contend that the approach employed by Cook and Moore is flawed because it relies on cross-state variation from a single year. They contend that a study of variation in state regulations over time is needed to distinguish these effects from other state-level factors that might affect educational attainment, such as state expenditures on education. Using matched cohorts from the Monitoring the Future Survey and 1990 Public-Use Microdata Sample, they use two-sample instrumental variables technique and find that teen drinking has no independent effect on educational attainment. A limitation of their analysis, however, is that cohort effects could be driving their

null finding as opposed to the true relationship between schooling and educational attainment because they do not examine the same individuals over time.

The debate regarding the usefulness of instruments and IV approaches was addressed again by Chatterji (2006) who used data from the 2000 National Educational Longitudinal Study (NELS:88) to model educational attainment at age 26 conditional upon current alcohol use in 10^{th} and 12^{th} grade. Chatterji employed a bivariate probit technique to simultaneously model substance use and specific educational outcomes (high school completion, GED, some college or college completion – each separately) and used as additional instruments state beer taxes and the percentage of the state population living in dry counties. She finds through diagnostics of these instruments that they perform poorly as instruments and decides instead to explore plausibility of a causal relationship using Altonji's et al (2005) bounding technique. She finds no evidence supporting a causal association using this method. Other strategies have emerged to try to deal with the problem of identification of causal associations in this strand of the literature. Bray et al (2000), for example, use information from a longitudinal survey and assess whether the age of first use of alcohol, cigarettes, marijuana, and other illicit drugs are statistically associated with the probability of dropping out of school. This sort of prospective approach relies heavily on the notion that early use of these substances (prior to age 16) is highly correlated with dependent use later. While this notion is well supported in the literature, it may also be the case that adolescents who are able to initiate at a young age have environmental or personal factors that may make them less likely to complete high school as well (i.e., less parental supervision, bad peers, etc.). They attempt to control for some of these factors, and find that marijuana initiation in particular is positively related to dropping out of high school, although the magnitude and significance varied in a non-linear fashion with age. What is perhaps most surprising about the study is that early initiation of the other substances was not negatively and statistically associated

with high school drop out status, which raises serious questions as to whether it is truly the drug use that is being picked up by these measures or something behavioral.

McCaffrey et al (2008) try a completely different approach for evaluating the relationship between substance abuse and schooling. Using a very rich set of panel data from the Project ALERT evaluation, they examine whether persistent and heavy marijuana use over the 10th and 12th grade is associated with high school completion using propensity score weighting. They also consider separately the effect of drinking through a continuous quantity-frequency measure of use in the past month and year. They are able to obtain baseline information on the adolescents in 7th grade, before the kids participated in the Project ALERT drug prevention curriculum, and account for a variety of observable differences. When they conduct analyses that simply correct for baseline differences between the group, including participation in the ALERT program, they find that heavy and persistent marijuana use is still positively associated with high school dropout (odds ratio of 2.3). However, when additional time varying measures are added to a propensity score weighted regression, they find that the statistically significant association between marijuana use and schooling disappears, suggesting that much of the observed association between marijuana use and high school completion can be explained by peer influences and family bonds. The study does not speak directly to the issue of causality, but provides interesting evidence of the mechanisms through which marijuana use might be indirectly associated with schooling.

Overall, the findings remain fairly mixed in terms of the effects of substance use on schooling as well as earnings and labor force participation. While methods that attempt to deal with the endogeneity of substance use generally lead to a reduction in the observed association, the studies using these methods also have problems and instruments have subsequently been found to be either weak or invalid. Thus, the literature continues to evolve to explore these

relationships, in part because the negative associations remain so strong in observational data and studies are so inconsistent in their treatment of mechanisms through which substance use is allowed to affect the outcomes.

VI. Summary and Next Steps

This chapter reviews the literatures on the effectiveness of substance use prevention and the effect of substance use on education, employment, and earnings. While there is a fair amount of evidence suggesting that prevention programs for adolescents have short-term effects on consumption, there is very little evidence suggesting these effects remain through high school. But as noted by Caulkins et al. (2002), short-lived program effects can still potentially translate into important changes in terms of lifetime substance abuse. Research also shows that delaying the age in which a substance is initiated can have a large effect on the probability of becoming dependent and the duration in which the substance is used (Douglas, 1998; van Ours, 2002; Patton et al., 2007). This can have implications for future employment opportunities and earnings.

The economic literature on the casual effect of substance use on education attainment and labor market outcomes is notoriously mixed. While we discussed many factors that have contributed to this situation, perhaps the most important one to keep in mind is how substance use and abuse gets defined in these studies. While legal prohibition implies that substance use is bad, it is not clear why any use of a psychoactive substance would automatically translate into a poor labor market outcome or increased poverty. Indeed, in the case of alcohol, there is strong evidence to show that a linear relationship cannot be expected. Clearly, the level of consumption, duration of consumption, and timing of consumption all have important implications in terms of whether we should expect to see an impact on employment or earnings.

Another important factor relevant for studying the association between substance use and earnings is the extent to which statistical models are correctly specified for identification of mechanisms that are being considered. While the literature suggests that chronic substance abusers are less likely to be employed, it also suggests that chronic substance abusers are less likely to finish school and more likely to engage in crime. Analyses of the effects of substance abuse on later life-cycle outcomes needs to carefully consider the indirect mechanisms through which substance use might also influence those outcomes. The potential for endogeneity biases, caused by unaccounted for differences in ability, anti-social disorders, deviance, mental health problems, rates of time preference, or some other unobserved factor, to impact results abound and few studies have adequately dealt with all of this.

Attempts to try to tackle these econometric issues should be given serious attention not just so that economists can have a better understanding of the causal connections. Information is desperately needed to better inform policy makers of the substance use might play in contributing to adult poverty. If chronic substance use lowers educational attainment and/or earnings, either directly or indirectly, then programs that prevent or delay substance abuse during adolescence may be an effective way of raising future income and deterring some from becoming economically dependent on the system or others. Even if chronic substance use is just an indication of some other third factor that is really driving the correlation between substance use and future labor market outcomes, prevention programs may still be an effective way of reducing poverty - not because they stop substance abuse, but because they teach valuable life skills, resistance training and coping mechanisms that help empower youth to make better life choices. Moreover, because so many of the prevention programs are relatively cheap to implement on a per student basis, they could prove to be an extremely cost-effective strategy for reducing poverty later.

We strongly support additional research on the long-term effects of prevention programs and a more seriously look at the direct effect of participation in these prevention programs on economic variables, such as educational attainment and early job entry. These programs are relatively inexpensive and some do show promising results in the very short run. If we had to design the next round of experiments to improve our understanding of prevention programs, we would focus on the following:

- Comprehensive, evidence-based school programs that begin with middle-school students and provide sessions through high school. While many prevention programs do have booster sessions after 7th or 8th grade, the typical age for initiation into harder drug use does not come until after high school. One could imagine randomly assigning the grade when school-based prevention stops so we could get a better understanding of whether the timing of booster sessions matter.
- Additional long-term follow-up studies (through senior year of high school) of treatment and control groups for the "best practice" programs, with a special focus on human capital accumulation. This would allow us to determine whether program participation shows any real effect on school performance and health by the end of high school, not just substance use. Additionally, this would allow us to better understand the extent to which there is immediate decay or slow decay of program effects for youth impacted by the prevention program and whether additional boosters could prolong program effects.
- A large-scale replication of the Big Brothers Big Sisters community-based mentoring experiment, with a special focus on long-term human capital outcomes, to more carefully evaluate the effect of this program on substance use and economic well-being.

- An experimental evaluation of school-based drug testing that is coupled with a curriculum based prevention program, paying close attention to alcohol consumption, attendance, and the probability of dropping out of school. Special attention should also be given to the consequences associated with testing positive. Indeed, if the expected sanction for testing positive is small, we would expect the intervention to have very small effects, if any.

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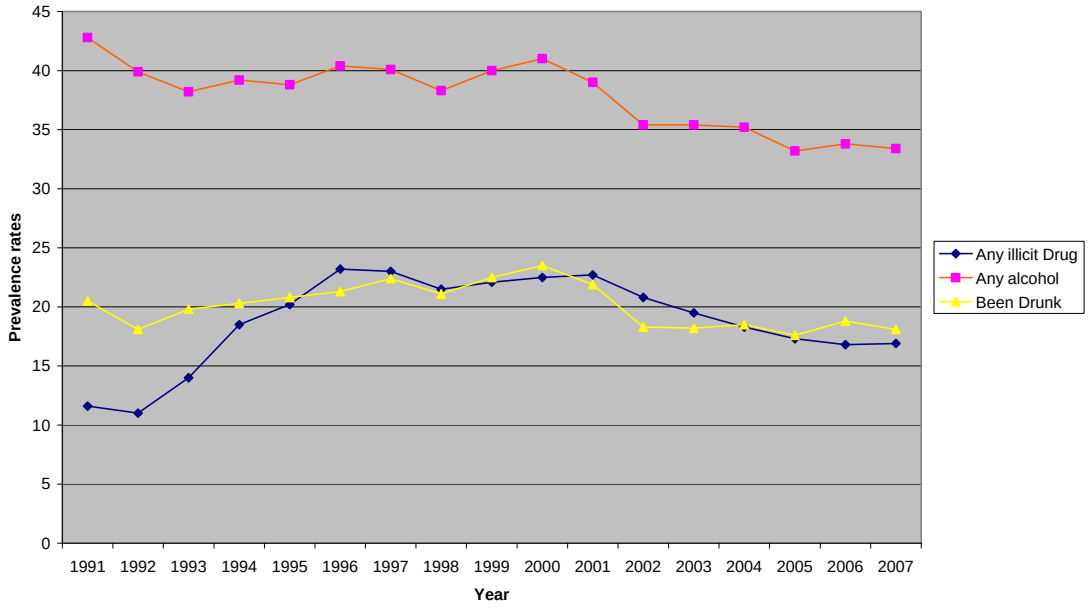
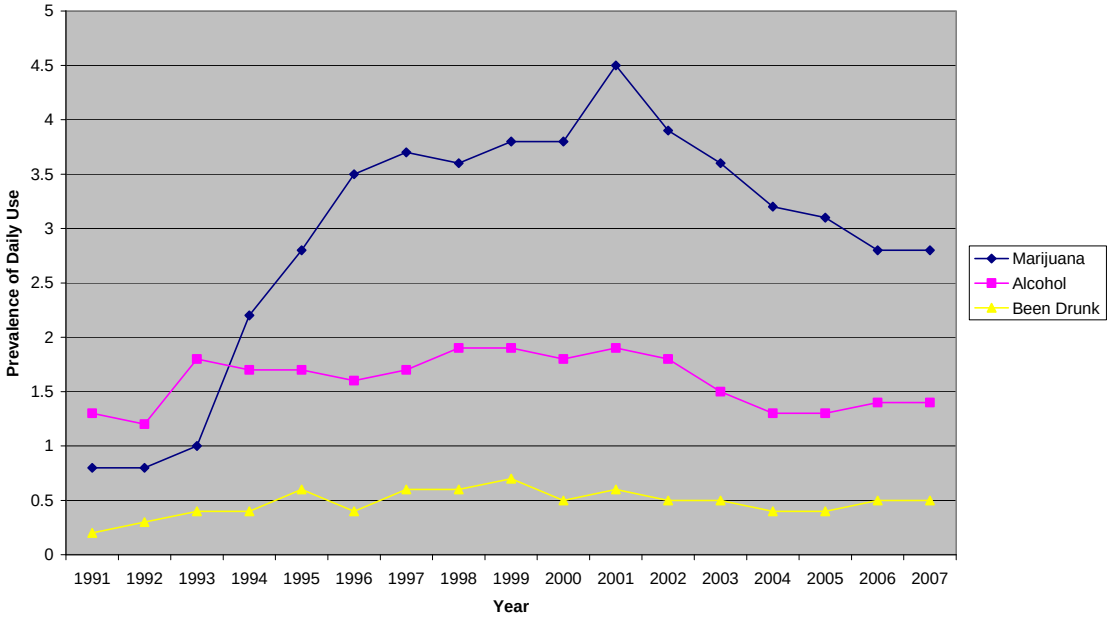
Figure 1
Thirty Day Prevalence of Substance Use Among 10th Graders
Source: Monitoring the Future



Source: Monitoring the Future (2008).

Figure 2
Rates in Daily Use of Substances Among 10th Graders
Source: Monitoring the Future



Source: Monitoring the Future (2008)

Table 1.
Long-term results of experimental evaluations of school-based prevention programs in the U.S.

Study	Program Description*	Evaluation Design & Sample	Alcohol Use	Tobacco Use	Illegal Drug Use
Adolescent Alcohol Prevention Trial (Taylor et al., 2000; Hansen and Graham, 1991)	10 sessions in 7th grade. Comprehensive social influences	Students in 12 schools in Los Angeles were randomly assigned to one of four prevention conditions (by school). Five waves of longitudinal data were obtained for 33.5% of the 3,027 students (through 11th grade). Authors use structural equation modeling to address data missing from Waves 2-5.	"Students receiving the normative education program had significantly lower average levels of reported cigarette and alcohol use, lower rates of growth for reported cigarette and alcohol use, and less deceleration of reported levels of cigarette and alcohol use as compared with the control group (information about consequences of use)."		Analyses based on five waves of data did not discuss illegal drug use. At 1 year follow up, normative education group demonstrated lower rates of recent marijuana use (2.2% versus 6.2%; $p<0.001$)
D.A.R.E. (Perry et al., 2003)	10 sessions in 7th grade. Resistance education is taught by a police officer in the classroom	24 middle schools in Minnesota were randomly assigned to D.A.R.E, D.A.R.E. Plus, or a delayed program. There were 6,237 students at baseline and 84% were surveyed at the one-year follow up. Used growth curve models to account for missing data.	No significant differences in outcomes between students assigned to DARE and students assigned to the control condition.		
D.A.R.E. Plus (Perry et al., 2003)	D.A.R.E., plus 4 sessions of a peer-led program, extra-curricular activities, and neighborhood action teams		"Among boys, those in the D.A.R.E. Plus schools were less likely than those in the control schools to show increases in alcohol use behavior and intentions, past year or past month alcohol use, tobacco use behavior and intentions, multidrug use behavior and intentions, and victimization" ($p<0.05$). There were no significant effects for girls.		

Hutchinson Smoking Prevention Program (Peterson, 2000)	65 sessions between 4th and 10th grades. Comprehensive social influences	40 school districts in Washington were randomly assigned to intervention or control condition. The study started with 8,388 3rd grade students and they were followed to two years after high school (94% follow-up)		"No significant difference in prevalence of daily smoking was found between students in the control and experimental districts…Moreover, no intervention impact was observed for other smoking outcomes, such as extent of current smoking or cumulative amount smoked, or in *a priori* specified variables, such as family risk for smoking."	
Keeping it R.E.A.L. (Hecht et al., 2003)	10 sessions in middle school (With booster activities and advertising). Resistance and life skills	35 school districts were randomly assigned to intervention or control condition (n=6,035 respondents; used multiple imputation to address attrition and missing values). Final wave of interviews was conducted 14 months post-intervention	Mean difference in past month use between intervention and control after accounting for Baseline level: -0.232 ($p<0.001$)	Mean difference in past month use between intervention and control after accounting for Baseline level: No significant difference	Mean difference in past marijuana month use between intervention and control after accounting for Baseline level: -0.175 ($p<0.001$)
Know Your Body (Walter et al., 1989)	2 hours of instruction each week during the school year from 4^{th} to 9^{th} grade. Normative and stress management	15 schools in the vicinity of New York City were assigned to either an intervention or a nonintervention group (n=1,105 eligible children, 911 participated at baseline, and 593 were interviewed at 6 years)		13.1% of control group had initiated use in 9th grade compared to 3.1% of those assigned to the intervention ($P<0.005$)	
Life Skills Program (Botvin et al., 1995)	15 sessions in 7th grade (Boosters in 8th & 9th). Cognitive behavioral resistance skills	56 schools were randomly assigned to intervention or control. 3,597 twelfth grade students represented 60.61% of the initial seventh-grade sample.	The intervention did not influence frequency of use at the 6 year follow-up, but it did reduce the probability of being drunk (40% vs. 34, $p<0.05$; vs. 0.33 $p<0.01$)	The intervention reduced past-week smoking (33% vs. 27, $p<0.05$; vs. 0.26 $p<0.01$) and past-month smoking (27% vs. 23, $p<0.05$; vs. 0.21 $p<0.05$)	Had no effect on marijuana use at the 6 year follow-up.

Life Skills Program (Botvin et al., 2001)	15 sessions in 7th grade (Boosters in 8th). Cognitive behavioral resistance skills	29 New York City schools were randomized to receive the intervention or be in the control group. 5,222 students (predominantly minority) participated in the study and 69% provided data at the 1 year follow-up	The authors measure substance use on a variety of scales, with 1="Never" or "I don't drink" and a maximum ranging from 6-11, depending on the measure. The mean scores for treatment and control groups both hovered between 1 and 2. The scores for the control group were marginally higher and statistically significant for the following measures: Smoking frequency*, smoking quantity**, drinking frequency**, drunkenness frequency*, drinking quantity**, inhalant frequency*. p<=0.05*; p<=0.01**.There was no statistically significant difference for marijuana frequency or getting "high" frequency.		
Project ALERT (Ellickson et al., 1993)	8 sessions in 7th grade (Boosters in 8th grade). Social influence model - resistance skills training curriculum	30 schools in California and Oregon were randomly assigned to three conditions: ALERT taught by health educators, ALERT taught by health educators with help from students, and a control. ~4000 students were assessed in grade 7 and six times thereafter through grade 12. The analysis sample at grade 12 constitutes 57% of baseline sample.	At the 6-year follow-up: "One the lessons stopped, the program's effects on drug use stopped. Effects on cognitive risk factors persisted for a long time (many through grade 10), but were not sufficient to produce corresponding reductions in use" (856)		
Project ALERT Plus (Ellickson et al., 2003)	11 sessions in 7th grade (Boosters in 8th grade). Social influence model - resistance skills training curriculum	55 middle schools in North Dakota were randomly assigned to ALERT, ALERT Plus (with high school booster sessions), or control. Of the 5,412 students enrolled in these schools. 4,689 completed baseline survey and 4,276 completed follow-up at 18 months after baseline	ALERTPlus did not influence alcohol initiation or current use, but it did lead to lower alcohol misuse scores (p<0.05) and less likely to engage in drinking that resulted in negative consequences (p<0.04)	ALERT Plus reduced cigarette initiation by 19% (p<0.01) and past month smoking by 23% (p<0.01)	ALERT Plus reduced marijuana initiation by 24% (p<0.01)
Project SHOUT (Elder 1993; Eckhardt et al., 1997)	18 sessions in 7th & 8th grade. (Boosters in 9th grade and 11th grade). Comprehensive social influence	22 schools in San Diego were randomly assigned to intervention or control condition. There were 3,655 participants and 2,688 (73%) were available to surveyed at the end of 9th grade.		"At the end of the third year, the prevalence of tobacco use within the past month was 14.2% among the intervention students and 22.2% among the controls . . ." p<0.001	

Project Toward No Tobacco Use (Dent et al., 1995)	10 sessions in 7th grade. (1 booster 8th) Comprehensive social influence	48 schools were randomly assigned to the intervention or control group (N=6,716). 52% of the sample was interviewed at the 24-month follow-up.		Weekly cigarette use increased 9% for controls and 5% for those assigned to intervention ($p<0.05$). Trial cigarette use increased 23% for controls and 16% for intervention ($p<0.05$).	
Project Towards No Drug Use (Sun et al., 2005)	12 sessions in one year of high school. Health motivation, social skills & decision making curriculum	21 schools were randomly assigned to control, classroom only, or classroom + (SAC). Of 1578 baseline subjects, 4-5 year follow up data were available for 46%.	"[S]ignificiant reductions were not found for 30-day use of cigarettes, alcohol, or marijuana use" (191).		Adjusted mean levels of 30 day hard drug use at 4/5 year follow up: Control (1.51%) Class (0.66%), SAC (0.3%) $p=0.02$ The authors note that this effect was evident at 1 year follow up, but not at the 2/3 year follow up.
Start Taking Alcohol Risks Seriously (Werch et al., 2003)	1 session in 6th grade and 1 in 7th grade. Materials sent to home. Second session is with nurse.	650 sixth-grade students were randomly assigned to the intervention or a minimal intervention control (a booklet to read at school)	At the 1-year follow up: "While mean alcohol consumption on all four measures of use was lower for neighborhood students receiving the intervention as compared to the control condition, these differences were not significant."		

*Heavily based on Table 4 in Skara and Sussman (2003).

Table 2.
Composite Estimate of the Two/Three Effects of a Composite, Hypothetical "Best Practice" Prevention Program

Substance	Measure of Use	Low Estimate	Middle Estimate	High Estimate
Marijuana	Lifetime prevalence	-4.9%	-10.9%	-14.0%
Tobacco	Lifetime prevalence	-4.3%	-16.8%	-21.5%
Alcohol	Past-month prevalence	-1.7%	-12.8%	-30.8%

Source: Reproduced from Caulkins et al. (2002, p. 64)

Table 3: Literature on the Effects of Substance Use and Abuse on Employment, Earnings and Educational Attainment

Study	Data	Outcome	SA Measure	Methods	SA and other intermediate factors controlled for?	Key Insight from Study
EMPLOYMENT AND EARNINGS						
Register & Williams (1992)	Young (ages 18-26) male workers in the 1984 NLSY	Log annual earnings, hours worked, and probability of employment	Continuous measures of the number of times cocaine and marijauna are used in the past 30 days. Also include long term use, defined as use 8 (5) years for marijuana (cocaine).	Two stage least square. Identifying instruments come from within the survey (e.g. religiousity) and may be problematic.	Education, marital status, and health are included as additional controls. Education and marital status are negatively associated with substance use in the first stage.	Cocaine use has no impact on wages or employment. Long term and on the job use of MJ are negatively related to wages, but off-the-job use is positively associated with wages.
Kaestner (1994a)	1984 and 1988 waves of the NLSY79 (sample was 23-32 years old in 1988). Models are estimated separately for men and women.	Log annual earnings	Measures of lifetime and past 30 day frequency of use of cocaine and marijuana. Heavy use measures based on lifetime frequency of use.	Both cross-sectional and panel (fixed effects) estimates are obtained using two stage least square estimation. Identying variables include frequency of religious attendance and non-earned income.	Education, experience, health status, and marital status are all included.	There find no statistically significant association between marijuana and log wages in the panel data analysis for males or females. For cocaine, the panel data models show a positive effect of lifetime frequency of use on log wages for females only (significant at 10% level). Women who never use cocaine have significantly higher wages in panel analysis.
Kaestner (1994b)	1984 and 1988 waves of the NLSY79 (sample was 23-32 years old in 1988). Models are estimated separately for men and women.	Usual number of hours worked in the past 12 months	Measures of lifetime frequency of use of cocaine and marijuana were examined. Heavy use measures based on lifetime frequency of use were also constructed.	Cross-sectional and panel estimates obtained using two stage least squares. Identifying variables include frequency of religious attendance and number of illegal acts committed prior to 1980.	Education, experience, and health status are all included. Panel data models are estimated separately by marital status.	The parameter estimates of the effect of illicit drug use were imprecisely estimated and had different signs depending on measure of drug use and the sample evaluated.
Kenkel and Ribar (1994)	NLSY 1979 Cohort. Separate analyses were done for men and women	Earnings, labor market hours	Four alternative measures of problem drinking, including DSM-III criteria for abuse, DSM-III criteria for dependence, binge drinking in the past 30 days, and number of days they had a drink.	OLS baseline estimates, individual and sibling fixed effects models, and IV models	Marital status, schooling, AFQT IQ test, and health problem indicators that could interfere with work	When individual fixed effects models are estimated, they find that problem alcohol use is associatd with a 1.3% increase in earnings for men and a 1.5% increase for women. When IV methods are used, they find large but statistically insignificant effects of problem drinking on income for males, but large positive and statistically significant effects of problem drinking on females' labor supply.
Mullahy and Sindelar (1996)	Alcohol Supplement of the 1988 National Health Interview Survey, individuals between ages 25-59 years of age. Models for men and women estimated separately	Employed, unemployed, or out of the labor force in the past two weeks before the survey	Alcohol dependence (DSM-III-R criteria), an indicator of total ethanol consumed, and heavy drinking (90th and 95th percentile for gender-specific distirbution).	IV methods. Instruments include state excise tax on beer, state ethanol consumption, and state excise tax on cigarettes	Schooling, health and marital status are all included as additional controls.	IV results for males do not support the conclusion that heavy drinking reduces employment or unemployment relative to being OLF. Findings for women is that problem drinking increases both probability of being employed and unemployed (hence lowers likelihood of being OLF). Net effect is ambiguous.
Zarkin et al (1998)	1991 and 1992 cross sections of the NSDUH, focusing exclusively on young males between the ages of 18 and 24.	Self-reported hours worked at all jobs in the past month.	Alcohol, cigarette, marijuana, cocaine and other drug use in the past month and in one's lifetime. Past month Alc and MJ use controlled for quantity and frequency using categorical variables	OLS IV estimation. Instruments included self-reported health risk and difficulty obtaining each substance. Only past month use was instrumented out.	Education, marital status, nonlabor income, number of children and self-reported health status were all included as additional controls.	In general they find no consistent relationship between drug use and hours worked. However, light marijuana use (1-3 joints in the last month) is positively and statistically associated with hours worked in the 1991 data and negatively and statistically associated with hours worked in the 1992 data. Specification tests support the 1992 results, but the inconsistency in results in adjacent cross sections of the

Table 3: Literature on the Effects of Substance Use and Abuse on Employment, Earnings and Educational Attainment (continued)

Study	Data	Outcome	SA Measure	Methods	SA and other intermediate factors controlled for?	Key Insight from Study
EDUCATIONAL ATTAINMENT						
Cook and Moore (1993)	Various waves of the NLSY79 data	High school completion, college entry and college completion	heavy and binge drinking in high school	IV and reduced form methods. Instruments include state variation in beer taxes and minimum legal purchase ages for alcohol.	No	Heavy drinking during senior year of high school reduces likelihood of finishing high school and graduating college.
Dee and Evans (1993)	NELS:88, repeated cross sections of the 1977-1992 Monitoring the Future Surveys & 1990 Public-Use Microdata Sample	Educational attainment	Use of alcohol in the past 30 days, binge drinking in the past 2 weeks.	IV methods and reduced form models of the effect of beer tax and minimum legal drinking ages on educational attainment	No	Reduced form estimates based on PUMS data show that teen exposure to MLDA of 18 had a small but statistically insignificant effect on high school completion, college entrance and college completion.
Bray et al (2000)	Longitudinal survey of 1392 adolescents ages 16-18 in a southeastern school system.	Dropped out of high school between the ages of 16 and 18 (evaluated for each age)	Initiation of each of four substances prior to age 16, 17 and 18. Substances are alcohol, cigarettes, marijuana and other illicit drugs.	Logistic models of the probability of dropping out of school. Specification tests couldn't reject exogeneity of age of first use variables, but may be due to weak instruments.	One of the first economic studies to fully consider polysubstance use.	Marijuana initiation is positively related to dropping out of high school, although the magnitude and significance of this relationship varies with age of dropout and other substances used.
Chatterji P (2006)	2000 follow up of the NELS:88. Models estimated separately for men and women.	Four measures of educational attainment by age 26: High school completion, GED, some college, finished college.	Alcohol use in the past 30 days in the 10th and 12th grade, as well as an indicator of binge drinking in the past 2 weeks from both waves.	Bivariate probit techniques. IVs include state beer taxes and percentage of the state population living in a dry county.	Prior cigarette use in 8th grade.	Results from the bivariate probit models relying on her IVs are unreliable because of the poor performance of her IVs. Attempts to bound the causal association using Altonji et al (2001) method provide no evidence of a causal interpretation.
McCaffrey et al (2008)	Project ALERT Plus sample of middle school students in South Dakota	High school drop out status (as reported by school administrator as well as self-reports)	Persistent and heavy marijuana use in the past 30 days in both 9th and 10th grade. Quantity-frequency index of alcohol use in the past month and year.	Propensity score weighting adjusts sample for baseline differences in 7th grade as well as differential participation in the Project ALERT curriculum.	Cigarette use at baseline (7th) and in each wave during high school is considered.	Even after adjusting for propensity weights, marijuana users are 2.3 times more likely to drop out. Statistical significance disappears when measures of cigarette use, family bonds, or peer effects are included.